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PUBLIC HEARING

STATE WATER RESOURCES CONTROL BOARD

DIVISION OF WATER RIGHTS

STATE OF CALIFORNIA

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

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Held in

Resources Building

Sacramento, California

Wednesday, December 15, 1993

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

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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:

HAL THOMAS

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
ADOLPH MOSKOVITZ
Attorneys at Law
Kronick, Moskovitz, Tiedemann & Girard
400 Capitol Mall, 27th Floor
Sacramento, California 95814

For State Lands Commission, Department of Parks and Recreation:

MARY SCOONOVER
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

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E X H I B I T S

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SACRAMENTO, CALIFORNIA
WEDNESDAY, DECEMBER 15, 1993, 8:00 A.M.
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HEARING OFFICER DEL PIERO: Ladies and Gentlemen, this hearing will come to order. I'm sorry I'm ten minutes late. If I hadn't driven around a wreck on Garden Highway, I would have been even longer.

Where were we when last we left yesterday?

Mr. Smith, were you on?

MR. SMITH: Yes.

HEARING OFFICER DEL PIERO: This is a continuation of the hearing regarding the City of Los Angeles' water rights licenses on tributaries to Mono Lake.

Mr. Smith?

MR. SMITH: Yes. I have one question. I guess these aren't working.

MR. CANADAY: You have to click the thing there.

HEARING OFFICER DEL PIERO: There you go, Dr. Smith.

CROSS-EXAMINATION BY THE STAFF

Q Thank you. I have one question, but I'd like to pose it in a series of questions about the exports at the lower -- Upper Owens at the -- at the portal.

Now, if I'm not mistaken, you said that the THA, total habitat available, would be greatest at

approximately 250 cfs. Is that correct?
A BY DR. SITTS: Yes.
Q Okay. And for you, Mr. Smith, the department's recommendation is for a cap of 200 cfs in the flow coming out of the portal?
A BY MR. GARY SMITH: That's correct.
Q So would that combine with the natural amount that's in the river?
A I think I need to clarify that. I may have misunderstood or misspoke.
Q I'm asking the question for clarification.
A It is a maximum of 200 cfs in the Owens River downstream, immediately downstream, if you will, of the east portal, so that includes base flow in the Owen River plus augmentation from the Mono Craters Tunnel.
Q Okay. So now to be precise about this, we're talking approximately 50 natural spring flow in the river and approximately 150 from the portal. Okay?
A Correct.
Q Okay. So that would be your recommendation as a cap?
A Yes.
Q Would there be any circumstances where you would like to have more as a stream augmentation, a stream maintenance flow? I want -- this is very important to

me because I'm running a computer model on this thing and also considering some restoration stuff, so I need to know whether you could consider at times of the year higher flows?
A Under natural conditions, the flow in the Upper Owens could go above 200 cfs. At that time -- that's a natural condition, but at that time, I would say that there should not be augmentation from the Mono Crater Tunnel.

If there is a need, an opportunity for additional water from the Mono Craters Tunnel, it should not cause the Upper Owens flow to exceed 200 cfs. There are opportunities, I believe, for some augmentation during the irrigation periods to make up for stream flow losses due to irrigation.
Q Okay. I'm not trying to belabor this point, but I just want to make it very, very clear. We've been talking a lot in terms of Rush Creek and also in terms of Lee Vining, having stream maintenance flows that go over the banks. Okay?
A Correct.
Q Okay. Now, you're saying, in other words, that you don't want to have anything like that happen in the Upper Owens?
A Artificially.

Q Artificially.
A Artificially. What I'm saying is when those higher flows, the flows that naturally occur in the system and naturally overbank -- first off, we have no control over that and, secondly, the department is

comfortable with that. What we want to avoid are circumstances where we have artificial overbanking particularly for long-term -- on a long-term basis. There -- it might be a good idea for Mr. Wolff to address the potential problems and the options, having flows in excess of 200 cfs.

Now, if it occurs naturally, that's the most -- that's the thing that we really don't oppose.

Q You're answering my question, thank you.

Mr. Wolff, would you like to elaborate?

A BY MR. WOLFF: First of all, I think I should explain a little bit about the -- people have been talking about flushing flows or stream maintenance flows.

Q Yes.

A I think the issue on the Owens River is entirely different than in the Mono Basin streams because the Owens River is a river that has a lot flatter gradient. The average channel slope is about, on an order of magnitude, less than the streams that we studied, that is Parker and Walker Creeks. And the bed

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of the channel has a lot finer sediment in the Owens River than those channels. So in terms of flushing flows, which people were talking about in the Mono Basin streams, that really is not an issue on the Owens River.

The flow -- the natural flows in the Owens River are always sufficient all the time to mobilize the bed sediments and to -- in a sense, then, keep the fine sediment flushed from the bed. So you don't need to release flow in order to achieve any kind of a flushing flow effect from the Owens River. The river is, because of the nature of the sediments and the bed of the river, there is -- it just is an issue.

In terms of overbank flows in the Owens River, I'm not -- I don't know much about the riparian vegetation situation or anything like that, so in terms of needing overbank flows for maintenance in that sense, I can't address that. The concern I would have, though, in terms of sustained overbank flows, it becomes, to me, a channel stability problem because the Owens River, historically, because of the flow augmentation from the Mono Craters Tunnel -- and with significant overbank flows, there has been a lot of channel evulsions. The channel has changed courses, meander bends have been cut off and whole new channels have formed. Part of

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that, I believe, is due to the significant overbank flows.

So my recommendation in terms of overbank flows is it's a natural process, and I don't think it's a problem if the flow goes overbank occasionally. That's just a natural process in a river. I think the concern that I might have, and I believe this is the concern of the department, is if the flows are -- if you have sustained overbank flows, unnaturally, due to augmentation, then there's a potential for channel stability problems. And I think that should be avoided, and I think that's what Gary Smith was saying in terms of if the flows are -- if there's really a

high run-off year, if the flows are getting way up above 200 cfs naturally, then the -- the flows from the tunnel should be limited under those conditions.
Q Dr. Stine had suggested that we have something in the way of perhaps an extended channel along the side, or perhaps something like a pipe in order to, you know, take these additional flows if they were available.
A Uh-huh.
Q Would you have anything inherently against something like that?
A No, I wouldn't. If it's economically feasible to build that, it would be a good solution from a

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technical standpoint. From a technical standpoint, the degree that you could relieve high flows from the main river, that's a good thing. And that was exactly what was done with the north ditch on the Conley -- what was it? The Nyo (phonetic) Ranch area there, and I guess some of the work that a Basco (phonetic) did on the meander bend cutoffs in that area show that that channel was at least partially effective in limiting changes on the main channel from the augmented flows.
So that one situation there might act as a model for success of doing that over a greater length of the river.
Q And you, Mr. Smith, in terms of Dr. Stine's suggestion?
A BY MR. GARY SMITH: I think that that suggestion deserves some examination. We haven't formed an opinion on it, yet, the department, and therefore, I can't -- I can't give you a clear response to your question. I do believe it deserves evaluation. I think it may have merit.
MR. SMITH: Thank you.
Q BY MR. HERRERA: Good morning, Gentlemen.
I'd kind of like to start with discussions about the Upper Owens after L.A.'s diversions up to the time that you began your studies, Dr. Sitts, a little bit.

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You did a little bit of a historic background of the stream channel during that time frame, did you not?
A BY DR. SITTS: Yes.
Q Did the stream, itself, stream channel, react to high flows? How did it react to those high flows? Did it come to some sort of equilibrium with those higher flows as they were diverted out of the Mono Basin?
A The theme in our report is that it did make some adjustments, and we don't have evidence to conclude that, you know, it was finally adjusted, but it certainly did make a number of adjustments. It entered in some areas and it appears to have widened, and we base this on prior century comparisons of channel widths to what we measured,as well as 1944 aerial photographs. Whatever that exhibit is up there on the board, 105 or 106, the 1944 series of photographs on the Upper Owens indicated that there was some straightening, obviously, a widening going on as well.
Q And that's occurred over quite a period of time; is that correct?
A It's occurred over decades.

Q What was the kind of the magnitude of those flows? The higher flows?
A The higher -- the higher flows, the combined flows with east portal and the baseline flows were up in the

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neighborhood of about 380 on a continuous level, and we're getting averages well over 200, 230 or so on a monthly average.
Q So as a monthly average in the Upper Owens with L.A.'s export operations, it's been over 200 cfs pretty consistently?
A Yes. Yes. However, I'd add that some of the figures, you can also tell that the fluctuations in those flows during the course of a year or in the course of a month change quite dramatically so that, for instance, in Exhibit 62, Figure 9, you can indicate there, you know, February might have had a flow somewhere around 100 but, in fact, a portion of the month was 200 and the other portion was 100. And it changed in the course of one or two days. Same thing happens in July where we see a change where it falls from about 300 down to in the neighborhood of 120 or so within the matter of a few days.
Q No matter what, those flows were all higher than the natural flow of the Owens at east portal?
A By definition, yeah.
Q And that's in the magnitude of --
A On an annual average cfs basis, it's on the magnitude of 92.
Q 92. Okay. So there's an additional 92 cfs at

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east portal?
A Yes. On the average.
Q Let me ask you this, if this Board was to reduce those flows back to, let's say, an extreme condition of no export of water, which has occurred for the last few years, how do you expect the stream channel to react to that below east portal?
A The stream channel?
Q Yes.
A I would expect that it would adjust to some extent. It would probably narrow. There would be more deposition. There would still be continuous flow all the way to the river.
Q At what flows did you conduct your studies?
A Our studies were conducted at baseline conditions and a little lower, I think somewhere in the neighborhood of 50 cfs below east portal.
Q 50 cfs and --
A And that's an approximate. We didn't always measure the flows when we were out there in the course of our days, but it was an augmented condition well into the drought.
Q And we heard testimony earlier that there was a release of water primarily for the fishery study in the Upper Owens, and that that was to provide a higher flow

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of some sort?
A Yes. The instream flow study was a short duration in October of '91 where the flows were elevated to the

neighborhood of 200 cfs below east portal for a few days for the purposes of taking measurements.
Q So for the purpose of your study, you essentially did a low flow somewhere around -- if my memory serves me correctly, around 55 cfs?
A On Page 100 in Table 38 of Exhibit 62, it specifies the flows at which we measured our instream flows at each of the locations.
Q The high flow was 210; is that correct, according to that?
A In Table 38, the highest flow is below Benton Crossing and that's 218.
Q And what was the high -- what was the high flow at east portal?
A The high flow at east portal? Probably was around 178 or 175 and in Table 38, I'm using the highest number of Hot Creek which, you know, was before the major accretion.
Q During your studies, you did not actually observe flows at east portal in excess of 200 and --
A Not to my knowledge.
Q Okay. So in making the recommendations you made

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for higher flows, the problems with higher flows above 200, is that an extrapolation of the data?
Two questions. I can see Mr. Wolff reaching for the mike, but number one, is in effect to the fishery. We talked the other day with Mr. Payne (phonetic) regarding extrapolation of two and a half times upward for various flushing flow scenarios and the effects of that on various aspects of the IFIM. Is that essentially how you got to your 270 cfs at Hot Creek confluence? I'm still a little bit confused how you got that 270.
A The 270. I'm going to take a crack at responding to your question and Mr. Wolff may add some more detail to clarify it. The 270 cfs was a recommendation for below the confluence of Hot Creek. It was based not on the IFIM, it was based on the computations of bank full capacity in Table 9, which I went over with Mr. Satkowski.
I took a look at the numbers in that table and observed 280, 290, down in the neighborhood of the confluence of Hot Creek. Those cross-sections were in the vicinity of where the two streams come together. The 270 was to stay below the 280, 290 numbers. However, it appears that I was upstream a little bit further than the model was estimating flows for, so you

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may want to loosen up on that 270 number at this point. But it was derived from the estimates of bank full capacity and recognizing that Hot Creek added more water to the river, the channel was bigger there, and it could accommodate more water. And the idea was to try to stay within the natural fluctuations of extreme conditions in the river.
Q Natural conditions prior to the export of water?
A Natural conditions being and extreme conditions being without the augmentation. What would occur there based on the in basin conditions.

Q Mr. Wolff, do you have something to add to that?
A BY MR. WOLFF: I guess what I would add in terms of any kind of a recommendation that the Board might make in terms of limiting flows, I think that probably the best way to do that would be to manage the river based on the flows just below the east portal. If you do that, if you -- the flows at east portal are known. Below hot creek, there is no gauge there on the river. The flows we have our model -- which estimated the flows there based on the flows at the east portal and based on what we estimated coming from Hot Creek. But there is no gauge there, and I think in terms of a river management plan, I think it would be easiest to base your management on the flows at the east portal

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and let the river below Hot Creek adjust naturally to any inflow from Hot Creek.
Q Let's discuss that "react naturally". On Page 67 of the Upper Owens report DFG 62, it talks about irrigation channels, and on that -- the first paragraph below the -- the heading, Irrigation Channels, it indicates 11 operational open irrigation channels were identified with eight, three, and zero in the upper, middle, and lower reaches respectively. And I believe further on in the discussion in this report, it identifies the rough guess of the amount of water being utilized by these channels.
How would you react to the natural -- let the stream come to natural conditions if we have 11 various irrigation channels coming out of this reach of the stream?
A I guess my conclusion based on the channel stability analysis, now, this in -- has nothing to do with the fisheries or anything like that at lower flows, but when we're talking about channel stability issues, we're talking about very high flows of 200 cfs or more above the confluence with Hot Creek and potentially substantially more due to the Hot Creek inflows. And I think that the amount of flow in those irrigation channels starts to become kind of in the

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gray area of the accuracy of the analysis. So I don't feel like the -- in terms of channel stability, I don't feel like those irrigation canals are a major issue.
If anything, during really high flows, there might be excess water out there, and they're relieving some of the pressure on the main channel. But in terms of -- I don't think the -- I don't think they're a big issue in terms of management in terms of channel stability.
Q Do these irrigation structures have any control structures on them for release of water from the main channel out into these irrigation canals?
A BY DR. SITTS: Some have flash boards.
Q I think you had, Dr. Sitts, in your report, you indicated that there is some problem with these irrigation channels to the fishery. Could you elaborate a bit on that?
A Sure. Two problems we can identify are the entrainment effect, the fish actually go into the

irrigation channel. They go out to the pasture, and they're stranded there. They die. The other aspect of it is that if water is withdrawn from the main channel, and downstream in the main channel the flow is less, and from the habitat area, flow relationships, we see a decline in the habitat area. And this would occur

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during, of course, the time that there's irrigation diversion there.
Q You made some recommendations on how to correct that problem.
A I -- yes. We made the recommendations to try to minimize the effects of those either by some type of screen or perhaps coalescing intakes and maybe taking a careful look at the amount of flow that's actually needed to divert, to cultivate, and irrigate the pastures.
Q Mr. Smith, have you looked into that situation as well, as a representative of the department, as to the problem of stranding fish from these irrigation canals?
A BY MR. GARY SMITH: Only from the perspective of preparing it for the Board.
Q The department hadn't looked into this prior to the preparation for the Board?
A I think Curtis Milliron should respond to that question.
A BY MR. MILLIRON: As part of the Crowley management plan, that issue has been looked at in the past and will be addressed in the plan for all the tributaries to Crowley including Comnicki, McGee, and the other tributaries that are not affected by portal flows. So

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that's been something that we've discussed with Los Angeles for many many years, discussing fish screens and recently getting into a dialogue of how we can operate these diversions in such a manner that we might reduce the impact of fish. Nothing substantial has come out of that yet, but it's a section in the management plan that we hope to work on in the future.
Q Are all of these canals on L.A. DWP lands, or are they on other privately held lands?
A The irrigation canals that I'm referring to are on Los Angeles property. But the private land holders do irrigate.
Q And they have a similar situation on private land as well?
A Irrigation canals are generally constructed the same and generally, depending upon the function of that particular canal, they're either left on for long periods of time and take a lot of flow, or they can take smaller amounts of flow, and be turned on and off frequently.
A BY DR. SITTS: The locations of the irrigation canals that we observed in our habitat mapping and walks up the river are indicated on Page 68 of Exhibit DFG 62.
Q DFG 62. Thank you.
Mr. Milliron, let's discuss a little bit more

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about the restoration or the -- I guess it's

restoration of this problem or resolve to this problem that is identified by Dr. Sitts in the report. Do you agree with screening and those sorts of techniques that would be required to alleviate that problem?
A BY DR. SITTS: Screening is an alternative that I've explored. It can alleviate the problem. There's a maintenance aspect, an initial cost aspect, and so it's not a problem that easily goes away with a central fix. Screens require annual maintenance as well as perhaps weekly maintenance, and so there's -- it's a real commitment if that is the method of -- to alleviate the problem.
I think that one of the big problems, if I might, is that there's really very minimal lack of understanding and control over the turning on and off of canals in regards to how that affects fisheries. We know that many fish are entrained. We've electroshocked diversion ditches and have caught many fish, and we know that they die, many of them die, when the canals are turned off.
Q Mr. Smith, these reports are Fish and Game publications. Are these recommendations that have been developed in here, one of which was screening to alleviate that problem, is that the recommendation of

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the department?
A BY MR. GARY SMITH: We would like to see those diversions screened or another mechanism put in place to prevent or minimize the entrainment of fish in the irrigation ditches.
Q Is it the department's recommendation that in this decision the Board is attempting to do here that we impose these types of restorations or recommendations upon private land holders as well as L.A. DWP?
MS. CAHILL: Objection to the extent that that calls for any kind of legal conclusion.
HEARING OFFICER DEL PIERO: You can go ahead and answer the question.
MR. GARY SMITH: From a biological perspective, it would be desirable to implement some mechanism, whether that's screening or combining -- one's a diversion or minimizing diversions, some mechanism to avoid entrainment.
MR. MILLIRON: There are alternatives to screening, and screening is effective. It's used extensively for anadromous fisheries on the north coast, and we have had screen shock personnel out to the Crowley tributaries to look at the kind of diversions we have and to look at the feasibility of screening. The answer there is that it is feasible, it

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will work. But the maintenance problem, as I discussed, is a real concern.
Also, looking into the potential of using electric fencing to discourage the movement of fish into diversion ditches. I think there are lots of alternatives out there and the management plan is looking to move into those in terms of all of the tributaries to Crowley. Something needs to be done. It's a significant issue.

