PUBLIC HEARING

STATE WATER RESOURCES CONTROL BOARD

DIVISION OF WATER RIGHTS

STATE OF CALIFORNIA

---o0o---

SUBJECT: AMENDMENT OF CITY OF LOS ANGELES' WATER RIGHT LICENSES FOR DIVERSION OF WATER FROM STREAMS THAT ARE TRIBUTARY TO MONO LAKE

---o0o---

Held in

Resources Building

Sacramento, California

Tuesday, November 9, 1993

VOLUME X

---o0o---

Reported by: Kelsey Davenport Anglin, RPR, CM, CSR No. 8553

BOARD MEMBERS

MARC del PIERO
JOHN CAFFREY
JAMES STUBCHAER
JOHN W. BROWN
MARY JANE FORSTER

STAFF MEMBERS

DAN FRINK, Counsel
JAMES CANADAY, Environmental Specialist
STEVE HERRERA, Environmental Specialist
RICHARD SATKOWSKI, Engineer
HUGH SMITH, Engineer

COUNSEL AND OTHERS

For the U.S. Fish and Wildlife Service:

ERIKA NIEBAUER
Assistant Regular Solicitor
Office of Solicitor
Pacific Southwest Region
2800 Cottage Way
Sacramento, California 95825

For the Sierra Club:

LARRY SILVER:

For California Department of Fish and Game:
Ô

VIRGINIA CAHILL
McDonough, Holland & Allen
555 Capitol Mall, Suite 950
Sacramento, California 95814

For the U.S. Forest Service:

JACK GIPSMAN
Office of General Counsel
U.S. Department of Agriculture

For the National Audubon Society and Mono Lake Committee:

BRUCE DODGE
PATRICK FLINN
Attorneys at Law
755 Page Mill Road
Palo Alto, California 94304

For California Trout:

RICHARD ROOS-COLLINS
CYNTHIA KOEHLER
Attorneys at Law
114 Sansome Street, Suite 1200
San Francisco, California 94104

COUNSEL AND OTHERS

For the City of LA and LA DWP:

THOMAS W. BIRMINGHAM
JANET GOLDSMITH
Attorneys at Law
Kronick, Moskovitz, Tiedemann & Girard

400 Capitol Mall, 27th Floor
Sacramento, California 95814

For State Lands Commission, Department of Parks and Recreation:

JOHN STEVENS
Assistant Attorney General
1515 K Street
Sacramento, California 95814

For Meter Water District of Southern California and LA MWD:

VICTOR GLEASON
Attorney at Law
1111 Sunset Boulevard
Los Angeles, California 90050-0153

FRANK HASELTON
Haselton Associates

JOHN ARCULARIUS
MARY SCOONOVER

Ô

I N D E X

DAVID F. HANSON

ô €

SACRAMENTO, CALIFORNIA

Tuesday, November 9, 1993, 9:00 a.m.

---o0o---

HEARING OFFICER del PIERO: Ladies and Gentlemen, this hearing will come to order, please. For those of you that have not been here before, this is a continuation of the hearing regarding the Amendment to the City of Los Angeles' Water Rights Licenses for the diversion of water from streams that are tributary to Mono Lake.

My name is Marc del Piero. I'm Vice-Chairman of the State Water Resources Control Board. I've also been acting in the capacity of Hearing Officer in this matter. With me today is my good friend and colleague Mr. John Brown who is also a member of the State Water Resources Control Board.

When last we left yesterday, we had concluded presentation from witnesses on the behalf of the L.A. Department of Water and Power. They will be actually coming back for both redirect and recross at a subsequent date.

Mr. Birmingham, I understand you have two witnesses that are available only today that are here today; is that true?

MR. BIRMINGHAM: That is correct, Mr. del Piero.

HEARING OFFICER del PIERO: Will it be you or Ms. Goldsmith introducing them?

MR. BIRMINGHAM: Ms. Goldsmith.

HEARING OFFICER del PIERO: Good morning, Ms. Goldsmith.

MS. GOLDSMITH: Good morning, Mr. del Piero.

MR. FRINK: Mr. del Piero --

HEARING OFFICER del PIERO: I'm going to swear them in a second. Just relax.

MS. GOLDSMITH: That was my first comment, too.

HEARING OFFICER del PIERO: Why don't you go ahead?

MR. FRINK: I just wanted to kind of exhort everyone to move it along today. I did a rough count. We've got 138 witnesses to go. Some of those may have dropped out. But if we go at the rate of one witness per day, which seemed to be what we were approaching yesterday, and 15 hearing days per month, we'll still be here August 1 beginning the rebuttal portion of the hearing. That could go for a couple of months. I don't think that would please Judge Finney, anybody in the room, certainly not Staff or the Board.

A couple of suggestions, maybe, on ways to speed things up, and I talked to Ms. Goldsmith about this and she was in agreement, to make more of an effort to make witnesses available for cross-examination as a

panel. In some ways, it's more awkward, but usually you ask a question once instead of asking it repeatedly to several witnesses before you hit the person who's most qualified to answer.

Secondly, sometimes our cross-examination seems to focus on trivial details instead of the major issues, and if we could make more of an effort to focus on the

key issues.
And finally, I think we've all got to recognize that there are qualified experts that are going to disagree no matter how long we cross-examine them, but in any event, this is just kind of my plea to everyone to try and move it along so we're not still here a year from now.
HEARING OFFICER del PIERO: Ms. Goldsmith, you want to introduce your witnesses?
MS. GOLDSMITH: Yes. Before I do, I'd like to make a slight amendment of Mr. Frink's comments about my assent. I certainly agree that redundant cross-examination is occurring and perhaps can be avoided. I do think that it's important to allow parties the flexibility as to whether or not they present their witnesses as a panel or singly. I am presenting Dr. Kimmerer and Dr. Melack this morning as a panel in the interest of time because I feel that

they can be presented that way, but that's not the case with all our witnesses and probably not with the witnesses of other parties.
This morning I'd like to introduce and call as our witnesses Dr. John Melack and Dr. Wim Kimmerer.
Dr. Kimmerer -- neither of them, I believe, has yet been sworn.
HEARING OFFICER del PIERO: Is there anyone else -- I guess we're going to have just you two gentlemen today for the bulk of the cross-examination, so why don't you stand and if you'd answer in the affirmative, do you promise to tell the truth during the course of this proceeding?
THE WITNESSES: I do.
HEARING OFFICER del PIERO: Thank you very much. Please be seated, and I'll let you begin.
DIRECT EXAMINATION BY MS. GOLDSMITH
Q Dr. Melack, would you state your name and spell it for the Reporter?
A BY DR. MELACK: John Michael Melack, J-O-H-N M-I-C-H-A-E-L M-E-L-A-C-K.
Q How are you employed, Dr. Melack?
A I'm a professor at the University of California Santa Barbara.
HEARING OFFICER del PIERO: Speak into the mike,

please.
MS. GOLDSMITH: Are these on?
HEARING OFFICER del PIERO: Yes. They're on if you talk into them.
Q BY MS. GOLDSMITH: L.A. DWP Exhibit 23, which I believe you have a copy of, is your curriculum vitae. Is that a true and correct statement of your qualifications, education, and experience?
A Yes, it is.
Q Could you briefly give the highlights of your experience and qualifications concerning studies of Mono Lake?
A Very briefly, I did my Ph.D. work in eastern Africa on saline lakes beginning in 1971, and then

began working at Mono Lake in 1978 and have continued there ever since. That's included, hence, publishing a variety of papers as well as serving on a number of advisory boards including the National Academy of Sciences Study.
Q L.A. DWP Exhibits No. 25 through 31 list a number of papers and publications. Are these papers and publications of which you have partial authorship?
A Yes.
Q And do they contain information and analyses that you relied on in forming your conclusions concerning

0011

Mono Lake?
A That's correct.
Q L.A. DWP 32 and 33, Exhibits 32 and 33, are papers that were authored by others. Are these papers on which you relied in forming your opinions and preparing your testimony?
A They were used to suggest certain kinds of data analysis that we used.
Q L.A. DWP Exhibit 22 purports to be your testimony. Is it true and correct?
A Yes, it is.
Q Do you have any additions or changes?
A No.
Q Could you briefly summarize your testimony for us?
A Could I get a sense of timing here in where are we in terms of --
Q 20 minutes.
HEARING OFFICER del PIERO: We're here until we're done.
MS. GOLDSMITH: He's concerned about the 20 minutes.
DR. MELACK: I was told I should try to keep my summary to 20 minutes.
HEARING OFFICER del PIERO: You should try to keep your summary to 20 minutes inasmuch as one of the -- I

Ô

0012

always like saying this because it always gets a laugh. One of the prerequisites for serving on this Board is you have to be able to read, so since you've presented your documentation and presentation in writing in advance, we've done that. And so you have 20 minutes to summarize it.
DR. MELACK: Terrific.
Okay. In many ways, the ecology of the open waters of Mono Lake is so well-known that you can use that information to come to, I think, very informed judgments about how the lake is currently functioning. In particular, over the last 14 years, we've conducted an intensive year-round sampling and analysis program which has provided us with a very great source of information. And based on that, then, we can evaluate the changes through time in the populations of the algae and brine shrimp and can, I think, come to a fairly reasonable conclusion that the lake is certainly what we judge as a healthy ecosystem, and I'll describe a little bit later some of those data and the basis for saying it's healthy. But I think it's clear that not

only my evaluations, but the National Committee deliberations also concurred that Mono Lake, by any standard of an ecologist, would be judged as a functioning, healthy ecosystem as it's been in

0013

existence over the last 14 years which, for the record, spans lake elevations from 6372 to 6381.

I'd like to make two other introductory summary comments regarding this deliberation and then proceed to present some information I think that would support my claims about the lake being a healthy ecosystem. The first is when one tries to evaluate changes, ecological changes, one has the establish some kind of criteria, and that's always a difficult question and, it often becomes somewhat arbitrary.

In the case of the Draft EIR done for Mono Lake, the criteria of 25 percent change from some reference level was used for the open-water community. And I would contend that this, although perhaps reasonable as an arbitrary decision, doesn't hold up to the realities of what the lake actually experiences. And if you look at the real data, you'll find that that's a much too narrow bound of criteria and that, in fact, one should have extended that bound quite a lot further and, in fact, used even a different criteria which is the criteria of long-term trends, not simply a variation around the mean.

The second point is that when you look at a system like Mono Lake, you have to think of it as an ecosystem. In other words, as a functioning collection

0014

of organisms in a particular environmental setting and, therefore, you can't simply look at one single factor, for example, salinity, which tends to be done. You can't say the salinity is "X" and, therefore, the lake is in good or bad shape. You have to, in fact, evaluate the populations in the context of the other species and their physical and chemical environment. If do you that, in fact, what you find is that in the case of the key organism in the open water, that is the brine shrimp, Artemia monica, that salinity by itself does cause negative effects on the population as salinity increases, but these effects are mitigated and modulated by effects on food supply and other factors.

So I think it's important that we keep in mind that we're really looking at a complicated ecosystem, not simply a physiological experiment.

With that, in the way of introduction, I'd like to present a little bit more information about the lake as it currently exists, and I'm going to restrict my attention to the open water part of the lake; that is, the lake where animals and plants are free floating or free swimming in contrast to those which are attached to the bottom near shore.

I'd like to bring your attention to the first figure which is designed to show two things. One is to

Ô

0015

remind us that the ecology of Mono Lake is driven by only a very few species, as is very typical of

high-saline bodies of water. If you look all over the world, you'll find saline lakes, and many of these are like Mono Lake, highly productive and contain very few species. In Mono Lake's case, there's really only one major species of animal living in the waters itself off the shore, the Artemia monica population. This animal is then -- uses for its food free floating algae or phytoplankton.

The resource which tends to limit the growth of the algae and, in turn, the production of the shrimp is nitrogen, and we find that, in particular, ammonium is an important form of nitrogen which occurs in the lake and is produced by a decomposition of organic matter in the sediments and by the excretion of the brine shrimp. So when one thinks about the biological dynamics of Mono Lake, one has to take into account the fact that its resource base is depending on a supply of nitrogen and, of course, light, and those resources feed -- supply resources to phytoplankton or algae which are then, in turn, eaten by the brine shrimp. And we have a very tightly coupled system between the algae and the shrimp and the nutrients.

The next figure emphasizes the changes through

0016

time in any given year of the Artemia population. I think it's important to remember that we're looking at changes now. We're thinking about change, and we're thinking about change that occurs every year and we're trying to see long-term changes that might be caused by human affects rather than natural causes.

What I'm showing you here is a typical annual cycle of the Artemia's life history, which is that the shrimp, every year, hatch from small resistant eggs called cysts, which reside in the bottom of the lake. During the period from January through May, these hatch. They develop into adults. These adults male and female produce young, live young, which, in turn, mature, and then both generations produce cysts. So every year the population goes from essentially zero to some large number, tens of thousands per square meter, and then declines to zero again. And every year the cysts and the sediments are the source that, in fact, becomes the basis for the next year's generation.

So, now, although these general patterns of cyst-catching Artemia growth and decline occur each year, there have been large differences from year to year that we've observed. We've also observed differences in the amount and production of the algae.

And what I'd like to now talk through a little bit

0017

is some of the reasons why we've preserved these variations in the amount of algae present, in their productivity, and in the amount of shrimp present. So I think it's important to bear in mind causes for variation when one tries to evaluate whether or not there's an ecological impact currently being imposed on the lake by changing water diversions.

The lake naturally, and I don't want to go into a limnological lecture here, but it's important to realize that lakes in the north temperate zone

typically are what's called stratified thermally. That is, they heat up in the surface waters during the summer and then mix during the winter period. And this annual cycle of mixing and stratification affects the supply of light and nutrients to the algae.

In the case of Mono Lake's history over the last 15 years, we have experienced that annual cycle. We've also experienced periods in which due to large flows of fresh water into the lake, the lake became chemically stratified and did not mix from top to bottom each year. The jargon used to describe that condition of partial mixing is meromictic, and it basically means the lake is no longer injecting these nutrients from deep water into the surface waters on an annual basis. And this led, then, to very dramatic changes in the

Ô

0018

abundance of algae and their productivity.

So the first major lesson that we've learned, then, from this 14-year data set is that adding water in large quantities such as occurred during the 1983 snow melt season, which was very large because of the very large El Nino in California as well as other parts of the world, resulted in a large input of fresh water which raised the lake level, which diluted the surface waters, which reduced the vertical mixing, which, in turn, led to a large decline in the abundance of algae and the productivity of the phytoplankton.

The second major point that has come out of these long-term records is that the Artemia populations change dramatically from year to year depending on the size of the first generation: that is, the generation which emerges from cyst hatching. In the years where we have small spring generations, we tend to have very large summer generations. In years where we have low spring generations, we tend to have moderate summer generations, and this is because the food supply of the shrimp is more or less depleted by the size of the first generation.

So again, we have evidence here of the importance of food supply affecting the brine shrimp population dynamics which comes back to my initial point that we

0019

have to evaluate the ecosystem of Mono Lake in terms of its interacting populations, their resource supply, and their temporal dynamics.

Now, to look more rigorously at these changes through time, I want to look at some time surveys data; that is, data that run through this 14-year record, and evaluate the kind of patterns that we observe in those records and think about these records in the context of how might you judge the ecological health, if you will, of Mono Lake, which I think is one of the issues here. How do we tell whether an ecosystem is healthy or not? And I would submit that there are various ways of doing that. Most are based on either the number of organisms present, number of species present, and their variations in time and space.

In the case of Mono Lake, we have very few species there, although they have sustained their populations

through time. So I think we should look instead at not the loss or gain of species, but instead the change in abundance through time.

And the next transparency, the next picture, shows this. It would be easier if I could actually walk up and point here. Can I take one of these microphones and walk up and do that?

HEARING OFFICER del PIERO: I don't know if it

0020

will -- just talk loud.

DR. MELACK: That one may reach. Is that permissible to do that? Thank you.

What I'm showing here is a record of changes in the abundance of aology measured with a unit called Chlorophyll-A, which is the amount of the photostatic pigment and, on the bottom panel here, the abundance of Artemia, that is the brine shrimp, in thousands of animals per square metered lake area. The dark blue lines show the actual measured data which were sampled every two weeks to every month over this period. The dashed line shows the 12-month moving average which smooths out some of these variations that occur in any given year.

The message of these data -- we look first at the algae. During the period of -- I'm sorry. This is Figure 10 in my direct testimony. During the period of meromixis or reduced vertical mixing, we see a very pronounced decline in the abundance of algae in the lake. This is now high lake levels, reduced vertical mixing, lower algal abundance. Prior to that, during lower lake levels, 6372 to 6375, we have higher chlorophyll levels and then, again, once we mix the lake and, again, have declining lake levels, the chlorophyll populations actually increase again. So

Ô

0021

there's a correlation here, if you will, between vertical mixing and amount of algae.

It's also, I think, very important to recognize that if you look at these data and you look at only a three-year record, you can find evidence of very marked declines and very marked increases in the abundance of the algae. I think this is a very strong warning to any management decision that you can't make a decision based on a short time series. You can be very easily misled. You have to look at at least five years, and preferably longer, to evaluate whether or not a system is changing.

If we now look at the abundance of the brine shrimp, again, we see large variations, as I mentioned earlier, from high numbers to almost zero each year, which is typical of the cycle or the life history. More importantly, though, if you look at the dashed line, what you find is that over this whole 14-year period, there's really no trend whatsoever. The populations just basically bound along more or less on an even keel which would, by my eyes, suggest that the lake is actually in very good shape, even in the face of large changes in algal abundance and primary production. So the shrimp, in fact, seem to be

modulated against changes in algal abundance which vary

0022

quite substantially due to this natural predation of a period of meromixis.

Now, an additional point I'd like to make is in terms of evaluating these data. You can not just look at time series, you can also look at various statistics of variance and variability, and there's a variety of these you can use. The easiest ones are ones that look at the natural range and compare that to the overall average, and if you do that, what you find is that for the phytoplankton, you compare the percent variation, long-term range of variation to the mean. The variation's about 180 percent over this period. It doesn't mean a whole lot to you, but it's maybe worth pointing out that this number is certainly much greater than a 25 percent variation which was used in the Draft EIR criteria.

It's also low by comparison to many other lakes. It's low by comparison to Lake Tahoe, for example, Lake Washington, Lake Huron. So even other large lakes have, in fact, more variability than does Mono Lake.

Secondly, if you look at the productivity; that is, the growth of the algae, the growth of the phytoplankton, and the growth of the brine shrimp, we again find that the range, the annual range in values compared to the mean extends from, in the case of the

0023

algae, about 200 percent to about 50 percent of the mean. And in the case of the brine shrimp, about 170 percent to about 70 percent. Again, we're looking at natural variability associated with largely changes in nutrient supply and climatic factors which are causing the lake to experience variations. And these variations must, then, be taken into account if one's actually going to evaluate the health of the system.

One last major area here is the use of models, mathematical models, as a way to judge how Mono Lake has been functioning. What I've been emphasizing to date in this summary is real data. Now, model results have also been used and, I think, sensibly in the EIR, although the use of models is a dangerous exercise unless the models are properly considered. And whenever you build a model, you have limitations and assumptions built into it, and one has to be very, very careful when one uses model results. I am of the opinion that perhaps some of the model results that were used in the Draft EIR were not properly considered. My own research group has developed two of these models, so I'm not criticizing others. I'm criticizing myself here, and it's a function of what models can do for us.

We built two models. One was a physical model

Ô

0024

based on vertical mixing on dynamics. The second was a plankton model based on analysis of the brine shrimp, the algae, the nutrients in the lake. And I'll emphasize here, then, the second of these, the plankton model, and point out that by combining a detailed

analysis of population dynamics with an analysis of algal growth and nitrogen cycling, we were able to -- excuse me, assess the way that not only salinity, but also food supply and nutrient supply affected the Artemia populations. And we found that, in fact, the long-term impacts of changing salinity were very much modulated by changes in food supply.

I would hasten to add, though, that use of these models beyond their validated range; that is, outside the range of known information is difficult and is made difficult because we don't know, in fact, how the lake may change in species composition or in overall ecological condition.

So I would be, I guess, interjecting a lot of caution in the use of model results as a basis for evaluating ecological impacts, especially when you have at your disposal a very long data set which is based on real observations.

HEARING OFFICER del PIERO: Doctor, your 20 minutes are up.

0025

DR. MELACK: Yeah. How's that?

HEARING OFFICER del PIERO: That's fine.

(Laughter.)

MS. GOLDSMITH: Are you done?

DR. MELACK: Yeah.

MS. GOLDSMITH: Can we put up the last exhibit?

DR. MELACK: Sure. You can put up the last exhibit which basically summarizes the information I've been talking about and indicates the changes to this period of lake level. The solid line and the vertical panels show the concentration of chlorophyll; that is, the algal abundance, and the little histograms there with the little animal pictures show the abundance of the Artemia. And, again, these emphasize the fact that the lake has experienced changes through time, but the Artemia itself has maintained, I would say, a healthy stable population during this period.

HEARING OFFICER del PIERO: Thank you.

Dr. Kimmerer, is he going make a presentation, also?

MS. GOLDSMITH: Yes, he is.

HEARING OFFICER del PIERO: Why don't you begin, Sir?

MS. GOLDSMITH: Can I ask him a couple of questions, please?

0026

HEARING OFFICER del PIERO: Sure. Certainly.

Q BY MS. GOLDSMITH: Would you give us your name and spell it for the Reporter, please?

A BY DR. KIMMERER: Yes. My name is William J. Kimmerer, K-I-M-M-E-R-E-R.

Q How are you employed, Dr. Kimmerer?

A I'm a scientist with the consulting firm of Biosystems Analysis, Incorporated.

Q Is L.A. DWP Exhibit 42 a true and correct copy of your curriculum vitae?

A Yes, it is.

Q Would you briefly summarize your experience and education for us?

A Yes. I have a bachelors degree in chemistry and a Ph.D. in biologic oceanography from the University of Hawaii and about 15 years of experience in oceanography and principally in modeling of lower trophic levels of ecosystems.
Q Is L.A. DWP Exhibit No. 41 a true and correct copy of your testimony here today?
A Yes, it is.
Q Are there any changes that you want to make to it?
A No.
Q Would you briefly summarize your testimony for us?
A Yes. And I'll do it in quite a bit less than 20

minutes. My testimony's fairly short.

First of all, my expertise in this area is not as a Mono Lake expert or as a brine fly expert, but as a modeler, and just a little bit further on what Dr. Melack said about modeling, I'd like to talk briefly about how a modeler thinks about these sorts of issues.

A model is simply a compendium of what we think we know about a system, and it can be a representation, either a picture representation or a word description, or, in many cases, a mathematical description or a computer representation of what we think the ecosystem is doing. And in the case of the alkali flies, what we have to do is we have to build models that are based on what we think we know and, actually we know relatively little about the brine flies compared to what we know about the brine shrimp.

Dr. Melack said that in the case of the brine shrimp, there's quite a bit known about their life history, their life cycle, and what limits their abundance in the population size. Unfortunately, we know considerably less than that about the alkali fly population. We know absolutely nothing about what happens to them as adults and, therefore, we -- we're not really in a position to write a model that

describes how the population changes over time.

What we do know is we know that -- or we think we know that substrate limits the number of alkali flies in the larval stages, and we think we know that there are salinity effects -- or is reasonably certain that there are salinity effects on the growth rate and the development of the alkali flies through their larval cycle. And that's really it.

So given that limitation, we're not justified in writing a model that purports to describe how the population will change over time and that, unfortunately, is what the Draft EIR contains is an attempt at a population model. It has a large number of assumptions, many of which are poorly stated or poorly backed up, but the main point is the model itself is totally inappropriate and unfit for the purpose at hand.

Now, going back to the -- to the effects of variation in lake level on the alkali fly population, there are basically two. One is that as lake elevation

drops from its current stand, or rises from its current stand, you'll see changes in the availability of hard substrate which we believe limits the size of the population of larval alkali flies. And therefore, we'd expect to see a proportional change in the population

0029

size, all else being equal. Of course, we don't know that. If we reduced the -- if we reduced the lake elevation from the alternative -- the preferred alternative in the Draft EIR, 6383.5, to the L.A. DWP management plan alternative, that would decrease the substrate by about 11 percent. At the same time, the growth rate of the alkali flies would decrease by about 2 percent, and so the overall production would decrease in proportion.

The population of alkali flies is rather large. As with the brine shrimp, it appears not to be in serious trouble. It appears to be a healthy population as far as anybody can tell, and so outside of any effects of alkali fly abundance or production on the availability of food to birds, my opinion is that there's relatively little effect on the alkali flies themselves other than just a change in numbers that would result from changes in lake elevation. That's it.

MR. FLINN: Madam Reporter, would you mark that part of the tape, please? We want to refer to that in cross-examination. Thank you very much.

THE REPORTER: Sure.

HEARING OFFICER del PIERO: Thank you very much.

Ms. Cahill? Or is it Mr. Thomas? Okay.

Ô

0030

CROSS-EXAMINATION BY MS. CAHILL

Q Hello, Dr. Melack, Dr. Kimmerer. I'm Virginia Cahill representing California Department of Fish and Game. I hope to complete my questioning in less than 20 minutes.

Dr. Melack, when you talk about a healthy lake, is that the same as an ecologically healthy lake in terms of functioning as it did in its pre-diversion state with its original natural biota?

A BY DR. MELACK: That -- the answer to your question is really a little difficult to give because there's essentially nothing known about the lake in its pre-diversion condition. My judgment is based on looking at many saline lakes around the world and seeing them experience very wide changes in salinity. Let me give you one example.

In east Africa we looked at lakes that had salinities as low as 10 to 20 grams per liter, up to 70 to 80 grams per liter. In this full range soda lakes, much like Mono Lake, the lakes did extremely well, sustained millions of flamingos, sustained large populations of phytoplankton and, indeed, they would be judged as healthy. And so my judgment's really based on analogy, which is to say that yes, indeed, I would say that Mono Lake is as healthy now as it was earlier.

0031

Q Let me focus on what we do know about Mono Lake

not by analogy to others. Was the species diversity in Mono Lake higher prior to diversion?
A Species diversity of the plankton community?
Q Whatever species you'd care to address. Was there a greater species diversity prior to diversion?
A The only evidence that we have of that species diversity in the lake itself were data gathered in the mid and early sixties by David Mason which showed that there were one or two species of rotipher present in the lake then, and they're not there now.
Q Are you aware of any other species that were present in the lake then that are not present now?
A Not in the open-water plankton, no.
Q Are you aware of any indication that the pre-diversion lake ecosystem was not functioning normally within a natural range of variations for hundreds, if not thousands, of years?
A If you'd look at lake history over time spans of hundreds and thousands of years, there's no lake I know of on the earth's surface which didn't undergo very large changes from dryness to large volumes of water, even lakes on the scale of Lake Tanganyika. So I think it's really a misnomer to ask the question the way you're asking it. It's not even appropriate to think

0032

in those terms.
Q But so far as you know, in, let's say, the early 1900s, the lake was functioning in a natural fashion?
Q I'm sorry, but you're putting -- you're not making scientific sense. You're talking fantasy.
Q Can your model accurately predict shrimp productivity under pre-diversion conditions?
A I don't believe so. And I say that for two reasons because to say a model can accurately predict something, a model must, in fact, be validated against conditions that one knows. One can't simply write a mathematical construct and extend it beyond the bounds of validation. Now, it's really impossible to validate a model for conditions that don't currently exist.
Q Why is it that your data set begins in -- is it 1979?
A It begins then because I was hired at the University of California in 1977, and I began my research there in 1978. And I initially developed some methodology and began a routine data collection sampling scheme in 1979.
Q And the lake levels in the period that you've studied have a variation of approximately what, ten feet?
A From 6372 to 6381.

Ô

0033

Q And since 1940, what has been the overall variation in the lake elevation?
A The lake has fallen dramatically. It's fallen 14 meters from 1941 to 1981, and then since then, it rose about three meters, and now its fallen again at about two.
Q And can you give that to me in feet, since I think your earlier answer was in feet?