Q I guess from the answer I got here that the recommendations that are provided in DFG 62 are that they are the recommendations of Fish and Game to this Board?

MR. THOMAS: Objection. This was asked and answered several times. We've been very clear that our recommendations are contained in the report.

HEARING OFFICER DEL PIERO: I'm going to sustain the objection. I need to caution you so we don't have the problem for the rest of the day, Mr. Thomas, Ms. Cahill needs to make the objections.

MR. THOMAS: As long as our staff can maintain some kind of deference to what we've been concerned about all along, I will maintain deference.

HEARING OFFICER DEL PIERO: That's fine.

MR. HERRERA: I guess the problem I'm trying to

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have here is I'm getting different answers, Mr. Del Piero, as to what is the recommendations here, and I'm just trying to determine what that is.

HEARING OFFICER DEL PIERO: It was indicated earlier that if there are questions that are left unanswered in regards to the position of the department, those should be prepared in writing and they can be addressed at the end. If that's posing a problem for you, Mr. Herrera, that's one way of addressing the situation. The balance of it is basically what's in writing. There's some representations that have been made by the representation of the department, one can reasonably assume that's the position of the department or that representation would not been made unless they choose to object.

If the department has mutually inconsistent recommendations, it's up to the State Board to remedy that problem.

MR. HERRERA: Okay. Thank you.

Q BY MR. HERRERA: Mr. Milliron, I would like to discuss a little bit of Lake Crowley problems here that you identified. That in 1989, 1990 there was a fish kill that you attributed to a low lake level in Crowley; is that correct?

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A BY MR. MILLIRON: I identified that there was a fish kill. It wasn't that there was a low lake level. We've had much lower lake levels without fish kills. Rather, it was the management of the stores of water in Crowley and the rapid drawdown of the reservoir through lake bottom sediments that resulted in the fish kill. I have a couple of slides that might better illustrate that point, if you want to see them now.

Q I'd like to pursue this a little bit.

A Okay.

Q Since the '89-90 occurrence of this fish kill, have there been any additional fish kills in Lake Crowley?

A Nothing of substance, no.

Q And you would attribute this to more applicable operations of lake Crowley by L.A. DWP?

A I attribute it to lake level management, water

storage management, if you will.
Q To your knowledge, during this time frame, was there any export of water from Mono Basin to contribute to Crowley Lake?
A It was the fall of '89 and spring of '90. Not being intimately familiar with the tunnel flows, I can't answer that.
Q Let us assume that since that time frame, that

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L.A. has not contributed any sizable amounts of water to Crowley but yet they've operated the lake in a fashion that has not caused a fish problem. Is that true? Would you consider that to be true?
A Since that time, there has not been a fish kill problem like that that we've experienced.
Q Early on, we heard discussion, I believe, during L.A.'s presentation, that at one time, there was an algae problem in Crowley Lake, and it was a single occurrence situation where they treated it or someone treated it with copper sulfate. Does that algae problem exist, continually exist at Crowley?
A Algae in Crowley has been a problem since Crowley's had water in it. The problem insofar as it may not be as compatible as water skiers would like it to be. It's certainly a component of the ecosystem there, and it's kind of a two-edged sword. Anglers might not like to have as much algae in the water and yet if we remove the algae with some kind of a treatment as has been done at one time, then the resultant fish growth is impacted. And that is the reason why -- I believe that's the reason why L.A. does not treat the algae in Crowley at this time is because it does have a direct impact on the growth of fish. The loss of their food.

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Q In your opinion, would a reduced inflow and/or lower lake levels in Crowley intensify this problem?
A That's a difficult question to answer. I don't think I can do that. We've had -- this water year, we've had a lot more water than we've had in the past, and we've had some algae problems in some of the areas of the lake that I wouldn't have expected, so I can't really answer that.
We did have a fish kill that may have been due to or exacerbated by algae in the seventies and when I looked at the record, the lake level was quite high during that period so it didn't seem to correlate with lake level.
MR. HERRERA: I believe this concludes my questions. Thank you, Gentlemen.
HEARING OFFICER DEL PIERO: Mr. Canaday?
Q BY MR. CANADAY: Mr. Milliron, I was doing other things when you were testifying earlier yesterday, this panel. When was this management plant, Crowley Lake management plan due? I don't recall.
A BY MR. MILLIRON: Due?
Q When will it be available, rather than due? I don't like deadlines, either.
A 1994? 1994.
Q 1994.

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A Shoot, it's being written down.
Q Sorry.
A Thanks.
Q Mr. Smith, on -- we've had discussions about management of the Upper Owens River, and I want to get clear in my mind of the best way to manage the flows in the river or the management point. Would you agree from a management perspective it's better to try to make flow determinations or decisions with the existing flow gauges that are below, immediately below the east portal rather than relying on some additional flow measurement below Hot Creek?
A BY MR. GARY SMITH: In general, yes. If circumstances were to change, it might be appropriate to install gauging devices downstream. I think Mr. Wolff stated a moment ago that basing the Upper Owens flows on flows immediately downstream of the east portal is a logical start. And subsequent to the Board making its decision and monitoring -- following up evaluation, if it's determined that another gauge would be appropriate, I think that should be considered. But to begin with, I think right now, the one gauge is probably sufficient.
Q So your advice to the Board in this decision is that that would be the -- the point that they should
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consider with some sort of monitoring program?
A Yes.
Q Dr. Sitts, you were questioned yesterday about your decision not to use Smith and Aceituno preference curves in the Upper Owens. We've heard testimony today that the Upper Owens is significantly different in its -- Mr. Wolff described slope? Was it the --
A BY MR. WOLFF: Yeah. The channel slope.
Q The channel slope as compared to Lower Rush Creek?
MR. BIRMINGHAM: Excuse me. I'm going to object on the grounds that it misstates the evidence. Mr. Wolff's testimony was restricted to his study on Walker and Parker. He did not go into Rush.
Q BY MR. CANADAY: Walker and Parker; is that correct?
A BY MR. WOLFF: Yes, that's correct.
Q Mr. Smith, in your development of the Smith and Aceituno studies and your studies of streams on the east side of the Sierra, those were generally high gradient streams issuing from the Sierra's themselves, correct, from the escarpment?
A BY MR. GARY SMITH: Generally, as I explained to Mr. Birmingham, there were areas of lower gradient.
Q Were there streams that, based on your experience in the Upper Owens, that were very similar to the Upper
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Owens or dissimilar to the Upper Owens?
A We actually sampled the Upper Owens.
Q You did?
A Yes.
Q Dr. Sitts, your decision to not use Smith and Aceituno, that was in consultation with Mr. Smith, wasn't it?

A BY DR. SITTS: Yes.
Q So the department looked at that and decided in consultation with you that you better use site specific preference curves; is that right?
A Yeah. It was a mutual agreement to go forward as we did.
Q You talk, Doctor -- Dr. Sitts, you talk about grazing impacts and water quality on the Upper Owens. Was your focus primarily on private lands, L.A. DWP lands, or a combination?
A The livestock and water quality?
A Yes.
Q We didn't distinguish between ownerships. There seemed to be cattle grazing from one end to the other.
Q But could you distinguish between the -- were there places on the river that it was a greater problem than others?
A Yes. In the lower reach, we did not get into the

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water to do criteria measurements because there were concerns about the quality of the water, the health of the water, and the health of people being in that water, so we avoided that.
Q And the ownership of those lands were?
A That's below the electrical transmission, and that's L.A.
Q I'd like to, now, move to Walker and Parker Creek studies. I'm referring to, first of all, DFG Exhibit 56, which is the Walker Creek stream evaluation report 92-1, Volume One, and Page 118. And it's the last paragraph.
A Okay.
Q Could you read that, please?
A You would like me to read it?
Q Yes, please.
A The entire paragraph?
Q Yes, Sir.
A "It was expected that the present flow regime would continue to provide productive fish habitat until an instream flow study could be conducted and optimal flows were in place. Fish habitat from the conduit to Rush Creek has been provided for under the present flow regime. The Basco (phonetic) Environmental '91, 1992, this regime has supported healthy trout and diversion

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populations of aquatic invertebrates. Further, water temperatures have been within the optimum range for trout and the channel location appeared stable, period."
Q Thank you. Mr. Smith, is it the position of the department that an additional instream flow study is necessary to develop instream flow recommendations to the Board?
A BY MR. GARY SMITH: Once the -- those channels, in this case, the Walker Creek channel, begins to obtain some state of dynamic equilibrium, we would recommend that another study be conducted at that time, yes, for refinement of our flow recommendations.
Q I refer you to DFG Exhibit 161, which is a letter to Division Chief Ed Anton (phonetic) dated June 21st,

1993. The subject of the letter is Walker Creek, and the stream evaluation report 92-1. I'll read the middle paragraph. Stream evaluation report 92-1 was prepared pursuant to Sections 10003 and 10004, stream protection standards. Other Public Resources Code, Assembly Bill 1580, Chapter 1241, statutes of 1989, and Fish and Game Code Sections 5937 and 5946. The stream flow requirements identified are stream flows necessary to keep fish in good condition as required under Fish and Game Code Sections 5937, 5946. So the

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recommendations in this report are adequate to meet those conditions, but we need another instream flow study?

A Excuse me. They're adequate to meet the conditions for minimum flows in these conditions to keep fish in good condition given the state of the stream. Once that stream has evolved, there's a need to refine those flows.

Q Do you have a time scale when that would occur?

A It would be speculation, but again, ten years -- five years, ten years. I would have to have the state of the stream evaluated today to give you a more refined estimate.

Q Dr. Sitts, I'd like to refer you to the Parker Creek stream evaluation report 1992-2, Volume One, which is DFG Exhibit 58, and specifically Page 119.

The second paragraph, would you please read that into the record?

A BY DR. SITTS: Second from the top?

Q Yes, Sir.

A "Flow recommendations in the plan were designed to facilitate optimization of fish habitat conditions by refining the flow regime. The strategy was based on the expectation that the recommended regime would continue to maintain productive fish habitat in the

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stable channel from the conduit in Rush Creek until the refined flow regime was in place."

Q Thank you. I guess my question to you, Mr. Smith, is the same. We have a letter to Mr. Anton (phonetic) dated June 21st, 1993, relative Parker Creek, and instead of reading the whole paragraph, I'll read the last paragraph or the last part of the middle paragraph of that letter, and this is DFG Exhibit 160 relating to Parker Creek.

"The stream flow requirements identified are stream flows necessary to keep fish in good condition as required under Fish and Game Code 5937 and 5946." So my question to you, again, is essentially the same. The department's recommendation is, as it stands today, is to maintain 59 -- meet the fish in good condition under 5937 or 5946. However, the department intends to, at some later date, conduct an instream flow study to optimize the flow conditions.

A BY MR. GARY SMITH: Well, that's not quite the same question. The difference was the department intends to conduct a stream flow assessment. That's the little wording difference there. My response to your question is roughly the same, and that is the stream flows

are -- would be the minimum given the state of the stream, the minimum, and based on our judgment, they

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are needed to comply with 5937 and refinement of these flows in the future is essential.
Q Dr. Sitts, I hate making you the straight person on this, but these are your reports. I'll refer you to DFG 60, which is the South Parker Creek stream evaluation report 92-3, Volume One.
A BY DR. STITTS: Okay. Just a second. I can handle it.
Q Page 47, Dr. Sitts.
A Sorry. 47. Okay.
Q And it's under the heading "Restoration and Optimization," and it would be the last paragraph under that section. Could you read that?
A Sure. "The development of a fishery in the study area under optimized conditions is not recommended as the habitat and fish production would be small and intermittent. Optimization could increase the 1.6 cfs average annual flow by an estimated .3 cfs at the conduit crossing. Further, the estimated 50 percent exceedence flow at the conduit and at the Rush Creek confluence are 0.8 and 0.1 cfs and do not appear significantly increased under optimized conditions. Reference Figure 15 and 16."
Q Thank you. Dr. Sitts, did you find any fin fish in South Parker?

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A No.
Q Mr. Smith, I'll refer you to Exhibit 162, which is a letter to Ed Anton (phonetic), division chief --
MR. FRINK: Excuse me, Mr. Del Piero and Mr. Canaday. I have a question regarding the relevancy of questioning the stream flow recommendations on South Parker Creek. It's my understanding, Mr. Birmingham, that the Department of Water and Power has ceased diversions from South Parker Creek and does not intend to reinstitute them, is that correct, in that that stream is not specified in your water right license?
MR. BIRMINGHAM: That's correct.
MR. FRINK: I think we can skip over this line of questioning regarding minimum stream flow requirements because their diversions have not been under their license in the first place.
MR. CANADAY: The question I was leading to is that there are no vertebrate fin fish and the Fish and Game's recommendation pursuant to 5937 and 5946 refers to non-vertebrate fin fish; is that correct?
MR. GARY SMITH: That is correct.
DR. SITTS: Non-vertebrate fin fish?
Q BY MR. CANADAY: You answered it. You knew what it meant.
A BY MR. GARY SMITH: I think you and I are the only

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ones who know what that means. Non -- there are no fin fish that I'm aware of in South Parker Creek downstream of L.A.'s diversion facility or past diversion facility.
Q Mr. Parmenter, you've been sitting patiently for

several days and to make your trip over here worthwhile, I do have a question for you.
A BY MR. PARMENTER: Mr. Canaday.
Q I'm referring to your testimony, and your testimony relates to what section of the Owens River?
A The Middle Owens, specifically the first 16 miles where I've done my work.
Q And for the record, would you, so that we're clear, where on the Owens River that is?
A The upstream point would be Pleasant Valley Dam extending downstream to Five Bridges Road.
Q And what is the management objective of the department on that section of the stream?
A Self-sustaining populations of wild trout. There are more specific management objectives.
Q And I was interested in some of the electrofishing reporting that you have in your testimony. And your testimony describes brown trout density estimates range from 1.2 to 3.7 adult fish per linear foot of stream?
A That's correct.

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Q Now, those were in the sections that you sampled. Is that -- is that kind of density -- would one expect to find that density in that -- along that whole section of stream?
A In general, yes. The electrofishing has a limitation in that you can only sample water that's, say, less than an inch or two below the top of your chest waders. So in deeper water, you might expect either greater densities or greater sustained crop rates.
Q And the year that this sampling took place?
A It's occurred in '74, '77, '79, '80, and '92.
Q And these -- well, but these kinds of densities were identified --
A The densities reported from the 1992 sampling.
Q Was the -- the average monthly flow in the Owens River during the months that you sampled in 1992 different than the long-term average for October? Do you recall?
A Yes, it was. I don't know what the average for the month of -- actually, the sampling occurred in September. And I'm not sure what the average flows were during September of 1992, but at the time of the sampling, they were about 100 cfs and had been at a low level for some period of time before that. That's

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quite a bit below the long-term average.
Q So the reduction in flow in the Middle Owens River in your opinion is not -- since 1989, has not impacted the fishery significantly?
A It hasn't, by any means, precluded the existence of an outstanding fishery.
MR. CANADAY: Thank you. That's all I have.
There's your trout stream, Mr. Del Piero.
HEARING OFFICER DEL PIERO: Yeah, I know. I've been taking notes. Good.
Redirect. Ms. Cahill, good morning.
MS. CAHILL: Good morning. Good morning to the panel.

REDIRECT EXAMINATION BY MS. CAHILL
Q Dr. Sitts, let me try to ask you a series of straightforward questions to perhaps dispel some of the confusion there's been about the Walker and Parker reports. If you would take either the Walker report or the Parker report and turn to Page 1, please.
A BY DR. SITTS: Okay.
Q And both of those reports, in fact, provide, don't they, that the purpose of the investigation was to provide a plan to restore and optimize environmental conditions of degraded portions of the respective creeks?