A 14 meters is -- a little more than three times 14.
Q So the variation you've observed is approximately a fourth of the total variation that there's been. You did not observe the lake at three-quarters of its elevation in this century?
A No.
Q And let me ask you again, you're aware of only two species that were present prior to diversion that are not there now?
A Be very careful how you say that. I said that open-water plankton species. I didn't say species. Okay?
Q All right. Are you aware of species other than open-water plankton species that were present in the lake prior to diversion that are no longer present?
A I don't think I'm qualified to talk about bird species diversity which may change or may not have

0034

changed.
Q Aside from birds, you distinguished between open water and species that might be present, I understand, within the water along the shoreline. Are you familiar with any species in that category that used to be present that are not now?
A That's a little hazy because if you look -- there's a gradient along the shore between wetland and lake, and if you start including the swampy region along the shore, you look at fresh water species. Their dynamics haven't been very long examined. And I don't personally know very much about that, but I think certainly, the possibility exists for there to be other species present in fresh water habitats in Mono Lake that currently exist. But in terms of actual salt lake species of any consequence, I don't know any evidence for salt lake species being any different near shore now than earlier.
Q Okay. So with regard to salt -- how did you categorize them?
A Salt lake species.
Q Salt lake species, you know of only two that are no longer present that were present prior to diversion?
A With the caveat that I'm really thinking mostly about the offshore waters.

0035

Q Thank you.
Dr. Kimmerer, in your testimony, you make a reference to anecdotal evidence of high populations of alkali fly. To what were you referring?
A BY DR. KIMMERER: I was referring to records of large numbers of fly pupa on the surface that were so abundant that people could scoop them up.
Q Let me show you an article which is in -- has been submitted as Department of Fish and Game Exhibit 99. I know you have this, but it's probably easier if I -- have you seen this article before?
A No, I have not.
Q Would you take a moment to look on Page 9? There is a photograph, and if you would read beginning about halfway down the text to the side of that photograph where the sentence begins, "The dead and decaying

individuals." You might want to start with the sentence before for it to make sense.
A "Dead and decaying individuals, this is flies, cast ashore mixed with suds and larvae to form food for an army of small flies, the larvae of which I found alive in the water. These flies are so thick that they form a black zone or band two or three feet wide next to the water all around the lake. A belt of flies 100 miles long, as one writer puts it."

0036

Q And the photograph -- could you read the caption under the photograph?
A It says, "South beach of Mono Lake showing dark zone of live flies."
Q And is this consistent with the other anecdotal reports you are familiar with?
A I don't know that its consistent or inconsistent.
Q Do you have any reason to doubt the truth of this account or --
A No.
Q -- this photograph?
Mr. Piero, I don't know if it would be appropriate to move admission of DFG 99.
HEARING OFFICER del PIERO: You can move it now, or you can move it later on. It's up to you.
MS. GOLDSMITH: I'd like to move its admission at this point.
MS. GOLDSMITH: I would object to its admission at this point. I don't think there is a foundation for it, and I would prefer that you did it --
MS. GOLDSMITH: Okay. Thank you.
HEARING OFFICER del PIERO: Thank you very much.
Mr. Dodge -- oh, Mr. Flinn.
MR. FLINN: The low end of the food chain falls to me, Mr. del Piero.

0037

HEARING OFFICER del PIERO: Somehow I could have anticipated that, Mr. Flinn.
(Laughter.)
MR. FLINN: And while I am sorting out my notes, I would ask, John, if you could --
HEARING OFFICER del PIERO: Evolution continues. Take heart.
(Laughter.)
MR. FLINN: Several million years.
-- if you could set up the items there.
MR. DODGE: You'll see my food preference when I cross-examine on the California gull.
HEARING OFFICER del PIERO: Uh-huh.
CROSS-EXAMINATION BY MR. FLINN
Q My question -- I'm going to start, Dr. Melack, with you and go on. I want to start with your general questions about your written testimony here. You wrote this?
A BY DR. MELACK: I did.
Q And I take it you were personally responsible for its content, not DWP's lawyers?
A Yes. I was responsible.
Q And would it be fair to say that you exercised

your own best independent scientific judgment as to what to emphasize to the Water Board in this written

0038

testimony?
A That's correct.
Q And you understood this was an important public policy issue when, in your testimony, you wanted to highlight the things that you thought was most important for this Water Board to know?
A That's fair enough.
Q And, in fact, the Department of Water and Power, and ultimately, its ratepayers, have paid you a lot of money or your university a lot of money over the years to develop this?
A As long as you qualify the money was paid to students, not me.
Q Your salary is not supported at all by the grants that DWP gets you?
A Very little. Very, very little.
Q Now, you say, on Page 19 of your testimony, quote, my analysis is derived primarily from actual measurements of conditions in the offshore water of Mono Lake." The actual measurements you're referring to are those from 14 years of data that you collected from 1979 to 1992; is that right?
A Yes.
Q Okay. Now, we've got our own example of some of your testimony here. It's not as fancy. I'm going to

Ô

0039

walk over here and try and keep my voice up. If it falls, someone holler. It's not as fancy as the nice ones that the L.A. ratepayers have supplied us, but I'll try and make do.
This is from Figure 1 of your testimony. You do recognize that, don't you?
A Yeah.
MR. BIRMINGHAM: Excuse me, Mr. Flinn. Your statement that the L.A. ratepayers aren't supplying this. Does this mean you're not going to make an application under 1021 --
MR. FLINN: Not currently supplying.
HEARING OFFICER del PIERO: Gentlemen, let me point out the jurisdictional authority for that is somewhere else not in this room.
MR. BIRMINGHAM: May I ask the Reporter to mark that admission that we're not going to have that application?
HEARING OFFICER del PIERO: I'm sure there's a judge somewhere who would love to take that matter up with both of you, but not --
MR. DODGE: The authority relates -- I've given Mr. Flinn relates to Dr. Melack and Dr. Kimmerer, not Section 102125.
MR. FLINN: We can fight about the five dollars --

0040

HEARING OFFICER del PIERO: That's living proof, Mr. Birmingham, that the food chain continues with us, also. Mr. Dodge is interested in pursuing that.
Q BY MR. FLINN: Your written testimony, Dr. Melack,

describes four different periods that cover this record of lake elevations. You recall that from your written testimony?
A BY DR. MELACK: I do, yes.
Q On Page 7 you tell us that the -- we were monomictic up until 1982; is that right?
A That's correct.
Q So I'm going to draw a line here, the '82 border, and I'm going to shade blue on Figure 1 for the monomictic period. Okay? And then we were meromictic from '83 to '87. Is that right?
A Not exactly. The lake -- we made a distinction between the period of meromixis and the period in which there was gradual deeping and eventual turnover, so the lake didn't really turn over until the end of 1988. So there was a gray period in between, in between '87 and '88, that we call transition to holomixis.
Q Am I reading your testimony correctly where -- on Page 7, where you say, "The period can be divided into four time periods"?
A That's correct, yes.

Q Monomictic '64 to '82. Maramictic '83 to '87. Am I reading that right?
A That's correct, yes.
Q So if I were to draw a line on this graph following your written testimony --
A That's fine. Sure. Sure.
Q I can draw a line here at '87 and I make the meromictic period red there, and then you say, "From '88 to '89, we were in this transition period." Am I reading that right?
A That's correct.
Q I'll make that red, too, but I'll hash it the other way.
And then from 1989 to 1992, we are back to monomictic, right?
A Correct.
Q Okay. Now, you say in your direct testimony that this monomictic condition is rare. You recall that?
A No. I didn't say that. I said the meromictic condition is rare.
Q I misspoke, yes. The meromictic condition. The red condition is rare.
And your direct testimony also said that that has a negative effect on the food supply for the shrimp?
A What I said was that it reduces the algal
Ô

abundance in basic primary production. Whether or not that has a negative effect would be indicated by whether or not the shrimp are doing well or not doing well. And it turned out the shrimp didn't seem to respond dramatically to that change.
Q What do shrimp eat?
A The shrimp eat phytoplankton.
Q And what does the meromixis do to phytoplankton productivity?
A It reduces the abundance. But you cannot say that that has a negative effect unless you see evidence that

there is, in fact, a negative effect.
Q We'll get to the negative effect.
In your testimony you say that during this period from meromixis -- monomixis to meromixis transition, monomixis, that the population was, quote, remarkably stable." Do you recall that testimony in your written work?
A Yeah.
Q Okay. Now --
A Population of brine shrimp.
Q Yes. The population of brine shrimp.
Now, are you familiar -- would you flip down the -- no. Flip up the chart? No, no, no, John, the other chart. Flip that up. No. The other chart

here. I'll just do it.
Are you familiar with something called Auxiliary Report Number 13 submitted to Jones and Stokes?
A Give me the title.
Q I just know it as the Mono Basin Auxiliary Report Number 13, and it's got your name on it. It's a modeling analysis of Artemia dynamics of Mono Lake.
A Yes.
Q And you're the Melack that's referred to on this document?
A That's correct.
Q And that's not an accident, that was deliberately -- you're listed as a co-author of this article?
A That's correct.
Q Okay. On Pages 3 and 4 -- and I'm going read it aloud because it may not be visible to everybody else, you and your co-authors tell us, "Over the past two decades, Mono Lake has been the subject of extensive research making it one of the best-studied saline lakes in the world. A major component of this research has been done by University of California Santa Barbara researchers and consists of an extensive monitoring program conducted 1982 to 1992."
A couple of preliminary questions that UC Santa

Barbara, that's you and your team, right?
A Yeah.
Q Okay. And this 1982 to 1992, is that historical record that's in your direct testimony; is that right?
A Yeah. It's referring to a shorter period of that testimony.
Q "During this time, both the lake level and salinity changed," and then there's the part we put in bold. "Despite this extended data record, the direct observation of effects on salinity in the Artemia population is difficult and unlikely to be detected even if present. The past decade included a period of unusual climatological conditions at Mono Lake, changes in the physical mixing regime of Mono Lake associated with the onset, persistence, and breakdown of meromixis dramatically alter plankton dynamics and most likely obscured effects due to changes in salinity."
Was that language language that you approved in this document of which you were co-author?

A Yeah. I think it's actually appropriate.
Q Good. Given that salinity effects are obscured by the historical record on which you testified your analysis is primarily derived, let me ask you is it not correct that we have available to us other data from your group about -- on shrimp populations? Could you

0045

answer that? Strike that -- let me withdraw the question.

Do we have -- we have data from experiments that you conducted on the response of shrimp to salinity, don't we?

A Yes.
Q And I have blown up here Table Number -- Table -- Figure 2 from the EIR Auxiliary Report Number 12 from Dana, Robert, Jellison -- excuse me, Dana, Jellison, and Melack and, again, that's the -- you're the John Melack that's listed here?

A That's correct.
Q And this again was research that L.A. ratepayers paid for; is that right?

A Yes and no. I mean, it turns out that the bulk of that research was actually paid for by a grant from the Packard Foundation.

Q Okay. Now, am I reading these graphs correctly -- there are 12 of them by count, that on the horizontal axis we have salinity? Is that right? In every one of these?

A That's correct.
Q Okay. And 50 micrograms per cubic liter, that's the -- excuse me. 50 grams per liter, total dissolved solids. Is that the measure there?

0046

A That's correct.
Q Okay. And is approximately 50 correspond to the pre-diversion lake elevation, close numbers?

A Yes, it does.
Q And 100 is down close to the 6372 range, the low end of DWP -- close to the low end of DWP's management range?

A That's correct.
Q Now, I just want to see if I can read these and -- first of all, there's both some data points and some straight lines and curves. Those straight lines and curves were placed there by you and your team, weren't they?

A Yes.
Q And then again they're not an accident, they're deliberate additions to these charts?

A Yes.
Q And am I just, to sort of read these things, that this first one up in the upper left-hand corner percent non-hatching cysts as we go from 50 to 150 following your curve, we get a greater percentage of non-hatching cysts as salinity increases?

A You should be careful, though, because actually, although you're quite correct the axis starts at 50, the actual data points don't begin until about 70. So

0047

the actual record of real measurements goes from about 70 grams per liter up to, I think, it's about 160.
Q Okay. Now, is it a mistake that these curves go all the way to 50, or is that deliberate to extend those curves all the way to 50?
A When you fit a curve with that many points, you can certainly extend that curve down.
Q That wasn't my question. I didn't ask you whether you could extend it. My question was a much simpler one. Was that a mistake, or was it deliberate?
A It was deliberate.
Q Okay. And, again, following the deliberate curve for their second one, the mean day of hatch, am I reading this correctly that as you increase salinity, you get a longer duration for the hatch? Takes longer to hatch?
A Yes. Yes. Yes.
Q And can you pronounce that word N-A-U-P-L-I-A-R?
A Naupliar.
Q Naupliar survival decreases with salinity?
A With salinity.
Q With increased salinity?
A That's correct.
Q And as I go through each one of these, we again see basically negative effects on either reproduction,

on the size of the animals, on their adult survival with salinity changes?
A That's certainly correct, and I actually acknowledge that quite freely in my direct testimony and today that, in fact, you do find, if you do laboratory bioassays, you do, in fact, find very convincing evidence that increased salinity decreases the reproductive capability of the brine shrimp.
Q Okay. Now, having agreed with the conclusion that any salinity effect was obscured by meromixis, you deliberately chose not to put these graphs in your testimony that you wanted to emphasize to the Water Board; is that right?
A That's a clear distortion of the evidence. Okay? Let me explain why.
Q My question wasn't whether that was a distortion of the evidence. Again, I'm trying to --
MS. GOLDSMITH: Mr. del Piero --
MR. FLINN: I'd like to finish my question.
HEARING OFFICER del PIERO: Wait. Wait. Wait. Dr. Melack, when I talk, everybody's quiet.
DR. MELACK: I'm sorry.
HEARING OFFICER del PIERO: You wish to object?
MS. GOLDSMITH: I do.
HEARING OFFICER del PIERO: On what grounds?

MS. GOLDSMITH: I believe Dr. Melack should be allowed to finish his answer.
HEARING OFFICER del PIERO: Actually, I'll allow Dr. Melack to complete his answer when he responds to the question. Okay?
So if you want to respond to the question asked,

Dr. Melack, either affirmatively or negatively, I'll be happy to allow you to expand on your response.
MS. GOLDSMITH: Mr. del Piero, I believe he did respond and perhaps the Reporter could read back --
HEARING OFFICER del PIERO: I'll be happy to have the Reporter read it back.
Mrs. Anglin, please read back the last question Mr. Flinn asked.
THE REPORTER: Sure.
(Whereupon the record was read as requested.)
HEARING OFFICER del PIERO: Is there a response? Ms. Anglin, is there a response after that question?
(Whereupon the record was read as requested.)
MS. GOLDSMITH: Mr. del Piero, in the event that you don't think that that's a response, I'll object to the question as being argumentative.
HEARING OFFICER del PIERO: That's probably an objection that I'll sustain.
Now, Mr. Flinn, if you want to reask the question

0050

in a less argumentative fashion, then I'll direct Dr. Melack to respond.
MR. FLINN: Sure. No, I'll move on to another point given the limited time here.
What I'd like, John, if you could, somewhere, pass out the document --
HEARING OFFICER del PIERO: Except before you go on.
Dr. Melack, Mr. Brown and I both share the same interest that he just indicated it to me, and so I'll ask you the question because I have the prerogative. We'd like you to respond to the question. Obviously, we aren't asking it in an argumentative fashion. Did you intentionally choose not to incorporate that information into your submittal to the Board?
DR. MELACK: Actually, I did include it. In fact, if you look on Page 4 of my testimony, I state that salinity bioassay laboratory experiments of the effects of salinity on individual organisms indicate gradual effects of increasing salinity on nearly every life-history parameter, hatching, mortality, growth, and reproduction of the only macrozooplankter in Mono Lake, the brine shrimp, Artemia monica. And with that sentence, I then cite two or three papers, I cite Dana and Lenz 1986, and I cite Dana et al., which is the

Ô

0051

source of, in fact, these plots.
I then proceeded to point out that, as I did in my brief summary to you today, that physiological laboratory assays must be put in an ecosystem context. And this use of this exhibit, this quote, is a very good example of how you can take things out of context. In fact, the message being given in that --
HEARING OFFICER del PIERO: Doctor. Doctor, this is not a forum for you to testify your reasoning. It's a request for information. We got it. Thank you very much.
DR. MELACK: Sorry.

HEARING OFFICER del PIERO: Mr. Flinn, why don't you proceed now?
MR. FLINN: Yes.
Q BY MR. FLINN: Dr. Melack, we have put up a document from Auxiliary Report Number 13, and this document is Table 17 following Dr. Jellison's July 24th, 1992, letter to Jones and Stokes. In the document, the complicated reference there is something of a mystery, and I'll try and clear that up a little bit.
Do you recognize that from the report of which I understand you're a co-author?
A BY DR. MELACK: Yeah. Although, I'm not sure -- can I ask you a question?

0052

HEARING OFFICER del PIERO: Yes, Dr. Melack, you can, although keep in mind I haven't sworn an oath today. I reserve the prerogative of making up the answers as I go along.
(Laughter.)
DR. MELACK: The documentation that's being used here is documentation that is, in fact, what is being referred to correctly in terms of the numbers of auxiliary reports. Unfortunately, those reports didn't reflect the actual final documentation provided to Jones and Stokes, so some of these data are, I would judge, preliminary in that context.
HEARING OFFICER del PIERO: Well, that's not a question.
DR. MELACK: Okay. That's a statement. I guess the question was should I clarify that chronology? I'm sorry, that was the question, or is that irrelevant?
HEARING OFFICER del PIERO: Dr. Melack, you're obliged to answer the questions. This is cross-examination. And in regards to that, I'd appreciate it if you answered them as succinctly but as completely as you can recognizing that you can't go on for hours on a single question. We do have time limitations here.
At the same time, the Board is interested in

0053

getting adequate information to be able to render both an intelligent and appropriate decision in terms of the direction to proceed from the Court of Appeals, and that's what our desire is. And that's the best advice I can give you.
Mr. Flinn, proceed.
DR. MELACK: Thank you.
Q BY MR. FLINN: I want to focus on this top chart here. Am I correct that this is the result of the model run by your team, not Jones and Stokes, but your team at UC Santa Barbara, of the effects of different lake levels on the -- this is your plankton model results; is that right?
A BY DR. MELACK: That's correct.
Q The top chart is the monomictic condition, right?
A That's correct.
Q Okay. Now, am I right in reading -- the columns NHV and NHH, are those measures of ammonium?
A Yeah.
Q Okay. And am I reading that correctly that as the

lake gets less saline from -- this median bar is 6377, and that as we go up to 6390, we get lower ammonium production in your model? The lower ammonium availability?
A Lower ammonium concentrations.

0054
Q Low -- concentration. And that negatively affects the brine shrimp's food supply, right?
A Not necessarily. What determines the growth rate of the algae is the supply rate of ammonia, not simply the concentration.
Q But you have the same figure going up as you get more saline, right?
A Excuse me?
Q You have the same figures with regard to ammonium concentration going up as the lake gets more saline? We go from 6372 to 6360, that figure goes way up, right?
A The concentration -- the relative -- the percent relative change is positive.
Q Okay. Meaning there's greater concentration, right?
A There is greater concentration, that's correct.
Q And is that the effect that you talked about of the excretions -- increased excretions by the brine shrimp as the water gets saltier?
A To a large extent, that's correct.
Q And that was this feedback mechanism that you were telling us about in your oral testimony, that the lake gets up, and the -- so brine shrimp excrete more, and thus, there's more ammonium and thus more food, and

0055
the -- so the brine shrimp have more food to eat. Is that basically this feedback mechanism you were telling us about?
A That's part of it. One piece of it. Yes.
Q Let's go over here to lake-wide totals the "PP" stands for "primary production;" is that right?
A That's right.
Q And the "SP" stands for "secondary production;" is that right?
A That's correct.
Q Is the secondary production a measure of the brine shrimp?
A It's a measure of the growth rate.
Q Yes. Okay. And am I reading this correctly that at 6390 -- not withstanding the decrease in ammonium concentration, at 6390 we have a 50 percent increase in this brine shrimp growth rate?
A That's what those results show, yes.
Q Okay. And again, although you mentioned your modeling, and you gave us all our caveats, this was a document, this graph was a document you chose deliberately not to highlight in your testimony.
MS. GOLDSMITH: Objection. Argumentative.
HEARING OFFICER del PIERO: I'll sustain that

objection. If you wish to reask the question, you

0056

can. That's twice, Mr. Flinn.
MR. FLINN: I appreciate that, and I tried to appreciate it a little bit differently. But rather than trial and error --
HEARING OFFICER del PIERO: Came out the same way.
MR. BIRMINGHAM: The inflection was a little different. You're doing well, Pat.
Q BY MR. FLINN: Do you have an opinion -- can you tell us in this meromixis-meromixis -- monomixis-meromixis issue focusing on the meromixis, what the difference in density between the less dense water on the top and the more dense water on the bottom is?
A BY DR. MELACK: In what sense do you mean? The numbers or --
Q The numbers or -- how big of a difference does it take to get this condition?
A It might be easier for people to understand salinity differences not density units, but it takes a gram or two per liter. But you get strong stratification with five to ten grams per liter differences.
Q Okay. Now, finally -- one other question, we passed around that exhibit, did we, John? 219? Would you hand the witness Exhibit 219 first?

Ô

0057

HEARING OFFICER del PIERO: Thank you.
Q BY MR. FLINN: While that's being passed out, would you identify Exhibit 219 -- yes. This is National Audubon Society in Mono Lake Committee Exhibit 219.
Would you identify this as a copy of a paper of which you are a co-author?
A BY DR. MELACK: That's correct.
Q And that is paper dealing with meromixis and nitrogen in Mono Lake?
A That's correct.
MR. FLINN: I would move the admission of Exhibit 219.
MS. GOLDSMITH: No objection.
HEARING OFFICER del PIERO: Fine. So ordered.
Q BY MR. FLINN: Would you turn to the next to the last page of that exhibit, Page 1038?
A BY DR. MELACK: Um-hum.
Q And would you read the second full sentence that appears on that page?
A Beginning "Nitrogen fixation"?
Q That's correct.
A. "Nitrogen fixation in benthic algal mass is nearly double at pre-1941 salinities than at current salinities."
Q Is that a --

0058

A Sorry. And then it proceeds to say, "On D. Herbst personal communication."
Q And did you review this document before it was published?

A Yeah. Yes.
Q And did you accede to that citation and that assertion in this document?
A Yes.
Q Okay. Now, you tell us that-- in your written testimony, that in DWP's management plan, the lake will be healthy, as you use the term. Do you recall that?
A I do.
Q What ranges do you understand the lake is going to fluctuate under DWP's management plan, particularly the low end? How do you expect that to get?
A Somewhere in the vicinity of 6372.
Q Now, if you were told -- asked to assume that there are errors in DWP's management plan with respect to the hydraulic or hydrologic modeling such that if their plan were followed, the lake would fall farther than minimum proposed in the plan, would that affect your conclusion that the lake is safe with the DWP management plan?
A It would depend on the extent to which it fell below that level.

0059

Q Let me ask you this, then, Dr. Melack. Did you ever hear of something called a "trial operation plan"?
A Yeah.
Q In 1989, isn't it true that you signed on to a DWP proposal to draw the lake down to 6370 just to see what would happen?
MS. GOLDSMITH: Objection.
THE WITNESS: No.
HEARING OFFICER del PIERO: On what grounds?
MS. GOLDSMITH: Relevance.
MR. FLINN: Your Honor, I will establish that this trial operation plan was submitted to Judge Finney in connection with the -- they asked Judge Finney to order them to draw the lake down pursuant to this trial operations plan, that Dr. Melack described this as a scientifically sound proposal. And I believe that it -- and he said so under oath in a declaration, and I believe it goes to his credibility. That if he says that drawing the lake -- go ahead.
HEARING OFFICER del PIERO: I'm going to overrule your objection.
As to the issue of credibility, I'm not going to rule in regards to credibility. The evidentiary requirements of this hearing are far more lenient than in a court. Okay? It's the purpose of this Board to
Ô

0060

generate as much factual information as possible. If you wish to pursue this matter, you need to lay an appropriate foundation, Mr. Flinn.
MR. FLINN: Okay.
HEARING OFFICER del PIERO: And I'll be listening very intently to hear you do that.
MR. FLINN: Sure.
Q BY MR. FLINN: You do recall the trial operation

plan?
A BY DR. MELACK: Sure. But you're misrepresenting the context of which we signed up to it.
Q I will show you the trial operation plan, but I will read from it. Page Two, it says, quote, the key feature is a program of continuing monitoring and research while the lake is managed to permit a decline to a minimum of 6370 feet." I'll show it to you and ask you if I have read that sentence correctly.
A Yeah. But you didn't read the rest of the paragraph which basically says that we're -- the minimum operating level would be 6372.5 and the 6370 is simply an extreme drought minimum to take into account the reality that we don't have absolute knowledge of hydrology.
Q So it's your understanding that L.A., under this plan, would be allowed to divert until the lake got to

6372 and then they'd have to stop?
A I don't remember the exact details of the plan, but more or less that was correct, that we were --
Q Dr. Melack, going back to Table 1 from your testimony, Judge Finney ordered all water available with the exception of a tiny amount for study starting in mid 1988 to go to Mono Lake. Do you understand that?
A Yes, I do.
Q And you understand that notwithstanding that order, the lake fell approximately three and a half, four feet or more?
A Sure. During an extreme drought in California.
HEARING OFFICER del PIERO: Mr. Flinn, your time is up. We're going to take a break. If you wish additional time, you can petition afterwards.
MR. FLINN: I am done with Dr. Melack. My only few questions now are -- I would like five minutes with Dr. Kimmerer.
HEARING OFFICER del PIERO: You need to make a showing, but you'll do that after the break.
(Whereupon a recess was taken.)
HEARING OFFICER del PIERO: This hearing has again come to order.
MR. FLINN: My application for an additional ten

minutes of time is based on the fact that I, at such a length, I will equal, not even unexceeding the length of the oral testimony which is substantially shorter, and the written testimony that I had to address. And I would also point out that Dr. Melack's written testimony in terms of just physical length was substantially longer than other witnesses, and that's the reason I needed the time.
HEARING OFFICER del PIERO: Your request is granted.
Q BY MR. FLINN: Dr. Kimmerer, at the very end of your oral statements, you may recall I asked the Reporter to mark something that you said and over the break, I wrote down from her transcript what you said. And I want to read it back to you.
You said, quote, my opinion is that there is

relatively little effect on the alkali flies themselves other than just a change in numbers that would result from changes in the lake elevation."

Now, Sir, with that in mind, am I not correct that you began your involvement with Mono Lake just back in 1991; is that right?

A BY DR. KIMMERER: Would you mind telling me who you are and who you represent?

Q I apologize. My name is Patrick Flinn, and I am

Ô

0063
one of the attorneys for the National Audubon Society and the Mono Lake Committee.
A Thank you.
Q With regard to that opinion that you expressed at the end of your testimony, am I not correct that you began your involvement with Mono Lake as recently as 1991?
A That's it, yes.
Q Okay. And how many times have you been to Mono Lake?
A Once.
Q You've been to Mono Lake once. And how long were you there?
A Several days.
Q Several days. And what kind of physiological or scientific study of the alkali fly have you done in terms of its physiology or its ecology?
A If you recall at the beginning of my direct testimony, I said I was not an expert on either Mono Lake or the alkali flies.
Q The opinion that you said that there is very little effect on the alkali flies themselves other than just a change in numbers as a result of change in lake elevation was not made in the capacity of an expert on Mono Lake or an expert on the alkali fly; is that
0064
correct?
A That's correct.
Q Let me move on. You testified that the Jones and Stokes model was something that you, in your expertise as a modeler, sort of reject out of hand as unreliable and shouldn't be used by this Board?
A Yes.
Q As an expert in modeling, you did develop your own model that relates to the alkali fly?
A Yes.
Q I'm going to pass around -- I believe it's National Audubon Society and Mono Lake Committee Exhibit 218 --
HEARING OFFICER del PIERO: Proceed, Sir.
Q BY MR. FLINN: I'll represent to you that the vertical dashed lines, the ones that are straight up and down in the middle of that graph, were placed there by someone other than yourself. But other than those vertical dashed lines, can you identify Exhibit 218 for us?
A Yes. I believe it's the output of the model that I developed for the Mono Lake alkali flies.
Q So this is one graph showing a plot of production, excess production, in terms of larvae and salinity as functions of lake elevation for 50 percent high-quality
0065
soft substrate that you did running your model?
A I think that's accurate, yes.
Q What would it take to make you more certain?
A To see the whole report.
Q Okay. Was the whole report submitted to Jones and Stokes as an auxiliary report?