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A That's correct.
Q And what did you mean by "restoration"?
A By "restoration," we meant replacing or recreating habitat loss.
Q And what did you mean by "optimization"?
A "Optimization" referred to providing habitat resources in addition to conditions that were restored.
Q And if you would turn, please, to Table 10 in the Walker Creek report.
MR. BIRMINGHAM: Ms. Cahill, do you have a page for that?
MS. CAHILL: Yes, it's Page 61.
DR. SITTS: Okay.
Q BY MS. CAHILL: And what does this table show?
A BY DR. SITTS: This table shows a list of restoration and optimization measures recommended for Walker Creek.
Q And is each measure clearly labeled as to whether it is restoration or optimization?
A Yes.
Q And with regard to those that are listed for restoration, in each case have you indicated the cause of the impact that you are trying to overcome by restoration?
A Yes.

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Q And what are those causes?
A Those causes are identified under the purpose cause at the far right column and in the footnote, it identifies the definition of the letter in the parenthesis. The C stands for effects of the conduit. I stands for irrigation diversion effects. L is for livestock, and R is for road construction. And then N refers to effects of the 1990 channel modifications.
Q And there is no suggestion, is there, that Los Angeles Department of Water and Power is responsible for all of those impacts?
A No.
Q But you would believe that they are responsible for the effects of the conduit?
A That's right.
Q And to the extent that they are the landowner, might they have some responsibility with regard to the livestock grazing?
A They may.
Q And to the extent that they are the owner, might they have some responsibility for the irrigation

diversions?
A Yes.
Q You have indicated that some of these measures you would still recommend and with regard to others, you

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might -- you might wish to see what's happened in the years since you did your study?
A That's correct. That's correct.
Q In your opening statement today one of the measures you mentioned was constructing a bypass channel around the conduit for continuity. Is that something you would still recommend?
A Yes.
Q And you recommended removing fish migration barriers. Is that something you would recommend?
A Yes.
Q And with regard to measures that might involve the recovery of riparian habitat, are those the sorts of measures that you believe there should be on-site assessment before they're implemented?
A Yes.
Q And we've discussed a bit about the possibility of putting the flow of Walker and Parker Creeks into the historic distributary channels. Is there something on Table 10 that indicated a recommendation to do that?
A Yes. The first item on Page 61.
Q Okay. And what were the measures included in that item?
A The restoration/optimization measure column second from the right had increase no distributaries

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downstream.
Q Among others?
A Among others, yes.
Q And that was considered a restoration; was it not?
A That's correct.
Q Thank you. And is there a similar Table 10 in the Parker Creek report?
A Yes, there is.
Q And does it -- does it also list both restoration and optimization measures?
A Yes, it does.
Q And does it attribute the causes of degradation in the same manner that the Walker Creek report did?
A Exactly the same.
Q And there, too, did you make recommendations with regards to rewatering distributary channels?
A Yes.
Q Actually, Dr. Sitts, I believe I may have been misleading my own witness with my questions. Would you turn to Table 10 of the Parker report?
A I am on Page 61 in the Parker Creek report at Table 10, first page.
Q And the recommendation there, increase flow distributaries downstream, what did you mean by that recommendation?

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A The recommendation referred to the utilization of historic channels that had been cut off by construction of the conduit.

Q Thank you. And that's in -- that's the first recommendation. It's one of the ones under restoration measure in sort of the first set of recommendations?
A Yes. It's in the first item on Page 61 under restoration measures. It's, I believe, the third phrase in that list.
Q Thank you. With regard to the fact that the IFIM on the Upper Owens River showed increasing total habitat area at flows up to 200 or 250, Mr. Smith, it's not your recommendation, is it, that flows in the Upper Owens River go up to 250, if that would require water that was needed in the Mono Basin either by Mono tributaries by Mono Lake; is that correct?
A BY MR. GARY SMITH: That's correct.
Q Dr. Sitts, did you also agree that that would be -- would you recommend taking the Upper Owens River to 200 if it would deprive the Mono Basin of water that was needed there?
A BY DR. SITTS: No.
Q But you did find that some incremental flows from the Mono Basin could be beneficial in the Upper Owens River if they were available?

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A That's correct.
Q With regard to the temperature conditions on Hot Creek, Mr. Smith, if temperature conditions on Hot Creek caused a limiting factor in the Owens River below Hot Creek, would that be a limiting factor due to natural causes or an artificially limiting factor?
A BY MR. GARY SMITH: Natural causes.
Q And similarly, with regard to arsenic. If arsenic were a limiting factor in the Upper Owens River below Hot Creek, would that be a naturally occurring limiting factor or an artificially occurring limiting factor?
A It would be an artificially occurring limiting factor.
Q Do you believe that arsenic is a limiting factor below Hot Creek?
A I don't have any evidence available to me that says indeed it is a limiting factor.
Q Mr. Milliron, has the department stocked any fish into the Upper Owens River below the confluence with Hot Creek?
A BY MR. MILLIRON: Catchable trout are stocked commonly throughout the fish angling season at the Benton Crossing Bridge in that area which is just below Hot Creek.
Q And are you aware of any losses of these fish

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since 1989?
A Not in regards to an arsenic or -- problem other than just the loss of fish -- I say this because there was a single incident where fish were lost. There was a fish plant that went sour, if you will, but it was the result of too much ice in the water in the truck coming over and a huge temperature change from the fish that were in the truck to the fish that were planted, so with that caveat, there's no indication that there's ever been a problem with fish planted and crossing, to my knowledge.

Q Is there a resident fish population downstream of the confluence of Hot Creek?
A Yes, there are, both salmonid and non-salmonid.
Q And are you aware of any losses of these fish since 1989?
A No.
Q Do spawning trout from Lake Crowley pass through the entire study reach on the Upper Owens River?
A Yes, they certainly do.
Q Dr. Sitts, with regard to influences that Hot Creek waters might have on Upper Owens, is there a -- is there a measure that you have considered that might reduce the influence of Hot Creek's natural influences on the Upper Owens River?

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A BY DR. SITTS: We have --
Q Actually, let me withdraw that statement, that question, and restate it. Have you given consideration to any measures to reduce the impacts of Hot Creek on the Upper Owens River?
A Yes. We discussed in the report the augmentations, but there are others that we have considered, and -- of more recent time, and it's not in the report. But it is possible to isolate the northern most tributary of Hot Creek from the Upper Owens River for a few miles and have it flow into the river where the southern most confluence comes in without too much trouble. It will allow most of the tributary, the north tributary, to flow and then just before it hits the river a couple of small bypass channels.
Q I think maybe you do want to draw this. This will be DFG 173.
A Okay. DFG 62, Figure 1, is a map of the -- DFG 62, Figure 1, is a map of the Upper Owens River. It includes the Hot Creek tributaries. There are three of them, the northern and southern. The northern one comes in a couple miles -- two miles upstream from Benton Crossing. The southern one may not get into there until a few hundred yards upstream of Benton Crossing, so there's a matter of about two miles

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between the northern most and where the southern most comes in. If there were small connections, links, between the northern and middle channel and the middle and the southern one, this could isolate the warm water from Hot Creek for a distance of two miles, and we would expect that this section would be cooler during the summer.
Q Thank you. Mr. Smith, with regard to the questions that you were asked yesterday about -- by Mr. Birmingham with regard to velocity adjustment factors, I believe that you stated that a -- an acceptable range of velocity adjustment factors, or VAF's, might extend from .1 to 10; is that correct?
A BY MR. GARY SMITH: For the 112 IFG, .1 to 10 is a general rule of them.
Q And with regard to a document that Mr. Birmingham showed you that reflected -- that had an indication that the majority of VAFs ought to occur within a range of approximately .8 to 1.2, what type of analysis would

this range probably apply to?
A I believe that range applies to a free flow regression IFG4.
Q And the Basco (phonetic) Owens River study, was it a one-flow IFG4 or a three-flow regression IFG4?
A It was a three-flow IFG4 -- excuse me, I'm sorry.

I misspoke. It was a one-flow IFG4.
Q So it was the type of IFIM for which the range of .1 to 10 would be appropriate; is that correct?
A Correct.
Q Mr. Milliron, I believe you did want to give a little bit further explanation with regard to the fish kill that Mr. Birmingham first asked you about and then you were asked about again by another party. First of all, was that a kill of trout in Crowley Lake?
A BY MR. MILLIRON: There were very few trout that were killed, at least very few dead trout that we found the following spring in shoreline surveys. It was primarily a Sacramento perch fish kill, and it was a significant Sacramento perch fish kill.
Q Would you first try to describe verbally what it was that happened, and then you can show your slides?
A Okay. In the fall of 1989, the lake was dropping rapidly and there was some concern that fish passage into the Upper Owens River would be impaired because the river was flowing over a delta that had not yet incised into a new channel. And so I went out and stepped in -- rather over this four inches of water that was skimming the surface and through several feet of muck that, upon subsequent visits, I noticed that the muck, this flat delta zone, had incised and that

the muck was then liberated into the lake. It wasn't really -- that was just before ice out that I had observed that or ice up.
The next spring just at ice out, we had a report that there were dead fish around Crowley. We did go out to the shoreline of Crowley and collected some 1300 dead perch, and 50 or so trout in the course of a half a mile or so of shoreline observations. And I was real concerned about the trout population for the upcoming season. As it turned out, the 1990 season was very good, opening weekend catch rates were right up there, and the season itself was also very good, so trout seemed to have been less impacted.
I had gone back, and I requested water quality data from the Department of Water and Power. They do take water quality samples going into and out of Crowley Dam -- going into Crowley from Benton Crossing and coming out of the Dam. There were no indications of elevated levels of chemical constituents such as arsenic or Mercury that were at a level that would indicate that there was a problem for fish toxicity.
Now, what I believe happened was that in the fall, Sacramento perch being a warmer water species, if you will, become less active and they segregate from trout and they go down to the bottom of the reservoir, and

they hang out in the bottom until the thaw in the

spring for the most part, whereas trout are still very active right through October when anglers are out there fishing. During that period of time when the reservoir in the river was being cut or was cutting or sizing the delta area, this sediment was sluicing into, I believe, the bottom layers of Crowley and affecting the perch population there.

That's also where water is withdrawn from the reservoir, so the water quality samples that L.A. was taking would have been from the same area of the lake where perch presumably were impacted and again, the levels of the constituent elements of Crowley through the DWP's analysis indicated that there were not -- there were not significant levels of arsenic, specifically, that would have caused a fish kill.

I believe that the fish died just because of the overall degradation of water quality and probably the development of anoxic or oxygen-depleting conditions.

Q And that would be due to the sediment?

A Due to the liberation of massive quantities of delta sediments.

Q If the lake -- once the lake was down, would this situation continue if the lake held at a stable level?

A This would be better illustrated at this point,

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then, if I might show a few slides.

MR. BIRMINGHAM: Mr. Del Piero, am I to understand that we are to be provided copies of the slides?

MS. CAHILL: You will be. I, unfortunately, don't have them today, but you will be provided as soon as we can get them made.

HEARING OFFICER DEL PIERO: So will we?

MS. CAHILL: Yes, of course. That goes without saying.

Let's name this slide DFG 173, since we didn't use that number after all.

MR. MILLIRON: This is --

MR. HERRERA: Excuse me. Before you get started. Your time is just about up. You have less than a minute.

MS. CAHILL: Mr. Del Piero, I would petition for no more than ten additional minutes.

HEARING OFFICER DEL PIERO: Granted. Go ahead.

MR. MILLIRON: This is a photograph, an aerial photograph of the Upper Owens River as it enters the Crowley Lake/Owens River arm and just to show, in this area here, that the delta that exists there, and I'm not going to get into a delta -- deltaic process conversation, but this graph does show --

Q BY MS. CAHILL: This will be DFG 174.

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A BY MR. MILLIRON: This is the water storage in Crowley Lake from the years 1979 to 1988, the ten-year period prior to the fish kill event, and the top of the orange indicates the maximum amount of water stored in any given month during that ten-year period. The bottom of the orange, the top of the green, indicates the minimum amount of water storage, and then there's a mean storage line. So you have the mean as well as the of range of water storage in Crowley.

Q This would be DFG 175.
A And during the water year 1989-90, there was quite a divergence from the -- that amount of water stored in Crowley that the previous ten years had, and it started in July where the reservoir dropped rapidly, not only did it drop rapidly, but it also dropped well below the previous ten years and that, then, therefore, exposed ten years' worth of accumulated organic debris and so forth, or muck is an okay term, believe it or not.
Q This would be DFG 176.
A And this just shows the period when there was the rapid drawdown. You see a level roughly greater than 15 feet of reservoir drawdown during that period of time. And so that's the -- the series of events that resulted in the fish kill. A rapid drawdown cutting through sediments that hadn't been exposed in years, so

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I believe that had the reservoir had been drawn down over the course of a longer period of time, the sediments could have been better assimilated by the system and would have moved through the system and not resulted in such a -- an overall degradation of water quality that did result in a fish kill.
Q So in other words, Mr. Milliron, do you believe that it was the fluctuation in water surface elevation more than the absolute storage amount that was the critical factor?
A The fluctuation as well as the exposure to lake bottom sediments that had accumulated over a long period of time, in this case ten years, that then were subject to being cut through. Now that they've been liberated, the reservoir could probably drop down maybe even at that rate. I don't know. I'm not suggesting this, certainly, but it would be less susceptible to this kind of event.
Q And since 1989, there's not been a reoccurrence?
A That's correct.
Q To your knowledge, does DWP have a Crowley operations plan?
A Not to my knowledge insofar as Mr. Hassencamp (phonetic) testified in his direct that they want -- that they considered Crowley recreation in the

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management and the storage of Crowley, but I have -- when crossed, he was not able to provide what kind of information has been used in order to incorporate -- or that has been incorporated into that management plan. So I -- I would say in terms of recreational fisheries, that they do not.
Q Would you recommend that the department be consulted if such a plan were to be drawn up?
A Yes.
Q Mr. Smith, let's make it absolutely clear what kind of fish we have on South Parker Creek. We do not have vertebrate fish on South Parker Creek below the conduit; is that correct?
A BY MR. GARY SMITH: Not that I'm aware of.
Q But we do have invertebrates which are defined as fish in the Fish and Game Code; is that correct?
A They can occur there, yes.

Q I think I have just one last set of questions and I'll make sure my clients don't have any others. Actually, I'm lying to you.

Mr. Milliron, to go back to the subject of Hot Creek and various geothermal influences on the Upper Owens River, are you familiar with any projects that might reduce the amount of warm water that would flow into the Upper Owens River?

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A BY MR. MILLIRON: Yes. One of my other functions with the department is I act as the department's representative on the Long Valley Hydrologic Advisory Committee and, as such, I have been dealing with geothermal development and the impact on aquatic resources for about six years. Since the construction and -- since the production of geothermal fluid or energy and the construction of the NP-2 and PLES-1 projects at Casa Diablo, there has been a decrease in the amount of geothermal fluid which reaches Mammoth Creek, either directly by springs that were geothermal springs that used to exist prior to the construction and operation of these geothermal power plants, or because of pressure changes within the system.

The long and the short of it is that geothermal power production has resulted in an impact to surface geothermal springs. They have -- some of them have decreased in the amount of output of geothermal fluid. Some of them have just simply dried up. So in Mammoth Creek, there certainly has been a decline in the amount of geothermal fluid which is the source of arsenic, Mercury, and other chemicals that may be of concern here into that system.

And Mammoth Creek is the major tributary to Hot Creek and events to Owens River.

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Q Thank you. Dr. Sitts, one last question. I didn't entirely hear your discussion with Mr. Herrera about the flows at which the IFIM was conducted. I would like to refer you to Page 100 of the Upper Owens report and the last paragraph on that page. And it indicates that there was a high flow of approximately 210 cfs below East Portal.

Is it your recommendation -- is it your recollection that there were some high flows as high as 210 cfs below East Portal during your study period?

A BY DR. SITTS: Yes, I'd confirm that.

Q Oh, and one last question. There was some discussion about screening irrigation diversions. Has the department, over the past years, made some attempt to handle some of the problems caused by irrigation in the Upper Owens River?

A BY MR. MILLIRON: On the Crowley tributaries in general, there has been an attempt by Phil Pister over 40 years ago, and myself, when we both came to the area, the obvious problem of fish entrainment and the diversions to -- that are off of the Crowley tributaries is real obvious, and we both addressed the issue. There has been an attempt. And to date, nothing has been done.

MS. CAHILL: Thank you.

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HEARING OFFICER DEL PIERO: Thank you very much.
Mr. Birmingham?
MR. BIRMINGHAM: Mr. Del Piero, we've been at this for an hour and a half. Can we just take a short recess?
HEARING OFFICER DEL PIERO: Sure. You want to take a recess? I was going break at ten. Ladies and Gentlemen, we'll take a ten-minute break.
(Whereupon a short recess was taken.)
HEARING OFFICER DEL PIERO: Ladies and Gentlemen, this hearing will again come to order. Prior to me beginning, Mr. Birmingham, let me point out for those of you that are doing what I've been doing for the last half hour, sitting on my hands, a phone call is being made as we speak to certain individuals to make sure the heat comes back on in here, and I was assured by the Chairman of the Board and also the Executive Director on a conference call that I just had that they would take care of the problem. It was not difficult to motivate them because I told them if they didn't turn the heat on, I was going to leave, and they were going to have to come down and hold the hearing. It's the first time I ever heard both of them say, "Yes, Sir," at the same time. We'll see what we can do.
(Laughter.)

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HEARING OFFICER DEL PIERO: Mr. Birmingham, please proceed.
MR. BIRMINGHAM: Thank you.
RECROSS EXAMINATION BY MR. BIRMINGHAM
Q Good morning, Gentlemen. Dr. Sitts, I'd like to begin with you for a moment, if I can, and ask you some questions about Table 10 in the Walker and Parker Creek reports. First, let's talk about Table 10 in the Department of Fish and Game Exhibit 56, which is Walker Creek stream evaluation report 92-1, and I believe you testified that Table 10 begins on Page 61; is that correct?
A BY DR. SITTS: Yes.
Q Now, the right hand column of Table 10, which extends for a number of pages, lists the purpose or the cause of a particular restoration or optimization measure; is that correct?
A Yes.
Q Now, the causes are listed at the bottom of each page of Table 10, and it -- with an asterisk that states, "Provides for restoration of effects of the conduit C, irrigation diversions I, livestock grazing or trampling L, road construction R, or 1990 channel modifications M;" is that correct?
A Yes, it is.

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Q So from that do we take that it if there is a C associated with a particular restoration or optimization measure listed on Table 10, then that C indicates that the restoration or optimization measure is intended to ameliorate the effects of the conduit?
A The C relates to just restoration, and in the case of C, it would be in relation to the conduit.

Q Well, let's go through Table 10, if we can. Is it correct that only two of the restoration measures that are listed in Table 10 are intended to deal with the effects of the conduit?
A It appears so, yes. I find two only Cs after the word "restoration."
Q Now, there are a number of restoration measures that are intended to deal with the effects of irrigation diversions; is that correct?
A Yes, that's right.
Q Now, isn't it correct that prior to the Department of Water and Power diversions from Walker Creek, irrigation was a -- irrigation water was diverted from that stream for irrigation of Cane Ranch?
A From the aerial photographs that I've seen, yes, that there was irrigation going on well before the conduit.
Q And isn't it correct that there was livestock

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grazing that was going on along Walker Creek well before the Department of Water and Power began its diversions from Walker Creek?
A That's what I had surmised.
Q Now, there are a number of restoration measures that are associated with restoring the effects of the 1990 channel modification; is that correct?
A Yes.
Q So as I understand it, these restoration measures with the letter M after them in Table 10 are restoration measures that are designed to restore conditions that were damaged as a result of restoration activities that were conducted in 1990?
A Yes.
Q Now, let's talk for a moment, if we can, about Table 10 in Department of Fish and Game Exhibit 58, which is the Parker Creek stream evaluation report.
A Okay.
Q That also begins at Page 61; is that correct?
A That's right.
Q Now, if we look through Table 10 in the Parker Creek report, it's correct, in it, that only one of the restoration measures identified is intended to correct the effects of DWP's conduit?
A There is only one location at the conduit

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diversion facility, and there are a couple of things that are recommended there, but that's the only item which we associate with C.
Q And there are a number of items that are intended to correct the effects or restore the effects resulting from irrigation diversions; is that correct?
A Yes.
Q Now, with respect to Parker Creek, isn't it right that prior to the Department of Water and Power's diversions from the stream, irrigation water was being diverted for irrigation of lands along Parker Creek?
A It appears so.
Q And with respect to livestock grazing, is it correct that before the Department of Water and Power began its diversions, livestock grazing had an impact

on Parker Creek?
A It appears so.
Q And again, with respect to Parker Creek, there are a number of restoration measures that have the letter M after them; is that correct?
A That's right.
Q And those restoration measures are intended to correct the effects of the restoration work that was done in 1990?
A Yes, that's right.

Q Now, in terms of specific recommendations, let's look at Page 3 of Exhibit 56, the Walker Creek report. Page 3 of the Walker Creek report states, and I'm looking at the second full paragraph on Page 3, last sentence. It states, "Thus implementation of the restoration plant provided here depends in part on the extent of natural recovery over time." Is that correct?
A Yes.
Q Now, Mr. Roos-Collins asked you some questions yesterday about your proposed restoration measures, and you said that you would still recommend implementing those proposed restoration measures to the extent that degraded conditions still exist in Walker and Parker Creek. Is that correct?
A Yes.
Q But as you sit here today, you do not know to what extent the degraded conditions still exist in those streams; is that correct?
A That's partially correct. In regard to riparian conditions, those are much more dynamic. In regard to a number of other conditions, I'm pretty sure that they're exactly as they were.
Q But as you sit here today, you can't tell us which of those conditions are still exactly as they were and

which of those have changed?
A I can do that for some.
Q Now, with respect to the Upper Owens River study, Mr. Dodge asked you some questions that were followed up about proposed aquatic habitat development management plans for the Upper Owens River, and you were -- you, based upon a review of 218, Page 218 of Department of Fish and Game Exhibit 62, describe some of the measures that might be implemented under an aquatic habitat development management plan, and on Page 218, the last measure is using a low-level intake to Mono Craters Tunnel to keep the Upper Owens River cool. Do you see that listed on Page 218?
A Yes.
Q Now, as I recall, you said that after this had been drafted, you discovered that that problem had been fixed. Is that right?
A I discovered that the indications were that the intake was already low and fixed in the reservoir, and there wasn't much you could do about it.
Q Now, let's talk for a moment about temperature problems at Hot Creek, below the confluence of Hot Creek. In response to questions by Ms. Cahill, you

referred to Department of Fish and Game Exhibit 52 -- I'm sorry, Figure 1. 62. Thank you, Mr. Milliron.

Department of Fish and Game Exhibit 62, Figure 1. And you testified that one of the ways of ameliorating the temperature problem during certain months below the confluence of Hot Creek with the Upper Owens River would be to divert water out of two channels into a third channel; is that correct?
A That's close.
Q What was it that you said? I want to make sure that we have it correct.
A I indicated that a way to ameliorate these effects would be to consider putting the lower portion -- diverting the water in the lower portions the lower and middle trenches of Hot Creek into each other and then finally into the lower branch and then into the Owens River.
Q I'd like to show you a quadrangle map that has been marked and introduced into evidence as L.A. DWP Exhibit 79, and it is a -- a map that was prepared in 1914, and I'd ask you to take a moment and review it. Particularly that portion of the quadrangle that depicts the area of Hot Creek.
Have you had an opportunity to review L.A. DWP Exhibit 79, Dr. Sitts?
A Yes.
Q Now, when you look at L.A. DWP Exhibit 79 and

compare it with Figure 1 from Department of Fish and Game Exhibit 62, it's correct, isn't it, that the three channels of Hot Creek depicted on Figure 1 are apparent on L.A. DWP Exhibit 79?
A Yes. This Exhibit 79 which was reprinted in 1950 shows three branches of Hot Creek.
Q And it's a map that's based upon 1914 data; is that correct?
A It says, "Edition of 1914."
Q Thank you.
So the temperature problem that is associated with the portion of the Upper Owens River between the north branch of Hot Creek and the lower most branch of Hot Creek would have existed in a state of nature; is that correct? Maybe my question isn't clear. I'm restricting my question to the portion of the Upper Owens River between the northern channel of Hot Creek and the southern most channel of Hot Creek.
A They would have existed in 1914. Whether they were nature or not, I don't know. And that shows obviously three, and they are similar in shape.
Q Thank you, Dr. Sitts.
Talking some more about the Upper Owens River, there were questions concerning this proposed limit of 200 cfs in the upper portion of the Owens River. Now,

I want to make sure I understand the position of this panel with respect to this issue. As I understand your statements in Department of Fish and Game Exhibit 62, the channel of the Upper Owens River had adjusted to the higher flows that existed in that portion of the

river because of diversions out of the Mono Basin by the Department of Water and Power; is that correct?
A BY MR. WOLFF: We state in there that there has been some adjustment, I don't know that there has been total adjustment; that is, the river's in equilibrium.
Q Well, I asked you this question yesterday, Mr. Wolff, and just so that the record is clear, Page 53 of the Department of Fish and Game Exhibit 62 states, doesn't it, that the present channel appears to have adjusted to the larger flows?
A That statement is in the report.
Q I'd like to -- this is the follow-up on a question that was asked of Mr. Dodge -- or by Mr. Dodge. Page 211 of Department of Fish and Game Exhibit 62, it states that -- talks about the maximum THAs, and then it states, "It is recognized that given the present channel, widened by high augmented flows, a future lower-flow regime may lead to a narrower channel and a smaller optimal instream flow." Is that stated on Page 211?

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A Are you directing this to me?
Q To anybody on the panel. You can answer it if you can, Mr. Wolff.
A Let me find the location. I didn't write this section.
A BY DR. SITTS: That's toward the end of the second paragraph?
Q In fact, it's the last sentence; is that correct?
A BY MR. WOLFF: That's correct.
Q Now, in other words, the present channel has adjusted to the higher flows, but if we put flows limited to 200 cfs in that portion of the river, it will narrow and ultimately there will be smaller optimal instream flows; is that right?
A I don't think that's quite right. You just said limit the flow to 200? I think there's an issue of duration here. If you ran the flow at 200 cfs continuously, the channel would probably continue to enlarge. So your question can't be quite answered just by the limitations.
Q But the first part of my question is that the channel has adjusted to the larger flows. You've said that that's stated in the report.
A Yeah. And I state -- my opinion is that some adjustment has occurred. I don't think that it can be

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clear that an equilibrium channel exists. In other words, if L.A. continued to operate the way they have, some additional adjustments could occur in the future.
Q Let me ask you an interesting question. That is a theoretical question because it relates to Rush Creek, and I know you haven't studied Rush Creek. You haven't studied Rush Creek, have you, Mr. Wolff?
A That's correct.
Q Dr. Sitts, I'm going ask you the same question. Have you studied Rush Creek?
A BY DR. SITTS: No.
Q I'm going to ask you to assume that the bottom lands of Rush Creek and the bottom lands we've referred

to as that portion below The Narrows, I'm going ask you to assume that as a result of flows in the stream, Rush Creek has widened and Rush Creek has straightened. Do you understand the assumptions that I'm asking you to make?
A BY MR. WOLFF: Repeat them. I didn't get the first part of them.
Q As a result of the flow pattern in the last 30 years in Rush Creek, the stream has widened, and it's lost some of its sinuosity. In other words, the channel has straightened somewhat. That's what's happened on the Upper Owens River; is that correct?

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The higher flows have caused the stream to widen and to straighten.
A BY DR. SITTS: There's too much going on in Rush Creek to make a reasonable association with the Upper Owens.
Q Is your response the same, Mr. Wolff? There's too much going on in Rush Creek for you to be able to answer it intelligently? Any question I would be able to ask you about Rush Creek?
A BY MR. WOLFF: Probably so. The two channels are very different dynamically. So I think any kind of comparison could get into big trouble. So I wouldn't be comfortable doing that.
Q What is the basis of the opinion that's expressed here on Page 211 that it is recognized that given the present channel widened by high augmented flows a future flow regime may lead to a narrower channel and a small optimal instream flow.
A Well, can I answer the first part of that. I can't answer anything about the optimal instream flow, but the basis of the channel narrowing would be an adjustment of the channel to the lower flows through deposition and growth.
Q Is that a general hydrologic principle?
A Well, it's the way an alluvial channel where the

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sediments are available and are mobilized, that's the way an alluvial channel will adjust. That doesn't mean all channels will do that, but in the case of the Upper Owens River, I believe that would happen.
Q Mr. Milliron. You talked about some proposed management for Crowley Lake, some proposed measures for the management of Crowley Lake; is that right?
A BY MR. MILLIRON: Yes.
Q Those proposed measurements, management measurements, aren't intended to -- let me back up for a minute. It was your testimony, wasn't it, that the -- the fishery in Crowley Lake has not suffered because of lower flows into the lake?
A I don't know if I said that specifically, but I think the fishery in Crowley Lake is in good shape.
Q Fishery in Crowley Lake is in good shape.
A Overall.
Q So the proposed management schemes that you outlined in response to questions by Mr. Roos-Collins last night are not intended to protect the fishery in Crowley Lake; is that right?

A I'm sorry, Mr. Birmingham, would you repeat that question?
Q Well, let me state it differently. The proposed management measures you discussed in response to

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questions by Mr. Roos-Collins are intended to make the big trophy trout more accessible to fishermen. Is that right?
A That's correct. These are department management recommendations which I believe should be incorporated in the overall management scheme that DWP employs.
Q And the purpose of it is to make the fishery more accessible to fisher people?
A Well, more specifically, to make the fish more accessible to fisher people.
Q And the management proposals that you've outlined, they're not required to keep fish in -- let's use the term "good condition" biologically?
A The fish themselves are -- appear to be present in the system, and so this is -- these recommendations are based on the fishery, not so much the biological needs of the fish. However -- however, the Sacramento perch in the production, in the latoral zone of Crowley would benefit biologically by having the -- that nursery habitat maintained in appropriate condition, and the large trout which forage on the Sacramento perch would also benefit by having those available. And then, of course, the final link there is that the fishery benefits because the anglers now have their accessibility to the large fish. So it's not that

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there's not a biological benefit to proper management because clearly there is.
MR. HERRERA: Excuse me, Mr. Birmingham, your time has elapsed.
MR. BIRMINGHAM: I make an application for an additional 30 minutes.
MR. DODGE: I would --
HEARING OFFICER DEL PIERO: Mr. Dodge, let me preface your comment by indicating we're not going to break for lunch today because of the limitation in terms of the availability of your witness. I have no difficulty with people eating in this hearing room. We broke early. I anticipate taking about a ten-minute break, but the balance of that time is going to be spent in terms of direct testimony. Now --
MR. DODGE: I really think in fairness we have a certain number of days left to complete the direct and, you know, a lot of those days are my case. And I think these questions are, with all due respect, are at the cutting edge of irrelevance to this proceeding. And so normally, I would object to further questioning, but in light of --
HEARING OFFICER DEL PIERO: And we won't take a dinner break either if this continues because --
MR. DODGE: -- my respect to Mr. Birmingham, I

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would be happy to concede him 10 of my 20 minutes.
HEARING OFFICER DEL PIERO: It is now quarter after ten. I had hoped to get done with this panel

last night. We didn't. We got here early this morning in order to get this matter resolved. It's two and a quarter hours into the day and so however long this takes, Mr. Birmingham, if you want 20 minutes, you're granted your 20 minutes, but not -- during the course of this entire process, I've not told anybody they can't have the additional time. I just want everybody else to know, we're going to get this and the next panel done today. We won't -- and if it necessitates us not taking any breaks except for the Court Reporter, I will do that, also. And if attorneys are upset about the fact they have to get up during the course of testimony to leave to take care of whatever they have to take care of outside, that's just the way it's going to be because we need to get moving.

Now, proceed, Mr. Birmingham.

MR. BIRMINGHAM: Thank you very much.

Q BY MR. BIRMINGHAM: Mr. Milliron, you mentioned in response to my question a minute ago Sacramento perch. Sacramento perch is a species of fish that was introduced into Crowley Lake illegally; in that right?

A BY MR. MILLIRON: Yes.

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Q And the Sacramento perch are a species which feed on juvenile trout; is that correct?

A I don't have evidence to support that, and I would suspect that that's not a significant issue in Crowley insofar as juvenile trout -- trout appear mostly in the streams. I think the biggest predation on juvenile trout occurs mostly in the irrigation canals.

Irregardless of how Sacramento perch got into Crowley, they have established themselves as a desirable sport fishery and they do benefit trout at least in terms of large trout forage.

Q Do Sacramento trout -- I'm sorry, do Sacramento perch -- Sacramento trout certainly don't, but let's talk about the species Sacramento perch. Sacramento perch compete with young trout for available planktonic foods. Is that correct?

A I have no indication that that is, in fact, occurring in Crowley. I think that Crowley's very rich, and we put a tremendous number of fish into Crowley and growth rates are very good, so there's no indication that there's any growth-limiting problems in Crowley.

Q Now, last night you told me that you had reviewed the March 1989 fish management plan for Mammoth Lakes basin and certain adjacent waters, Mono, Madera, and

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Fresno Counties, California, prepared by the Department of Fish and Game. You have reviewed this document, haven't you?

A In 1989.

Q I'm going to show you this document, and I'm going to mark a portion of it.