A Yes, it was.
Q And this -- was this figure attached as an auxiliary report?
A Yes.
Q Which number would that be?
A Sorry. I have no idea.
Q I'll put this question out perhaps to the Staff or to others. I was told by the person who gave me this, Dr. Herbst, that this document was not an auxiliary report and is not in the record, and that's the reason --
A I'm sorry. I guess I misunderstood the auxiliary report. I mean part of the testimony or part of the Jones and Stokes Draft EIR.
Q You understand that there's the Draft EIR, and there was a series of 20 or plus auxiliary reports --
A Okay. I misunderstood that. No. This is not an auxiliary report.
Q Okay. But your best recognition of this is of a
Ô

0066
chart from a report you wrote of your running of your model?
A Yes.
MR. FLINN: On that basis, I would ask that Exhibit 219 -- excuse me, 218 be admitted.
MS. GOLDSMITH: Objection. I don't believe an adequate foundation's been made.
MR. FLINN: For purposes of this hearing, your Honor, I would argue it is adequate. We don't have, given the pressures of time, the ability to get the whole report. It's not something we ever got a copy of.
He recognized it as best he could. Obviously, on redirect, he can have some problem with it --
HEARING OFFICER del PIERO: I'm going to allow it to be recognized, and he does have the opportunity to object or dispute it later on. But at least at this point in time, his testimony is that he thinks this is what was produced by the model he worked with. I'll accept it based on that. I would point out -- I'm sorry.
MR. FRINK: Yes, I have a comment. It is included in the record that the Board already introduced. It would be a part of SWRCB Exhibit No. 2, Division of Water Rights files 0.50, Special Studies Mono Lake.

0067
MS. GOLDSMITH: Withdraw my objection.
HEARING OFFICER del PIERO: Mr. Flinn, proceed.
MR. FLINN: That makes things a lot easier.
Q BY MR. FLINN: I just want to see if I'm interpreting this graph correctly. If I can walk over to it.
There are -- there is a kind of a sloping line here, a curved line falling from approximately the upper left-hand corner down to the lower right-hand corner is that sloping line a line showing salinity?
A Yes.
Q And that corresponds with changes in the lake level from 6320 to 6440. Is that correct?
A That's right.
Q And am I reading this model correctly that you show the -- roughly the production and excess pupae maximized at somewhere between 6380, or a little bit below, and 6390, perhaps?
A Under the assumptions of this particular run of the model, that's right.
Q Okay. Do you -- that's approximately, isn't it? What the Jones and Stokes model run found in Figure L-21 in Appendix L, in terms of where we have maximization at lake level versus lake level?
A I don't remember exactly where that maximum was. I assume it was somewhere around there.

0068
Q Okay. Now, this model here that you developed, it assumes, does it not, that there is no increase in the density by which the flies can live on hard substrate as salinity decreases?
A That's right.
Q Okay. And if there were to be evidence that, in

fact, you can pack more flies on to the same size rock if there's less salinity, that would tend to make your model be on the conservative side in terms of productivity; is that right?
A I don't know what you mean by "conservative" here.
Q You would tend to show fewer -- less lower productivity at a given lake level than would otherwise be present?
A You would show a steeper change in productivity with the lake level elevation. Is that what you're saying?
Q Wouldn't our graph move to the right there with the assumption that you would get greater densities if lower salinities were included in this model?
A It wouldn't move to the right. It would move up.
Q The peak would move to the right?
A Possibly.
Q Okay. Now, likewise, your model does not include any effect of the difficulty that -- strike that. Let

0069

me back up.
Are you aware that as the salinity increases, the bodies of the alkali flies just get smaller than they would be at lesser salinities?
A Yes. As with the brine shrimp, there are a number physiological changes that occur.
Q And would you dispute that the smaller the fly, the harder it is for them to go around and get food?
A I don't know if that's true or not true.
Q If you assume that this is true, that a smaller fly is harder -- it's harder for a smaller fly to get food than a larger fly, your model would not include that effect of salinity; is that right?
A No. I don't believe that's right. We included a function of, well, the effect of salinity on growth rate which would include the ability of the larvae to obtain food.
Q Only growth rate, not absolute size?
A Right.
Q Okay. Assuming that -- not only does -- the growth rate is not only affected by salinity but their absolute size -- that specific salinity effect was not included in your model; is that right?
A That's partly right.
Q Now, the next question I have has to do with what

0070

was assumed to be substrated. I take it that your model assumed hard substrate was comprised of hard rocks and pumice blocks and things like that in the lake bed; is that right?
A Right.
Q And soft substrate was considered to be very poor habitat and thus -- strike that.
And the availability of this hard substrate in your model is a limiting factor of the productivity of the flies; is that right?
A That's correct.
Q Okay. Now, your model did not contain any factor

allowing for possibility that flies use submerged vegetation as a substrate with equivalent or close to equivalent densities as they do hard substrate?
A That's right. And again, this figure is taken somewhat out of context. If you look at the report that contains this figure, I believe it discusses the idea that at higher lake elevations than what we've seen historically, we don't really know what the availability of substrate was because of this question as to whether aquatic vegetation, was suitable substrate, which it may be, and the issue of how much of this vegetation was present at any previous time in history, which we don't know.

0071

Q Now, assuming that the photograph that Ms. Cahill showed you and the historical reports are accurate, that there once were these vast quantities of flies are true, would the use of "vegetation" as a hard substrate be an, at least, an explanation for why there would be some flies at those higher elevations?
A Well, first of all, I'm not satisfied that we know that there were more flies at a higher lake elevation than there are now or that there had been in recent times, so it's a bit hard for me to answer that question.
That photograph that's in this exhibit, of course, the reproduction is poor. It's hard to tell what it shows, but -- I was astounded on my one visit to Mono Lake as how many flies were along the shoreline then, too. So -- and I took some photographs to clearly show the flies from quite a distance, so I'm not convinced that there was a time when the long-term mean fly abundance was substantially higher. It may have been true. It may not have been true. I don't know.
Q Dr. Kimmerer, you misunderstood my question. I didn't ask you whether you were convinced of the truth of those historical accounts, or I didn't ask you whether or not you thought that was a clear copy of the photo.
Ô

0072

I asked you to assume the correctness of those reports. And my question is assuming the correctness of those reports, would not the use of submerged vegetation as a substrate, as an equivalent substrate habitat, explain such high levels at high lake levels?
A Yes. If previous levels of fly abundances are higher than they are now, that's one way to explain it.
Q Okay. Now, does your model that you run contain any assumptions about the effects on the availability of food for the flies with increasing salinity? Or does it assume food source is constant?
A It assumes that the effects of food are those that we saw -- those that were seen in the experimental results on growth rate and size of the flies.
Q And you're confident of that?
A Of what?

Q What you just said? You have no doubt about that validity of that assumption in the model. Strike that.

You have no doubt that that assumption was used in the model?

A I believe that's correct. That's my recollection of what we did.

Q Okay. Now, you testified both written and orally that if we looked at the difference between 6383.5 and

0073

DWP's proposed lake level, that there would be an 11 percent decrease in hard substrate and a 2 percent decrease in growth rate. Do you recall that?

A Yes. That's a calculation that I made since doing this model.

Q Okay. Have you calculated what the actual effect on percentage -- on productivity would be?

A No.

Q Is there a reason why you didn't do that calculation?

A I just didn't do it. I had no reason to do it.

MR. FLINN: I'm through.

HEARING OFFICER del PIERO: Thank you very much, Mr. Flinn.

Mr. Roos-Collins? Where is he?

He's absent. You have no questions.

Ms. Scoonover?

MS. SCOONOVER: We have no questions.

HEARING OFFICER del PIERO: Ms. Niebauer? Good morning and welcome back.

MS. NIEBAUER: Thank you. Erika Niebauer representing United States Fish and Wildlife Service.

CROSS-EXAMINATION BY MS. NIEBAUER

Q Dr. Melack, I have a couple of questions for you. You stated in your direct testimony that you were

0074

appointed as a member on the National Academy of Sciences committee that prepared that important study entitled Mono Basin Ecosystem Effects After Changing Lake Level.

Can you tell me what the importance of that particular report is as it relates to the written testimony that was filed in this proceeding?

A BY DR. MELACK: I'm not quite sure of the context of your question. Let me start to answer and see if it's correct. If not, you can redirect me.

When the U.S. Congress asked the National Academy to do that study, it was at the encouragement of people's interest in Mono Lake as a natural resource, and I felt that the process of selection of members of that committee, which is based on a national search for experts with no remuneration so it is all volunteer, results in a very scholarly, thoroughly reviewed document. In fact, after the documents are written, they're reviewed by outside reviewers, and so in contrast to all other documents that we have on the table in front of us; that is, the EIR and the Corey report, this document went through much more vigorous review. So I felt that it deserves attention because it does represent a greater effort by the National

Academy to produce a credible document.

0075

Q And I think that your testimony actually stated that that was one of the most or the most unbiased study of Mono Lake issues to date; is that correct?
A Yes.
Q And you, as you've just stated, you compare that NES report with the Corey report and the DEIR. My question for you -- is that correct?
A Well, in terms of major review documents, they're, I think, comparable, yeah.
Q Would you classify yourself as a research scientist?
A Yes.
Q Were the members, the other members of the committee that helped prepare that NES report, would you classify them as research scientists as well?
A That's correct.
Q But there are many scientists that aren't research scientists; is that correct?
A I think not.
Q Most are research scientists?
A I think if you're a scientist, you must be doing research, otherwise you're no longer functioning as a scientist.
Q Let me ask you this question, then. Maybe that's a bad way to phrase it. Are there not some scientists

0076

who review research that has been prepared by other scientists such as yourself?
A Well, we all do that. That's part of the process. You actively do research and you review other people's research. It's sort of a give and take.
Q I guess what I'm trying to get at, Dr. Melack, and I'm not trying to be cute or ingenious or anything, but I'm trying to establish what is the difference between you, your credentials, and the credentials that those scientists who reviewed your work, analyzed it, and synthesized it in the DEIR, what is the difference between those two types of scientists?
A I wouldn't make a distinction about -- let me answer your question, then. This might take a couple of minutes here. The processes are very different.
In the case of the EIS report, what they do is they pick people in a variety of areas, not necessarily people who have any experience at Mono Lake in this case, but who are recognized ornithologists, germologists, hydrologists, and those people are asked to review the information that one can use to evaluate an environmental issue in this case. The people doing the Corey report were just as much active scientists as the people doing the EIS report, the people doing Draft EIR, not the consulting company, but the people doing

0077

research that went into that are just as active scientists.
My context of that is how the document is eventually reviewed and when you use the word "scholarship," that implies an academic world view

which means that it's reviewed in the academic context, as are scientific publications that are put out in the open literature. That review process is different than the review process of an EIR. It's still a review process. I wouldn't call it a scholarly one. It's an equally strong review process. Corey's report wasn't reviewed by any outside agency or any outside individual, so the people doing it are the same, but the review process is different.
Q Okay. Thank you.
When was that NES study or report published?
A 1987.
Q So six years ago. Has additional research been conducted on the aquatic productivity and wildlife resources of Mono Lake since that time? Since 1987?
A Oh, yes.
Q Did that report, the NES report, acknowledge the inadequate data base on which the report was based and recommend that additional research be conducted?
A I think that's -- every scientist always says

0078

that. I'm sure it said that.
Q Was one of the goals of that study to specify the critical lake level needed to support then current wildlife populations?
A You mean the congressional mandate to do that?
Q I think in the front of the actual report there's a listing of a number of goals, and I believe that that was part of the congressional mandate. I'm asking you, do you recall --
A I'm not sure of the exact wording of how that was written. That result was -- wasn't a lake level, it was a range of gradient of change that was actually presented.
Q Just a minute.
MR. BIRMINGHAM: We have a copy of the document right here. Perhaps Dr. Melack would like to read that.
HEARING OFFICER del PIERO: Gee, I've never seen some of them appear all at once.
Q BY MS. NIEBAUER: The preface at Page 7, Number 2, could you -- I'll read that I guess. Number 2 says, "The critical water level of Mono Lake needed to support current wildlife populations." The question is, was that one of the goals of that report?
A Yes. Yeah, it was.

0079

Q Was the NES study charged with discovering the optimal or the most secure or, in your terminology, the healthiest lake level for wildlife populations?
A I don't think in the sense that you're asking it. It wasn't charged to pick a particular lake level. It was charged with evaluating the evidence and trying to evaluate where we knew enough to say, "This was safe," and, "This is unsafe." It didn't target a lake level level, as you notice in the document itself.
Q Well, the question is, was the charge of the report to discover what was the best for current wildlife populations or for wildlife populations?

A Not -- no. That wasn't the way it was actually carried out.
Q Did the 1987 NES study make a recommendation as to the lake level that was necessary to support then current populations of wildlife?
A What it did, it presented a figure in the summary conclusions which showed Figure 6.3 which has solid lines, dashed lines, and dotted lines, and for different parts of the community, brine shrimp, brine flies, et cetera. It showed levels that the resource was being maintained, was slightly effective, and the resource was severely affected. And that was as far as this committee was willing to go in terms of

0080

identifying lake levels that might be causing harm to the ecosystem. That's on Page 210.
Q On page -- Page 206?
A Um-hum.
Q The first paragraph, summary and conclusions, second sentence?
A Um-hum.
Q Talking about the resources of the Mono Basin ecosystem, aquatic bird, shoreline, upland environments are affected by changes in lake level, and it reads, "Some of those resources would be adversely affected if lake level rose above the current level, 6380, and others would be adversely affected by lower lake levels;" is that correct?
A Yes.
Q Was there an overall recommendation that was made by the report that essentially the 1987 lake level was necessary to support then current wildlife populations? Do you recall that?
A I don't recall that kind of wording, no.
Q Now, let me move on here. Drs. Denman, Belk, and Serina Bolin published an abstract in the Bulletin of Zoological Nomenclature in September of 1990 in which they stated that Artemia monica is a clearly defined as endemic to only one unique salt lake. Do you know that

Ô

0081

to be true?
A I'm not absolutely sure. I know that certainly the largest populations occur in Mono Lake, but I've also thought that people have found species from ponds near Fallon, Nevada, which seems to breed with Mono Lake species. So I'm -- frankly, I wouldn't call myself an expert in population genetics of Artemia.
I understand their work there, and I'm under the impression that you can find populations which can, in fact, breed with Mono Lake brine shrimp from a few other ponds, but certainly on a population scale, those ponds are much smaller than Mono Lake.
Q Do you know of any lakes in which Artemia monica is found other than Mono Lake?
A And perhaps small ponds in the vicinity. No.
Q You made a statement in response to questioning by Virginia Cahill, Department of Fish and Game counsel, that there were two rotiphers, I believe you said, that were found at Mono Lake previously but that are

currently gone.
A Um-hum.
Q Do you recall making that statement?
A Yes.
Q And you also made a statement in response to questioning regarding other species that were

0082

previously found at Mono Lake that were no longer there. The statement that you made was that you were only qualified to address open-water species. Do you recall making that statement?
A Um-hum.
Q Does a healthy functioning ecosystem such as Mono Lake typically include more than just open-water species? In the case of salt lakes, the benthic communities certainly are important as part of the ecosystem. They're relatively minor, usually, in abundance and contribution, but it's a little hard to say. It depends on what your criteria for healthy is.
If you looked at animals which fed on open-water plankton, they would be quite content if they only had open-water plankton. But I think it's certainly true. The benthic communities are typically part of most lakes, sure.
Q So then the answer to the question does a healthy functioning ecosystem typically include more than just open-water species is yes?
A Um-hum.
Q Well, if that's so and if you've said that you're only qualified to speak to open-water species, then can you tell me, are you qualified to give an opinion regarding the overall health of Mono Lake?

0083

A I think so, because I certainly understand what's going on in the -- I've studied it as an expert, and a research scientist is different than being able to read literature about that region.
MS. NIEBAUER: Thank you. That's all I have.
HEARING OFFICER del PIERO: Is Mr. Gibson here?
Okay. Mr. Hasleton is not here.
Is there anyone else who wishes to cross-examine the witnesses?
Ms. Goldsmith, redirect?
MR. FRINK: Excuse me, Mr. del Piero.
HEARING OFFICER del PIERO: I keep forgetting about you guys.
MR. FRINK: It's easy to do.
HEARING OFFICER del PIERO: Out of sight, out of mind, looking over the top of your heads.
CROSS-EXAMINATION BY THE STAFF
Q BY MR. HERRERA: I have a few questions of Dr. Kimmerer to begin with.
Dr. Kimmerer, you were a subcontractor to Jones and Stokes in the preparation of the Draft EIR?
A BY DR. KIMMERER: Yes, I was. Let me rephrase that. My company was.
Q You were charged by your company to prepare material for Jones and Stokes?
Ô

0084

A Yes.
Q What was your initial involvement with that? What was your initial proposal? Your company proposed to do for Jones and Stokes?
A We were asked to do -- to work on the modeling aspects of the preparation of the EIR.
Q Can you be a little more specific? Work on the modeling aspects?
A Well, initially, we were going to work both on the alkali fly population modeling and the brine shrimp modeling. We later -- well, we realized that that was not appropriate, the latter, because of the amount of time and money available and the amount of work that had been done already and the fact that it was in good hands, in my opinion.
Q Is that -- on the shrimp modeling?
A Yes.
Q So you did not get further involved other than your initial involvement on the shrimp modeling?
A Sure.
Q Now, again, you mentioned that you reviewed or you worked on the fly model that was prepared by Jones and Stokes. Could you discuss a little more what your involvement was with that? What you actually did?
A Yes. Dr. Herbst and I got together and basically

spent time putting together this model. He being the expert on the alkali flies, and I being familiar with modeling techniques. And we put together a production model that we suggested to Jones and Stokes was about as far as we could go with the available data, and I wrote that up as a report and submitted it. And that's it.
Q You reviewed -- in other words, you reviewed Jones and Stokes' model and prepared some of the material that suggested changes to that model?
A No. I wrote a model to start with. Jones and Stokes took that model and added a bunch of stuff to it that, in my opinion, was not appropriate and came up with their model.
Q And when did you review that material that you suggest is inappropriate?
A After the Draft EIR. I'm sorry. I got a copy of it, but I really reviewed it after the Draft EIR came out.
Q Were you given an opportunity to review it before the Draft EIR came out?
A I'm not quite sure. I believe I got a copy of it, but I get a lot of things to review. I don't review them all.
Q I find that kind of surprising, something that

you'd been involved in that you didn't review it if you had the opportunity before the draft came out. Is there any particular reason? You didn't have the time --
A I didn't have the time.
Q You just didn't have the time to review it?
A I've got a stack of things this high that I should be reviewing now.

Q I can feel for that.
And you developed -- you developed your own model, and that was submitted to Jones and Stokes about when? In fact, I could tell you the date of the letter -- the cover letter to Jones and Stokes. It's part of our exhibit that was earlier identified. It was dated August 17th, 1992.
A Yeah. That sounds about right.
Q Okay. Was this model also prepared with input from Jones and Stokes?
A Yes. They provided input on, as I recall, the relationship of elevation to salinity and the relationship of hard substrate area and soft substrate area to lake elevation.
Q Okay. And in the development of your model, was anybody else involved in the review of this model?
A Russ Brown of Jones and Stokes was involved.

Q But outside of Jones and Stokes --
A And from L.A. DWP as well.
Q Was that L.A. DWP's staff?
A Yes.
Q And Ed Herbst, I assume --
A Yes. David Herbst and I worked -- we produced the model together.
Q Was this sent out for any other review?
A No, it was not.
Q I think that concludes my questions to Dr. Kimmerer.
Dr. Melack, I've just got a few for you. Earlier -- earlier on, you were discussing -- we were discussing with Mr. Kimmerer his involvement in the preparation of the Draft EIR. Would you describe your involvement -- were you a subcontractor to Jones and Stokes in the preparation of the Draft EIR?
A BY DR. MELACK: I don't think so. Our funding was actually through the Department of Water and Power. It wasn't a subcontract derived from Jones and Stokes.
Q You were charged to do specific things, though, to prepare information for the Draft EIR, or could you elaborate on what your involvement was there?
A Yeah. We and you and Jones and Stokes met several times outlining a plan of action which involved model

development, primarily, which we then did. So it's a joint decision between you, them, and us.
Q And was it not the charge of your group or -- and Jones and Stokes and ourselves to look at developing or possibly working with your models to discuss or develop some sort of way to predict or analyze the possible effects of various lake levels?
A Definitely. Yeah. That was certainly one of our goals.
Q And how did that go? Was that successful, or -- were we able to work with the higher lake levels, lower lake levels that are outlined in the Draft EIR?
A I think it varied. We were -- in the case of the physical model that we developed, which was -- we had a large head start on, we were able to produce useful

predictions of meromixis, not over the full range of potential lake levels because we were constrained by the existing physical chemistry of the lake that we had in hand.

In the case of the plankton model, we were, I think, less successful because of the inherent comprehensive modeling plankton communities and associated limnology. And so from my assessment, I think we started from the position of strength with the Artemia population, but we had difficulty assembling

0089

the nutrients and algal populations into what I would consider to be a truly predictive model, which is very much the state of the art. It's very hard to make predictive models of plankton population.

Q In your testimony, you suggest lake models of 6372 to 81 -- I believe that's correct -- is a healthy environment for shrimp. That was the basis for much of your investigation because you did have, for lack of a better term, hard data or actual data to represent those conditions. Again, these were lake levels you had that sort of hard data on.

Now, in predicting other possible effects, you recognize that you were, during a rare event, as you termed it, meromixis, as the foundation for that information, how did that affect, again, your limitations on extrapolating that information for, let's say, higher lake levels?

A Okay. Higher than 6381?

Q Yes.

A The fact that it was meromictic I don't think had much effect on that extrapolation. What it had an effect on was that we had less years, we still had several years, we had less years to look at what you might say the normal condition. But in terms of beyond the period of record, whether it was miramictic or not, Ô

0090

is less of an issue.

I think, in fact, the opposite is really true, that with not having the lake be meromictic, if you take that aside and do that as a treatment, if you will, an interpretable agreement, what we saw then was how does Mono Lake respond to a large reduction in primary production? That reduction could have been caused by salinity. It could have been caused by meromixis. It could have been caused by a number of things. So in a sense, what we were able to observe was how the lake responded to a major predation in terms of its brine shrimp response.

So I would argue that, in fact, the meromixis was a very useful predation and, in a sense, gave us a strong indication of what could happen if our predictions of salinity effects are correct.

Q Well, if we're talking about, again, maybe in a little simpler form here, the meromixis has occurred by a rapid influx of fresher; is that correct?

A That's correct.

Q And that's that rare event. Now, if the -- as the Draft EIR suggests, these various lake level

alternatives would not be achieved in a rapid fashion. Would that still give your meromixis scenarios? And I'm not sure that those would prevail. Have you looked

0091

at that?
A Yeah. You're asking a little different question. If you're asking when we did the simulations with the model called DYRESM with the incidence of meromixis, for that simulation, we used the lamp input, hydrological input data over the 50-year period, which included various water supplies. Is that what you're asking me? I'm not quite sure.
Q What I'm getting at is that yeah, you have a meromixis that occurred from a rapid influx of water. And yet if we're looking at operational scenarios as suggested by the EIR where that would not occur, again, how would that -- if your evaluation is stating that you're not going to go through a meromictic period, if we're operational, as the EIR suggests --
A Well. Maybe I should -- I guess I'm not sure if we're getting off track or not. The -- my point a few minutes ago about the effect of meromixis and our ability to interpret the ecosystem response was really in the context of if we examine the evidence that we have or suggest that -- if you take the bioassay experience and you predict a decrease in production with increased salinity, if you decrease production in the whole lake by some mechanism, in this case by decreasing nutrient supply, that then provides you with

0092

a very powerful basis for evaluating primary production effects on the ecosystem. Regardless of whether the lake is meromictic or not, the effect is still the effect of reducing production.
That effect then gave us an ability to evaluate how the lake changed when the production decreased by 50 percent or more. That's what I was trying to say, and it's kind of irrelevant whether the lake is meromictic or not to make that particular judgment.
Q Well, one of the things that struck me, and I don't remember the particular comments, but in your earlier testimony, you suggested that as the nitrogen levels were going down, the shrimp population were going up, but at the same time, so was the lake level. And you were having a little bit -- you were trying to qualify that question a little bit earlier, I believe your testimony is.
If we're suggesting again that the nitrogen levels were going down and lake levels were going up and yet shrimp populations were increasing, could you explain that a little bit more? We had a dilution factor of what you suggest was the controlling factor to shrimp.
A When you say "my testimony," you mean --
Q Earlier today?
A My response to the questions by Mr. Flinn? Is
Ô

0093

that what you're referring to?
Q I believe so, yes.
A And he showed a -- in fact, this exhibit over here

where he based on our model predictions, we showed -- at different lake levels, we showed different changes in different concentration of ammonia.
Q And ammonia was coming down and populations were going up as lake level was rising.
A That figure is a model simulation. It's not based on the actual data that we have.
Q It could be. I don't recollect.
A That was a model simulation. We actually haven't, today, talked about the actual data in that 14-year period, which would directly address your question. And we could do that. I'm not sure we want to do that.
Q I don't think so. I'm just curious when we're saying one thing, we didn't discuss the dilution factor here at all.
Again, did you -- there's other various questions that we discussed pre-1941 conditions as to shrimp populations. How would you characterize the shrimp in pre-1941 conditions? Was that a healthy ecosystem?
A I don't have the slightest idea. If I were just to guess, I would suggest that it was, but I have no basis in fact to say that.

MR. HERRERA: I think that concludes my questions. Thank you, Dr. Melack.
HEARING OFFICER del PIERO: Mr. Canaday?
Q BY MR. CANADAY: Dr. Melack, you testified to what lake elevations that your 14-year record corresponds to. Can you relate that to the salinities that you studied? The range?
A Yeah, I can. It probably would help if I try to remember that the highest were up in the vicinity of 90 --
Q 98?
A I was going say 98 grams per liter, and the lowest would have been in the surface waters. Just after the meromixis began, they were as low as -- I'm not exactly sure. I'm guessing 72 or so grams per liter, 75 or so. That's in one of these reports. I just don't remember exactly what number it was, but it's in that vicinity.
Q Okay. Is some of your reluctance to using scientific judgment to express an opinion on the impact of brine shrimp productivity and algae productivity and primary productivity at lake levels higher than 6381 the fact that you have no data for that? Is that correct? Actual field data?
A That's why -- I'm not saying I won't do it, but I'm a little bit more reluctant to have to do that and,

therefore, I'm trying to say when you look at the model results, you have to be cautious.
Q But based on 14 years of record and the information you have at hand and the expertise you had at Mono Lake, in your professional opinion, do you expect that the brine shrimp productivity would significantly decline at higher lake levels and lower salinities?
A It depends on how high you get. It depends --
Q Let's say --
A You get there.

Q 6383.5.
A Oh, no. I wouldn't think that there would be any difference from the record I showed you today if you went through that level.
Q 6390?
A I'm less sure, but I wouldn't think that there would be very large changes.
Q 6410?
A Then I would have to say that Mono Lake would be a different lake, and I wouldn't be willing to speculate.
Q The NES report that's been talked about today that was completed by the august group of scientists, wasn't the focus of that report based on the fact that the lake level was going to be declining?

0096

A That's a fair statement, yes. That was a concern. How things changed as the lake level fell.
Q Right. And so the emphasis of the research and the recommendations was not based on evaluating a lake level that may be rising from that particular point of reference in the document. is that correct?
A Yes and no. We certainly evaluated prior lake levels. There's ample data in there for higher lake levels as well.
Q But only up to a point of 6381; is that correct?
A I don't think so. That's my memory. I thought we had --
Q As far as lake productivity?
A As far as the lake productivity, on these figures here, that was 6420 and above, 6430.
Q So scientists were willing to make projections based on data that their data sets were significantly lower than that on the lake level, but yet they were willing to make value judgments based on the data at hand; is that correct?
A I think I got your question. Will you just restate it, though, just one more time, please?
Q Well, in your earlier testimony, you said that it's important to base evaluations and recommendations or analyses based on the data sets at hand. And you

0097

suggested that this is the most unbiased study, the NES report that had been done, but yet there are -- here's a group of scientists making recommendations for lake levels, and the focus of the study was that lake levels that were declining from the point. But yet they were willing to make, based on the data at hand, data up to 1986, make projections of the values of the resources at higher lake levels. Is that correct?
A Well, perhaps. I think you ought to be careful, though, how you say that. If you look at the resources at hand which I'm speaking of, brine flies, brine shrimp, if you look at those two, what the NES report does is it says, "Resource maintained." That's a pretty neutral statement. The resource is maintained at levels from 63 -- I guess for Artemia, it would be 6368 up to -- this goes to about 6425.
We're saying it's maintained. I mean, that's -- I think -- yeah. To that extent, your statement is

correct. People are willing to say that the resource is being maintained. It's not threatened in that range. It's not a very strong statement, but that's what it's saying. Resource maintenance.
Q You don't dispute that recommendation?
A No. I wouldn't entirely dispute that, no.
Q Getting back to the EIR that was produced by Jones

and Stokes and getting back to earlier statements you made about scientists doing research, am I to assume that, from your statements, that the staff at Jones and Stokes could not be considered scientists?
A I said -- what I was trying to imply is that they weren't academic scientists as were the scientists that composed the Corey board or the NES board. They still could be scientists, they're not research scientists.
Q Do you know that for a fact that they're not research scientists?
A I know among the ones who I interacted with that's the case. Whether it's true across the board, I certainly do not know.
Q So what you're saying is that -- is that unless you're attached to a university under some grant that, therefore, you're out of the mainstream of science?
A I wasn't making any value judgment at all. I was simply making a statement. I wasn't judging pro or con. I was simply stating that there's different kinds of individuals. In fact, I quite carefully said that there was no distinction among the people involved, just that they had different kinds of jobs.
Q So then you're telling me you would admit that they are scientists, then?
A Some are certainly. Some are engineers.
Ô

Q Um-hum.
A There's a difference there I would make as a distinction.
Q Dr. Kimmerer, did I hear your testimony earlier that you are under the employ of a consulting firm? A BY DR. KIMMERER: Yes.
Q Do you consider yourself a scientist?
A Yes.
MR. CANADAY: Thank you. That's all I have.
MR. FRINK: Staff has no further questions.
HEARING OFFICER del PIERO: No further questions.
CROSS-EXAMINATION BY THE BOARD
Q BY HEARING OFFICER del PIERO: I need a clarification. Something that -- Dr. Kimmerer, you were an employee of a contract firm that produced the initial draft model on the flies; is that true?
A BY DR. KIMMERER: That's true. I produced the draft.
Q In your capacity as an employee of a contractor?
A Yes.
Q When was that?
A It came out this August 1992.
Q And you -- was that consulting firm subcontracted to Jones and Stokes at the time?
A Yes.