MS. CAHILL: Mr. Del Piero, I object. Mr. Birmingham did this all day yesterday where he has one copy and he wanders over and hovers over the witness. Last night there were no copy facilities available, but between then and now he should have been

able to copy the pages to refer to.
HEARING OFFICER DEL PIERO: Do you have copies, Mr. Birmingham?
MR. BIRMINGHAM: I don't, Mr. Del Piero, but again, I'm not introducing this as an exhibit, I'm simply using this document as a means to cross-examine this witness on his opinions which I am entitled to do. This is a Department of Fish and Game publication. Ms. Cahill represents the Department of Fish and Game. Their office is across the street. They may have copies of it available. Ms. Cahill is correct. I should show her the passage I'm going to ask the witness to read before I show it to the witness, and for that I apologize. But --

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HEARING OFFICER DEL PIERO: You should, Mr. Birmingham.
Q BY MR. BIRMINGHAM: Okay. Mr. Milliron, I'm handing you the 1989 Department of Fish and Game fisheries management plan, and I have marked a paragraph which appears on Page 20. And I would ask you to read the paragraph that I've marked from Page 20 into the record.
A BY MR. MILLIRON: Starting with "it has been suggested"?
Q Yes.
A "It has been suggested that Crowley Lake no longer supports the number of large trout that it has in the past. It seems most likely that the illegally introduced Sacramento perch now compete directly with the young trout for available planktonic food with subsequent adverse impacts on trout survival. While perch do provide forage for large trout, the food chain has been lengthened and an overall decline in the production of top line predatory fish may have occurred. Despite possible declines in the abundance of large trout, Crowley Lake remains a fishery of national importance."
Q Thank you.
Now, having reviewed, re-reviewed this portion of

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the 1989, March 1989, Department of Fish and Game fisheries management plan, does that change your opinion concerning the competition between young trout and Sacramento perch for available planktonic food at Crowley?
A Not in the least, especially given that we've had a trout -- excuse me, a perch die off of major magnitude since that document was written and that perch have only recently reestablished themselves in their former fishery position, if you will. And I have no indication that there's been an impact either positive or negative to trout growth during that period of time.
Additionally, trout are spatially segregated. Young trout, juvenile trout are reared in tributary streams to Crowley. Sacramento perch are not in tributary streams to Crowley.
I think a bigger issue in this regard would be simply that providing habitat for young perch is

appropriate.
Q Mr. Milliron?
A Yes, Sir.
Q Again, my time is very limited, and I don't want to cut you off, but a large portion of the answer that you just gave was not responsive to my question. My

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question was limited to whether or not review of that portion of the 1989 report changed your opinion. That is a question that can be answered yes or no, and in light of the very limited time that we have and a unavailability of witnesses, I'm going to ask that you just respond to my questions, if you will. All right?
A Yes.
Q Thank you.
Now, let's talk for a moment about that -- the fish kill that we've had discussed here. That was a fish kill that occurred in 1989?
A 19 -- during the period 1989, early 1990. 1990 is when we noticed it, when the ice -- when Crowley became ice free, and we found many dead Sacramento perch along the shoreline in a decomposed state.
Q Now, you say "many." You found 25; is that right?
A No. I testified earlier, I believe, that I found 13 -- roughly, 1300 dead Sacramento perch and about 50 dead trout in about a half a mile of examined shoreline.
Q You had some slides that you used in explaining what you thought was the cause of this fish kill in 1989, 1990. Could we put those slides back up, please?
A In a moment. Is there any particular slide you want me to show?

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Q Yes. You had a slide, Mr. Milliron, that showed the levels of Crowley Lake between 1979 and 1988, and I believe that that was marked as Department of Fish and Game Exhibit 174.
A More specifically, it's not the levels but the amount that's total storage in acre-feet.
Q Okay. Now, that represents the period, again, 1979 to 1988; is that correct?
A That's correct.
Q And generally during that period, the level of storage is constant? Somewhat constant? How would you characterize it?
A Well, what I have here is all the data represented within that orange range. It's the range of storage, so you have the maximum line for any given month during that ten-year period at the top of the orange. The minimum amount of storage at any given month at the bottom of the orange, and then the mean of all ten years is represented by the white line.
Q Now, you had a slide, I believe it was Department of Fish and Game Exhibit No. 175.
A This one?
Q And that shows 1979 to '88 storage; is that correct?
A Well, the only difference between this slide and

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the previous one is that the red line superimposed

represents the water year 1989-90 from April through May represented by the red line.
Q Did you have an additional slide, Mr. Milliron, which showed a drop in Crowley Lake storage in July? Which exhibit is this?
A It would be the next one after the last one.
Q So this would be Exhibit 176; is that correct?
A I did not keep track of the numbers.
HEARING OFFICER DEL PIERO: I believe that's correct, Mr. Birmingham.
Q BY MR. BIRMINGHAM: Now, let me ask you some questions about this. This is storage for the calendar year 1989; is that correct?
A Well, I believe -- yes. Yeah. That's what it is. January, February, through December.
Q Now, as we go through January through June, there is a -- there is a decline, then an increase, and then a sharper increase in storage in Crowley Lake; is that correct? January through June?
A Yes.
Q Now, in -- starting in June, there is a substantial decline in the storage in Crowley Lake; in that right?
A Yes.

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Q Now, Crowley Lake is -- we established yesterday is a storage facility that was built in -- well, I'm sorry. You were not aware of why it was built. So let me state the question differently.
It's correct, isn't it, Mr. Milliron, that it was in June of 1989 that the El Dorado County Superior Court entered a temporary restraining order that prohibited the Department of Water and Power from exporting water out of the Mono Basin?
MR. VALENTINE: Objection, your Honor. These questions have been asked and answered. This may have been kind of a belated objection because I could've been making it in the last ten minutes, he's been through this with Mr. Birmingham and others yesterday.
HEARING OFFICER DEL PIERO: The answer is he didn't know -- he's already indicated a couple of times, Mr. Birmingham, he doesn't have direct knowledge of the case in 1989, so why don't you proceed.
MR. BIRMINGHAM: Thank you. What I'd like to do, if I may, is I'd like to try and refresh the witness' recollection.
HEARING OFFICER DEL PIERO: Mr. Milliron? Were you a participant in that litigation?
MR. MILLIRON: No, I was not.
HEARING OFFICER DEL PIERO: It's difficult for him

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to have his recollection refreshed, Mr. Birmingham, if he wasn't a participant.
MR. BIRMINGHAM: I think I can establish a foundation at least to be able to try and refresh his recollection.
I'm done with this slide, Mr. Milliron, so you can turn that off and resume your seat, if you will.
Q BY MR. BIRMINGHAM: Now, Mr. Milliron, you were responsible for the management of Crowley Lake for the

Department of Fish and Game in 1989; is that correct?
A Yes.
Q And as part of your responsibilities, you would have followed the Department of Water and Power's operation of Crowley Lake; is that right?
A Not necessarily. There's -- this data was acquired after we saw effects. I have a lot of other responsibilities besides the management of Crowley Lake and the department has never been afforded the opportunity to have much of an impact at all on management and storage in Crowley Lake.
Q After the fish kill that we've been talking about, did you -- were you interviewed by a reporter for the Los Angeles Times by the name of Richard Roberts?
A In all likelihood. I've been interviewed often by reporters including Richard Roberts (phonetic) who

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don't always report exactly what I say.
Q I have that same problem.
HEARING OFFICER DEL PIERO: I won't say anything.
Q BY MR. BIRMINGHAM: I'm showing you an article from the Los Angeles times, and I'll ask you if it refreshes your recollection as to whether or not you were interviewed by this reporter named Rich Roberts for the Los Angeles Times regarding the fish kill that we've been talking about?
A Now, do you want me to specifically read any part of this?
Q Just take a look at it and see if it refreshes your recollection as to whether or not you were interviewed?
A Well, I certainly remember the photograph on the front here and that's Owens Weir and I remember talking to -- not Mr. Rich Roberts so much as an agent of his in regards to the weir. How much of -- I don't specifically remember what conversation we had on the Upper Owens -- excuse me, the Crowley fish kill. If I were to study the article it might help.
Q Why don't we take a minute and I'll just ask you to study -- to study the article. There are a number of paragraphs which are circled in green ink, and I'd ask you just to take a moment, look at them, and see if

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it refreshes your recollection about about this fish kill.
A You have a lot of paragraphs here. I'm not a speed reader.
Q While you're doing that, I'll see if there are some other questions I can ask of another witness so we don't waste a lot of time.
HEARING OFFICER DEL PIERO: Mr. Dodge?
MR. DODGE: I object to this line of questioning. It's repetitive and only marginally relevant. We're just wasting our time here.
HEARING OFFICER DEL PIERO: Mr. Dodge, there's 11 minutes left on Mr. Birmingham's time.
MR. DODGE: Thank you.
Q BY MR. BIRMINGHAM: I'll just ask you one question about that article, Mr. Milliron. On the extreme left hand column -- Mr. Milliron?

A BY MR. MILLIRON: Yes.
Q On the extreme left-hand column, there's a paragraph there, it's the last paragraph I pointed out to you.
A This one?
Q Actually, I'm sorry. It's this paragraph right here. Would you read the last paragraph that I'm marking? Just read it into the record.

0088

A Out of context? He said -- I don't know who "he" refers to. I guess me?
Q Well, if you look at the preceding paragraph it quotes you.
A How far back do you want me to go?
Q Just the preceding paragraph.
A "Milliron said, I don't think we can really say we've hurt the Crowley fishery. He said the DWP has been concerned and cooperative in preserving fisheries all along the eastern Sierras."
Q Did you tell the reporter or the agent for the reporter that DWP has been cooperative in preserving the fishery all along the eastern Sierras?
A I have no idea if I said that or not. I generally try to give as much credit to the Department of Water and Power as they -- yes. I may -- I'm generally quite flattering. Let me make a note, if I might, I think it's needed for clarification that that -- what was the date of that article?
Q April 25, 1990.
A Yeah. There wasn't -- that was actually before the angling season started, and so we really didn't have a good indication as to the magnitude or the impact of any fish kill. We certainly know a lot about it now. Sacramento perch were impacted. The fishery

0089

for Sacramento perch essentially was gone for all of two years and didn't return until the third year. The last year was its first real come back or this season, and the trout fishery was very good that season so --
Q Let me just ask you, in your experience, has the Department of Water and Power been concerned and cooperative in preserving fisheries all along the eastern Sierra?
A I've had some good times and I've had some not so good times with the Department of Water and Power personnel, and I think that I would like to end it on a more positive note than a less positive note in the spirit of future cooperation which I hope will occur, and I will say that water and power is interested in fisheries in the eastern Sierras, and I think there's lots of room to do good things.
MR. BIRMINGHAM: I don't have any further questions
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Birmingham.
Mr. Dodge. Excuse me, Mr. Dodge, one question.
Mr. Milliron?
MR. MILLIRON: Yes.
HEARING OFFICER DEL PIERO: Does your information upon which you do your analysis for the Department of

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Fish and Game come from the Los Angeles Department of Water and Power?
MR. MILLIRON: Some of it does.
HEARING OFFICER DEL PIERO: How much of it?
MR. MILLIRON: Well, all the water storage information that you have here is water and power data.
HEARING OFFICER DEL PIERO: Other than the biological information, how much of the information do you rely on that comes from the L.A. Department of Water and Power?
MR. MILLIRON: Most.
HEARING OFFICER DEL PIERO: Is that --
MR. MILLIRON: I'm trying to separate out what you're really asking me. I have a large amount of information that I've requested and received from the Department of Water and Power.
HEARING OFFICER DEL PIERO: Can you do your job without it?
MR. MILLIRON: I certainly couldn't make the graphs that you saw today without that information.
HEARING OFFICER DEL PIERO: Thank you.
Mr. Dodge?
RECROSS EXAMINATION BY MR. DODGE
Q BY MR. DODGE: Dr. Sitts, let me return to one of my few areas of interest with this panel, the
0091
distributaries on Parker and Walker Creek, now dry. They held water until 1940, correct?
A BY DR. SITTS: I would assume that there was some water in those before 1940.
Q Okay. And, in fact, until 1940, they held water and irrigation water came out of them; isn't that right?
A I don't have firm data on that, but they seem to distribute water to the pasture land.
Q Assuming they held water until 1940, and looking at Table 10 on the Parker Creek study.
A Okay.
Q Now, as I understand it, the rewatering of the distributaries on Parker Creek is not listed on Table 10, at least I couldn't find it, but my question to you is hypothetically, assuming that the distributaries held water until 1940 and then they were thereafter dried up by the diversions, would you agree that if rewatering the distributaries were listed on Table 10, that it would have a capital C after it?
A Yes.
Q It would be conduit influence?
A Yes. And perhaps we can talk about that first item in Table 10.
Q All right.
0092
A Under the column called "Restoration Measures," second from the right?
Q I see that.
A We go down to the third line, right under that heading, and we see the increased flow distributaries downstream.
Q It's already in the table, then.

A It is.
Q I misread that, Sir.
A And restore and CR in the next column on purpose.
Q Thank you.
Mr. Smith?
A BY MR. GARY SMITH: Mr. Dodge.
Q I had a question about the three Hot Creek channels. Can you tell us whether or not, in fact, the two northern most Hot Creek channels as they enter the Owens River are, in fact, artificial?
A I have heard discussions on both sides of that issue. I do not have the knowledge to answer that question other than to say that others have informed me that those are man-made channels. On the other hand, others have informed me that they're naturally formed channels. The issue has some controversy associated with it.
Q Thank you.

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Does anyone else on this panel know the answer to my question?
A BY MR. MILLIRON: What was the question, please?
Q Whether of the three Hot Creek channels now entering the Owens River, the two northern most are, in fact, artificial channels?
A I'll only relate the following comment that I heard from Mr. Gary Giacomini (phonetic) who is associated with the family who has run that operation for many years, in a public meeting, and I don't even recall which one it was, but he clearly stated that their family, the one he married into, is responsible for the diversion of Hot Creek into several distributary channels which, in his context, a reason to bring that up, benefited the Upper Owens River because it helped facilitate additional cooling. And he was therefore referring to that beneficial effect that they have had by, in fact, diverting from one to three channels. So that was a comment that they made.
I would also add that grazing in Long Valley has gone on for be a exceptionally long time, well before the 1911, I believe, map that was handed to Dr. Sitts.
Q Did Mr. Giacomini (phonetic), if that's the right pronunciation of his name, did he indicate which of the channels was the natural channel?

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A No, he did not.
Q So you have -- you have some evidence that they are artificial channels, however persuasive it may be, but you can't tell us which one is the natural channel?
A That's correct. He doesn't even claim that they are -- that they are responsible for diversionary channels.
MR. DODGE: Thank you. I have no further questions
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Dodge.
Mr. Roos-Collins?
RECROSS EXAMINATION BY MR. ROOS-COLLINS
Q Good morning. Mr. Wolff, pursuant to water rights held by the City of Los Angeles, is water diverted from

the Upper Owens River for irrigation?
A BY MR. WOLFF: I don't know anything about their water rights, but I know that water is diverted out of the Upper Owens River. Actually, let me clarify that. I think I do know something about their water rights. It's in our report here.
We had information from the State Water Resources Control Board that -- it stated some quantities on a particular water right. Let me just check that to make sure that's one of Los Angeles'. It was for the Jacobs

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east and Jacobs west diversion, and if my recollection is correct, that is a water right owned by Los Angeles.
Q I believe you're referring to Page 19, the final paragraph of DFG Exhibit 62?
A Okay. Yes. Okay. There it is. It's the first sentence says, "State Water Resources Control Board records include L.A.DWP's statements of water diversion used for two diversions, Jacobs east and Jacobs west."
Q Let me read a portion of the Draft Environmental Impact Report, Volume One, Page 3-A-13, in the section entitled Upper Owens River. Quote, Significant diversions are made from the Owens River and Hot Creek for irrigation of L.A.DWP and private grazing pasture lands. L.A.DWP records indicate that an average of 20,000 acre-feet a year are diverted for irrigation of its lands."
Do you concur with that statement?
A Well, I don't know enough about their records to know about the total quantities, but I don't have any information that would dispute it.
Q On a continuous basis, what does 20,000 acre-feet per year equal in cubic-feet-per-second flow?
A Well, if you average 20,000 acre-feet out over an entire year, that equals roughly 29 cfs.
Q And the base flow of the Upper Owens River not

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including augmentation, is what?
A Well, correct me if I'm wrong, Rick. It's 76 cfs?
A BY DR. SITTS: 76.
Q Let's return to the Mono Basin. Parker Creek. Does the City of Los Angeles divert water from South Parker Creek for irrigation?
A BY MR. GARY SMITH: I believe water is -- excuse me. Water is diverted out of South Parker Creek upstream of the Lee Vining conduit for irrigation purposes on private lands, and I believe also Department of Water and Power lands.
Q So notwithstanding the termination of export which Mr. Birmingham discussed, it is your understanding that L.A. continues to divert water from South Parker Creek for irrigation?
A If they are the landowners, water is being diverted on the property, yes.
Q What about Parker Creek, itself?
A The same situation, I believe.
Q What about Walker?
A I don't think so on Walker Creek. I'm not positive on that one. When we get upstream of the

conduit, I get a little fuzzy.
Q Dr. Sitts, Mr. Birmingham asked you questions this
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morning about irrigation diversions and grazing prior to 1941 on both Walker and Parker Creeks. Do you recall those questions?
A BY DR. SITTS: Yes.
Q Do you have an opinion as to when the City of Los Angeles acquired the water rights on Walker Creek that had previously been used for irrigation?
MR. BIRMINGHAM: I'm going to object on the grounds that this calls for knowledge of a percipient witness, not an opinion by an expert.
HEARING OFFICER DEL PIERO: Overruled. Do you know the answer to the question?
DR. SITTS: No. Is he asking for an opinion --
HEARING OFFICER DEL PIERO: He's asking you for a date. Do you know when?
DR. SITTS: No.
HEARING OFFICER DEL PIERO: Proceed, Mr. Roos-Collins.
Q BY MR. ROOS-COLLINS: Have you read chapter G -- excuse me, Chapter 3-G of the Draft Environmental Impact Report?
A BY DR. SITTS: No.
MR. ROOS-COLLINS: Thank you.
Mr. Smith, what is Cal-Trout's next in order?
MR. SMITH: Next in order's 31.
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Q BY MR. ROOS-COLLINS: Dr. Sitts, I show you now Cal-Trout 31, which is a record of the meeting of the Board of Fish and Game Commissioners of the State of California, August 10th, 1927, and I ask that you read the first paragraph on the second page beginning, "Resolved further that the Fish and Game Commission does hereby order Cane Irrigation Company."
A BY DR. SITTS: You want me to read this into the record?
Q Read it to yourself.
A Okay.
Q Other than what you just read, do you have any knowledge about any order by the Fish and Game Commission to the Cane Irrigation Company regarding screening of its irrigation canals as described in Cal-Trout Exhibit 31?
A No.
MR. ROOS-COLLINS: Thank you. No further questions.
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Roos-Collins.
Mr. Valentine?
MR. VALENTINE: No questions. Thank you.
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Valentine.
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Mr. Haselton?
MR. HASELTON: Just a few.
RECROSS EXAMINATION BY MR. HASELTON
Q Mr. Parmenter, you had made reference to the wild trout program of the Middle Owens River which, if I

recall, extends from Pleasant Valley, the dam, down to Five Bridges.
A BY MR. PARMENTER: That's correct.
Q And what was the approximate length of that?
A 16 miles.
Q 16 river miles?
A Yes.
Q What when was that program implemented?
A On the Middle Owens, I think it was 1978.
Q Okay. Do you recall what were the components of that program?
A There was a special angling regulation instituted, a policy change whereby the department ceased stocking of hatchery trout, and a monitoring program assumed.
Q To your knowledge, was there any physical manipulation to that portion of the river?
A I'm aware that there has been attempts to manipulate and control the river.
Q As a part of that wild trout program?
A Attempts to manage habitat. It's not necessarily

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the same thing as the management of the fish population in the abstract.
Q Mr. Smith, I just want to take a few moments and just revisit Page 218 of Exhibit 62, which have -- describes habitat development measures. We talked about it a little bit last night.
A BY MR. GARY SMITH: All right.
Q Have you or your department ever completed a similar program that included all of these measures in this area in the Owens River watershed?
A I have not. And frankly, I'm not familiar with the history of activities over the past decades, so I really can't respond to that with respect to department activities.
Q Mr. Parmenter, would you -- is it safe to say, then, that probably a principal if not the principal component of the wild trout program is the adjusted regulations?
A BY MR. PARMENTER: No.
Q And then what are other -- what are the other components?
A A primary focus is in habitat protection.
Q Okay.
A There -- as you questioned earlier, another focus is in habitat restoration, when that's possible and

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appropriate.
Q Um-hum.
A That's been -- at least on the flow levels, that's been -- because of course excess, that's been a very minor component.
Q I'm sorry?
A There have been efforts to stabilize eroding banks by rip-wrapping with local rock and efforts to revegetate using artificial propagation of willows which have failed. I'd say almost 100 percent failure, and I'm currently in a project to restore native cottonwood over storage along the stream, and it's proceeding.

Q One last question for Mr. Smith, have you or the department approached any of the private landowners regarding this habitat development plan that's described on Page 218?
A BY MR. GARY SMITH: Excuse me. I spoke briefly with Mr. John Arcularius on this matter several weeks ago, just briefly.
Q Could you characterize his response? In a civil manner?
A His response was -- he was supportive -- I'll to have paraphrase it. He was supportive of actions which would reopen abandoned channels. He wasn't too

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supportive of heavy construction, I think would be the best term. Heavy construction activities.
MR. HASELTON: That's it. Thanks a lot.
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Haselton.
Mr. Frink?
MR. FRINK: No questions.
HEARING OFFICER DEL PIERO: Mr. Satkowski?
Mr. Herrera? Mr. Canaday?
MR. CANADAY: No.
HEARING OFFICER DEL PIERO: Thank you very much.
Gentlemen, thank you for your time. We appreciate your efforts.
MS. CAHILL: At this time, we would like to offer exhibits into evidence and in order to facilitate it, we have made up a list of the NAS/MLC exhibits as well as the DFG ones. Let me first just on the record clarify when we introduced Dr. Sitts' errata sheet yesterday, we gave it Exhibit No. 17-A, but it should be clear that those errata did not apply only to Exhibit 17, they applied to Exhibits DFG 17, 25, 57, 58, 59, 60, and 62.
HEARING OFFICER DEL PIERO: As are indicated on the documents.
MS. CAHILL: Right. DFG Exhibits 88 through 94

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were not identified by any witness. They were photographs taken during the field investigation primarily on the Lee Vining Creek. Exhibits 98 through 104 related to the duck testimony. We would offer into evidence DFG Exhibit 1 through DFG 176, Cal-Trout Exhibit 5 and Cal-Trout exhibits -- I'm sorry.
MR. SMITH: Got you so far.
HEARING OFFICER DEL PIERO: Mr. Smith is fast but he's not that fast.
MS. CAHILL: Do you have the most current lists?
MR. SMITH: Yes
MS. CAHILL: All of the DFG Exhibits 1 through 176. Cal-Trout 5. Cal-Trout 5-A through 5-T. NAS/MLC 1-U, 1-W, 1-A, B, 141, and that's also SLC and DPR 1. NAS/MLC 142, 159, 161, 162, 163, 164, 176, 177, 178, 179, 180, 81 --181, 182, 183, 184, 185, 188, 192, 205, 206, 207, 208, 209, 210, 211, 212, and 213.
MR. SMITH: Thank you very much.
HEARING OFFICER DEL PIERO: Any objections?
MR. BIRMINGHAM: Yes.
HEARING OFFICER DEL PIERO: Mr. Birmingham?

MR. BIRMINGHAM: With respect to those Department of Fish and Game exhibits that were not identified during the course of the proceeding --

HEARING OFFICER DEL PIERO: Which were not identified prior to the proceeding?

MR. BIRMINGHAM: During the proceeding. There are -- DFG exhibits that involve -- that are the written testimony of witnesses that have not appeared.

HEARING OFFICER DEL PIERO: Yes.

MR. BIRMINGHAM: And there are other Department of Fish and Game exhibits that were not the subject of testimony of witnesses who have appeared, and we do object to the admission of those -- of those exhibits.

HEARING OFFICER DEL PIERO: Ms. Cahill?

MS. CAHILL: We can go through -- with regard --

HEARING OFFICER DEL PIERO: Excuse me, Ms. Cahill.

Mr. Birmingham, you need to be more specific as to that. Because at this point we're dealing with a whole lot of exhibits here and -- you need to track this, okay? Now, please articulate those exhibits to which you have an objection.

MR. DODGE: May I suggest that this be done tomorrow after people have had a chance to, you know, make a complete list so we could proceed today?

HEARING OFFICER DEL PIERO: Are you prepared to move forward with this now?

MR. BIRMINGHAM: No, Mr. Del Piero, I'm not because I do not have a list of those exhibits that were -- that were identified during the course of the proceeding. I'm going to have to go back through all of my notes and check those that have been identified. I can tell you the names of the witnesses who have not appeared, and I think if I get together with Ms. Cahill, it will only take a few minutes to establish those that were not identified by any of the witnesses.

MS. CAHILL: Let me ask just with regard to the Basco (phonetic) witnesses, before Dr. Sitts leaves, I had asked him whether he had solicited those resumes and statements, testimony from subs and employees of Basco (phonetic).

Dr. Sitts, let me ask you again. Did you review both the testimony and the qualifications?

DR. SITTS: Yes.

MS. CAHILL: And did you receive those directly from the persons named?

DR. SITTS: Yes.

MS. CAHILL: And do you have any reason to believe they're not true?

DR. SITTS: No.

MS. CAHILL: Thank you. I'm laying that groundwork for when we have that argument.

HEARING OFFICER DEL PIERO: 8:30 tomorrow morning we'll take up this issue.

Thank you very much, Gentlemen.

Ladies and Gentlemen, we're going to take -- who's

up next, Mr. Flinn and Mr. Dodge, you've got your panel, or is it -- is it Dr. Stine only or --

MR. DODGE: No. It's a combined panel.

HEARING OFFICER DEL PIERO: It's a combined panel. We're going to take -- we're going take ten minutes right now to allow you to seat your panel and get prepared since we're transitioning from one party to another, and then at about 12:15, about an hour from now, actually an hour from when we start again, we'll take a 15-minute lunch break between 12:15 and 12:30, and then we'll be back on again. Okay? Ten minutes.

(Whereupon a short recess was taken.)

HEARING OFFICER DEL PIERO: Ladies and Gentlemen, this hearing will again come to order.

MR. FLINN: Mr. Del Piero, perched briefly as I am at the pinnacle of the food chain, I will yield this to Mr. Dodge.

MR. DODGE: I just wanted to repeat what we agreed a couple of days ago that we would have a panel of four people, Dr. Herbst, Dr. Stine, Mr. Shuford, Dr. Winkler, and we'll start with Dr. Herbst. All questions relating to him whether on direct or cross

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will be dealt with by Mr. Flinn, and then I will deal with questions to the other three members of the panel.

HEARING OFFICER DEL PIERO: Thank you very much.

MR. DODGE: We'll start with Dr. Herbst.

HEARING OFFICER DEL PIERO: Mr. Birmingham?

MR. BIRMINGHAM: And it's our understanding that we have an agreement with Counsel that has not been blessed yet by the Hearing Officer that the cross-examination of this panel which was formed in order to expedite this process would be conducted jointly by Ms. Goldsmith, who will examine Dr. Winkler and Mr. Shuford, and Mr. Moskovitz will cross-examine Dr. Herbst.

HEARING OFFICER DEL PIERO: Very good. Any objections to that process?

MR. FLINN: No. We stipulate to it.

HEARING OFFICER DEL PIERO: I see nodding heads.

MR. FLINN: We agree.

HEARING OFFICER DEL PIERO: Good. Mr. Flinn, proceed.

MR. FLINN: First of all, we might just have the panel members introduce yourselves briefly by stating your names for us.

DR. HERBST: David Herbst.

DR. STINE: Scott Stine.

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MR. SHUFORD: David Shuford.

MR. WINKLER: David Winkler.

HEARING OFFICER DEL PIERO: Mr. Shuford, have we met before?

MR. SHUFORD: I don't believe so. I attended one date of hearing.

HEARING OFFICER DEL PIERO: You all -- please stand up and raise your right hand. Do you promise to tell the truth during the course of this proceeding?

(All say I do.)

HEARING OFFICER DEL PIERO: Please have a seat.

DIRECT EXAMINATION BY MR. FLINN

Q Now that you're under oath, what are your names? Just kidding.

Dr. Herbst, could you identify National Audubon Society and Mono Lake Exhibit 1-G as your testimony in this proceeding?

A BY DR. HERBST: I can.

Q Could you tell us, Dr. Herbst -- actually, before you do, Mr. Herrera, I was hoping you could give me ten-minute and five-minute warnings.

MR. HERRERA: Will do.

Q BY MR. FLINN: Dr. Herbst, could you give a description of your background and professional qualifications, please?

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A BY DR. HERBST: Yes, I can. I have a Ph.D. in zoology and entomology from Oregon State University. I'm currently a research biologist at the University of California Santa Barbara, and I'm stationed at the Sierra Nevada Aquatic Reserve Laboratory, which is in the eastern Sierra.

I've conducted research at Mono Lake I think for longer than anyone continuously dating from 1976. My work has dealt primarily with the physiology and ecology of the alkali fly and algae which inhabit the near shore lake bottom environment, but I've also done research on brine shrimp as well.

Q Now, Dr. Herbst, could you briefly summarize your testimony for us?

A Yes. What I would like to do is present data that's not in the record or I don't believe has been considered completely. I think that all the evidence in this hearing needs to be weighed in order that we can evaluate lake level changes in the broad historical sense; that is to say, in the sense of the kind of lake level changes -- the kind of lake level changes that have occurred since the time of diversions to present-day conditions, which span a range of salinity conditions from 50 to 100 grams per liter and about 40 to 50 feet in lake elevation. This should include not

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only monitoring information but should also include information drive from modeling predictions and from experimental studies as well.

So what I'd like to do in the course of my testimony here is present some of this information to you, or a summary of most of this information. The results of my studies have typically shown that salinity is the environmental factor of primary importance in controlling growth and productivity of the aquatic ecosystem. Though there are other factors that are also important, they usually compound the impact of the salinity problem or only partially offset the problems caused by salinity.

The basic reason that salinity is such an important variable is derived from the fact that the organisms that live in Mono Lake have a need to maintain blood and cell salt concentrations at a constant level. It's a fundamental aspect of their physiology that they maintain this salt balance

otherwise they can't survive and grow and reproduce, and there's no way to avoid the cost that's associated with this osmole regulation, so any increase in salinity that the organisms in Mono Lake experience will always cause a stress. Salinity always will be a stress factor in the physiology of these organisms, and

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the data I have collected by and large demonstrate that salinity translates from being not only a physiological stress factor, but it also inhibits population growth and productivity of the aquatic organisms in the lake as well.

So what I'd first like to consider is a model of alkali fly production that was part of the Environmental Impact Report.

Q If I could interrupt very briefly there. A version of this was marked as one -- on one part of Mono Lake Committee and National Audubon Society Exhibit 66. We've made it a little clearer and bigger, and this is the same exhibit, but we've identified this as Exhibit 66-B, as in boy. I take it back. This is 66-A, as in alpha.

A All right. What's inside the box here is elements that were included in the model, and they emphasize the influence of habitat area on the abundance of flies. And by "habitat area," I mean the amount of hard substrate which is Tufa rock versus soft substrate which is in the lake, fly larvae and pupae, the alkali fly larvae and pupae have varied preferences for these two kinds of substrate. So depending on the lake elevation, there's varied amounts of these two types of habitats in the lake, and that will influence overall

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abundance of larvae and pupae and either decrease or increase the abundance of flies in the model.

As a secondary effect, salinity was incorporated in terms of its effects on the growth of larvae, the development time and size of larvae and pupae were incorporated and that also has an influence on the abundance of flies. However, there are important elements of both habitat considerations and salinity considerations that were not incorporated into the model that's in the EIR that make the model in the EIR really conservative in the sense that it under estimates potential beneficial effects of high lake levels and potential adverse effects of low lake levels.

In terms of habitat, one of the most important features that's not considered in the model is the presence of latoral vegetation or near-shore vegetation being submerged as lake levels come up. Not only do fly larvae and pupae use Tufa and rock as a habitat for attachment, they can also use latoral vegetation. At this point, I'd like to use this -- let's see, NAS and MLC 49 and NAS and MLC 50, photographs that show the attachment of fly pupae and larvae to submerged vegetation in the lake.

Now, these are attached pupae and larvae that

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occurred only in this last year when we had a very

small rise in lake level and relatively little vegetation being inundated but nonetheless, the vegetation that was inundated in this particular small rise in lake level permitted a new habitat for fly larvae and pupae to attach to. So I just wanted to establish --

HEARING OFFICER DEL PIERO: Excuse me. Just for clarification purposes, the latoral vegetation that you're referring to is not vegetation that grows in the lake, itself. It's vegetation that's been inundated because of lake levels going up?

DR. HERBST: That's correct.

HEARING OFFICER DEL PIERO: And does it generally die off?

DR. HERBST: It generally dies off. However, there are certain kinds of vegetation, because of the root system that they have, that appear to persist for at least as long as ten years. During the lake rise that occurred in the early and middle eighties, there was a substantial amount of vegetation submerged that is called the sticelous (phonetic) and the sticelous (phonetic), the salt grass, has a root system that actually goes below the surface. And so even though the vegetation dies, substantial portions of it can

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remain embedded in the substrate and can still provide a substrate for attachment. So though it dies, it and still persist as an attachment site.