0100

Q They were? And the draft model that you prepared was pursuant to the contract that was entered into?
A Yes.
Q That was being -- that was ordered by the Court of Appeals?
A Right.
Q Okay. And you were employed in that capacity to produce that?
A Well, it was one of my jobs.
Q Okay. I just -- I'm trying to get this straight. When did you get a copy of the draft from Jones and Stokes, the Environmental Impact Report?
A I really don't recall. I get lots of things to review, and --
Q When did -- circulation of the draft is required to be done pursuant to the guidelines of SEQA.
Mr. Frink, do you know what the date was that that was circulated?
MR. CANADAY: May 26th, 1993.
Q BY HEARING OFFICER del PIERO: May 26th. Okay. Were you still employed to review that work?
A BY DR. KIMMERER: My contract with -- or our contract with Jones and Stokes had finished by that time, and I was --
Q Your contract did not require you to review the

0101

document?
A No.
Q It did not?
A No.
Q Do you recall when you received the model after Jones and Stokes had added the other stuff to it? Those were your words?
A Yeah. I received the Draft EIR --
Q I'm not talking about the Draft EIR. I mean the model which is your work product.
A I believe they sent me a copy sometime before that, but I really don't remember when.
Q Would it be in your records?
A Yeah. I'd have it somewhere.
Q Would it be in Jones and Stokes' records?
MR. CANADAY: Yes.
Q BY HEARING OFFICER del PIERO: And when did your contract terminate with Jones and Stokes?
A BY DR. KIMMERER: Shortly after I submitted that report in August 1992. I'm not sure of the termination date of the contract, but the work was completed. And we ran out of money so we stopped. I don't know the actual contract completion date.
Q Okay. You stopped because you ran out of money in terms of reviewing the documentation that had been

Ô € €

0102

ordered by the Court of Appeals?
A I was not asked to do any further work for Jones and Stokes.
HEARING OFFICER del PIERO: Okay. It's a quarter to 12. We'll start with redirect at one o'clock. This hearing's adjourned.

(Whereupon the lunch recess was taken.)
HEARING OFFICER del PIERO: Ladies and Gentlemen, this hearing will again come to order.

REDIRECT EXAMINATION BY MS. GOLDSMITH

Q Dr. Melack, I'd like to ask you a few questions on redirect.

First of all, has the Los Angeles Department of Water and Power ever attempted to constrain the publication of data which has been produced by you or your group?

A BY DR. MELACK: No. Not at all. In fact, if anything, they have encouraged us to produce some publications in the fully reviewed scientific literature.

Q Is your use of the term "fully reviewed" the same as is also referred to sometimes as "peer reviewed"?

A That's correct. When we publish papers, what we do is we submit them to an editor of a journal, and that editor sends in anonymous review. And then

they're criticized and sent back and eventually published in journals, and that's the standard way scientific information is deemed legitimate or valid. Until papers are actually reviewed and published, they remain in the realm of personal communications or unpublished data. Such information can be used but does not pass the same stamp of approval that a peer review article does.

Q And has most of the data that you've collected been published in one form or another in peer review journals?

A I wish I could say most of it. Most of it, yes, but in all of it mainly because we're behind. But we do aim to publish everything we do in peer review journals.

Q And is there any interference in that process other than the constraints of time?

A Interference by -- no. The constraints are just merely time.

Q Turning to questions that were raised at an earlier time by U.S. Fish and Wildlife Service, is a criteria of 85 grams per liter an appropriate or relevant water quality standard to apply to a saline lake like Mono Lake?

A I would say not. Those criteria are really

arbitrary and depend on the particular organisms that live there and the ecosystem at issue. And in the case of Mono Lake, we have clear evidence that the lake functioned very nicely. Salinity's higher than that.

If I looked at the Dead Sea in Israel, one would see salinity at 300 grams per liter and clearly, the ecosystem is functioning. So it depends on your ecosystem. There's no arbitrary way -- that cut-off is a legitimate criteria.

Q In his cross-examination of you, Mr. Flinn asked you questions concerning a quotation which he put up on a chart taken from Auxiliary Report Number 13 in which there was a question about salinity effects being obscured. And you stated in your answer that it was

important not to take that statement out of context.
Can you tell us what the meaning of that statement is in the proper context?
A Yes. What I've been trying to do here, both in my written testimony and in my oral testimony, is to raise the issue that when one tries to look at environmental change, one has to look at it in the context of the whole ecological system and, therefore, one can't look at a single factor like salinity and say that's the dominant factor causing all of our changes. And therefore, when one has a data set like we have which

Ô

0105

shows natural variability, in this case caused by some climatic changes, those effects can, in fact, override other effects, and the argument is really not to say that salinity isn't a factor. It is a factor. We know it's a factor. That data show it's a factor, but it's only one factor. And food supply, vertical mixing, water supply rates, species composition are also factors. And so the evaluation of a particular influence really depends on the ecosystem context. And that was really the point of that quote. It wasn't to emphasize that we couldn't see salinity effects, period. It was to say that they need to be put in the context of the overall ecosystem.
Q Is that true for all of the different sorts of factors which affect the ecosystem such as temperature or --
A Yeah. But the real challenge of ecology, like the challenge of economics, is to look at very complicated systems and evaluate what's really the key factors and how these factors interact with one another. So the thrust of this has been to try to make the decision-making process sensitive to the realities of complicated systems and not let us be lulled into the feeling of security by picking on any factor and then basing our decisions on one single factor which can be

0106

very misleading and often, in fact, wrong.
Q Is this confounding effect or complicating effect of the interaction of various factors true of all lake levels or just --
A It's true of all ecosystems, and Mono Lake is no exception.
Q Now, Mr. Flinn also put some graphs from -- there was a report by Dr. Jellison which showed a 50 percent increase in secondary production and if I could put those up and identify the graph a little more clearly. I believe it's Table 17 from the Auxiliary Report Number 13.
How would you evaluate the accuracy of the salinity results and effects that are shown?
A These are simulation model results based on a variety of equations that Bob Jellison and I put together, and when you do such an analysis, what you then do is you vary some of the values that you assign to the terms in those equations. And one of the approaches is called the sensitive analysis where you systematically change parameters and look at how the

system responds to those variations. You also compare your results to known conditions.

In the case of a sensitivity analysis that we did, we found that the values that we predict would vary

0107

between 10 and 20 percent depending on what we varied. So the aggregate effect of the sensitivity analysis which suggests those values are probably plus or minus about 15 percent.

In addition to that, you compare your results to the natural variability, and so as I mentioned in my testimony, and it is documented in the direct written testimony, if you look at secondary production in Mono Lake over the last -- it's an eight-year record, the variation is from about plus 70 percent to minus about 40 percent. In other words, from a mean value, we observed over this eight- to ten-year period variations of between 40 and 70 percent. So in other words, a 50 percent increase or decrease could be judged as within the natural variability.

On the other hand, if that increase persisted through time many years after another, in other words, if there was a clear trend of values going higher and higher, then one would be more comfortable with the notion that, in fact, there was a significant effect. But this particular simulation was a one-year simulation. It wasn't really based on a trend.

So again, I was mostly just trying to raise the issue of caution, and when you make a decision, you base your decision on the best available information.

Ô

0108

And when you use models, you have to be careful that you include in your appreciation of those model results the uncertainties of those models just like you do with any other kind of data. And it's not saying the models are wrong or right or bad or good. It's just something that the reality is that they intrinsically have uncertainties and those uncertainties are part of life, and we have to, then, bear those in mind when we make our judgments.

Q Mr. Flinn also asked you about NAS and MLC Exhibit No. 219, which is a paper by Jellison, Miller, Melack, and Dana, and he had you read a sentence from the end of the paper which says, "Nitrogen fixation and benthic algal mass has nearly doubled at pre-1941 salinities than at current salinities," which is cited, "D. Herbst personal communication."

Does this mean that there would be twice the nitrogen fixation if the lake returns to the pre-diversion levels?

A No, it doesn't. And the reason is that the area of benthic algal mats and the species composition of those mats and the what's referred to the oxidation reduction status in those mats and the amount of trace metals present all affect nitrogen fixation rates. It is a very complicated process. It's very sensitive to

0109

a whole range of environmental conditions one of which is salinity, but in general, people find salinity as

being one of the less important factors. You have many factors involved, and you also have the issue of what area is even covered by benthic algal mats, and that's an issue as well. Although the lab experiments by themselves are valid scientifically, the extrapolation of those data to the lake at a different level is pure guess work.

Q Now, you were asked by Ms. Niebauer concerning your opinion that the lake's ecosystem is healthy. And the focus of that line of questioning, I think, was whether or not your opinion is limited to the phalagic zone or extends more broadly. Is your opinion that the lake's ecosystem is healthy limited to the phalagic zone only?

A No, it's not. My own research is generally limited to there, but I have long-term contact with everyone studying Mono Lake and have had many contacts with David Herbst, for example, who's done a great deal of the research. And we keep in touch with unpublished documents and, in fact, that's why we cited an unpublished work, because we work in close contact. So I think that my own knowledge base clearly extends beyond the phalagic zone and includes all the recent

0110

research going on in the lineral zone as well.

Q I have one question for Dr. Kimmerer.

There was some discussion, a great deal of discussion, as a matter of fact, before lunch concerning what was said to you, what you were asked to do, and what the development process was for the brine fly model that was included in the DEIR.

Have you had a chance to investigate that question over the lunch hour? Have you learned anything?

A BY DR. KIMMERER: Yes. My recollection at the time was -- before lunch was I had received something from Jones and Stokes, and I couldn't remember whether I was asked to review it or not asked to review it. I was clear that it wasn't part of any contract.

I did discuss this with Dr. Russ Brown, who is here today. He was my contact with Jones and Stokes, and he confirmed my suspicion that I had not been asked to review it. I got a copy of the model in March just as an informational copy because I worked on the -- the production model that was used as a basis for this model in the Draft EIR.

Q You were not asked to review it?

A I was not asked to review it.

MS. GOLDSMITH: That's all I have on redirect.

HEARING OFFICER del PIERO: Ms. Cahill?

Ô

0111

RECROSS EXAMINATION BY MS. CAHILL

Q Dr. Melack, in response to a question by Mr. Flinn as to whether you had incorporated certain graphs into your testimony, you directed our attention to Page 4 of your direct testimony in which you state that, "Salinity bioassay laboratory experiments of the effects of salinity on individual organisms indicate gradual effects of increasing salinity on nearly ever life-history parameter of the only macrozooplankter in

the Mono Lake brine shrimp."
     In order to make it clear, would it be correct to say that these gradual effects of increasing salinity are adverse effects on the organism?
A BY DR. MELACK: You're getting to the realm of semantics here. Adverse is a value judgment, and if science is supposed to be value blind, I guess I should say that I wouldn't use that adjective.
     You could say that the populations, as was described, I think, quite well by Mr. Flinn in his cross-examination, that the plots show that survivorships decline and growth rates decline. In that sense, you use a word like "decline rates" or "longer duration times," then I would be more comfortable, but I wouldn't use the word "adverse" or something like that. I think that's inappropriate for

0112

a scientific document.
Q   In order to let the reader know that these effects are effects that would -- the lay person would consider to be adverse, what is the scientific way of expressing that, that these are not beneficial to the organism? Is "decline" the word that you --
A   Yeah. I would say -- of course, you have to be careful because in some cases they -- longer generation time is a -- it would slow up the reproduction rate of the population. Again, I think you're losing the forest for the trees in a way because you're missing a whole line of this argument, which is that individual factors in and of themselves don't tell the whole story.
Q   Those factors, based on those laboratory results, taken as a whole, would they tend to show that there was a trend toward a more healthy or less healthy situation for the brine shrimp with increasing salinity?
A   Let me try to not be at all evasive. If you extrapolate the data to salinities to 130 grams per liter or more, it's clear that brine shrimp population will not survive in the lake if that lake level is below 6360. So there's no question that at some point, you have a clear negative effect on that population.

0113

In the realm of salinities that we're currently talking about, changes of a few percent, it's very hard to say whether those are positive or negative effects. So it's a question of the range in which you're talking, whether or not you would really deem it as a true negative or positive effect.
     I'm not trying to mitigate the effect being real, but I think you just have to be careful about how you conjure these two points because they vary depending on which range of lake levels you're talking about. In a range from 6372 to 6385, you probably won't even see a salinity effect.
Q   But the trend is -- if you've used the word "negative" in most of those cases, the trend with increasing salinity was toward negative impacts?
A   But it's kind of irrelevant because --
Q   Just yes or no.

A The trends for the physiological assays is negative, that's right.
Q Thank you.
Dr. Kimmerer, I have just one last follow-up with you. You quite rightly stated in response to a question by Mr. Flinn that the copy of the Condor article that I gave you was not very clear, and because the original is so much clearer, I would like to show
Ô

0114

it to you. And I will try to have better copies made for all counsel.
But would you take a look at this, please?
MR. THOMAS: Are you going on the stand?
MS. CAHILL: Mr. del Piero, I'll bring it up to you, too. It is true the original makes quite a difference.
Q BY MS. CAHILL: Dr. Kimmerer, having reviewed what that statement says about a band -- a dark band of alkali fly completely around the lake and having viewed the photograph, would you conclude that it is possible that in the pre-diversion condition, the abundance of alkali fly was greater than it is today?
A BY DR. KIMMERER: It's possible.
Q And, in fact, based on your review of that photograph, do you conclude that it's likely that at some time pre-diversion, there was a population of alkali flies that was greater than what exists today?
A With or without that photograph, I would conclude that at some time in the past there have been higher or lower populations than there are now.
Q Thank you.
A That photograph doesn't really say much to me to answer that question one way or the other.
MS. CAHILL: Thank you. That's all I have.

0115

HEARING OFFICER del PIERO: Thank you very much.
Mr. Flinn?
MR. FLINN: If I could have one moment?
RECROSS EXAMINATION BY MR. FLINN
Q Dr. Melack, I want to revisit meromixis a little bit here, follow up on some questions that you answered to the lawyers who followed after me. If we were to draw on table -- Figure 1 here, your lake level chart, a kind of a -- your smoothed brine shrimp population curve, it would be sort of wavy but relatively constant through this period; is that right?
A BY DR. MELACK: We can show that, actually. It's on one of the other exhibits.
Q But it's not against lake level?
A Yes, it is.
Q It is? You have one against lake level?
A The last one, Figure 11, the overlay is lake level.
Q Oh. Is this the one?
A Yes.
Q Okay. Great. Much handsomer than ours.
Now, am I sort of right that we've got this monomixis period here in which the shrimp were having to endure salinities substantially higher than they

endured when the lake was in its pre-diversion
0116
condition? Is that right?
A Oh, sure, yes.
Q Okay. And then you said that this meromixis substantially reduced food supply, right?
A Initially, yeah. Where those bars are kind of grayish, that means there's less food. When the bars are bluer, there's more food.
Q As we got bluer, we got back down to the monomixis salinity levels again, right?
A That's correct.
Q So we were sort of out of the -- out of the salinity frying pan but into the nutrient fire?
A Those are your words.
Q We never had both at the same time, did we? We never had both the light gray or light blue bars at the same time we had the higher salinities, did we, in your 14-year historical record?
A Well, we do it careful because those data there are showing you annual average values and, in fact, if you look at individual years, we do, in fact, have situations where there were much, much lower algal abundances in early parts of the growing season. And so we -- and we could, therefore, evaluate food independently of meromixis, so that -- based on those data in front of you, you're correct but, in fact, we
Ô
0117
have other data which would allow us to do other sorts of analyses.
Q Didn't you tell us that we shouldn't even take five years or less than five years as a trend? We certainly shouldn't take less than one year as any kind of a trend, should we?
A That's a different issue. What we're talking about when you talk about food supply and shrimp abundance, is we're talking about a mechanism that affects the population. That's a different kind of analysis than a trend. You're talking apples and oranges here. So maybe you should clarify where you're coming from.
Q I will clarify it. We never had, for a period of an entire year, an entire season, a condition of both the low nutrient and the high salinity at the same time for a whole year, for a whole season; isn't that right?
A During this period of record, that's right, yeah.
Q Now, we talked about the model. Just for clarity's sake, this document, which is Table 17, following the letter, this is produced by your team not Jones and Stokes, right?
A Yes.
Q Okay. And so the decision to use 6390 and to draw these graphs was a decision your team made and you
0118
approved of, right?
A I wouldn't say that. The decision to use those elevations was a joint decision made by the State Water Board and the courts and everyone involved. We picked certain levels as being appropriate levels to evaluate.

Q And 6390 was one of them?
A Yes, exactly.
Q And you told us a lot about uncertainty. You told us about how you can't -- just recall to us your uncertainty testimony. Is there a better model of what would happen at lake levels outside of your historic record than the one your own team did?
A There's no better model, no. Although -- well, at this minute, there is a better model. There was not a better model at that point.
Q At this minute?
A In other words, we didn't stop working on this problem when we turned in the results to Jones and Stokes. We've continued to try to improve upon the model based on more data and more experimental results.
Q In the record available to the Water Board, that's the best thing we have?
A That's the best we had available. That's correct.
Q So assuming that the Water Board has to live with these uncertainties you've warned us all about, you

would commend to them the best model, wouldn't you?
A What I would also commend to them is the importance of looking at real data if one has an option to look at real data versus model results.
Q And did you, yourself, take data for more than five years with the lake level at 6390 or thereabouts?
A No, of course not. Those data don't exist.
Q Okay. Now, you did answer some questions, I believe it was Mr. Canaday or Mr. Herrera, in which you were asked would bad things happen to the shrimp population as the lake rose, and he asked you 6383.5 or 6390 and 6400 or 6410. And I recall you saying something like you wouldn't perceive much of a change at 6390.
Do you generally recall that testimony?
A Yes, I do.
Q Okay. Now, if we were to follow the best model available to the Water Board currently, the change between 6377 and 6390 in monomictic conditions is, in fact, in a positive direction, isn't it?
A Yes.
Q Now, in answer to other questions, and I don't know who asked them. It might have been Ms. Cahill, but I'm not sure. In answer to other questions, you were asked about studying the lake at a higher lake

Ô

level and you said, I believe you described the lake level above 6400 or 6410 as a "different lake."
Do you recall that testimony?
A Yes.
Q Now, let me ask you to assume, and I'm not going to examine you on the public trust doctrine. I'm going to ask you to make some assumptions. I'm going to ask you to assume that the California Supreme Court has instructed all of us in the room that a public trust value is the preservation of Mono Lake in its natural state. And I am further going to ask you to assume that the natural state means the lake above 6410.

Do you follow me so far?
A Um-hum.
Q I take it that the so-called healthy lake that you're telling us about today is a different lake than the lake the Supreme Court has commanded us is a public trust value.
HEARING OFFICER del PIERO: Is that a question, Mr. Flinn?
MR. FLINN: Yes. I want to make sure that I'm following his definition of --
HEARING OFFICER del PIERO: Dr. Melack?
MR. FLINN: Do you understand that question?
DR. MELACK: It didn't sound like a question. It

sounded like a statement. What's the question?
MR. FLINN: Madam Reporter?
THE REPORTER: Sure.
MR. FLINN: Could you read it back?
(Whereupon the record was read as requested.)
DR. MELACK: Yeah. Just like I'm a different person ten years from now. That's a kind of a nonsequitur, what you're saying.
Q Now, I want to turn to exhibit -- let me back up. One more thing.
Do you have an opinion, Sir, as to the health of the migratory duck population at Mono Lake currently?
A Do I have an opinion?
Q Yes.
A No. I don't have an opinion about that.
Q Okay. Exhibit 219, your meromixis and nitrogen article, and you used the word "guesswork" in answer to one of Ms. Goldsmith's questions. Do you recall that?
A I do, yes.
Q Okay. Now, when Dr. Herbst told you or communicated to you that, quote, nitrogen fixation in benthic algal mats is nearly double at pre-1941 salinities than at current salinities," and you saw fit to include that in your paper, was Dr. Herbst guessing that fact, or had Dr. Herbst actually done some

scientific experiments to show that fact?
A You've really distorted the prior questioning. The question was whether the whole lake supply of nitrogen would be doubled at pre-diversion salinities, and the answer to that question was, based on the testimony I gave, that that would be pure guesswork.
The experiments that Dave did were laboratory experiments, which he has yet to have published but which he showed us the results of and which we deemed were sufficiently accurate to include in that paper. So I was not in any sense implying that Dave's work was guesswork. I was simply saying, in answer to Jan's question, that that was -- extrapolating those data to the whole lake was guesswork.
Q Is it equally guesswork to say that there would be no benefit in nitrogen fixation if you decreased salinities to pre-41 levels?
A Say that again?
Q Would you be guessing just as much if you were to testify that there would be no benefit in overall

nitrogen availability in Mono Lake if you reduced the salinity to pre-diversion levels?
A Based on our existing knowledge, yes.
Q Dr. Kimmerer, a couple of follow-up questions. In the interim I've had the availability -- I've had the

0123
chance to read the whole report in which the one document was just a part. And I want to ask a couple of follow-up questions because I think there was some un -- lack of clarity in my prior questions.
I was attempting to ask you on direct examination what -- whether four different assumptions were allowed for in this model. Whether or not there was an increase in habitat density as salinity decreased. Whether or not there was an effect of the smaller adult body size making it harder to get food, vegetation as a hard substrate. And the final one I asked you was whether or not there was a consideration of the effect of salinity on the fly food source, and I recall you telling me that that was considered.
Did I hear you wrong?
A BY DR. KIMMERER: I didn't hear that question. The question that I heard was, was there an effect of body size on the ability of the fly -- the larvae to find food.
Q Okay.
A I answered that question.
Q Yeah. Let me reask it, then. Is it not correct that you assumed -- your model assumes across all the salinity ranges a constant food supply to the flies?
A It assumes that the -- that changes in the food

0124
supply are not -- are not affecting the flies other than through growth rate which is included as an effect and the body size.
Q Growth rate is assumed to change as a function of salinity, right?
A Well, okay. On Page 4, all the assumptions and all the -- the information that's used based on the experimental results is listed, and so it's all right here. If you want me to go through it, I will.
Q Actually, let me focus on a different thing. The experimental data that you're referring to in which -- which showed that as you increase salinity, growth rates decreased. You understood that to be the experiment?
A Yes.
Q In those experiments, was the food supply kept constant?
A It was not manipulated.
Q Right. So it was a constant food supply for the flies?
A I don't know that it was constant, but it was not manipulated.
Q So it was the same amount of food with cross salinities?
A I didn't say that. If the salinity changes, and

0125
that changes the food supply during the experiment,

then the food supplies could have been different.
Q Okay. But you don't know one way or the other about that, do you?
A It was not manipulated. Okay? It was not expressly changed.
Q Do you know whether or not the food supplies changed?
A No, I don't know.
Q Okay. If I ask you to assume that the food supplies were not changed.
A Okay.
Q Then the effect of food availability as a function of salinity would not be incorporated into your model; is that right?
A The effect of food concentration would not be, but availability also includes the ability of the animals themselves to get food. So in that case, it could still be affected.
Q Now, you conclude -- you discuss -- if I can find it here -- on Page 8 of your report, that -- the bottom paragraph under Changing Elevation. You say, "Changes in lake elevation influence both biomass and production, although there was little effect of lake elevation on the time at which biomass saturates."

Ô

0126

Then you say, "The peak in production occurs at a lake elevation of about 6384 feet." You cite Figure 6.
That's your conclusion from your model?
A That's my conclusion from this set of runs in this model which has not been -- I've not done the full sensitivity analysis. This is not a complete piece of work here.
Q Okay. And again, assuming that the Water Board does not have an infinite time, and we all don't have an infinite budget, and assuming that this is the best available data to the Water Board, then this would be your conclusion?
A It wouldn't take an infinite amount of time and money.
Q Assuming --
A Quite finite, actually.
Q Assuming that this is the best available information to the Water Board, that's your conclusion?
A This is what -- this is what the model produces given the assumptions.
Q I didn't --
A A model -- let me just explain this. A model is just the result of the series of assumptions that you put into it. If you make the assumption clear, then either you agree with the assumption or you don't. If

0127

you don't agree with the assumptions, then that leads you to do one thing about it, which is to try to revise the model according to your new assumptions.
If you agree with the assumptions and the model is done correctly, then you can't escape the conclusions, okay? Given these assumptions, I assert that I believe this model is done correctly. This is the conclusion you'd arrive at.

Q Okay. Then you go on to say, "Production decreases sharply above and below that. In particular, the decrease below the present elevation of 6375 feet is precipitous."
A Um-hum.
Q That has the same degree of certainty and conclusiveness in your view that the prior sentence has; is that right?
A Yes.
Q Okay.
A I think the term "precipitous," of course, is a choice of words, but -- it's a steep choice. It's a steep decline, precipitous may be an overstatement.
Q Who picked the word "precipitous" for this report?
A I did.
Q At the time you thought that was a good word?
A Yeah.

0128

Q Who -- you weren't being paid by DWP at the time you wrote this report, were you?
A Indirectly? Yes.
Q But through Jones and Stokes?
A Yes.
Q And now you're being paid directly by DWP?
A Yes.
Q One final question to Dr. Melack. A duck question. Since you don't have an opinion upon whether or not the migratory duck population is healthy or not, let me ask you to assume that it is not healthy. Does that affect your conclusion about the health of the ecosystem?
A BY DR. MELACK: Not at all because the migratory duck population depends on resources scattered all over North America, and I think it's going to be hard-pressed to think that in contrast with the grebes, that Mono Lake is a potential resource for those migratory ducks.
Q Do you have any idea how many ducks there were before diversion?
A I have read the same material that most people have read about that.
Q What did you read?
A I read the Draft EIR, and I read various testimony
Ô

0129

of various people about the subject. At face value, they're highly speculative numbers.
Q They're high numbers, aren't they?
A Sure there's high numbers, but "high" is a relative word, Mr. Flinn.
Q Sure. Let's say they decline from a million to less than 10,000. Under your definition of a healthy ecosystem, that's okay?
A Hardly.
MR. FLINN: Thank you.
HEARING OFFICER del PIERO: Thank you very much.
Mr. Roos-Collins?
MR. ROOS-COLLINS: No questions.
HEARING OFFICER del PIERO: No questions.
Ms. Scoonover, I take it you have none?

MS. SCOONOVER: No questions.
HEARING OFFICER del PIERO: No questions.
Ms. Niebauer? No questions?
Anyone else have cross? Staff? Mr. Canaday, don't go away mad. You have a meeting with Senator Leslie?
MR. CANADAY: Yes, I need to brief Senator Leslie.
HEARING OFFICER del PIERO: You have a nice afternoon, Sir.
MR. CANADAY: Thank you.