HEARING OFFICER DEL PIERO: Does it break down frequently? Does it break down quickly?

DR. HERBST: It eventually does break down.

HEARING OFFICER DEL PIERO: How long?

DR. HERBST: Some of the mats of the sticelous (phonetic) that I saw in the lake in the early nineties, had to have been inundated from that early to middle period of the early eighties. So it had to be a period of years of at least some five to ten years and could quite possibly be longer than that.

HEARING OFFICER DEL PIERO: Thank you.

DR. HERBST: So including this effect of latoral vegetation, there's actually more habitat that could become available to flies as the lake levels get higher even though some of the rocky substrate that's in the lake gets so deep in the water it's no longer accessible.

There's important salinity effects that were also not incorporated into the model and those include the influence of salinity on larval survival, growth of the algal food resource to the flies, the effect of salinity on the size of pupae, and the ability of

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adults to emerge from those pupae, and the influence of salinity on adult body size and the reproductive ability of those adults. These were all effects that were not incorporated into the model here and were they to be incorporated, it would actually produce a model that would show there are more beneficial effects for higher lake level conditions and more adverse effects for low lake level conditions. So I just want to emphasize that that is conservative model.

Now, this is basically the same sort of model that was used both in the Jones and Stokes version of the alkali fly model, and the model I developed with William Kimmerer. Jones and Stokes modified the model that Kimmerer and I produced in such a way that neither of us agreed with what they did, but the outcome, the results of both of those models are basically identical. They showed that the population and abundance of the flies should be maximized based on primarily on these habitat consideration that an elevation range between about 6380 and 6390.

I'd like to move on to an experiment that was done in 1991 as a part of the Environmental Impact Report research work and these are microcosm experiments --

HEARING OFFICER DEL PIERO: Can I see it?

DR. HERBST: Should I turn these?

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These diagrams here show a series of tanks between --

Q BY MR. FLINN: Let me interrupt real briefly again just as a matter of procedure. This is a clarified version of the chart that was Mono Lake Committee and National Audubon Society Exhibit 52, and we mark this clarified version as 52-A.

A BY DR. HERBST: These microcosm tank experiments were specifically designed to simulate ecological conditions in the near shore like environment which I'll sometimes refer to as the benthic ecosystem, and it's the habitat where the alkali fly develops, where the larvae and pupae of the alkali fly live. And each of these tanks from 50 to 75, 100 to 125 grams per liter, were set up out of doors. These tanks are about a meter on a side and contain about 200 gallons of water. They were set up in the early part of the summer with water in them that had been adjusted to each of these different salinity levels. And then added to those salinities were sediments from the lake which contained algae, the eggs of the flies and the larval stage of the flies, and a variety of other benthic micro-organisms and invertebrates that came not only from Mono Lake but from other habitats, both more saline and less saline than Mono Lake. So we were introducing a varied

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community of organisms to the ecosystem.

HEARING OFFICER DEL PIERO: Excuse me. What was your control?

DR. HERBST: The control? With reference to 100 grams per liter, which is what the salinity of the lake was at that time, would be this tank right here.

HEARING OFFICER DEL PIERO: Okay.

DR. HERBST: So what these experiments -- what these charts here depict is the productivity of these tanks in terms of the emerging flies -- and you can see we have our little fly icons on the graphs, and the amount of benthic algae growing in the tanks is depicted by the depth of this shaded area here at the bottom of the tank, and then the body size of individual flies is depicted by these pie diagrams. And the slice out of each pie diagram indicates the total percent fat in those individual flies. So as you

can see, as you go from these high-lake-level-low-salinity conditions, there's a dramatic loss of overall productivity of the population, many fewer flies emerging as we go up to the higher salinities and, in addition, there's also a dramatic reduction in the amount of algae growing in these tanks.

On a per-individual basis, the flies that emerge

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from these different experimental tanks also decreased as we increased the salinity, and the proportion of fat that makes up the body content of flies of decreasing size also decreases as the salinity increases.

HEARING OFFICER DEL PIERO: Do you have specific percentages on these pie charts?

DR. HERBST: Yes, I do.

HEARING OFFICER DEL PIERO: Where?

DR. HERBST: At -- at 6415, 18 percent fat, 15 percent fat at 75, and 10 each at the two higher salinity levels.

HEARING OFFICER DEL PIERO: Thank you.

DR. HERBST: Yep.

MR. MOSKOVITZ: Excuse me, Mr. Hearing Officer. Could those exhibits be labeled with their numbers when they're up on the board so that we can identify them by the numbers that --

HEARING OFFICER DEL PIERO: Sir. Mr. Flinn, if you would be kind enough, do you have a marker there, Sir?

DR. HERBST: The important thing I want to emphasize about these experiments is they're sort of halfway between being a laboratory experiment and an actual change in the lake environment itself. It's about the best experimental manipulation we can do to

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try and simulate what would happen realistically under natural lake conditions, and we're not just seeing what happens when we change salinity in terms of the effects on individual organisms or on single populations or species, we're looking at the composite effect of salinity on the entire community. So these experiments were -- only got very cursory mention in the Environmental Impact Report, and I think it's really important that we use this information that's available to us.

In addition to these microcosm tanks experiments, since 1991, I've also completed a set of experiments having to do with the influence of salinity on the nitrogen budget of the lake and on -- the effects on the brine shrimp, Artemia, and I'd like to outline those results right now.

Q BY MR. FLINN: Just before you do, Dr. Herbst, let me interrupt and ask you if can you identify National Audubon Society and Mono Lake Committee Exhibit 75 as a copy of the paper you wrote or co-authored on salinity and nitrogen fixation?

A BY DR. HERBST: That's right. That's my paper.

MR. FLINN: And while he's setting that up, for record, this is a version, clarified version of what appeared on National Audubon Society and Mono Lake

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Committee Exhibit 66, and we identified this as Exhibit 66-B, as in boy.
MR. HERRERA: Ten minutes, Mr. Flinn.
DR. HERBST: What I want to do is just set up my discussion of the influence of nitrogen fixation by talking about the nitrogen cycle in Mono Lake with regard to the sources and losses of nitrogen. A lot of discussions of the nitrogen budget in Mono Lake is focused on internal cycling of nitrogen within the system. That is to say, nitrogen that comes from sediments that by decomposing organisms becomes released as ammonia dissolved into the lake water, is taken up by organisms, by algae living in the lake, and then through death, goes back into the sediments.
In addition, shrimp that live in the water will also excrete some nitrogen and this ammonia that they excrete can also be available as a nutrient source. So let me back up a bit and just say that nitrogen is the limiting nutrient in the lake, so it's particularly important for us to consider this.
But one of the things that's been largely glossed over, I find, is that there are important losses of nitrogen from the Mono Lake system. And one of the ways nitrogen is lost from the system is that after death, certain kinds of nitrogen compounds that are
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tied up in these dead organisms are refractory, or non-reactive to decomposition by bacteria, and so they're buried in the sediments where they're no longer available for recycling back up into the lake. So nitrogen is lost from this internal cycle by that.
In addition --
HEARING OFFICER DEL PIERO: Wait. Wait. Wait. Wait. I need to understand that. Explain what you're talking about in terms of refractory --
DR. HERBST: Refractory or non-reactive nitrogen is --
HEARING OFFICER DEL PIERO: I understand that. Tell me what --
DR. HERBST: There are particular kinds of nitrogen molecules that are difficult to break down --
HEARING OFFICER DEL PIERO: What are -- where is it coming from in terms of --
DR. HERBST: Well, for the most part, it's protein compounds that contain nitrogen, and some of those protein compounds are more difficult to break down than others.
HEARING OFFICER DEL PIERO: Okay.
DR. HERBST: And so some of that material is buried in the lake sediments and continues to be piled up as the years go along and you can never get access
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to it again. So it disappears from this nitrogen cycle.
In addition, nitrogen's also lost from the system as ammonia gas from the lake water where it's dissolved as ammonium, there are conditions of mixing and solubility that alter whether or not the ammonia can stay in the lake and that result in the expulsion of

ammonia gas fron the lake. So we have a large amount of nitrogen lost from the lake as well to the atmosphere as well as we're having some loss from the system here.

Now, Mono Lake's not a closed system. If it were, and we had these losses going on, eventually this internal nitrogen cycle would run itself down. There'd be no way for new nitrogen to get into the system to supply new nitrogen for the growth of the organism. So there must be some external sources of nitrogen that get into the system as well that allow this balance to occur. A balance is necessary because we're losing nitrogen sediments into the atmosphere.

One of the ways new nitrogen can get in is through atmospheric precipitation. Another way might be through stream flow, but by and large, the calculations that have been done suggest that that's a relatively minor contribution. There's not that much nitrogen

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that can get into the lake that way. And probably the best way or the most abundant way that new nitrogen can get into the lake from the outside and, in fact, the way a lot of new nitrogen gets into ecosystems on a global scale, is through a process known as nitrogen fixation.

And nitrogen fixation is a process whereby nitrogen gas from the atmosphere becoming dissolved in the lake water is taken up by certain kinds of bacterial organisms, cyanobacteria, which are often called blue-green algae. These blue-green algae change this nitrogen gas into a reduced form of nitrogen, our old friend ammonium here. And so this process can bring new nitrogen into the system, can bring nitrogen from the external environment back into the lake system and help replenish that which is lost by burial or by the expulsion of nitrogen gas.

So one of the things that I've become particularly interested in is the influence of salinity on this process of bringing new nitrogen into the system, so what I specifically did with colleagues at the United States Geological Survey is to do experiments on cultures of cyanobacteria taken from the near shore, the Toro Lake environment where the conditions are just right for nitrogen fixation, brought them into the

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laboratory and exposed them under several kinds of experimental conditions to a series of salinities to see how that would influence nitrogen fixation. And what we found was that current salinities of about 100 grams per liter, the rate of nitrogen fixation at current salinities is only about half that which we see at lower salinities, at about 50 and 75 grams per liter.

However, in addition to considering the effects of salinity on the rates of nitrogen fixation, we also need to take into account the area of the lake bottom over which nitrogen fixation is occurring. So if we look at both salinity and lake area affects on nitrogen fixation, we see the following result.

MR. FLINN: Let me interrupt you right here. That

is graphic depiction of data in a table contained in Exhibit 65. We would mark this as Exhibit 65-A.
MR. HERRERA: Five minutes, Mr. Flinn.
DR. HERBST: So over her on this axis, we have --
MR. MOSKOVITZ: May I make an inquiry to see whether I want to object or not?
HEARING OFFICER DEL PIERO: Yes, Mr. Moskovitz.
MR. MOSKOVITZ: Is what you've marked as Exhibit 65-A a graphic representation of the same graph or another representation of a graph, or is it simply a

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representation of data that appear in some other form?
MR. FLINN: If you look at the numbers on Page 11 of Exhibit 65, the elevation feet column on the left-hand column of that is the X axis of this graph, and if you look at the nitrogen fixation in millions of moles, I guess, molecules of nitrogen -- is that right? Moles?
DR. HERBST: Moles, it's not molecules.
MR. FLINN: -- of nitrogen. The last column on the right represents the Y axis, and each one of those data points are the numbers that appear on the column under nitrogen fixation.
MR. MOSKOVITZ: Just one more inquiry, please. Your Exhibit 64 on Page 5, lower right, has a -- a graph that I believe purports to depict the same kind of information that's shown on Exhibit 65-A, although the orientation is reversed. Is -- is Exhibit 65-A the same in terms of what is shown by the graph as is on Page 5, lower right-hand graph in Exhibit 64?
DR. HERBST: Yeah, it is.
MR. MOSKOVITZ: It is?
DR. HERBST: It's a percentage -- rather than absolute numbers, it's graphed as a percentage, though, of the maximum value there. So here we have absolute numbers of potential nitrogen fixation lake wide, and

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on the graph that you're looking at, it simply shows where the maximum value is and takes that to be 100 percent and relates everything else to that 100 percent value. So it's just a more simplified way of looking at this relationship.
MR. MOSKOVITZ: With that explanation, I will not object to a new exhibit being presented.
HEARING OFFICER DEL PIERO: Mr. Herbst, please proceed.
DR. HERBST: Sure. So what these data show is that at these low lake levels, we have a substantial loss of potential nitrogen fixation lake wide, both because there's less area available over which this fixation can occur and because there's a substantial inhibition of the process of fixation at these lower lake levels and higher salinities. It's maximized at an elevation of 6390 which corresponds approximately to 75 grams per liter, and declines at higher elevations because of the fact that even though there is equal rates of overall nitrogen fixation at these higher lake levels and lower salinities, nonetheless, there's less actual latoral benthic area around the lake as a whole, and so less area over which fixation can occur. And so

you see a drop in the total amount of nitrogen that can come into the system.

Once again, though, along with this data, we need to consider each of the different elements of these experiments that allow us to be able to try and predict what the overall in the case of these things should be on productivity of the system. But let me just once again emphasize that the maximization based on both salinity and lake area affects would be an elevation of 6390.

HEARING OFFICER DEL PIERO: Please, Mr. Herrera, make accommodations for this in terms of time. I want to get this clarified.

Ziano bacterial nitrogen fixation. The source of the bacteria?

DR. HERBST: They grow in the lake.

HEARING OFFICER DEL PIERO: Is it your representation that this represents the only sources of nitrogen?

DR. HERBST: Of external nitrogen? No, not at all. There are other sources of nitrogen and other sinks for nitrogen, but by and large, they appear to be insignificant.

HEARING OFFICER DEL PIERO: Excrement from the birds?

DR. HERBST: Well, excrement from the birds is another possibility. But excrement from the birds by

and large should also be internal to the system because they're at the lake feeding on the organisms which are taken nitrogen from the lake itself --

HEARING OFFICER DEL PIERO: That's assumed in the biological update bubble that's reflected on the chart; is that correct?

DR. HERBST: Yes.

HEARING OFFICER DEL PIERO: I just want to be sure I understand what you're representing.

DR. HERBST: Sure. Let me finally go on to experiments that I conducted with the brine shrimp, Artemia, last year as well. Previous studies that have been done on the effects of salinity on the growth of brine shrimp have examined salinities as 75 grams per liter and above, never below that particular salinity. So if we want to address the concerns of how shrimp might be able to grow and develop at salinities more comparable to what they were historically, that is to say, at 50 grams per liter, then we need to examine that low salinity level. Once again, all we have right now or previous to these data are data from 75 grams per liter and above. So in order to address whether or not they're doing any better or worse at 50 grams per liter, these experiments needed to be conducted.

So I removed cysts from Artemia, which are dormant

eggs that live in the sediments of the lake, incubated them at different salinities, and then watched the growth and development of the shrimp from those experiments.

MR. FLINN: Again, briefly interrupting, this is a

combination of exhibits, Mono Lake and National Audubon Society Exhibits 201, 202, and 203. We would mark this combination as Exhibit 201-A.

DR. HERBST: I can't quite get it all on here. Does this go down any more?

MR. CANADAY: It goes forward.

HEARING OFFICER DEL PIERO: You must be a Stanford grad.

DR. HERBST: So from the experiments where I hatched cysts in these different salinities in Mono Lake water, the cyst hatch was approximately the same across all the treatments and the survival to this stage of the experiment was the same across all these treatments. So no real difference in hatching success or survivorship across these treatments.

What was significant, though, was that as we go from the low-salinity condition to the high-salinity conditions, you can see that there's a shift in the body size and age distribution curves to the left. What these bars show in these hatched areas are the

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number of individuals that were in the adult stage. Then once again, left to right, we have increasing size classes. So what we see at this lowest salinity at the 50-gram-per-liter level that had not previously been examined is that there is both a higher proportion of the shrimp that have developed into the adult stage and moreover, they've developed into a larger body-sized shrimp than we see at these lower salinity levels. So there appears to be both delays in development and smaller body size.

Q BY MR. FLINN: Briefly, can you just relate each one of those salinities to lake levels for us?

A 50 grams per liter would be 6415. 75 grams per liter would be 6389. 100 grams per liter would be 6373.

So finally, if we consider all this information together and try to search for an optimization between all these different factors, we can look at habitat availability as being one factor that's maximized between elevations of 6380 to 6400. That's where the best rocky habitat is available on the lake for for the flies.

In terms of the beneficial effects of low salinity at 75 grams per liter or 50 grams per liter, the most beneficial effects would be between elevations of 6390

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and 6415. So the range of overlap where we optimize both good habitat and good salinity effects are between 6390 and 6400.

In addition, the nitrogen fixation data also suggests that 6390 is the best condition in -- for which new nitrogen can be introduced into the ecosystem, new nutrients can come into the system.

I believe that's all I have.

MR. HERRERA: Mr. Flinn, that's time.

HEARING OFFICER DEL PIERO: Mr. Flinn?

MR. FLINN: I'll actually probably be revisiting my questions on a redirect anyway, so I'll just hold off.