0130

HEARING OFFICER del PIERO: I wish it were me.
MR. CANADAY: It's more fun here.
(Laughter.)
MR. BIRMINGHAM: For Mr. Canaday's benefit, can I move to strike that?
(Laughter.)
HEARING OFFICER del PIERO: Gives you some idea of how exciting his social life is, doesn't it?
MR. DODGE: I have a procedural question.
HEARING OFFICER del PIERO: Thank God. We were worried you were going comment on my comment.
(Laughter.)
MR. DODGE: One of Mr. Flinn's statements or questions, and there seemed to be some doubt as to whether it was that statement or question, led me to a question about what illegal tag teaming is, and my question is am I allowed to object to Mr. Flinn's questions?
MS. GOLDSMITH: Mr. Dodge, be my guest.
HEARING OFFICER del PIERO: I don't know, Mr. Dodge. Let me take that under --
MR. THOMAS: We'd like a ruling on that. I have several thoughts on that matter.
HEARING OFFICER del PIERO: Why don't all of you interested brief me on that, and I'll render a decision

0131

later on whether you can object to Mr. Flinn, although I don't think that my ruling's going to have any bearing on whether or not he objects to Mr. Flinn. Okay.
Mr. Herrera?
MR. HERRERA: Yes. I just have a few more questions here.

RECROSS EXAMINATION BY THE STAFF

Q Dr. Melack, going back to your statement in your testimony that you were concerned about the Draft EIR's use as a 25 percent criteria of significant effect. Are you aware that in the example you gave of a natural variability of upwards of 200 percent as common or as a natural occurrence in the lake, are you aware that the Draft EIR used the 25 percent criteria, not 25 percent of 200 -- but as 25 percent of 200, I mean, which, in a sense, is a 50 percent change?
A BY DR. MELACK: I'm aware that it was a plus or minus 25 percent. That's correct.
Q But in reality it was a 50 percent change in the range, yes. Because there's 25 percent of 200? I was wondering if you were aware of that point, that it wasn't just 25 percent, it was 25 percent of the change

which is -- you're saying naturally occurred was 200 percent. The Draft EIR assumed that 200 percent was

0132

there, subsequently they evaluated 25 percent of the 200.
A I understand that. Although, just to clarify something here, what was actually used in the Draft EIR was not the observed natural variability. What was used was instead results from model output and what was taken -- there were -- model output over a course of about ten years, those model outputs then were taken, and the range of model outputs was used as the range of natural variability. And then of that, 25 percent was taken. So, in fact, what was used was a model output, not real natural variability. But even if -- I mean, I understand that. But even if they had used natural variability, I guess my argument to you would be we should use what we've observed. There's no basis for taking 25 percent or 50 percent of that number. We should use the full range that we observed.
Q In that full range, what would you consider significant then?
A I would consider nothing that exceeded -- I would consider nothing that did not exceed that -- am I saying this right? I would consider only a variation which exceeded that range as being significant.
Q So everything within -- any change within the 200 percent example that you gave is not considered

0133

significant?
A That's what I would say. That's correct.
Q Even over your long-term -- is 14 years considered long-term?
A That's correct.
Q Okay. I have another question relating to -- you also suggested long-term monitoring as part of the future, and would be, I'm assuming that's for areas, especially those areas that you don't have any hard core or have any actual results or studies on.
Do you still suggest you need to do monitoring for those areas that you have a 14-year basis to determine the effects on it?
A Well, it depends on what range of elevations you're dealing with.
Q Let's say the State Board selected 6377. Do you still consider we need to do monitoring, long-term monitoring at that level?
A At a reduced level. I think it's only sensible, just like we monitor our own personal health, that you monitor an ecosystem that you're interested in. You never know what happens. Maybe there's an accident, and somebody dumps a load of petroleum products into Mono Lake. It would be nice to know what happens if that happens. So I think it's prudent to monitor

0134

important natural resources even when we have a good knowledge base. And if we extend our levels outside of our knowledge base, it becomes even more critical.
Q So in other,words what you're advocating is just

continued monitoring regardless of the lake levels?
A I'm advocating that, yes.
Q In your discussions as well, you indicated that we need to look at the ecosystem as a whole. We need to look at not just the specific instances of salinity or nutrient loading or any of that effect. Does that take into consideration the additional nutrient loading or the additional ecosystem associated with the fresh-water streams depositing or adding things to the ecosystems in Mono Lake?
A By all means.
Q Are they considered part of that ecosystem evaluation?
A The inputs of water and associated chemicals in the streams is certainly part of the ecosystem as would be if, for example, there's increased development and nutrient supply from human activities. Any of those things would constitute part of the analysis, sure.
Q In your studies, have you evaluated the effects of the various instream flows there?
A We've evaluated the effect of the fresh water as

it is affects mixing, and we've measured the concentration of nutrients in the fresh waters as a source of nitrogen and phosphorous to the lake.
Q You did evaluate, then, the nutrients that were being contributed by the instream flows?
A That's correct, yes.
Q What range -- again, that was from 6372 to 81 is the range you evaluated what the flows would be?
A Yeah. It was during the period over the last -- actually, roughly, about ten years we've done that. But it included the very high flows during the El Nino years and has included the drought flows, which were essentially zero. That's correct.
Q We've heard a fair amount of testimony regarding grazing activities in the watershed in which, in some cases, it was extensive. That may have added a fair amount of nitrogen to these streams. Would you consider the nitrogen contribution these streams had to be high, moderate?
A No. It's quite low, actually.
Q And why is that?
A Why is it low? It's low because most of the water that comes out of the Sierra is melting snow, which has extremely low nitrogen content and is basically flowing through fairly unweatherable terrain. And there is a

potential effect, you're right, on the water quality, but we certainly haven't seen that.
Q Mr. Tillemans testified that there was an incidence just in passing of 2,000 sheep that crossed the county road on Lower Lee Vining Creek. I would suspect that just their presence would add a significant amount of nutrients?
A Your point is well-taken. I should add a certain caveat here, and that is that we don't sample the streams daily. We sample them every couple of weeks, and so what you described is certainly a real

possibility. The odds are we wouldn't have sampled that.

To really do that kind of analysis correctly, you have to put out -- which is possible, a monitoring device which samples the water regularly. You could see an effect, perhaps, from that, but the data I have certainly wouldn't allow us to make that judgment. Although, it's a really interesting point you're making.

MR. HERRERA: I think that concludes my questions. Thank you.

MR. FRINK: I believe Mr. Smith has one quick question.

Q BY MR. SMITH: One quick question. I see you have

authored a couple of articles with Dr. Jellison?

A BY DR. MELACK: That's correct.

Q Would you consider him a research scientist?

A That's a loaded question. He was a Ph.D student of mine, so I certainly would be very biased in my interpretation of him.

Q Would you respect his opinion?

A Yes, for sure.

MR. SMITH: Thank you.

Q BY MR. FRINK: I wanted to follow up along that line. I understand that a number of the papers that you have done on Mono Lake were jointly authored with Dr. Jellison; is that correct?

A That's correct, yes.

Q On Page 1 of your written testimony, you've stated that, "A management plan which maintains lake level elevations from 6372 to 6381 would be warranted." Is that your recommendation to this Board that it adopt a management plan calling for lake elevations of between 6372 and 6381?

A Based on the status of the off-shore community, that's a fair range of elevations, yes.

Q Okay.

A That's a -- the answer is, therefore, not categorically yes, it's yes in the context of these

Ô

data that I'm describing in this chapter.

Q Okay. Would you have another basis for recommending any alternative lake level or lake level management plan?

A You're asking me to serve the function of this whole --

Q I'm asking you what your recommendation, as one who has studied at least some aspects of the Mono Basin for a number of years, is what is your recommendation to the Board as to the lake level alternatives that they should aim to achieve?

A Okay.

MR. DODGE: I think the question, to the extent the witness should be allowed to answer it, has been asked and answered. He said based on the offshore community, which is what he studied, that that lake level would be consistent with his research. But to go further than that and ask him to, in effect, make the

decision this Board is being asked to make, I think is improper.
MR. FRINK: I'll withdraw the question --
HEARING OFFICER del PIERO: Actually, I have to tell you, I know the Board members want an answer.
Do you have a more specific answer than the range you gave? Your response to that question is either yes

0139

or no.
DR. MELACK: No.
HEARING OFFICER del PIERO: No. Then I'm going to accept the range that you gave us as being a range within which you believe, given the context of the statement you made. Is that correct?
DR. MELACK: That's correct, yes.
Q BY MR. FRINK: I believe Mr. Smith asked you a couple -- a couple of minutes ago if Dr. Jellison is the sort of research scientist whose opinion you would respect; is that correct? And you answered yes, you would?
A That's correct.
Q Are you aware that in the hearing before this Board in Mammoth Lakes on October 5th, that Dr. Jellison recommended that the Board adopt an alternative providing a water elevation in Mono Lake of 6390?
A I'm aware of that. Yes.
Q After working closely with him on a number of papers and hearing that he has recommended a significantly higher lake level than is recommended in your testimony, would you want to reevaluate your recommendation in any way?
A No, I wouldn't.

0140

Q Have you discussed with Dr. Jellison the reasons for his recommendation?
MR. BIRMINGHAM: Excuse me, Mr. del Piero. I'm going to have to object to this. Mr. Frink is bringing into this evidentiary hearing a statement made during a policy session at which we had absolutely no opportunity to cross-examine Dr. Jellison. We'd be more than happy to bring Dr. Jellison in here and let him testify, but we were left with the impression that the statements made in those policy sessions were not evidentiary, and so I'm going to object to these questions.
HEARING OFFICER del PIERO: Mr. Flinn?
MR. FLINN: I take a contrary view because I don't believe the question was designed to elicit the fact of Dr. Jellison's testimony or his view as an evidentiary fact. The pending question is simply did he discuss with his colleague a particular view and that may well -- his discussions with his colleague is a fairly typical thing for experts to do. They're a team together. They discuss things. I think that the pending question is admissible.
HEARING OFFICER del PIERO: I think the policy statement submitted by Dr. Jellison during the course of the policy statements, pursuant to the Board's own

Ô

rules, that document itself is not admissible as evidence.

Alternatively, I'm going to overrule the objection rendered by Mr. Birmingham because the questions that Mr. Frink is asking go directly to conversations that took place between not only colleagues, but co-authors in terms of some of the documentation that's been entered as evidentiary material by many parties including the L.A. Department of Water and Power. I know the other Board members are interested in knowing whether you've had conversations with him about that issue.

DR. MELACK: Fair enough. Let me respond two ways. One is Bob and I have worked together a lot and the testimony that I submitted, the written testimony, I worked with Bob, and he actually, in a sense, passively endorsed that testimony as a statement of the state of knowledge that studies have given us and what that says about the lake's functioning. So I don't want to put words in Bob's mouth, but I know he agrees with me that the lake certainly has been healthy and surprisingly stable over these last 14 years.

His opinion about higher lake level is a freely-expressed opinion. I might, perhaps, give you another way of evaluating that. If you look at the

National Academy of Sciences' book on Mono Lake, and if you read the foreword, I think it is, which was written by the chair of that committee, in that foreword he includes comments that he elicited from the members of that committee subsequent to the completion of that report. And he was asking people what their views of Mono Lake were as an aesthetic, as a natural site. And those comments were very positive, and people are struck certainly by the beauty of Mono Lake.

And so I think it's only reasonable to expect that Bob, who has lived there for 15 years, would clearly endorse a whole variety of aesthetic features of the Mono Basin as being positive at higher lake levels and, at the same time, and quite consistently, agree that the plankton communities are indeed healthy in terms of lake levels substantially less than that.

So both are legitimate points of view and that Bob is legitimately expressing, or could legitimately express, both points of view.

Q BY MR. FRINK: Have you any indication from him that the basis for his recommendation was solely visual or aesthetic considerations?

A BY DR. MELACK: I'm not sure that's a fair question.

Q Did he state that, you know --

HEARING OFFICER del PIERO: Wait. Wait. Wait,

Mr. Frink.

Mr. Melack, I get to determine what's a fair question.

(Laughter.)

HEARING OFFICER del PIERO: Mr. Dodge -- not Mr. Dodge, not Mr. Birmingham, certainly not Mr. Flinn. Just me. So if you'd be kind enough to

respond to the question, and I'll have Mrs. Anglin reread it back to you. Okay?
(Whereupon the record was read as requested.)
DR. MELACK: Not that it was solely that, but I certainly know that it wasn't based on the scientific evidence we presented here. It was based on much more than that. Personal friendships, personal experiences, I know were a factor.
Q BY MR. FRINK: Have you taken into account in your recommendations visual or aesthetic factors, or are your recommendations based solely on the work you've done regarding brine shrimp and the lake ecosystem?
A Throughout this whole proceeding, which has now extended for many years, I've tried to take the position that the decision making should be based on the variety of evidence. And one should be scientific, and so I've taken upon myself to try as much as possible to provide to the decision-making people what

0144

I consider to be credible scientific evidence and, therefore, what I presented here was that. It was an attempt to be as objective as possible to make available to the State Board as objective as possible data with no -- as much as is humanly possible to avoid, no bias associated with those interpretations.
Q But answering my question, did you take into account visual or aesthetic considerations in making your recommendation?
A No, I did not.
MR. FRINK: Thank you.
HEARING OFFICER del PIERO: Any other questions of the staff? Questions by Board members?
Gentlemen, thank you very much for your time. We appreciated it very much.
We have -- is it correct -- I'm not quite sure. Mr. Birmingham, are you going to be doing the next witnesses, or is it --
MR. BIRMINGHAM: I will be doing the next witnesses.
HEARING OFFICER del PIERO: Okay. And you have two, Sir?
MR. BIRMINGHAM: Mr. Frink has requested that we present Dr. Hardy and Mr. Hanson as a panel.
HEARING OFFICER del PIERO: Is Dr. Hanson here?

0145

MR. BIRMINGHAM: He is here, and we will present them as a panel.
HEARING OFFICER del PIERO: Let's go ahead and proceed that way.
Mr. Frink?
MR. FRINK: Mr. del Piero, before we get into the next panel of witnesses, I believe there will be one housekeeping detail.
Mr. Birmingham, Los Angeles Department of Water and Power, in connection with the testimony of these witnesses has identified Exhibits 22 through 33. Do you wish to offer those into evidence at this time?
MR. BIRMINGHAM: Pursuant to Mr. del Piero's request at the conclusion of Dr. Chapman's testimony,

we were going to offer all of our --
HEARING OFFICER del PIERO: All of them at the same time.
MR. FRINK: Okay.
MR. BIRMINGHAM: I understood that was the Hearing Officer's --
HEARING OFFICER del PIERO: That's correct.
MR. BIRMINGHAM: Thank you, Mr. Frink.
HEARING OFFICER del PIERO: Why don't you go ahead and call your next two witnesses, and I'll administer the oath if they've not been --

0146

MR. BIRMINGHAM: They're next-door. May we take a recess to bring out the material --
HEARING OFFICER del PIERO: We'll take ten minutes.
(Whereupon a recess was taken.)
HEARING OFFICER del PIERO: Mr. Dodge?
MR. DODGE: Mr. Chairman, Mr. Birmingham and I were talking over the recess, and we have a couple of procedural issues to bring up. I think both of us are in agreement. The first is that I had indicated earlier that I was not aware until an hour ago, or a few minutes ago, that these two witnesses were going to be presented as a panel and that I was not ready for Mr. Hardy. You indicated that I'd be allowed to examine him tomorrow morning.
HEARING OFFICER del PIERO: That's correct.
MR. DODGE: The second issue, I would very much like to send Mr. Flinn home and just --
MR. BIRMINGHAM: We do agree on that.
(Laughter.)
MR. DODGE: I asked Mr. Birmingham whether when we finished with his fish witnesses he intended to call another witness tomorrow, and he indicated he did not. Now, if the Board is going to stop when we finish DWP's fish witnesses, I will send Mr. Flinn home. If you're

Ô

0147

not going to stop, then I can't do that. And I wondered whether I could get an advance ruling on that.
MR. BIRMINGHAM: Our situation is this, Mr. del Piero. We had intended to present Dr. Hardy and then Mr. Hanson as individual witnesses as opposed to a panel. We have one more fishery witness, Dr. Emil Morhardt who will testify, presumably, tomorrow. If -- in addition, doctor or Mr. Hanson has a scheduling conflict this evening, and we were informed during the recess that we were going to go with this panel tonight until we finish. That presents some problems inasmuch as Mr. Hanson was not aware of that and does have a scheduling conflict tonight.
So what we had hoped was that we could go until approximately five o'clock this evening, break, and then resume with his testimony tomorrow. And then I think the remainder of his testimony tomorrow and Dr. Morhardt's testimony would consume most of tomorrow.
Therefore, we had not anticipated calling our next

lake witness, Dr. Joseph Jehl, until the next hearing date which would be the first date next week.

HEARING OFFICER del PIERO: Can you have him here tomorrow?

0148

MS. GOLDSMITH: Dr. Jehl?

HEARING OFFICER del PIERO: Yes.

MS. GOLDSMITH: I'll have to call him. I think it would present some problems.

HEARING OFFICER del PIERO: Losing a day because of witness scheduling poses a problem, too, and I'm not -- understand. I have full appreciation for the problems in terms of scheduling witnesses on this --

MS. GOLDSMITH: In all candor --

HEARING OFFICER del PIERO: The reason that these two were asked to be brought on as a panel was for us to us try and improve what is taking an inordinate amount of time in cross-examination. Whatever benefit would have accrued in terms of that will be completely lost, plus some, if the time line that you have laid out, Mr. Birmingham, and I'm not being critical, I'm just telling you, the time line, the way you've laid it out is, in fact -- in fact, results, we will not have gained a minute and, in fact, probably lost more time than even I could have anticipated.

MR. BIRMINGHAM: We do have -- we do have some witnesses here. We would be calling them out of order, but we could put them on tomorrow if necessary.

HEARING OFFICER del PIERO: Are you going to be prepared to cross-examine?

0149

MR. DODGE: Well, yeah. More to the point, I'll be able to send Mr. Flinn home if he's talking about Mr. Tillemans who I plan to examine.

MR. DODGE: We can put Mr. Tillemans on tomorrow, and --

HEARING OFFICER del PIERO: Mr. Tillemans, were you intending to leave tomorrow, Sir?

MR. TILLEMANS: No, I'll be around. I wasn't intending to testify tomorrow.

(Laughter.)

HEARING OFFICER del PIERO: That will teach you to show up here.

MR. BIRMINGHAM: If you order him to be here, he can't go anywhere. But that would actually create additional need for us not to go late into this evening because we would want to sit down with him on his testimony.

HEARING OFFICER del PIERO: I appreciate that. You know -- you aren't ready to do Mr. Hanson today? Right?

MR. DODGE: I am ready to do Mr. Hanson.

HEARING OFFICER del PIERO: I'm sorry. Mr. Hardy? You are not prepared to do. What do you need Mr. Flinn for?

MR. DODGE: I don't.

Ô

0150

HEARING OFFICER del PIERO: For any of these witnesses?

MR. DODGE: None of the fisheries witnesses and if the next witness tomorrow is going to be Mr. Tillemans, I don't need him for that either.

HEARING OFFICER del PIERO: Where's the -- what's his name?

MS. GOLDSMITH: Dr. Jehl is in San Diego, and we had anticipated taking Thursday to prepare his testimony.

HEARING OFFICER del PIERO: This hearing's not continued on Friday, it's Monday.

MS. GOLDSMITH: That's right.

HEARING OFFICER del PIERO: You're working on Veteran's Day?

MS. GOLDSMITH: I'm not a veteran. I may change that status after this hearing.

MR. BIRMINGHAM: I'm taking Veterans Day off in honor of any of those members of the Board who are veterans.

(Laughter.)

HEARING OFFICER del PIERO: I'm out of quick repartee.

MR. DODGE: Mr. Birmingham has sunk to a new low.

(Laughter.)

0151

HEARING OFFICER del PIERO: Either that or he's risen to a new high. I'm not sure.

MR. DODGE: I would note that the National Audubon Society is comprised of 100 percent of veterans. Millions of them.

(Laughter.)

MR. BIRMINGHAM: I think we've resolved the question that we needed to resolve with the exception of Mr. Hanson --

HEARING OFFICER del OFFICER: That Mr. Dodge is as full of it occasionally as we think he is? Is that the question that needs to be resolved?

Okay. Look. Let's do this. Let's go with Mr. Hanson now. Okay? Mr. Hardy, we're going to go with you first thing in the morning. Okay?

Mr. Birmingham, you need to be prepared to have your other fisheries specialist and our good friend over here ready to go tomorrow.

Mr. Flinn, go home. Okay?

MR. FLINN: Thank you.

HEARING OFFICER del PIERO: Look at the smile on that man's face.

MR. BIRMINGHAM: The ratepayers of the City of Los Angeles thank you.

HEARING OFFICER del PIERO: Mr. Hanson?

0152

MR. HANSON: I have not been sworn yet.

HEARING OFFICER del PIERO: I'll swear at you in a second here, Sir.

Mr. Hardy, if you'd be kind enough to go take a seat in the audience, we'll take you up first thing tomorrow morning, Sir.

Now, for the attorneys, we'll be here at nine o'clock tomorrow, and if I have to go through lunch tomorrow, we're going to go through lunch tomorrow to get all the witnesses out of the way. Let me point out

the obvious. From now on, I know it may be difficult, but I want you all, all of you, to anticipate that we are going to go without any major breaks. So you need to have your witnesses all lines up. And in the case of witnesses that may be coming from a long distance, you need to be prepared to have them here. So they need to be notified in advance of what's taking place.

It's not my intent to cause people to be miserable during this process. We're going to go into night sessions with the express purpose of moving this process along. I don't intend to schedule night sessions if people aren't going to be here to be examined. So -- and since I intend for them to be examined, I expect them to be here. Okay.

Mr. Birmingham -- Mr. Hanson, would you please

Ô

0153

stand and raise your right hand? And the appropriate response after I get done is "I do" or "yes."

Do you promise to tell the truth during the course of this proceeding?

MR. HANSON: I do.

HEARING OFFICER del PIERO: Please be seated, and Mr. Birmingham, why don't you begin?

MR. BIRMINGHAM: Thank you very much, Mr. del Piero.

DIRECT EXAMINATION BY MR. BIRMINGHAM

Q Mr. Hanson, I'm placing before you a pile of documents that I'll ask you about in a few moments. But first, would you please state and spell your last name for the record?

A BY MR. HANSON: Hanson, H-A-N-S-O-N.

Q And, Mr. Hanson, where are you employed?

A I'm employed as EA Engineering Science and Technology.

Q And L.A. DWP Exhibit 13 is a document entitled the Direct Testimony of David F. Hanson. Is that direct testimony which you prepared in connection with this proceeding?

A Yes, it is.

Q And I've placed in front of you L.A. DWP Exhibit 14, which is a document entitled Professional Profile

0154

of David F. Hanson. Is L.A. DWP Exhibit 14 a true and correct copy of the professional profile of David F. Hanson?

A Yes, it is.

Q And does that document, L.A. DWP Exhibit 14, accurately state your educational --

A Yes.

Q -- and work experience?

A Yes, it does.

Q And your professional qualifications?

A Yes.

Q L.A. DWP 15 is a document entitled EA Engineering Science Technology 1990 Instream Flow Analysis for Lower Rush Creek, Mono County, California, Preliminary Draft. Is L.A. DWP Exhibit 15 a document which you prepared?

A Yes, it is.

Q And did you use L.A. DWP Exhibit 15 in connection with forming opinions which you would express -- that you have expressed in your written testimony?
A Yes.
Q L.A. DWP Exhibit 16-A is a document entitled EA Engineering Science Technology, 1990 Draft Report Fish Population in Lower Rush Creek 1985 to 1989. L.A. DWP Exhibit 16-B is a document entitled EA Engineering

0155

Science and Technology Draft Report Fish Population in Lower Rush Creek.
Are L.A. DWP Exhibits 16-A and 16-B documents on which you relied in forming opinions which you expressed in your written testimony?
A I relied principally on 16-A. I did not rely hardly at all on 16-B.
Q Were you involved in the preparation of 16-B?
A No, I was not.
Q Does L.A. DWP Exhibit 13 accurately state your testimony or -- let me state that differently. Are there any corrections that you'd like to make for L.A. DWP Exhibit 13, the direct testimony of David F. Hanson?
A Yes. There are a couple of labeling questions or labeling errors I'd like to correct.
Q Would you identify those, please?
A Yes. On Page 49, Figure 2, there's a mislabeling of the lines. The legend shows the dashed line representing EA study, the solid line representing CDFG studies. Those should be switched. The dashed line is the CDFG study, the solid like is the EA study.
Also, on Page 51, CDFG Figure 18, again, there's mislabeling of the icons; the dot is labeled on that graph as "adult," it should be "fry." And conversely,

Ô

0156

the square is labeled "fry" and that should be "adult."
The other point that I'd like to make is that the testimony that I prepared here was based on information I had while I was reviewing the Draft EIR. I am aware now that there is a more up-to-date report for Lee Vining Creek than the one that I developed my testimony on.
Q Is that a more recent report developed by the Department of Fish and Game?
A I believe it is, yes.
Q And is it your understanding that that more recent report was issued by the Department of Fish and Game subsequent to the preparation of your written testimony?
A Yes.
Q Is it possible that the Department of Fish and Game report would change some of the opinions that you've expressed in your written testimony?
A Yes, it may.
Q But you have not reviewed that report for a presentation of your written testimony; is that correct?
A That's correct. Not to the extent that I have my

testimony today.

0157

Q Would you briefly summarize L.A. DWP Exhibit 13, the direct testimony of David F. Hanson?
A All right. I'll start with a summary of my education and experience. I have a bachelor's degree in zoology from UC Santa Barbara in 1953, a master's degree in wildlife science and fisheries from Utah State University in 1978. At the end of my master's program, I worked one year for a consulting firm W.F. Sigler and Associates, that's S-I-G-L-E-R, in Logan, Utah. The work I did for that firm was a development of a population model for cut throat trout in the Truckee River.

In 1978, I joined EA Engineering Science and Technology and since 1980, have worked mostly in instream flow studies using the instream flow incremental methodology or the IFIM. I'll use that term quite a bit throughout my testimony today. Over the course of the last 12 to 13 years in doing instream flow studies, I've worked on 50 or so different streams and rivers mostly in California but also in Oregon, Montana, Pennsylvania, West Virginia, Ohio, Georgia, and New Zealand.

The instream flow studies that I've been involved with in the Mono Basin comprise the Lower Rush Creek study. I was involved in the instream flow studies of

0158

Lower Rush Creek. That's what generated the report that I produced. I've also participated in the instream flow studies on Upper Rush Creek working on hydroelectric projects for the Southern California Edison Company. That section of the Upper Rush Creek that I worked on is the reach between Waugh and June Lake.

I've also worked on the performance of instream flow studies of Upper Lee Vining Creek between Hilary and Saddlebag Lake and on Mill Creek, one of the other tributaries that flows into Mono Lake. All those studies were done for Southern Cal Edison Company.

Also, throughout the eastern Sierra-Nevada, I have done IFIM studies, again, related to hydroelectric projects on Bishop Creek, Birch Creek, McGee Creek, and Green Creek, also on Independence Creek, and Big Pine Creek.

In addition to those experiences, I've worked electrofishing surveys on several of these streams; those are Rush Creek, Green Creek, Upper Rush Creek, Upper Lee Vining Creek, and Bishop Creek.

Now, the focus of the testimony that I am going to provide today is recommendations for minimum stream flows in Rush Creek and Lee Vining Creek. These recommendations are based on instream flow studies

Ô

0159

done by California Department of Fish and Game and EA, in the case of Rush Creek, and the California Department of Fish and Game and EA, in the case of Lee Vining Creek.

And I thought that before I got into my

recommendations, I would briefly go over what the instream flow incremental methodology is. It's a fairly complicated methodology for developing a functional relationship between stream flow and habitat that is based on a sampling procedure whereby cross-sections are put across a stream, essentially, taking a tape measure and running it across a stream and identifying different stations along that tape measure that define different points on the stream having different depths and velocities and other physical characteristics such as substrate and cover.

These transects are the basic sampling protocol for the IFIM. They're placed in different so-called macro-habitat types throughout the stream such as pools, riffles, and runs, and what we sometimes call rock gardens.

Each of these different macro-habitat types are assumed to have different hydraulic characteristics, and that's the reason why there are different transects put through them. Generally, the sampling methodology

0160

involves some sort of stratifying random design where the strata, in this case Rush Creek, there were different reaches of the river that were identified as strata, and sampling was conducted in these macro-habitat types. So it's sort of like a two-stage stratified random design; the first level of strata being the reaches of the river. The second level being these macro-habitat types within the reaches, and then the samples, these transects across the river, these cross-sectional transects placed randomly within the different habitat types.

In Rush Creek there were a total of 75 different transects over the course of the river from Grant Lake down to the Mono Lake. At each and every one of these sampling locations, these cross-sectional sampling locations, a series of measurements are taken primarily of the bed profile to define what the river looks like from bank to bank.

That's very important from the standpoint of looking at depth changes. Water surface elevations are then measured at four -- in this case, four, sometimes three, as many as you want, but in this case, four different stream flows. In this -- in the case of the Rush Creek study, there were 13 -- excuse me. 13 cfs, 19 cfs, 60 cfs, 100 cfs were the stream flows at which

0161

water surface elevations were taken.

Velocity measurements are also taken in each of the cells at one or two or three of the different stream flows. Two of the stream flows velocity measurements were taken on Rush Creek. Those are used as, certainly, measurements of velocity and for simulation of velocities.

Based on all these measured bed profiles and water surface elevations and velocities, hydraulic simulations are performed to simulate what the changing river would look like in terms of elevation rise, spreading of the river at a broader range of stream flows generally than flows that you measure, and also

filling in the gaps. For example, wanting to know what the depths of velocities at 75 cfs are, having not measured those, you go to these hydraulic simulation models. They predict the depth and velocities in the cells over a broad range of stream flows.

Once you've predicted these depths and velocities, all these cells across the river, you apply what are called habitat suitability criteria, which are the means by which the model translates physical data, physical characteristics of the stream into habitat variables. So the output of this transformation is a term called weighted usable area, which is an index of

Ô

0162

the habitat, a measure of the usable space in the river.

And to visualize it, imagine yourself up in a helicopter or an airplane overflying the river at a given flow, let's say 50 cfs. You look down on the river, and there's a certain area of the river that's wetted, and this could be measured by plenimentary or some other technique.

That wetted area, that actual area of the stream that's wetted, is not necessarily always usable, and it's the depths and the velocities and some of the other physical variables that dictate how usable it is and, of course, those change as a function of flow. So this 50 cfs, this total wetted area, is modified to what we call weighted usable area.

So the weighting is -- it's weighting of the usability of that wetted area by how good the depths and the velocities are. Some depths are too deep. Some depths are too shallow. Sometimes the water's too deep or too slow for different species and lifestage under consideration in the model.

These habitat suitability criteria are these things that tell us whether it's too deep, too shallow, and they are the driving variables that dictate that the weighted usable area is from the wetted area.

0163

The output, then, of the modeling exercise is this functional relationship between stream flow and weighted usable area. Weighted usable area is often shown as its acronym, WUA. You'll see that in lots of reports.

So that's my brief description of the methodology. I hope that was understandable because some of those terms you'll be hearing in my testimony and that of Dr. Hardy and I'm sure other witnesses that will come before you.

As I said, the focus of my testimony is minimum flow recommendations for both Rush and Lee Vining Creeks. To summarize what I have in my testimony, in my written testimony, I've recommended that you consider flows in the range of 20 to 30 cfs as minimum flows for Rush Creek and 15 to 25 cfs as minimum flows for Lee Vining Creek.

Also, I described in my written testimony that in addition to these minimum flows, I recommend that some form of channel maintenance or flushing flows of the

kind that Dr. Beschta was speaking of yesterday. I don't provide recommendations for these flows, but I recommend that they be considered and that in that consideration, such factors as maximum flow or the flow necessary to perform the channel maintenance function

0164

is considered along with the duration of that flow and the frequency of that flow. But I will not speak to specific values for that particular function today.

So let me turn to the Rush Creek, give you a background regarding how I've come to these flow recommendations that I have for Rush Creek. In 1987, the Department of Water and Power and the Department of Fish and Game agreed to a joint instream flow study to be performed on Rush Creek.

It was agreed in this joint study that a single group would collect all these transect data that I was describing to you, these physical variables along these transects. A firm by the name of Beak, B-E-A-K, Consultants was selected for this purpose. It was also agreed as part of this joint study that data generated from the Beak study would be separately analyzed by the two departments, DWP and Department of Fish and Game.

My involvement in the field aspect of the study was the following: I met with the Beak Consultants and Cal Fish and Game representatives to discuss the delineation of the different reaches. Remember, this is the first level of stratification I was talking about. And also to review the selection of the transects within these reaches and these different macro-habitat types. I also reviewed some of the

Ô

0165

habitat mapping studies that had been done throughout the reach -- throughout each of the different reaches, to determine what the distributions of these macro-habitat types were in the various reaches. I also reviewed and participated briefly in the data-collection techniques. That's the actual collection of the data across these transects. That's physical data, water surface elevation, and velocity data.

Once the data then were delivered to me from the analysis, or rather from the field work, I performed my own independent analysis and Beak Consultants performed their own independent analysis, and there are some differences between these analyses. And that's one of the things about IFIM, people go down slightly different pathways in doing these analyses, and there are certain differences that I viewed in comparing the two analyses that I'd like to bring out.

The first of these differences is a slightly different approach to -- not slightly different approach, but a different way of dealing with one of the problems related to the hydrologic simulation. As I've mentioned at several of the flows, the plan is to go out and collect water surface elevations across the transects. And the model assumes in most cases, in all

0166

cases, that the water surface elevation is uniform from

left to right bank. And this is certainly generally the case in pools, as you all know, from walking on stream banks, walking down streams as fishermen or whatever, that water surface elevations on left and right banks in a pool are generally the same. This is what the model's looking for, and it only allows for one water surface elevation. But in Rush Creek, as in many other streams that I've studied, oftentimes you find transects running across the stream where the water surface elevations are slightly different, where there's a higher water surface elevation on the left bank than the right bank associated with some sort of hydrology pull --

MR. BIRMINGHAM: Excuse me, Mr. Hanson. I wonder if you would slow down your speech.

HEARING OFFICER del PIERO: Ms. Anglin, are you doing okay down there?

THE REPORTER: I'm doing okay.

MR. DODGE: I hate to agree with Mr. Birmingham, but I'm having trouble getting it all down, too.

HEARING OFFICER del PIERO: We won't hire you as a Court Reporter. Okay?

MR. DODGE: I won't ask why.

MR. HANSON: So one of the problems that's

sometimes encountered in these studies is the water surface elevations are not necessarily the same. When I say "the same," I'm talking about a tenth of a foot difference. When it gets to be a tenth of a foot or two-tenths difference on either side, then you can start to have problems with the fact that you have to average that value, and you're not going to have as accurate a simulation as you might otherwise have with the uniformity of water surface elevation. And EA specifically developed a version of the hydraulic simulation model that was able to deal with multiple water surface elevations across a stream channel that I don't think was available to the Beak Consultants who were doing the analysis.

So I think that's one of the -- one of the differences in hydraulic simulation that I can evaluate by looking at the two studies. You can see in the report I produced some of the cross-sectional profiles that, in some cases, show you these non-uniform water surface elevations across the transects lines.

Perhaps the more significant difference in the analyses done by EA and by the Beak Consultants falls on the habitat suitability criteria. The habitat suitability criteria that are generally used in these studies are derived from generally one of two sources.

Ô

The first being literature values, data taken from other streams have led to the generation of habitat suitability criteria, can be utilized and are utilized in IFIM studies on a regular basis and were in this case, too.

The other option is to collect site-specific data, generally, the preferred option is to collect site-specific data, and that is generally accomplished

by snorkeling, actually going into the river and observing fish at different depths and velocities and determining the frequency distribution of that use level, and then using that frequency distribution as the habitat suitability criteria for the analysis.

There was some differences in how this was done. There were independent habitat suitability criteria studies performed in Rush Creek. EA did its own independent habitat suitability criteria and developed site-specific criteria for the adult and juvenile life stages of brown trout. We utilized literature values for the fry life stage.

The analysis done by Beak Consultants utilized site-specific suitability criteria for juvenile life stage brown trout and literature derived values for adult fry and spawning life stages.

You notice that I didn't indicate that spawning

0169

curves or spawning weighted usable area curves were generated in the EA report. I elected not to include spawning curves in the report because of my belief in the fundamental problems associated with simulating accurately relationships between spawning and weighted usable area in these kinds of studies. That's based on my experience in these studies in that spawning habitat in Sierra-Nevada streams is generally sort of a localized small pocket of gravels that are very poorly sampled using cross-sectional transects.

Cross-sectional transects work quite well in other streams, coastal streams, anadromous rivers where you have large gravel beds that are known to be used by, say, Chinook Salmon or some other anadromous species. And cross-sectional transects can actually be used to some degree in a mapping process to show the area of usable space over the spawning gravels at different flows.

When it comes to sampling, cross-sampling widely distributed pockets of gravel in and amongst the boulders in Sierra-Nevada streams, I think the sampling methodology breaks down. And I generally don't use spawning curves in these studies, and it wasn't used in this particular case.

In developing my minimum flow recommendations, I

0170

have evaluated the flow recommendations for Rush Creek that were generated by the Beak report and that came to me as a letter, a copy of a letter dated June 21st, 1993, from the director of the California Department of Fish and Game. I believe that these flow recommendations were taken primarily from the results of IFIM study as shown in Beak's report, and I'd like to start showing some of my figures that I have here. So if I could get -- I could get Figure 2 put up, please.

HEARING OFFICER del PIERO: Mr. Hanson, I'm going to give you, because we asked to you slow it down, I'm going to give you about three more minutes.

MR. HANSON: Three more minutes?

MR. BIRMINGHAM: Actually, Mr. del Piero, I'd like to make an application for additional time. I believe

on our original notice of intent to appear we listed doctor -- excuse me, Mr. Hanson's testimony as taking approximately 40 minutes given the complexity of IFIM and the central -- central fundamental nature of the issue which he is addressing, and it's actually the ultimate -- one of the ultimate issues that the Board has to decide. I would make an application for an additional 20 minutes.

MR. DODGE: We have no objection to that. This is

Ô

0171

complicated stuff.

HEARING OFFICER del PIERO: All right.

MR. HANSON: I may take an additional 10 or 15.

HEARING OFFICER del PIERO: So long as --

Ms. Cahill?

MS. CAHILL: No.

HEARING OFFICER del PIERO: Mr. Roos-Collins?

MR. ROOS-COLLINS: No objection.

HEARING OFFICER del PIERO: Fine. You're granted 20 minutes.

MR. HANSON: I would also --

HEARING OFFICER del PIERO: I forgot to ask. Ms. Goldsmith, if you want to object to Mr. Birmingham's request --

MR. BIRMINGHAM: No, she won't.

MS. FORSTER: Don't take that.

MR. BIRMINGHAM: Don't worry. She won't. We are in a very unique situation in that Mrs. Goldsmith is the chairperson of the department in which I work at Kronick, Moskovitz, Tiedemann and Girard, and so we're approaching the end of the year, and she's going to evaluate me, and so Mrs. Goldsmith doesn't have to take anything.

HEARING OFFICER del PIERO: While you're preparing --

0172

MS. STUBCHAER: I was going to say while we're interrupting, could I ask a couple of questions for clarification?

HEARING OFFICER del PIERO: Sure.

MR. STUBCHAER: Regarding the analysis of the section you were describing, you were describing profiles that you took with a tape measure. Are those longitudinal or transverse?

MR. HANSON: They're cross-sectional. Parallel to the -- or rather perpendicular to flow of the stream.

MR. STUBCHAER: In engineering parlance, profile normally means parallel?

MR. HANSON: These are the exact opposite.

MR. STUBCHAER: And are the water surface elevations measured by sticking the stream and getting a depth from the bottom, or are they surveyed in with relation to a bench mark?

MR. HANSON: They're surveyed in with relation to a bench mark.

MR. STUBCHAER: You survey the section first and then the water surface?

MR. HANSON: Yes. You establish a bench mark along the stream's edge. Sometimes it's these things

we call head stakes that are sort of like pieces of rebar that are pounded into the side of stream. We

0173

assume they don't change from measurement to measurement, or sometimes it's a convenient rock. And then we simply -- it's just -- we're just using a level and a stadia rod, and we use the level and the stadia rod to measure the cross-sectional profile, the ups and downs of the stream, from left bank to right bank. And then we use the same technique for measuring the water surface elevation and, of course, from the difference between the water surface elevation and the profile, we calculate depth.

MR. STUBCHAER: Are the cross-sections tied together by level circuit?

MR. HANSON: Sometimes they are. In this case, they are not. There are different hydraulic simulation models that are available for use in this -- I didn't describe the model all that well, but it was developed by the U.S. Fish and Wildlife Service, I think, around 1978 is when it was first developed, and there are several different hydraulic simulation programs. Some that -- some in which the transects are hydraulically tied together, so you're surveying up and down the stream. All transects are tied together. Those typically aren't used in stream flow studies where you have higher gradient streams such as we have in California. They're more prevalent in studies in low

0174

gradient streams, say, in the midwest. But all of the studies that I'm aware of, with the exception of just a few that have been done in California, the transects are separated and are unique and independent measures of habitat, not tied together hydraulically.

MR. STUBCHAER: And then so the flows of each section are determined --

MR. HANSON: The flow at which you visit a stream, that's one of the most difficult things to measure. Sometimes you'll go out and you'll take a depth and a set of velocities at your transects and feel quite comfortable with your flows, but we're not talking about typical cross-sectional profiles that people from U.S.G.S. go out and measure stream flow. We're talking about pretty squirrely conditions where your estimate of flow can be off by several percentage points.

MR. STUBCHAER: You mentioned cells; is that correct?

MR. HANSON: Cells, yes.

MR. STUBCHAER: Do you use a current meter for the velocity in each cell?

MR. HANSON: Yes.

MR. STUBCHAER: Okay. Thank you.

MR. HANSON: Any other questions regarding the methodology?

0175

HEARING OFFICER del PIERO: No. Why don't you go ahead?

Q BY MR. BIRMINGHAM: You started to make a reference to Figure 2 and Table A from your testimony.

A Actually, what I'd like you to do, if you could, is open my testimony to Pages 30 -- excuse me, 48 and 49. Figure 2 is on there, but there's a table in there that I'd also like to talk about simultaneously.

In my evaluation of recommendation, I should say of different minimum stream flows for the streams, I look at the recommendations made by the California Department of Fish and Game on Rush Creek and the means by which those recommendations were derived. And I've shown in the testimony here, Table 33, which is one of the tables that was utilized in developing those recommended stream flows.

And I want to talk about that because I have concerns about them, the means by which those flow recommendations were derived. The basic methods that are described in the Beak report, and at least shown the results of here, is what you would call a habitat duration analysis very similar to a flow duration analysis, it's simply analogous to that. I think you probably understand a flow duration analysis based on all your experience, but let me describe how a habitat

0176

duration analysis is done.

For a given period of time, in this case the analyses are done on a monthly basis, all the available flow records, in this case they were daily flow records, are utilized in developing a sort of a time series of weighted usable area values. So let's say if you're dealing with the month of January, and you only have one year's worth of data, well, you have 31 values, 31 daily values of flow starting from January 1 running to January 31. And the process is to convert each and every one of the weighted usable -- excuse me, of the flow values, daily flow values, to daily weighted usable area values.

So now you have 31 weighted usable area values. Those are ranked, and the median value, or the 50 percent exceedance value, is selected, in this particular instance, this application of the method, as the amount of weighted usable area that's the target weighted usable area to support the fishery. And then the flow at which that weighted usable area value corresponds to is the recommended stream flow. And so there -- the process utilizes the weighted usable area curves, that's the mechanism by which you go back and forth between flow and weighted usable area, and flow duration analysis.

Ô

0177

In this particular case, for Rush Creek, I believe, there was a fairly long period of record 1937 to 1988, so we're talking about quite a few daily flow records for each one of months. That's an awful lot of data. And the problem that I have with the analysis is that I think it ran into one of the problems that plagues analyses of this kind in that the range of flows that were simulated in this case, you can see them on Figure 2, roughly, I'd say 5 cfs up to -- 10 cfs up to 100 cfs, is not high enough; that is, on the higher end, to provide a weighted usable area value for

some of the higher flows.
In other words, if you look at the flow duration curves in the Beak report for the normal wet and dry water years, you'll find that there are several instances in many months where flows in excess of 100 cfs occur. And this begs the question of -- has plagued me on other studies that I've done, is how do you -- what weighted usable area value do you give to a flow of 120 cfs, a flow of 150 cfs, if you are limited in your extrapolation to 100 cfs.
I think this study was limited to 100 cfs for a reason that I uncovered when I started doing my own independent analysis, and that was that when you go out and measure your weighted use -- excuse me. When you

0178

go out and measure your cross-sectional profile data, where you put the ends of your profile dictates the limits of your hydraulic simulation and that as you start simulating rising water surface elevations and they go above the banks that you've defined by the ends of your transects, you're in an area where you don't know what's happening. You don't know how the water surface elevation is going to change because you don't have information about what the bank profile does outside of your transects.
And it's a typical problem that I've dealt with in other studies and run into in other studies when you want to do a time series analysis and want to show weighted usable area through time. You want to convert your flow data to weighted usable data, and you have to make some judgment as to what you're going to do once you start out stripping the extent of your extrapolation values in the weighted usable area curves.
And I looked over the flow duration data that were presented in the Beak report and found that in several months, almost nine months in a wet water year and six months in a normal water year, there were flows in excess of 100 cfs. And I think that that problem that was encountered in this instance in running the percent

0179

exceedance analysis is shown to some degree in Table 33 that I provided in my testimony.
Let me point out that if you look at Figure 2 here, you can see that the adult weighted usable area curve rises up to a peak value at 100 cfs and that peak weighted usable area value is a little over 200,000 square feet, 208,477, to be exact. If you look, then, at the adult habitat curve, and I'm talking about -- not curve, the adult habitat columns in Table 3, I want you to focus on the median weighted usable area values and the flows associated with those, you'll see the value 208,000 -- 208,000 weighted usable area values, for example, in June. This is associated with a flow of 100 cfs, which is the recommended stream flow for that particular month.
I think the problem that I've just described is symptomatic of this result in that if 208,000 square feet of habitat is predicted to be the maximum habitat on Figure 2, it's unlikely that it is, in fact, the

true median value of habitat duration analysis. In other words, the median value means it's the 50 percent exceedance value where, in fact, if you look at the data, it's really the zero percent exceedance value on Figure 2. There are no curves. There are no weighted usable area values greater than 208,000, and I think

this is symptomatic of the problem that I was talking about, the concerns that I have about the analysis that was performed.

My recommendation is that you review this carefully in your consideration of flow recommendations from Cal Fish and Game. I also think that the analysis did not take a step back, so to speak, and look at the biological criteria that were described in Figure 2 for adult brown trout.

You'll notice that the figure sort of has this general sort of monotonic relationship where it rises to sort of a plateau and then very, very gradually rises all the way up to 100 cfs at its peak. I looked at the flow recommendations made in certain months, particularly the hundred cfs flow recommendation, and compared the amount of habitat predicted at 100 cfs to habitat that's predicted at lower flows and in my testimony. I point out that, for example, for the adult brown trout, at 50 cfs, the amount of habitat that's predicted by the model is 189,000 square feet. At 100 cfs, it's 208,000 square feet, roughly. That doubling the flow from 50 cfs to 100 cfs corresponds to about a 9.7 percent increase in habitat.

So the point I'm making is I think that when you do the percent exceedance analyses whether, in fact,

you actually come up with the correct median value, as I suspect may not have happened here, you still -- also want to step back and make sure whether it's making biological sense and whether the changes in flow provide corresponding changes in habitat. Something to that effect.

Also, as I pointed out in my testimony, the fry curves, 100 cfs for -- 100 cfs produces 224,000 square feet of habitat, whereas 20 cfs produces 216,000. So five times as much flow is increasing the fry habitat by about three billion percent.

Let me then quickly get to what I did in developing my flow recommendations. I looked at both the results of the EA data or the EA analysis and the Cal Fish and Game analysis. I was looking at the shapes of the curves. The EA analysis, which is shown on Figure 2, the -- you saw the line on Figure 2, showed weighted usable area reaching a peak value around 20 cfs. This is for adult rainbow trout.

The Fish and Game curve produced again the sort of monotonic curve that sort of generally rose higher and higher as flows increased, and what I did was looking at the Fish and Game results for adult brown trout, the juvenile, the fry, and the spawning, I developed a percent of maximum weighted usable area table. And

0182

that's figure -- rather table -- the next table. Oh, it's on there. Sorry. Table A. And what you're looking at there is percent of maximum weighted usable area at these different flow rates, 20, 30, and 40 cfs.

And based on that analysis, I was looking for a fairly simple range of value, percent of maximum values, that is used in instream flow studies to provide what is considered sometimes its optimal habitat, sometimes it's self-sustaining habitat. But it is a step down from the maximum habitat rather than going to the top of the curve. The maximum amount of habitat is generally held that 80 percent of the maximum habitat is -- will supply adequate habitat for a self-sustaining fishery.

And looking over Table Number A here, I presume that 30 cfs or I believe that 30 cfs, based on the Cal Fish and Game -- Cal Fish and Game results, will provide adequate habitat for the self-sustaining fishery of brown trout in Rush Creek.

Based on the result of of the EA analysis and the Fish and Game analysis, I've recommended to you a flow regime, a minimum flow regime between 20 and 30 cfs in Rush Creek. Again, I repeat, that I've also recommended a flushing flow or channel maintenance

Ô

0183

release, but I don't have specific figures for that. So this flow that I'm talking about here, the 20- to 30-cfs-flow range is the flow that you would fall to following release of higher flows for the purposes I've just described.

The additional evidence that I'd like to bring before the Board but won't speak to at length is the electrofishing data, the fish population numbers that have -- that EA has uncovered for Rush Creek over that period of time when the minimum flow was held at 19 cfs in Rush Creek. Initially, Rush Creek was held at 19 cfs for some number of years, and we conducted instream -- we conducted electrofishing studies throughout that period. And judging from the testimony of Dr. Morhardt, who will come following me, not directly following me, there seems to be evidence that the brown trout population in Rush Creek at that minimum flow range is comparable to other streams in the eastern Sierra Nevada.

The testimony you have today regarding Lee Vining Creek, as I said, was based on an earlier report. I think it's listed in the references to my testimony. That report, what I've said in my testimony, has flow recommendations based on what I would call a flow duration analysis only, and my testimony states that I

0184

thought that it was inappropriate that it, in a sense, ignored weighted usable area curves and that weighted usable area curves should not be flow duration analyses used to generate recommended stream flows for Lee Vining Creek.

I reviewed the curves in Lee Vining Creek and -- this is the table that appeared in the report that I reviewed. And looking at those data -- I didn't have the actual numbers before me -- I looked at these curves, and based on the same 80 percent of maximum weighted usable area criteria, recommended that stream flows somewhere in the range of 15 to 25 percent as a minimum would provide adequate habitat for a self-sustaining fishery of brown trout in Lee Vining Creek.

MR. STUBCHAER: Percent or cfs?

MR. HANSON: Percent of maximum weighted usable area. Did I misspeak?

MR. STUBCHAER: No. No. I misunderstood.

MR. HANSON: That concludes the oral presentation.

Q BY MR. BIRMINGHAM: I have just one question. An additional question.

The flows that you recommended in Rush Creek of 20 to 30 cfs and the flows in Lee Vining Creek of 15 to 25

0185

cfs, in your opinion, would those flows keep in good condition fish that existed in those streams?

A Yes.

HEARING OFFICER del PIERO: Thank you very much.

Ms. Cahill?

While Ms. Cahill is coming up, I would point out that pursuant to Mr. Dodge's request, that there are two veterans on the Board, Mr. Stubchaer and I. We are designated by the Chairman as the Veterans Affairs and Water Rights Decision-Writing Committee, so if you and Mr. Birmingham would like to join us on Veterans Day for our ceremonies, we would appreciate your coming.

Can we count on your attendance?

(Laughter.)

MR. BIRMINGHAM: I will be there.

CROSS-EXAMINATION BY MS. CAHILL

Q Good afternoon, Mr. Hanson.

A Good afternoon.

Q Let me just start by basically going over the corrections you made and one other that I think you might not have discovered.

I believe you have already made the correction on this table which indicated in the -- in your testimony that this was the Fish and Game weighted usable area curve and this was EA's. You've already mentioned that

Ô ’

0186

those are mislabeled and corrected that.

This table, Figure 18, from the draft report, have you checked to see whether the current Lee Vining Creek report has this table with these numbers?

A I've seen a figure that's similar to that, but it's not the exact same numbers, no.

Q And your testimony is not based on your new figures; is that right?

A That's correct.

Q Just let me tell you that this, in fact, is reproduced from your report. It corresponds to this

large blowup, and this is now the table. It's actually Figure 16 in DFG's final Rush Creek report. And as you can see, there are -- there are some differences.

Mr. Hanson, do you know, or can you tell us when you received a copy of Fish and Game's final report?

A I saw it for the first time this morning.

Q Do you know when Los Angeles received it?

A I do not.

Q Would you be surprised if I told you that it was sent out to the parties in August?

A Not necessarily. Yes -- no, I would not be surprised. It has a July date on it.

Q And you said that you received it after you had completed your written testimony?

0187

A I actually never received it. I saw it, as I said, for the first time today.

Q Thank you.

There is just one other minor error I think you ought to correct. In fact, you said it correctly in your oral testimony. On Page 45, the second to last sentence on the page you say, "CDFG only uses site-specific data for adult trout." I believe that was juvenile; is that correct?

A That's correct. Yes.

Q As you've explained already, there were common transects done and some common field work done by Beak and EA on Rush Creek; is that correct?

A Yes, it is.

Q And they used, then, common hydraulic data?

A There was just one set of hydraulic data.

Q In your opinion, is the main divergence between the two studies with respect to the habitat suitability criteria?

A I think so.

Q Do you have a problem with the calibration of the hydraulic modeling that Beak did?

A I haven't reviewed it carefully.

Q I think despite your careful technical explanation of IFIM, we're going to have to go one level deeper,

0188

and I apologize for having to get so technical so late in the afternoon.

Could you explain to the Board Members the difference between a utilization curve and a preference curve?

A That's technical.

Q And in order to assist you, I will put up those curves from your report.

A The difference between a utilization curve and a preference curve is that --

HEARING OFFICER del PIERO: Mr. Dodge, are you objecting?

MR. DODGE: No. I'm just remembering the spring of 1990 when I tried to master this, and I don't think I ever made it.

(Laughter.)

MS. CAHILL: Let me tell everyone who'd like to find it. That is a figure from L.A. DWP Exhibit 15, and it's Figure 3 in that report.

HEARING OFFICER del PIERO: Which aspect of it do you need assistance on, Mr. Dodge?
Mr. Birmingham, can you help him out?
MR. ROOS-COLLINS: Mr. del Piero, I'm reminded of the saying about old dogs.
(Laughter.)
Ô

0189
HEARING OFFICER del PIERO: No. It's too late in the day. We've got to do this in the morning. Not now, please.
Q BY MS. CAHILL: Actually, let me lead you through this, Mr. Hanson, and you can correct me if I'm wrong.
The utilization curves and preference curves are both subcategories of what we sometimes call habitat suitability curves. Would that be the expression you would use?
A Yes, it would.
Q And in the IFIM process after you have characterized the hydrology of the stream and you have measured in the cases of many IFIMs, you measure depth, velocity, substrate, and cover. Is that correct?
A Um-hum.
Q And in your particular case, you relied on depth and velocity?
A That's correct.
Q Having those measurements and then doing a picture of the stream, you then enter a curve that shows which depths and velocities the fish are predicted to be using, or you can correct that and state it more technically.
A Yeah. Well, it's what I was stating earlier when I was going through my little discussion of the

0190
method.
Once you -- I don't know if you said painted the picture, once you've simulated all your different depths and velocities in all these cells in all your different flows, the habitat suitability criteria are a definition of how suitable different depths and velocities are to the fish. Hence the suitability in the name.
You want me to get into utilization --
Q Why don't you just tell us briefly how would you develop the utilization curve.
A The utilization curve is developed through this snorkeling that I was describing where you get into the stream and you snorkle about looking for fish. You find fish, and you make observations of what depth and velocity those fish are at. The velocity is mean column velocity. The depth is just a water depth.
After collecting a set of data that generally is considered -- has to be above 150 separate observations, you develop a frequency distribution of those observed values. And where you find the fish most often, of course, is then the highest level of frequency. And the suitability curve that you see on this figure over here, the highest frequency is given a value of one. So where you see the fish most often in

0191
your observations through snorkeling is assumed to be the most suitable habitat, and that value is given a suitability of one.
Q And where you have not observed fish, what number would you assign?
A Where you have observed no fish, the suitability is zero.
Q And can you tell us when did you -- and you collected this on-site in Rush Creek?
A Yes, we did.
Q And you did that for two of the life stages?
A Yes.
Q And at what cfs did you collect those data?
A At 19.
Q And what year did you do it?
A I think it was '87. It could have been '88. I'd to have review.
Q Did you, in fact, collect part of your data in one summer, in the summer of 1988, and then collect additional data in 1989?
A I can't recall, but that may well be the truth.
Q Do you recall that perhaps in 1988 you selected the data macro -- by a macro-habitat length, by a particular run, riffle, or pool, and then in 1989 you collected it for 25-yard stretch of the stream?

Ô

â

0192
Q My recollection is that we collected it by 25 -- 25-meter stretches of stream.
Q Would it be appropriate to collect data by two different methods and then sum it?
A It necessarily wouldn't be inappropriate.
Q But it might introduce some error because they are not comparably collected. You have different sampling methods.
A I have been involved in instream flow studies where different sampling methods have been applied and the data have been logged and sensitivity studies have been employed as to whether, in fact, there was bias using one or more of the data sets, and it turned out in that particular analysis that it was not the case.
Q Is it sometimes considered that there is a possible bias in using use criteria?
A Yes.
Q And can you explain why that would be?
A Well, that gets to the issue of utilization versus preference curves. And the debate on this issue has raged for several years now, and it goes to the following: That the use of -- the observations of use may be biased by the availability of data, availability of what's out there.
Q Let me put you some questions. In other words, if
0193
you see a lot of fish in a certain depth of water, typically, you would assume that the fish liked to be in that depth of water; is that right?

A Yes.
Q But if -- and it may be that you see that there is very little of a particular kind of water, but when it is present, there are fish in it. So that you would not have many counts because there's not much of that type of water but that, in fact, when the water is available, the fish use it heavily.
A I don't know if I would agree that they would necessarily use it heavily. If it's used to some degree at one -- in one condition and it's more prevalent in another condition, I don't know whether that would necessarily imply that it would be used heavily in the second condition.
Q Is one of the ways to avoid the possibility of bias out of utilization data to also take into account the availability and then develop a preference curve?
A Yes. That's the issue of great debate in instream flow studies.
Q And, in fact, in your study, did you do that? Did you develop a preference curve?
A I did do it that way. I did it both ways. I developed utilization curves and I developed so-called

0194

preference curves.
Q And, in fact, you ran your model with both types of curves; did you not?
A That's true.
Q Okay. And to get back to Figure 2, isn't this a little bit apples and oranges inasmuch as the California Department of Fish and Game weighted usable area curve was the output with a preference curve run through the system, whereas this EA curve is the output that resulted when you ran use curves through the model?
A Well, there's another apple-and-orange factor in there in that one set of data was collected in Rush Creek and the other set of data was collected in streams outside of Rush Creek. The general conventional wisdom of IFIM practitioners is that it's better to collect the data in the stream and it is generally, and I think there's substantial backup for this, that when you collect the data in the stream, the utilization data are the correct data to use, and that a preference function or a modification of the utilization data to create preference data is not as appropriate as using utilization data.
Q But, in fact, this one was derived so that to get a comparison using the same types of curves -- I would

Ô

0195

refer you all to Figure 8 from L.A. DWP Exhibit 15 -- when you finally ran both your utilization data, which has this dip after the peak, and your preference curve, isn't it true that your preference curve has a bit more of the same shape of Fish and Game's than your output using the utilization curve?
A Yes, that's true.
Q If you decide not to use a preference curve but to use the utilization curve, is it recommended to collect data at a range of flows so as not to bias the results

toward the flow at which the data is collected?
A That would be recommended.
Q And did you do that?
A No. We collected data at 19 cfs, but the point of the preference function is that if your concern about the availability of habitat at the flow in which you collect your data, the correction, the availability correction to create the preference function will take care of that. That's the conventional wisdom. And I did that, and I evaluated that, and I generated both sets of curves to evaluate that eventuality.
Q Right. But the curve that we see now in Figure 2 was not the result of your preference curve?
A That's right. I still believe that the utilization curve is the better curve.

0196

Q Right. But it would be better with the utilization curve to take it at representative flows -- the danger -- isn't it true that what the danger is of taking it at a low flow would be that certain depths might not be observed at all and consequently, you don't know whether the fish will utilize the water at those depths?
A I don't think in Rush Creek that if it had been collected at significantly set -- let me start over again.
I believe that in Rush Creek if we had collected data at a lot of other flows -- in fact, to tell you the truth that was -- I don't want to get into it. But the curves wouldn't necessarily --
MR. BIRMINGHAM: Mr. Hanson, everything you're telling us is the truth; is it not?
HEARING OFFICER del PIERO: Mr. Birmingham, just said so, so it must be.
(Laughter.)
MR. HANSON: I don't deny that.
But if you look at Rush Creek, Rush Creek is a very shallow stream, at least it was in 1987. I know there was some debate that it's changed a little bit over the years. But in 1987, Rush Creek was shallow. You see that in some of the frequency distributions of

0197

available depth that we developed from the data.
Q BY MS. CAHILL: Let's go back, then, to the curve that shows your utilization curve, the figure with the three curves. Maybe --
A I'm not sure which one --
Q Now, in theory, is this curve a compilation of the observations you made in the stream?
A Yes.
Q And did you observe any fish in water over three feet in-depth?
A Few.
Q Few.
A If any.
Q If any. Does this curve include all the observations you made?
A It should.
The point I'd like to make regarding collecting data at higher flows is, if you look at the

cross-sectional profiles with those water surface elevations that we show in our report, you'll see that flows from 19 cfs up to 20 cfs don't add significant depth to Rush Creek. Rush Creek is a shallow stream. There's no question about it -- as shown in this availability plot. You can see that the most dominant depth -- you go out there and walk across Rush Creek in

0198

1987, you're going to walk across a shallow stream.
Q Let me just ask, though, is it your testimony that whatever observations you made would, in fact, show up on the utilization curve at the top of that figure?
A Yes, they did.
Q Okay. And you did not reject any data, any observations?
A Not that I'm aware of.
Q Okay. I would like to refer you to Page 9 of your report in which you state -- actually, it starts on Page 8. You say, "Problems -- " this is the second sentence in. "Problems were basically caused by the dynamics of low levels of use and availability at the tails of distributions; that is, in swift water and in deep water."
Would that suggest that you did make some observations in swift and deep water?
A It all depends on what you define as "swift and deep water." What I'm referring to in that sentence is that when you go through the correction technique, it's very volatile, particularly where you have small amounts of observations. We haven't described the correction technique, but just to describe it very briefly --
Q Let me just ask the question. The question is

0199

does that indicate that you had, let's say, for example, some observations in water more than three feet deep?
A Well, I'm not exactly sure.
Q Then on Page 10, the report says, "In order to eliminate the dominance of the incidental use and availability data at the tails of the distributions, the use distributions for both depth and velocity for both adults and juveniles were truncated at appropriate levels prior to application of the preference formula."
Does that suggest that some of the data at the tails of the curves was truncated? And what do you mean by "truncated"?
A Well, the curves were probably smoothed and maybe brought down to lower levels, the utilization or even the availability in developing the preference function.
Again, it's a problem associated with this volatile nature of the technique for making the correction. If you divide the utilization factor by the availability factor, and I know that doesn't make a lot of sense, but that division creates volatile

results that create great spikes of preference resulting from dividing a small number by -- excuse me, a large number by a small number relative to the

0200

different curves. And what I'm talking about there is an attempt that's done by all practitioners of IFIM in smoothing their data to some degree, maybe truncating some values in order to create a preference function that is smooth, that doesn't have some of this spiky nature.

Q In the end, both your use curve and your preference curve appear to assign a suitability of zero at three feet of depth. As a fisheries biologist, do you believe that brown trout adults will use water of three feet depth if it's available to them?

A Yes, generally they will. But again, we have to get back -- and I know you posed this question to the Jones and Stokes fisheries biologist, and their reply and mine's the same is, you have to consider the conditions available in the stream. Brown trout will live in lakes, however deep the lake is. Rush Creek in 1987 and probably today, as well, pretty much is very shallow. There is very little deep water, and when you go out into a stream and take random samples of fish observations, as we did when we went out to select these random 25-meter sections, you're going to be looking at shallow water in almost all cases.

And the data reflect that. The data reflect the reality of what's out in the stream and the reality of

Ô
#

0201

what the fish have to select from at 19 cfs and even at 60 cfs and 100 cfs, the increase in water surface elevation, if you look at these cross-sectional profiles, doesn't substantially change from 19 cfs to 100 cfs. It rises about a foot, maybe a foot and a half, and you're not going to be, even at those higher flows, you're not going to be finding much of Rush Creek at depths of 3 to 4 cfs.

There are pools in Rush Creek that are three, four, and five feet deep and adult brown trout are in there, but if you go out and randomly evaluate where fish are, as you should in these habitat suitability criteria studies, you will observe most of your fish in shallow water.

Q In terms, though, of overall preference for brown trout, if water of 3.2 feet deep were present, you would expect them to use it?

A I wouldn't say they wouldn't be in there, that's correct.

Q And if water is four feet deep, you would expect them to use it?

A That's correct.

Q And are there other, either preference or suitability curves, with which you're familiar where the researchers, once they reached the peak, basically

0202

extend this over with an assigned number of one for adult trout, so that if you began to have deep water,

your model will show that it was very suitable instead of, in your case, showing that it was entirely unsuitable?
A That is done in some cases. It's not done in all cases.
I'd like to point out that one of the things to consider in doing IFIM studies like this and developing these suitability criteria and making decisions like that to hold your depth criteria up to a maximum value, is what sort of an impact that will have on the simulation, the simulation of weighted usable area versus discharge, this curve that you're trying to generate at the end. And in the case of Rush Creek, the distribution of deep water is still very infrequent. The -- as can you see, the distribution that's shown on that curve, it's the middle curve, the availability, there is very little deep water in Rush Creek.
Q Did you collect -- did you collect these samples at all reaches of the creek -- of the stream?
A Yes.
Q Did you collect these samples in the return ditch?
A No. We were going to areas that were considered

0203

to be more stream-like. There are very few fish in the return ditch.
Q Are there fish in the return ditch?
A There are some fish in the return ditch.
Q And is the return ditch at this point functioning as some portion of the stream between Mono Gate One and Mono Lake?
A I don't really have any information to decide whether it's functioning as a -- I know there have been some changes as part of the restoration operation. But whether it's functioning with more fish than it had in 1987 -- I'll tell you, in 1987, it had darn few fish.
Q But had you collected data and put it into your use curve, you might, in fact, have found -- you would have found use in that stretch of the stream?
A I don't think we would have found many fish to observe in that stretch of stream.
Q In the end, tell us when the predominant depth of water in Rush Creek is?
A I'm not sure exactly what you mean.
Q You've said it's a shallow stream. What is the most common depth in Rush Creek?
A It depends on what the flow is. It's going to change as a function of flow.
Q At 19 cfs?

Ô

,

0204

A 0.4 feet.
MS. CAHILL: Mr. del Piero, I would apply for an additional 20 minutes. This is, I think, the heart of Cal Trout --
HEARING OFFICER del PIERO: It's granted.
MS. CAHILL: If you all are being put to sleep --

HEARING OFFICER del PIERO: Oh, no. We find this remarkably interesting. Please proceed.

(Laughter.)

MS. CAHILL: All right.

HEARING OFFICER del PIERO: That was del Piero who said that on the record.

Q BY MS. CAHILL: You don't believe that brown trout prefer a depth of 0.4 feet, do you?

A You mean as their most highly suitable --

Q Right. Is 0.4 feet even suitable for an adult brown trout?

A Not much.

Q Okay. And yet, you are recommending, as the low end of your flow range, a flow of 20 cfs. Let me quote to you from your report on Page 10, "If the preference curves do, in fact, reflect to preferences of brown trout in Rush Creek, once the bias of habitat availability has been removed, what we discover is that the greater portion of available depths in Rush Creek

0205

at a Mono Gate release of 19 cfs are in a range of values that are not preferred by brown trout."

So are you recommending a flow that results in releases that result in habitat that is not preferred by brown trout?

A I'm having trouble with the question. Would you repeat the sentence?

Q Are you recommending a flow range to the Board, the lower end of which is 20 cfs?

A Um-hum.

Q And from your own report what can you tell me about the depth of the stream at approximately 20 cfs?

A The maximum -- not the maximum depth. The depth that was most prevalent in the stream, based on the transects, is 0.4. That's -- you've got to remember, though, that the weighted usable area curves take into account all the other depths in the stream. The depths in the pools. The depths in the riffles. The depths in the runs. Everywhere. And while it still may be shallow throughout a good portion of it on the edges of the stream, that is all wrapped into the weighted usable area versus discharge curve that's generated as the output of the model.

Q And, in fact, didn't you find relatively low levels of weighted usable area?

0206

A Yes.

Q Okay. And your report says, "The reason for low levels of usable area in Rush Creek is the preponderance of shallow water as indicated by the availability curve shown in Figure 3," and that's the middle curve on your figure.

MR. BIRMINGHAM: Excuse me. Can you tell me from where you're reading?

MS. CAHILL: Page 20 of L.A. DWP Exhibit 15. The middle paragraph.

Q BY MS. CAHILL: "The predominant depth of Rush Creek at the 19 cfs Mono Gate release is 0.5 feet, which is shown to be utilized very little and predicted to be unpreferred." Is that correct?

A Um-hum.
Q What happens to the depths in Rush Creek when you go from 20 cfs to approximately 60 cfs?
A The depths increase by about one foot throughout most of the stream.
Q And so if adult brown trout prefer deeper water, would an increase from 19 cfs to 60 cfs be likely to create more water of the type that the adult brown trout prefer?
A Well, you've got to bear in mind that velocity is taking a role in this as well, and the velocities may

Ô
$

0207

be increasing to a point where there's a decline in suitability as a function of velocity. So the two may be offsetting each other.
Q Right. But you don't know for a fact that the velocity would, in fact, have made all that increased depth unsuitable?
A No. I'm not saying that it would make -- again, you have to remember that this is an analysis where you're taking a tremendous amount of data, 75 different transects, maybe 20 or 30 cells across each reach, or rather each transect, and all that information -- every one of those cells has a different dynamic. It's creating different depths at different flows, different velocities are occurring at different flows, and it all goes in a hopper, in some sense, and out spits this weighted usable area which is the discharge curve.
Q Right. But you have admitted that the depth of the stream overall would increase and that typically, assuming a velocity problem, the deep -- assuming no velocity problem, the trout would prefer a deeper water, the adult trout?
A Well, not based on the suitability criteria that we developed from the stream.
Q Well, but based on your own knowledge as a fisheries biologist, wouldn't adult brown trout prefer

0208

water deeper than is present at 19 cfs?
A They would be found in water that is deeper than what is present in 19 cfs.
Q And this is -- this is also from your report. It's the first of the series of transects -- transect cross-sections. Can you identify which this is? It's --
A I think that's one of the transects that's in the return ditch.
Q Yes. I believe it is. And you --
A Two of the transects.
Q -- all your suitability criteria data at a flow of 19 cfs. That's this lowest line, isn't it?
A Yes.
Q And so this would be the water surface elevation at 19 cfs?
A Actually, that's 13.
Q 13. You're right. 13, then 19, then 60, then 100.
HEARING OFFICER del PIERO: Stop. You need a

microphone. Okay?
MS. CAHILL: I think I can stay --
HEARING OFFICER del PIERO: No. Stop. You need a microphone. Please. Okay?
MS. CAHILL: I probably can stay here now.

0209

HEARING OFFICER del PIERO: Can you walk over to the Board. The cord will reach that far. You just need to take it with you. Okay?
Q BY MS. CAHILL: For those who want to find this, I know these aren't going very far. Again, these are the first transects in L.A. DWP Exhibit 15, and the four horizontal lines, is this right, Mr. Hanson, are the surface elevations at 13, 19, 60, and 100 cfs?
A Yes, that's correct.
Q And so on this top one if you went out at 19 cfs, and -- you would find that there was no availability of four-foot-deep water.
A Yes.
Q However, if you went out -- three feet deep. However, if you went out and measured at 100 cfs, you would, in fact, have some three-foot-deep water?
A Yes.
Q So had you measured at 100 in the return ditch and found adult trout, you probably wouldn't have had a utilization curve that showed zero preference at water three feet deep?
A Well, you have to take into account the distribution of the fish in the stream, and I think if we went into the return ditch, we wouldn't have found many fish to observe. Those in there, yes, would have

Ô

Ò

0210

been in deeper water. But in terms of the overall study that we would have done, the number of observations probably would have been small because there are few fish there.
Q I believe -- I can't remember whether on the Board field trip, we actually saw a fish caught from the return ditch, or we only heard tell of one.
MR. BIRMINGHAM: Are you offering testimony, Ms. Cahill? Because I'd love to cross-examine on that issue.
Q BY MS. CAHILL: All right.
Let me go very briefly back to, if I can, to some of the criticisms you have of the Department of Fish and Game report. You mentioned that in the -- in your testimony on Page 46, you indicate that you didn't use spawning habitat suitability criteria, and you thought it might be inappropriate to do so on Rush Creek. Is that right?
Did you look at Table 33, which is also reproduced in your report, had the Department of Fish and Game used only adult weighted usable area curves instead of using the spawning ones for the spawning months, would it have resulted in any lower flow recommendations?
A It doesn't look like it.

Q You also, on Pages 46 to 47, criticized Beak for

0211

not minimicking what you call natural flows. Is it true that Beak's recommendations were based on the median habitat that would occur in Lower Rush Creek in the absence of Los Angeles' water storage and diversion?

A I want to correct you. I don't think I criticized them for not mimicking stream flows or -- what I said in my testimony is that the small changes in stream flow that are recommended in the Beak report, I don't think have that much biological relevance because the curves in that range of flows are very flat from -- that is, the Fish and Game curves or the Beak curves.

Q And your own recommendations are for 20 cfs for certain months and 30 cfs for certain months. Do those mimic the natural hydrograph?

A They're not intended to mimic the natural hydrograph.

Q You've stated today that you're not making any channel maintenance flow recommendations; is that correct?

A That is correct.

Q Were you here for Dr. Beschta's testimony?

A Yes, I listened to it.

Q Did he make any channel maintenance recommendations?

0212

A I don't think he made any specific recommendations.

Q And are you today not making any recommendations for riparian vegetation maintenance?

A No, I am not.

Q Do you know if anyone from the Department of Water and Power is going to recommend flushing maintenance flows or channel maintenance flows?

A In a specific sense or a general sense, as I am?

Q In a specific sense will there be numbers provided to the Board?

A I'm not certain whether there will be.

Q With regard to your early testimony regarding the historical flows that exceeded 100 cfs, if, in fact -- assume that Beak did extrapolate out to 260 cfs and found the adult weighted usable area curve to continue to rises throughout that period, is it then likely that the relative order of the medians would not be affected?

A If I assumed that they were capable of extrapolating out beyond the 100 cfs and the curve continued to rise?

Q Yes.

A Would it not be -- what was the --

Q Well, would it -- would there be any reordering in

Ô

0213

the years in the habitat duration analysis?

A I'm not sure what you mean by "reordering in the years."

Q Well, you were saying that if there were flows

over 100 cfs, it was possible that the median might be different than what Beak saw because many of those were over the range of extrapolation.
A I think the median that was reported by Beak was an artifact of the limitation they had in not going over the 100 cfs maximum limit.
Q Right. But what -- if their number -- where they have reached the top to their extrapolation at 100, whatever the median is going to be over 100, so long as -- if they've set everything over 100 back to 100 and if the curve is still rising and never drops out to 250, then, in fact, it's not likely, is it, that anything would affect that 100? The median whatever it is would be over 100. Is that right?
A Well, the median weighed usable area could be at a flow higher than 100 cfs. Is that what you're saying? Depending on -- depending on the shape of the curve. anything could happen. If the curve continued to rise and rise and rise, the median could be well above 100 cfs.
Q Right. What's happening, though, is as you have

0214

the curve rise and you're ordering your habitats in order of the currents, so long as the curve continues to rise, those habitats will have the same order. Only if the curve dropped, would you then start to have to rearrange your habitats in order.
MR. BIRMINGHAM: Objection. Compound.
MS. CAHILL: This may be a horse not worth beating.
HEARING OFFICER del PIERO: I understood it the third time you asked it.
Do you understand the question?
MR. HANSON: I think what she's saying -- you're talking about the ordering of the --
HEARING OFFICER del PIERO: Mr. Hanson, I'm asking you, do you understand the question she asked?
MR. HANSON: I'm getting close. I think.
HEARING OFFICER del PIERO: I'm going to sustain the objection. You need to break it into parts. I think I know where you're getting to, but --
Q BY MS. CAHILL: The way Beak has done its habitat duration analysis is that it has taken the historic period of record, those flows, it has determined what habitat corresponds to each of those flows, it has then ordered the habitat from the least frequent to the most frequent, and it has then found the median level of

0215

habitat and gone back to determine what flow corresponds to that median level of habitat.
Now, when the amount of habitat is more than the maximum amount that was extrapolated, something over 200,000 square feet of habitat, anything over that Beak set the flow equal to 100, which was the maximum flow.
A Right.
Q Now, you indicated that if they had extrapolated out further, all those years might be rearranged, and wouldn't that happen only if the curve dropped again?
A It would depend on how many flows there are above 100 cfs.

Q If the median -- if the median habitat was already something over what corresponded to a flow of 100 then, in fact, even if which years the median would change, it would still be over 100. Isn't that right?
A I'm sorry. I'm getting a little bit confused again.
Q I think --
HEARING OFFICER del PIERO: Do you know the answer, Mr. Hanson?
MR. HANSON: I don't understand the question well enough --
HEARING OFFICER del PIERO: Fine. If you don't understand the question, I think, Ms. Cahill, why don't
Ô

"

0216

you move on.
MS. CAHILL: I will do that. Thank you.
Q BY MS. CAHILL: You indicated in your report, your IFIM, that the study area extended from Mono Gate One to Mono Lake. In fact, the weighted usable area curve that you presented in your Figure 2 at the beginning, that did not, in fact, include the return ditch trench, did it?
A That's correct.
Q And Beak did no transects below the county road. How were you able to model the section below the county road down to Mono Lake?
A We wouldn't have.
Q So in other words, the statement that it represents the stream from Mono Gate One to Mono Lake is not entirely accurate?
A You're right.
Q Were any additional data taken after preparation of this draft report that's L.A. DWP Exhibit 15?
A By EA?
Q By EA or anyone?
A Relative to --
Q Well, this is -- this is labeled as a draft report.
A Oh.

0217

Q Preliminary draft. Was there ever a final prepared?
A No.
Q Were there additional data taken?
A We have conducted several studies since this draft report was prepared, but it was -- none of those studies were done with the intention of modifying the draft.
Q And none of those have been submitted to the Board or the parties?
A No.
Q Were there additional analyses made since this draft report came out?
A No. There were not.
Q With regard to your electrofishing, EA's electrofishing data, were those sites randomly

selected?
A I don't believe so. Initially, I think they were selected by a team of biologists using professional judgment.
Q Were the sites restricted to discrete macro-habitat units?
A No.
Q Were the site boundaries at the boundaries of macro-habitat units?

0218

A I think generally that was true.
Q Does the lack of random selection of sites possibly affect the outcome?
A Oh, it always can. I mean, there's always the debate between what a professional judgment imparts some bias that if you selected your stations absolutely randomly would be outside of your data. My guess is that there probably isn't a sufficient enough bias to be concerned about associating and not randomly selecting the sites.
Q Let me just recap quickly. On Lee Vining Creek you have not reviewed the final DFG report?
A No.
Q And your recommendations were based on the draft?
A That's correct.
Q You gave some changes in weighted usable area, but again, since they're based on what is not the final recommendation, it may not be useful to go through that.
A I think I gave those on Rush Creek, not on Lee Vining Creek.
Q Your in-house model with regard to the split elevation at different portions of the stream, is that model used by anyone else?
A No. It's our own model. Although the U.S. Fish
Ô
&

0219

and Wildlife Service is developing that capability to have more than one water surface elevation across the transects.
Q The technique used by Beak is the standard technique used in the field at this time, isn't it?
A Yes, it is.
Q And where you did use public -- published criteria, where did you take them from?
A I took them from the Cal Fish and Game 1987 report.
Q And is that the same source of published criteria used by Beak?
A I believe it is.
Q And do you know why Beak did not develop site-specific criteria?
A Well, they did develop site-specific criteria for the juvenile life stage.
Q Do you know why they didn't for adult?
A They didn't see enough.
Q Yes. Thank you.
I'm getting close.
HEARING OFFICER del PIERO: That's good.

Q BY MS. CAHILL: One of your criticisms of the Lee Vining report was that it was based on flow duration only. If I were to tell you that the final is not

0220

based on flow duration only, would that alleviate that criticism?
A Yes.
Q And in the end, your conclusion with regard to Lee Vining, based on the no longer current figure, was that based on 80 percent of the maximum weighted usable area, you came up with 15 to 25 cfs as a minimum flow adequate for a self-sustaining fishery. Do you have any reason to believe that that would be the same as the historic fishery?
A No.
MS. CAHILL: I think that's all I have. Could I have just a moment to confer with my client?
Q BY MS. CAHILL: Could you tell us a little more about the studies you have conducted since this preliminary draft?
A The studies that I've conducted?
Q That EA has conducted?
A Well, there have been electrofishing studies that EA has conducted since then. There have been some transects data collected at some of the sampling -- some of the macro-habitat sites that were used in this analysis. There have been some additional studies, very detailed studies of depth and velocity used by fish as part of an epiery study that EA has done. We

0221

used Rush Creek as a sample stream for that.
I'm really not familiar with all of the details of those studies, but that's some of the work that's been done by EA since then.
MS. CAHILL: Okay. Thank you. I think that's -- wait. One last question.
Q BY MS. CAHILL: Was Rush Creek prior to 1941 a shallow stream?
A I don't have an opinion on that.
Q Have you heard anything about the existence of pools or deep water in Rush Creek prior to diversion?
A I've heard some debate on the subject, yes.
MS. CAHILL: Thank you.
HEARING OFFICER del PIERO: Thank you very much.
I assume Mr. Dodge is next, but Mr. Brown's got a couple of questions, so if you'd be kind enough to hold on for a moment, Mr. Dodge.
Mr. Brown?

CROSS-EXAMINATION BY THE BOARD

Q BY MR. BROWN: Mr. Hanson, at 19 cfs, what's the normal velocity in that stream?
A BY MR. HANSON: I'm not sure I can answer that without reviewing data, and I don't know if I have any data in the report to answer that question.
HEARING OFFICER del PIERO: Can you check and see?

Ô

r

0222

MR. HANSON: I don't think -- I'll look, but I don't think it's in here. I'm pretty certain it's not. I could look at some of the velocity distributions. There are velocity distributions shown in this report that can give you an idea --
Q By MR. BROWN: Just an estimate of what it is?
A BY MR. HANSON: At 19 cfs?
Q 19, 20. Three, four feet per second?
A I'd say it's more in the range of one to two.
Q One to two. My experience with brown trout, their habitat is generally in the banks as opposed to the rainbows and brooks being out in the middle of the stream for their habitat. Is that true in this stream?
A I actually didn't collect any of the snorkeling data in this stream, and I can't tell you exactly where all the brown trout were observed.
Q Are there many holes in that stream at 19 or 20 cfs?
A There are few holes in the stream at 19, 20 cfs or at 60 or 100 cfs in 1987. I know that there have been some pools created as a result of the restoration program, but the point I was making in the discussions a minute ago was that a good portion of Rush Creek is dominated by riffle habitat, which is a shallow habitat, and run habitat, and what we call rock garden

habitat. Very little of Rush Creek, when we were out there, was pool habitat or deep habitat.
Q Isn't that a preferred habitat for brown trout?
A The adults. When we electrofished, we found more adults in the deeper water than in the shallow.
MR. BROWN: Thank you, Mr. Chairman.
HEARING OFFICER del PIERO: Mr. Dodge.
Mr. Dodge?

CROSS-EXAMINATION BY MR. DODGE

Q Good afternoon. This feels like deja vu all over again.
A Yes, it does.
Q Didn't you and I discuss Exhibit 15 in the spring of 1990?
A I think we've done this before, Mr. Dodge.
Q It's dated --
HEARING OFFICER del PIERO: Mr. Hanson, you don't look anywhere near as old as Mr. Dodge.
(Laughter.)
Q BY MR. DODGE: It's dated April of 1990, and as I recall, you hurried to finish it for that interim stream proceeding, correct?
A You're right.
Q And I notice it's still a draft, a preliminary draft. Has it not been completed in the three and a

half years since you and I went over this?
A Well, that's true.
Q Are you still working on some of the points on this for cross-examination?
A I'm still thinking about them.
(Laughter.)
MR. BIRMINGHAM: Objection. Assumes facts not in evidence.

(Laughter.)
HEARING OFFICER del PIERO: Sustained.
Q BY MR. DODGE: Why would you do an IFIM study? Again, what is -- I think you've told us this, but what is the purpose of doing this study?
A The purpose is to develop this relationship between stream flow and fish habitat upon which flow recommendations are generally based.
Q And you have at least a preliminary draft for Rush Creek. Do you have one for Lee Vining Creek?
A I did no analysis on Lee Vining Creek.
Q The only IFIM analysis we have for Lee Vining Creek is the Department of Fish and Game analysis?
A Yes.
Q Now, you say -- and you said it twice, as a minimum, 20 to 30 cfs for Rush Creek. Now, if my math is right, 30 is about 50 percent more than 20, which

seems to me to be a fairly wide gap. Is there a reason for that?
A Well, the gap is based on consideration of the results of the EA analysis and the Fish and Game analysis or the range, I should say.
Q But a 50 percent difference seems like a large difference for a scientific study. Are these IFIM studies somewhat of an inexact science?
A Well, there is -- yeah. There's a certain amount of uncertainty as to the exact relationship that we're shooting for, this relationship between weighted usable area and discharge.
With regards to whether a 50 percent change in the range of flow is significant or not, we often deal with smaller streams where flow recommendations are made at a lower flow range over a much broader percent change -- percent difference in flows.
Q Isn't it true that even if -- even if the scientists agreed as to what the relationship was between weighted usable area and flow, there's still substantial room for disagreement as to what the recommended flow would be?
A Are you saying if they agree that the curve is correct, what the flow ought to be, and that's a very good point. There are several avenues that different

people who do IFIM go down in coming up with a recommended stream flow.
Q And one -- one scientist might look at weighted usable area curve and recommend one flow, and one of his or her colleagues might recommend a totally different flow. Isn't that right?
A Yes.
Q So there's -- even after you have the results, you have to use professional judgment in applying them?
A Yeah. There are different techniques to apply, too, it's not simply always just looking at the curve. Obviously, in the case of the Beak report, there was a habitat duration analysis. Sometimes population modeling is performed. We, on other streams, have used a population response model to evaluate what the best

recommended flow is.
Q And you, as I said, twice referred to 20 to 30 cfs as a, quote, minimum, end quote, flow. Would you agree that higher flows might be suitable for Rush Creek?
A Might be suitable. In what sense do you mean "suitable"?
Q I mean suitable in the sense of suitable fit habitat for brown trout?
A I think flows higher than that minimum are not going to be degrading to brown trout habitat.

Q Not going to be degrading?
A That's right. Well, it depends on how high you go. If you look at the curves that were generated by the EA study and the Cal Fish and Game study, you do notice a sort of general plateau over a broad range of flows. It seems to indicate that there's not potential for habitat degradation in that range as you go higher.
Q You're not here telling us that the DFG recommended flows are, quote, degrading.
A My testimony will be, right, that they will not lead to degradation of habitat, significant degradation of habitat.
Q What do you understand the goal of this proceeding to be as it relates to stream flow?
A Well, I believe the goal of this proceeding is to evaluate potential management regimes for the Basin including flow regimes in the stream as one component, overall management -- how to best manage water in the Mono Basin.
Q You say "best management." Against what standard, Sir?
A Well, I don't really know that much about all the standards, I suppose, that are being utilized in this exercise. There are certainly all the resources that are being considered such as the aquatic resources,

Â

0228

terrestrial resources, the lake --
Q I'm solely concerned with stream flows.
A Okay.
Q And what do you understand the goal to be?
A The goal to be with regard to this exercise or these hearings?
Q Yes.
A I would say the goal is to develop a flow regime that provides adequate protection for fish and other aquatic organisms in Rush and Lee Vining Creek.
Q Does the DFG recommendation do that?
A Yes. As does mine.
Q The reason I ask is that I went through your testimony fairly carefully, and at Page 45 you talked about a self-reproducing population. And then today you told us about a self-sustaining fishery, which I read as pretty similar to that. At Page 49, you talked about a fishery, quote, equivalent to other population in the streams of the Owens Basin, end quote. At Page 50, you talked about, quote, maintaining the brown trout population in Rush Creek, end quote. And then at Page 50, again, you said at 30 cfs we reach, quote, 80 percent of the maximum predicted habitat for all life stages, end quote.
Now, those all read to me as goals, arguably

0229

different goals.
A I don't know if I would agree that they're different goals. I view them more or less as the same goal. Maybe I just stated it differently in different places in the testimony.
Q Okay. But let me ask you in terms of the 20 to 30 cfs recommendation, does that relate to these different goals, or if they are the same goal?
A Yes, it does relate to the same goal.
Q And that goal again is?
A Is -- well, I don't know which way I'm going to put it. To maintain a self-reproducing population of brown trout in Rush and Lee Vining Creek.
Q Now, would you agree with me that if someone had a different goal, they might come up with a different flow recommendation?
A Absolutely.
Q Now, you told us that looking at Figure 2 that the habitat peaked at approximately --
HEARING OFFICER del PIERO: Mr. Dodge, I'm going to admonish you the same way I admonished Ms. Cahill.
MR. DODGE: I thought were you admonishing Ms. Cahill because of volume.
HEARING OFFICER del PIERO: No. Volume has no bearing on the tape recorder that's here. It's

0230

proximity as opposed to volume.
MR. DODGE: Because no one's ever said that I was too quiet.
(Laughter.)
HEARING OFFICER del PIERO: No. In fact,

Mr. Birmingham won't object to that assertion, either.
Q BY MR. DODGE: In Figure 2 you said, in response, I believe, to a question by Ms. Cahill, that habitat peaked at 200 cfs. Do you see that?
A For the Fish and Game curve, yes.
Q For the Fish and Game curve.
But isn't it true that you submitted a declaration before Judge Finney in '89 which said, in effect, that an adult and juvenile habitat at Rush Creek increased up to flows between 150 cfs and 180 cfs?
A That was from an earlier deposition, and I --
Q It was a declaration that you submitted to Judge Finney in September of 1989.
MR. BIRMINGHAM: Maybe Mr. Hanson could be given a copy of the declaration if he's going to be asked questions about it?
HEARING OFFICER del PIERO: Do you have a copy available?
MR. DODGE: I do, but I'd like to see what his recollection is first.

0231

HEARING OFFICER del PIERO: Okay. That's appropriate to ask him in regards to his recollection of a document like that.
MR. HANSON: I don't recall exactly what I said in the declaration, and I think what I said was based on preliminary data that I received from the Beak study, the Beak data that had been generated in the field.
Q BY MR. DODGE: Let me ask you to look at a declaration that I -- and ask you whether you signed it on September 8, 1989, and then if you did, I'll ask you to take a look at Paragraph 4 and see whether I've correctly summarized what you've said about habitat peaking in Rush Creek at 150 to 180 cfs.
HEARING OFFICER del PIERO: Take your time, Mr. Hanson.
MR. HANSON: I've reviewed it.
Q BY MR. DODGE: Is that a declaration that you signed?
A Yes, it is.
Q And did I accurately summarize in it terms of Paragraph 4?
A Yes. What it says is that flows sharply increase from 5 cfs -- excuse me, habitat sharply increases from 5 cfs up to 20 or 30 cfs followed by a gradual increase up to 150, 180 cfs. What I -- I frankly don't recall exactly what this is from. I mean, what data I was

0232

looking at, but I would tell you that I think what it is is preliminary analyses of the data that we were doing back in '89 when it was written.
Q Well, is it your testimony that it peaks at 100 cfs?
A No. My testimony is based on the more up-to-date analysis that I did in April of 1990 that it peaked at 20 cfs. I can't tell you the differences and the analysis that was done that led to that conclusion versus the analysis that was done that led to this -- not conclusion, but results. There may have -- you

notice that I took it up to 150 or 180 cfs? Well, if you've read this, you'd notice that I also indicated that we started overtopping our banks at 100 cfs in 8 of the 75 transects in the analysis, so that probably was one of the things that I changed or we changed going from that -- the materials in that declaration to what's in here, cutting back on the simulation up to higher flows because of the problem with the overtopping of transects.

So what I'm saying is that was based on a preliminary assessment of the analysis, and this was based, my present testimony, was based on the analysis that was done in 1990, which is more up to date and complete and more correct.

Q Let me change subjects with you, Sir. You said in your written testimony that as Rush Creek increased from 50 to 100 cfs, that you got a 10 percent increase in adult habitat. Do you recall that testimony?

A Yes.

Q Let me ask you -- aside from an adult habitat, are there other benefits of increasing from 50 to 100 cfs? Fisheries benefits?

A You mean from a weighted usable area perspective?

Q No. General fisheries benefits?

A Well, I don't have anything that I can think of off the top of my head that would improve fishery benefits other than maybe some of the studies related to geomorphological changes or the riparian changes of those flows, and I haven't reviewed that data well enough to answer the question specifically.

Q Will my change from 50 to 100 cfs move sediment?

A In the Cal Fish and Game report, there was an indication of flows above 60 cfs would move spawning gravels, but I don't feel that I have a good handle on all the studies that have been done relative to sediment transport in the system to know what flows are going to move sediments.

Q But there's a potential benefit for fisheries there in an increase from 50 cfs to 100 cfs, correct?

Ô

0234

A I think my testimony states that there would be a potential benefit to fisheries associated with normal channel maintenance or regular channel maintenance flows -- flushing flows which do move sediment, but I don't know what those values would be, whether it would be 100 cfs or whether it would be 200 cfs. I just don't have a clue.

Q Would an increase from 50 to 100 cfs have the beneficial effect of moving young fish, distributing them throughout the stream?

A I don't have an opinion on it. I haven't evaluated what the dynamics of the fry I presume you're talking about are in the river.

Q Would an increase from 50 to 100 have potential benefits for riparian vegetation?

A I would have to defer to Dr. Beschta to answer that question.

Q Now, the IFIMs look at the existing wetted channels, correct?

A Yes. In 1987, the IFIM looked at all the channels that were wetted, presuming that the transects went through areas where there were multiple channels and that sort of thing, but yes, in 1987, it would have included that consideration.

Q Now, if you went from 50 cfs to 100 cfs in Rush

0235

Creek but used some or all of the extra 50 cfs to rewater historic channels that are now dry, would that potentially have an increase in habitat over 10 percent?

A Say, going from 50 to 100 cfs, but you're leaving the 50 cfs in the main channel --

Q We're --

A Is that what you're saying.

Q Hypothetically, we're rewatering historic channels below the narrows of Rush Creek that are now dry and putting water year-round into those dry channels. My question to you is whether an increase from 50 to 100 cfs would potentially have greater than the 10 percent increase in weighted usable area that you testified to.

A There's a possibility that if you're opening up new areas, side channels with a higher flow and that higher flow isn't degrading the habitat in the main channel; that is, where the velocities are getting too swift, that the expanded areas off to the side would improve habitat. You would have an increase certainly if you're going to add adequate depths. It depends what the configuration of the side channels are in terms of the depths of the velocities that additional 50 cfs provided but, yes, there is the potential that there would be improved habitat greater than 10 percent

0236

that I showed.

Q Now, you developed site specific utilization information on adults, correct?

A Yes.

Q And that's a difference from what DFG did, correct?

A That's correct.
Q And you testified to certain water surface elevation measurements and those, you said, were done from 19 to 100 cfs. I believe there were four different flow, correct?
A Actually, it's 13 to 100.
Q Four different flows. But in terms of the utilization, that was just done at 19 cfs.
A That's correct.
Q Now, is there a problem with that?
A I don't have a problem with it.
Q If you look at -- look at Figure 3, which Ms. Cahill talked to you about and it shows the utilization basically going down to zero as the depths get to a little over two feet, correct?
A Yes.
Q And you wouldn't expect that as a biologist for adult brown trout, would you?
A Yes. The point is that fish -- adult brown trout

0237

can be found in a multitude of depths, depending on what is available in the stream. The distribution of depths that you observe in a given stream is a function of what is available to them, and as I was pointing out to Ms. Cahill earlier, Rush Creek does not provide many deep -- does not provide a lot of deep water even at the higher flows.
Q But you would find more deep water at 60 cfs than you would at 19 cfs, correct?
A You would find water -- the distribution that you would see would be similar to that distribution that you see in the middle curve there, add one foot.
Q So if were you looking at 60 cfs, you would have a lot more observations of two foot and above water wouldn't you?
A Yes. And to some degree, the correction factor that I applied going from the utilization to the preference function takes that into account.
Now, the point -- I'd also like to make one point here is that the depth criteria that were used here, first of all, reflect what was observed in the stream, and there may be a shift in depth if you collected data at 60 cfs for deeper water. And you could even go to the extent where you kept the depth criteria up to a high level, say, anything up to 100 feet is suitable.

0238

But I would tell you that the results of the analysis using that different -- those different curves showing preferences or suitabilities, or wherever you want to put it for deeper water would have, I think, a fairly small impact on the results of the analysis. There's two things to consider here in doing these analyses -- it's not being counted against my time, Mr. Chairman, is it?
HEARING OFFICER del PIERO: It is.
MR. DODGE: I would request an additional 20 minutes.
HEARING OFFICER del PIERO: You're granted an

additional 20 minutes.
MR. HANSON: Let me point out something that I've observed here. There's two levels of sensitivity to the model. Remember, there are different avenues to take in running an instream flow study. The sensitivity of the model is one thing to consider. Sensitivity of the decisions made, or what I call policy sensitivity, is another thing to consider. If these suitability criteria were extended to deeper depths, my sense is there would be some change in the suitability criteria -- excuse me. Some change in the output of the model, some sensitivity to that weighted usable area, but the sensitivity of the policy

decisions made from that new curve, I don't think would be that significantly different than what I've made here.
Q BY MR. DODGE: When I was a biologist --
HEARING OFFICER del PIERO: Mr. Dodge, just for the record, so it's clear, you need to make a showing of why you need the additional 20 minutes seeing as everyone else has.
MR. DODGE: I need the additional 20 minutes because this is very complicated and the witness is going on and on, sometimes unrelated to the questions.
HEARING OFFICER del PIERO: Proceed.
Q BY MR. DODGE: As a biologist, wouldn't you expect that curve on Figure 3, the top curve, to show a greater preference of brown trout for deep water?
A Again, I would not expect it to show that based on what is out there in Marsh Creek.
Q Let's -- you had very few observations of three-foot water at 19 cfs, correct?
A Probably true, yes.
Q In fact, you had very few observations of water greater than two feet; isn't that true?
HEARING OFFICER del PIERO: Excuse me, Mr. Dodge. I think it's already in the record that he -- you did not do the measurements yourself, did you?
Ô

b

0240

MR. HANSON: Oh, yeah, I'm not personally -- I thought he was referring --

HEARING OFFICER del PIERO: Are you referring to --

MR. DODGE: I'm referring to EA, yes.

HEARING OFFICER del PIERO: The last question was you did not or EA did not see very many examples of two-foot water?

MR. HANSON: That's correct.

Q BY MR. DODGE: So you had a very small sample in developing your utilization curve, a very small sample of observations of deep water; isn't that correct?

A It's because there's little deep water out there to -- if you go out in a random fashion, which is what we did, you're not going to see that much deep water.

Q Couldn't that small sample of deep water have affected that utilization curve?

A I'm not sure exactly how you mean. It's in there.

Q Aren't there inherent dangers in generalizing from a small sample?

A When we do a study like this, you did not collect the same number of data from different depths. You don't go out, for example, to collect 50 observations at one depth and 50 observations at another depth.

0241

Standard procedure is to go out in a different fashion and you will run into the depths in proportion to their distribution throughout the stream. And that's what we did.

Q Let's go back to Exhibit 15, Sir. Page 21. Do you have the IFIM on Rush Creek in front of you? Now, Page 21 you talk about habitat improvement, correct?

A Yes, I think so.

Q And you -- again, you say, as you've told us today, that Rush Creek is simply too shallow; is that correct?

A Yes.

Q And you talk about the possible habitat improvement by increasing the number of pools within the stream, correct?

A Yes.

Q And you told us today that -- today or at least in 1987 that there were a very small amount of pools in Rush Creek, correct?

A Yes.

Q So you're here recommending the creation of deep water habitat; isn't that right?

A I did so in that report.

Q And are you aware that the consultant under direction of Judge Finney has been doing just that?

0242

A I'm aware of that.

Q Now, you also, in this Exhibit 15 at Page 22, talk about planting riparian vegetation and putting in cover sources such as boulders, correct?

A Yes. I make that in reference to the return ditch only.

Q But wouldn't that make sense in all of Rush Creek?
A Well, no. The return ditch is essentially a ditch that, while it had very good depths and velocities to it, lack cover generally. My recommendation was that there's excellent depth and velocities, if you went in and put some boulders and created some -- instream boulders to create some overhead cover for fish, that would improve the value of that ditch considerably. And if it was more shaded with riparian vegetation, that would also improve the quality of the habitat. As I saw it in 1987 prior to the restoration program, it didn't have good fish habitat, and that was the reason why I stated earlier that we saw few fish in it.
Q Now, this -- this recommendation that pools be put in Rush Creek, that was made before Dr. Beschta and Dr. Chapman entered the scene, correct?
A That's correct. The notion behind that was if you want to get the depths up in Rush Creek, what I was saying was that by increasing flows, the depths are not

0243

increasing fast enough to achieve the depths that you want because the velocities are going to start to take over and degrade the habitat.
If you want to improve depth, I was suggesting that the river could be deeper in places, and that would do far better than trying to throw more water down the stream because the more water you put down the stream, the depth just wasn't increasing quickly enough.
Q And isn't it true, Sir, that the creation of pools would be a good thing from a fish habitat standpoint either at your recommended 20 to 30 cfs or at the DFG recommendations?
A More pools in Rush Creek I think would be beneficial, whether they can be created artificially or whether they'll occur naturally as a natural process. Either way, that, I think, would be beneficial to the trout population of Rush Creek.
Q Regardless of the flows?
A Yeah. I think there's a broad range of flows that really won't matter. As long as you've got some of those deep pools, you're going to improve habitat.
Q And when you wrote this report in -- on or about April of 1990, you felt that pools could be created in Rush Creek without an unacceptable injury to the

0244

riparian vegetation associated with Rush Creek?
A Yeah. Well, frankly, I wasn't thinking about that, whether the materials that were dug out from the stream would be put on the stream bank where the riparian vegetation was or not. I was simply theorizing if there was deeper water, pool habitat, it would improve conditions, and if that could be done artificially, then that would be beneficial for the stream.
Q And you didn't, in this report, call out any problems in doing that, did you?
A No, I didn't.

Q Now, flushing flows, you told us that you didn't have any recommendations today. Can you give us any standard by which you would recommend a flushing flow?
A Well, I know of some of the methods that are used for flushing flows. I been involved in some studies that have evaluated flushing flows and have some level of familiarity of the techniques. There are certain models that are sometimes applied, so-called incipient motion models, that will predict at what flows different particle sizes will be moved through the system.
There are other techniques that will evaluate what so-called a bank full discharge is, what that flow is,

the periodicity of that flow. This is the kind of thing that so-called effluvial geomorphologists deal with, and I have an understanding of it but not enough understanding to make my own specific recommendation.
Q If I were to give you the mean daily flow or the mean daily peak flow on any of these creeks, would that help you make a recommendation?
A Not really.
Q Now, just a couple more things and then I'll let you go. At Page 47, you talk about Rush Creek and a gravel replenishment or replacement program, and you said or you referred to, quote, some level of replenishment, end quote. And I take it that you meant by that that on some periodic basis there would be gravel replenishment?
A That's, yes, what I was assuming.
Q And is that, in your experience, common on regulated streams?
A I know where it has occurred, but I don't know if I would claim it to be common on regulated streams.
Q Would an approximate replenishment program of once every five years seem reasonable to you?
A I really don't know.
Q Is there any industry standard, to your knowledge?
A Not to my knowledge.

0246

Q Finally, just a couple of questions on the return ditch. I think you and Ms. Cahill established that DFG analyzed the return ditch and EA did not, correct?
A Well, we analyzed it. I mean, it's in our analysis, but it is not in the weighted usable area curve that you see there on Figure 2. It's not part of that calculation. In fact, we predicted a lot more habitat in the return ditch than rest of the stream. Based on the depth and velocity characteristics of that return ditch.
Q Now, if I were to tell you that the return ditch contains a lot of deeper water today, would that affect the comparison between your curve and the Department of Fish and Game curve?
MR. BIRMINGHAM: Objection. Ambiguous.
HEARING OFFICER del PIERO: Wait a second. Wait a second. You need to expand on what your objection is. I didn't think it was ambiguous.
MR. BIRMINGHAM: Deeper than what?
MR. DODGE: Substantially deeper than the rest of Rush Creek, is what I meant.
MR. BIRMINGHAM: With that clarification --
HEARING OFFICER del PIERO: I assumed he was talking about the baseline in '89, but go ahead.
MR. HANSON: I'm sorry. Would you repeat the

0247

whole question one more time?
HEARING OFFICER del PIERO: Sustained.
(Laughter.)
Q By MR. DODGE: If, in fact, the return ditch contains substantially deeper water than the rest of Rush Creek and you've talked about the difference between your curve and the DFG curve, I'm wondering whether that fact could bring the two curves closer together?
A Well, I'm not certain that the DFG curve did include the return ditch.
Q Assume it did, Sir.
A That might have some impact. I think the differences between the EA curve and the Fish and Game curve are based on habitat suitability criteria more than anything else.
Q The habitat suitability criteria?
A Yes.
Q And again, if I'm right, you used utilization and DFG used preference; is that right?
A I used utilization data from the stream. I also used preference data from the stream. Both those analyses are present in my report. Cal Fish and Game used preference data from streams off-site.
Q But Figure 2, your curve, uses utilization?
A That particular curve shown in that figure was

0248

based on the utilization data.
Q As I understand an IFIM in principle, you find utilization and then you look at the availability of the habitat -- you get the utilization and assign various values. You look in a given stream at the availability of that habitat and somehow you create a

preference from that; is that correct?
A Well, again, there is a debate on this subject.
Q That's one approach?
A That's right.
Q And, in fact, that is the approach that you started out to take in Exhibit 15, isn't it?
A I'm not sure I'd quite put it that that was the approach that I started out to take. It's the approach that I was generally using throughout the document. Are you talking about the correction approach or the utilization approach?
Q I'm talking about the fact that when you started the analysis in Exhibit 15, you were going to look at utilization and then availability and come up with a preference curve, correct?
A The general approach as we started out was to use the utilization data. As I've stated earlier, the -- again, general conventional wisdom is that if utilization data are collected in the stream upon which

0249

the IFIM study has been conducted, then this there isn't a need to correct for availability.
Q But if you look at Figure 2, and -- your curve, as you said, I think, peaks at about 20 cfs based on the utilization curve. What would your curve look like under the preference curve approach?
A Well, using the data from Rush Creek, it's shown in my report.
Q That curve would be more similar to the DFG curve, wouldn't it?
A A little bit. It would start to be a little bit more similar.
MR. DODGE: Just give me a second, Mr. Chairman, if you would.
Thank you, Mr. Hanson. No further questions.
HEARING OFFICER del PIERO: Thank you very much, Mr. Dodge.
Mr. Roos-Collins, do you have questions, Sir?
MR. ROOS-COLLINS: I do.
HEARING OFFICER del PIERO: Fine. We're going take a break for ten minutes and start again promptly at five o'clock.
(Whereupon a recess was taken.)
HEARING OFFICER del PIERO: We're back on the record again.

0250

Mr. Dodge, are you here somewhere? There you are. Mr. Birmingham, I understand we have a problem with the availability of the witness after two minutes from now; is that true?
MR. BIRMINGHAM: Yes. Mr. Hanson has a commitment in Walnut Creek this evening, and he promised his spouse that he would be back by seven o'clock and that he would leave here by five.
HEARING OFFICER del PIERO: Mr. Hanson, you're going to be here tomorrow morning?
MR. HANSON: Absolutely.
HEARING OFFICER del PIERO: Mr. Roos-Collins, I

understand your cross-examination is going to take at least 30 minutes; is that true?
MR. ROOS-COLLINS: That's correct.
HEARING OFFICER del PIERO: Given the nature of the hour, Ladies and Gentlemen --
MR. THOMAS: We have one procedural question with relation to Mr. Tillemans tomorrow. I'd like to have some explanation about the relevancy of his Crowley Lake testimony so we can file an objection now so that you could rule, and we could know of his availability or not tomorrow.
HEARING OFFICER del PIERO: He's going to be available tomorrow.

0251

MR. THOMAS: There is some question whether the discussion about Crowley Lake is at all relevant to the water rights licenses in the Mono Basin, so we would object to the testimony on Crowley Lake insofar as it's not relevant to the --
HEARING OFFICER del PIERO: Let me suggest that if you want to raise that objection, you can raise it tomorrow.
MR. THOMAS: We can do it tomorrow as well.
HEARING OFFICER del PIERO: Tomorrow's more appropriate than right now. I'll be happy to rule on it at that time.
MR. THOMAS: If you ruled in our favor, you wouldn't need to hear Mr. Tillemans tomorrow.
HEARING OFFICER del PIERO: Well -- we'll see everybody tomorrow morning and then if he gets to go home early, he can go home early, so he gets to spend one more delightful evening in the great metropolitan area of Sacramento. Okay?
Mr. Roos-Collins, you're on tap first thing in the morning.
Mr. Hanson, you'll be here bright and early ready to go promptly at nine o'clock.
MR. HANSON: I'll be here at eight, if you want.
HEARING OFFICER del PIERO: Actually, see --
Ô

0252

see -- I would prefer starting at eight o'clock, but in the interests of everyone else, I'm inclined to think I'd probably hear howls of objection. Is that true? It wasn't noticed at nine o'clock, I don't think. Was it? It was just noticed for day. Would people mind beginning earlier?
MR. DODGE: Our preference would be to begin earlier and end earlier.
HEARING OFFICER del PIERO: I understand that, but -- my preference is to finish sometime within the calendar year of 1993, so -- the second portion of your preference isn't necessarily going to be receiving a tremendous amount of consideration.
MR. BIRMINGHAM: Would 8:30 be an appropriate hour to start tomorrow?
HEARING OFFICER del PIERO: Ms. Anglin, can you be

here at 8:30?
THE REPORTER: Absolutely.
HEARING OFFICER del PIERO: Any objections to beginning at 8:30 in the morning?
We're going to adjourn for the evening and start again at 8:30, and then we'll continue with the cross-examination. I'll take up your issue after the cross-examination -- after both cross-examination and redirect and recross of this is completed.

0253

I would point out for the record Mrs. Anglin has some of the transcripts. I know she distributed some of them, but there are some that are available. I'd strongly recommend counsel for the various parties get together with her after this in order to make sure that they get all that she has available.
Any other questions before I close this out for the day?
MR. CANADAY: Mr. del Piero?
HEARING OFFICER del PIERO: Yes, Sir, Mr. Canaday?
MR. CANADAY: Of course, my standard admonition about the garbage, but I've been informed by Maureen Marche that we can calendar December 1st.
HEARING OFFICER del PIERO: December 1st?
MR. CANADAY: Yes. In December we were going to start on Thursday the 2nd.
HEARING OFFICER del PIERO: Yes.
MR. CANADAY: But we now have Wednesday the 1st as a day of opportunity.
HEARING OFFICER del PIERO: What happened to the other hearing that was scheduled for the 1st?
MR. CANADAY: I'm not sure. All I know is --
HEARING OFFICER del PIERO: There was another hearing scheduled for the 1st and, in fact, I was

0254

supposed to have a meeting with Mr. Pettit about it this afternoon at five o'clock, so --
MR. CANADAY: We can firm that up tomorrow morning.
HEARING OFFICER del PIERO: Would you call Mr. Pettit's office and make sure he's still here, so I can see him before we start calendaring the 1st of December?
Ladies and Gentlemen, I'll see you tomorrow morning at 8:30.
(Whereupon the proceedings adjourned at 5:01 p.m.)

---o0o---

REPORTER'S CERTIFICATE

---o0o---

STATE OF CALIFORNIA)
) ss.
COUNTY OF SACRAMENTO)

I, KELSEY DAVENPORT ANGLIN, certify that I was the official court reporter for the proceedings named herein; and that as such reporter, I reported, in verbatim shorthand writing, those proceedings, that I thereafter caused my shorthand writing to be reduced to typewriting, and the pages numbered 1 through 255 herein constitute a complete, true and correct record of the proceedings:

PRESIDING OFFICER: Marc del Piero
JURISDICTION: State Water Resources Control Board
CAUSE: Mono Lake Diversions
DATE OF PROCEEDINGS: November 9, 1993

IN WITNESS WHEREOF, I have subscribed this certificate at Sacramento, California, on this 16th day of November, 1993.

_____
Kelsey Davenport Anglin, RPR, CM, CSR No. 8553