Mr. Dodge.
HEARING OFFICER DEL PIERO: Mr. Dodge? Are we going need the screen any further?
Mr. Dodge?
MR. DODGE: No, I don't think.
HEARING OFFICER DEL PIERO: We're ready to go.

DIRECT EXAMINATION BY MR. DODGE

Q Dr. Stine, I would like you to summarize briefly a portion of National Audubon Society and Mono Lake Committee 1-U. You've previously testified about the -- if I may speak loosely, the duck oriented aspects of Exhibit 1-U, and I don't want you to repeat

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that. And you've also talked a little bit about wetlands at Crowley Lake, and I don't want you to repeat that, but there is on Exhibit 1-U from Pages 7 to 9, certain testimony about the physical situation with respect to certain islands at Mono Lake, and I'd like you to summarize that testimony, please.
A BY DR. STINE: I'd be glad to and I will make reference to something on the -- something on the qualifications that comes in here. I have written an auxiliary report, one of the five, Auxiliary Report Number 22 that is called Lake Fluctuation Induced Changes in the Size and Configuration of the Mono Islands, and it's that report that a lot of what I will be presenting here is based on.
I want to concentrate on the Mono islands, and I'll be referring to NAS/MLC Exhibit 159, which has been introduced previously, and to NAS/MLC Exhibit 142. And we'll start here on Exhibit 159. We can see that in 1930, and indeed for sometime after 1930, into the forties and fifties, we had two main islands in Mono Lake, Paoha Island near the center of the lake, which is an island composed primarily of lake bottom sediments that have been unparched due to volcanisms, and Negit Island, a smaller island here to the northeast of Paoha that is composed of hardrock,

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volcanic rock due to volcanic activity on the lake floor.
Perhaps not apparent to those of you far distant from NAS 159 here are a constellation of small islets just to the north of Negit Island, and we refer to these indeed as the Negit Islets and there are some rather clever names that have been tied to these things over the years by my gull-studying colleagues, and we'll be able to identify a few of those as we -- as we go along.
The islands here are of interest to the Mono Lake controversy for reasons that I don't have to dwell on. It's a gull-nesting area, has been for a long time and the gull colony here has, I don't think there's any dispute about this, been disrupted by coyotes from time to time. The purpose of the testimony here is to provide background for Mr. Shuford and Dr. Winkler in their discussion of the effect of coyotes and predation on the birds of the Mono Island.
Going then to Exhibit 142, MLC -- or pardon me, NAS/MLC 142, we can see the change in the islands that

have occurred, changes that have occurred as Mono Lake has dropped between 1930 on the one -- shown on 159, and 1982, shown on Exhibit 142. The islands have, in all cases, gotten larger. That includes the Negit

Islands as well as the major islands, Negit Island and Paoha Island.

Also, another constellation of islets, the so-called Paoha islets, have emerged just to the west of Paoha Island. Those islets are composed of soft sediments and are easily erodible. For the purposes of this testimony, I'll be concentrating on Negit Island and the Negit Islets, and we can see that by 1982, Negit Island was connected to the main land by a land bridge, a land bridge that is very well known, but to which there is often tied a misconception. That misconception lies in the idea, the incorrect idea, that as the lake level drops, Negit Island enlarges toward the mainland and the mainland enlarges toward Negit Island. And at some point these two land masses then coalesce into a land bridge or a causeway that can be crossed by coyotes.

What I'd like to point out here by way of slides -- Dave, if could you lower the -- by way of slides, is that it's a little bit more complicated than this. That, in fact, a third island, a third large island emerges -- let's see. What do we have in there? That's kind of interesting. Something hairy.

HEARING OFFICER DEL PIERO: One of those flies left over.

DR. STINE: Presumably this can be seen by everyone. It's a photograph taken in 1972 of Mono Lake. The lake elevation here would be about 6386, roughly --

MR. HERRERA: Excuse me, Scott. Could you elevate that so that the people in the back can see it?

DR. STINE: Sure. I'll tell you what. We can pull this back somewhat.

MR. HERRERA: Maybe elevate the projector itself.

DR. STINE: How's that?

MR. HERRERA: Much better.

DR. STINE: This, by the way, is NAS/MLC Exhibit 193. Again, showing Mono Lake in 1972, approximate elevation here is 6386 feet. Paoha Island near the center of Mono Lake, Negit Island to the northwest, and then this white blob here which is, in fact, not a reflection but a new island that has risen from the lake, emerged from the lake as the lake is falling. That island, itself, which will become the land bridge emerges first at an elevation of 6390 feet and luckily, we have a photograph that shows that thing that has come out within a few days or a few weeks, something like that, prior to the snapping of the photograph. So we know what elevation this comes out.

As you can see, it's this island, then, that grows

both island ward and land ward. It's that island growth that then becomes the causeway. As a result, we have actually two straits through here, straits being

linear water bodies between -- that lie between two land objects. We have what has been referred to by me and others now as Damned Straits that lies between the mainland and this island and Dire Straits which lies between Negit Island and the mainland. And, of course, as Mono Lake falls, then, these two straits become narrower and narrower and eventually disappear at the time the actual land bridge bridging event occurs which, by the way, for Negit Island is at approximately 6375 feet.

Now, I have prepared cross-sections that show the configuration of the straits here and the depth of the straits and the width of the straits at various lake levels coinciding with the -- some of the alternatives discussed in the DEIR. I would like to point those out in a second. For now, let me just show you where the transect would be. It would be from Negit Island across the land bridge and on to the mainland. I'll show one exhibit along that transect, I'll then show a second exhibit that goes from two of the islets out here, particularly Twain Islet, which is the largest of the Negit Islets that we can see on this map. From

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Twain Islet across the land bridge and on to the mainland, and then I'll show a third transect that's going to go from the tip, the northwestern tip of Negit Island on to Java, which is the islet -- Negit Islet closest to Negit Island. It will go from Negit Island to Java then to Twain showing another possible route for coyote crossing there and how that route changes in terms of depth and width of the straits at the various lake level alternatives.

But before I do that, let me just point out that this ultimately is what occurs when the lake gets down to about 6372 feet. Again, at 6375 feet, we complete the land bridge between the mainland and Negit Island, but at 6372 feet, in fact, this is at 6372.67, and is part of, its simply one of the photographs that went into the mosaic that composes Exhibit 142 --

MR. DODGE: We have labeled this Exhibit 142-A, and I believe copies have been distributed, correct?

Thank you.

DR. STINE: This photograph is taken when the lake is at 6372.67 feet. You can see that, of course, not only is the Negit land bridge complete, but Twain, the largest of the Negit Islets, and Java, another large Negit Islet here, both of which are of importance to gulls in ways that I won't go into, they are at this

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time, at this elevation, very, very close to being themselves land bridged. And so with that background, let's go to those transects that I threatened a moment ago.

And I should point out that I believe, Mr. Del Piero, you have been furnished with sort of a packet there? Yes. Okay. The first of these that we'd like to look at is called topographic profile Number One, and it is NAS/MLC Exhibit No. 198. And on this exhibit, what I have done here is to cross from Negit Island -- cross from Negit Island, which is shown

at the right of the graph here, all the way over to the mainland, and this hump-like feature, sort of dromedary-like feature in the center here is the land bridge, and you can see there's a low spot, a channel, a straits to either side of that high point of the land bridge.

Now, at an elevation of 6372 -- pardon me. 6372 feet, there is no water in either straits. So we can walk from the mainland to Negit Island without getting our feet wet. I haven't shown that here. The lowest elevation I've shown is 6377 feet. At 6377 feet, Dire Straits is, I've shown up here, approximately 662 feet wide, and as we go then to higher lake levels, we can see that not only does Dire Straits widen, for

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instance, in the second block diagram with a surface elevation of 6383.5 feet, Dire Straits has now widened to 2,280 feet, but Damned Straits all of a sudden has water in it as well. So Damned Straits now contains a straits that is 2260 feet approximately wide.

By the time we get up to an elevation of 6390 feet, that is a lake level 6390 feet, we've completely submerged the land bridge, itself, the land bridge is now no longer visible, and we have one water body that covers the land bridge, and so 6390 feet, we show a Dire Straits 4100 feet wide and a Damned Straits 1760 feet wide. Actually, there is a tiny, tiny island sitting there at that time, but it is very small. Nevertheless, it does provide a basis for constituting two straits there. And by the time we get up to the lake alternative, 6410 feet, we have a continuous waterway that is about a mile and a half or so wide, 8630 feet wide.

Okay. Now, going to the second topographic profile, topographic profile Number Two, we're now moving from the mainland to Twain Island. Twain Island, again, being the largest of the Negit Islets. And I've essentially done the same thing here in block Diagram Four. The first block diagram represents an elevation of 6377 feet, a lake level of 6377 feet. The

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reason I don't show a block diagram for 6372 feet, of course, is because Twain Island is actually land bridged at that elevation. But by 6377 feet, we have a little bit of water in Dire Straits. Indeed, at 6377 feet, we have a straits width there of 1663 feet, and that can actually be measured on the ground. By the time the lake rises to 6383.5 feet, the straits have widened, and we now have a straits width of 6190 feet and, of course, as we go higher and higher, then, the water -- waterway gets -- we lose the two straits. We get into one long straits, and we have a much, much wider band there, 10,550 feet between Twain Island and the mainland at that particular lake level, 6410.

Now, on topographic profile Number Three, as I say, what we're really doing here is going from Negit Island to Java and then to Twain. We're, in a sense, island hoping there, and I've prepared this simply to give an idea of how much water crossing there is protecting these islets in a sense from one another.

At an elevation of 6372 feet, you can see that the one straight here, which we've never named, we can come up with some asinine names, I'm sure, but the one straight between Java and Twain is very, very narrow, about 230 feet wide, and there's, in fact, no water between Negit and Java. It's essentially zero where we drew the

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transect.

By the time we get up to a lake elevation of 6377 feet, we've filled these straits a little bit more and, of course, we can see the numbers up here, 345 feet width between Negit and Java and 680 feet width between Java and Twain, and then in the final diagram here, 6383.5, we can see that it's a continuous waterway in there, 6383.5 we have a width of about 2,200 feet separating the -- separating the islands from one another.

I believe that concludes my testimony, which is, as I say, in preparation really for Mr. Shuford and Dr. Winkler. Thank you.

HEARING OFFICER DEL PIERO: Thank you very much. You want to break?

MR. DODGE: Would now be a good time to take our break?

HEARING OFFICER DEL PIERO: Ladies and Gentlemen, we will return at 25 after the hour.

(Whereupon a short recess was taken.)

HEARING OFFICER DEL PIERO: Ladies and Gentlemen, this hearing will again come to order. Since we've returned from the lunch break, Mr. Dodge, do you wish to proceed?

MR. DODGE: Yes, I do. I'd like to call back

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Dr. Stine for just a moment. It was pointed out to me by Mr. Canaday that I was -- something I was unaware of and I believe Dr. Stine was unaware of, and that is that there are a couple of differences between the exhibits we have offered in writing, if I can find those, those being Exhibit 198 and 199, and the blowups that were done last night.

And, Dr. Stine, can you confirm that?

DR. STINE: Yes, I can. Except that it was early this morning or was it last night? It was after midnight, which might be part of the problem, but in any case, this was an early draft and we ran off and -- this was an August draft, I believe, and we ran off and enlarged this one. So what would I like to do, if possible, is just make a couple corrections on here. What you have in your hands, those of you who have this stapled packet of exhibits, is correct. It's the enlargement up here that is incorrect. The straits widths on profile Number One should read 0, 662 feet approximately 2280 feet and approximately 3800 feet. So I'll make that change here.

On Damned Straits, the column should read 00, 1760 feet and 2260 feet. The two numbers on this are reversed. It's correct in the packet you have, so I'll make that change. And then an even simpler change on

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profile Number Two, I believe it is, the straits'

widths for Dire Straits should read 0, 1663 feet, and then 2700 feet instead of a blank line. That 2700 feet then should be followed by two blank lines. Damned Straits should read 00, 1200 feet, and then followed by two blank lines. The bottom -- the middle column there, as it were, should read 6190 feet -- I apologize. Let's see. No, the rest of that is correct.

Q BY MR. DODGE: The 6190 would be deleted.

A BY DR. STINE: 6190 is indeed deleted. That's correct.

I suppose the bottom line here is pay attention to what's in your hand rather than what's on the board or what was on the board. It's now corrected.

Q In any event, Dr. Stine, the materials submitted in your written testimony I believe in September of this year is correct.

A That is indeed the case and I thank, embarrassingly, I thank Mr. Canaday.

HEARING OFFICER DEL PIERO: Thank you very much. Those corrections will be noted for the record. Please proceed, Mr. Dodge.

MR. DODGE: Yes.

Q BY MR. DODGE: Mr. Shuford.

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A BY MR. SHUFORD: I'm here.

Q Can you identify for the record National Audubon Society and Mono Lake Committee Exhibit 1-P as your written testimony?

A Yes, I can.

Q And do you have any corrections to that testimony?

A No, I don't.

Q And is the testimony accurate?

A To the best of my knowledge, yes.

Q Could you summarize for us the information presented on Exhibit 1-P?

A I have a Master's degree in ecology from UC Davis, and I'm currently employed by Point Reyes Bird Observatory, and for the last 11 years, from 1983 through the present, I've conducted or overseen research on the ecology, population, size and reproductive success of California gulls in Mono Lake. Our work is focused on the Negit Islets, which during the period, have contributed 70 -- approximately 70 to 85 percent of the total population of nesting gulls at the lake.

And I also have extensive experience throughout California surveying population sizes and habitat needs of wetland dependent birds, particularly shore birds, and also including snowy plovers. And from our

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research at Mono Lake, my colleagues and I have made 13 reports or papers that deal with California gulls, and these have been used extensively by Jones and Stokes in providing information for the DEIR process.

And I was also contracted because of my expertise on California gulls at Mono Lake to comment on wildlife sections of the Draft Environmental Impact Report.

Before I get into some of the real specifics, I'd just like to set the stage for understanding, you know,

the importance of the California gull colony at Mono Lake and the factors that have been identified as influencing reproductive success of that colony.

As probably you heard many times, the Mono Lake California gull colony is the second largest concentration of California gulls in the world, the first being at Great Salt Lake. Despite the large size of it's colony, in 1978, the California Department of Fish and Game identified this colony or the California gull, in particular, in the State of California as a species of special concern, and the reason for that being the potential threats of water developments -- water diversions to that colony.

The current size of the California gull population at Mono Lake is between 60 and 65,000 breeding adults, and this colony dwarfs in size any other California

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gull colony within this state. The two next largest colonies are one on the coast at Alviso (phonetic) on San Francisco Bay, which is about 6800 birds, the most recent count, and at Clear Lake in Modoc County in the interior. That population has been somewhere between 5 and 10,000 adults.

In 1992, at the end of the recent six-year drought, the Mono Lake colony represented about 85 percent of the total population of California gulls breeding in California. And during that period because of the, you know, lowering of lake levels and reservoir levels, many of these other colonies were abandoned and the populations of other interior colonies were reduced by 65 percent, about 10,000 birds at those colonies.

And to give you a little more perspective of eight other interior California gull colonies in the state, during that period, 1992, five of these were not active because of water levels had dropped where they breed. The three remaining of those colonies, two of them only supported in total 400 -- approximately 400 California gulls. So what that means, if you look at the Great Basin in general, Mono Lake and Great Salt Lake really provide a refuge for California gulls to breed. These two colonies consistently have supported a large numbers of California gulls throughout most of their

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history and during these extensive drought periods.

Probably people have heard a lot about increases in California gulls historically, both at Mono Lake and also throughout the west. And I would just caution the interpretation of this data. There really were no systematic censuses of California gulls of the whole colony until 1976 when Dr. Winkler started his work, and throughout the west, because of these, you know, changing climatic conditions and colonies appearing and disappearing, it's really hard to go back to the historical record and add up the number of California gulls at any point in time. If you're adding up numbers from one site during a drought period, adding up numbers from a colony that's not enduring a drought period, you may be sort of mixing apples and oranges and not getting a really good estimate of the population size.

There's never been a systematic censusing of the California gull population in the west, and there's -- this has never happened over any period of time. So there's really no solid trend data on these species, and that's not to say California gulls haven't increased, but you should be really cautious in using that data to draw any major conclusions.

And getting on to the breeding biology and the

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basic factors that influence California gull at Mono Lake, there have been six key factors that have been identified. The lack of data on a lot of these factors shouldn't be interpreted as they don't affect the California gulls or they're not influenced by lake level at the lake. I think Judge Finney hit it right on the nail at the proceedings in South Lake Tahoe when he said that it seems like there's more that we don't know about the California gull than what we do, and if that impression is -- there's still that impression from the Draft Environmental Impact Report that this report did not adequately explain all the interactive effects of all these various factors that are affecting the California gull.

I think in a large part that's due to the fact that scientists, including me, have not been very successful in identifying exactly what has influenced the size of the colony or its reproductive success in given years. So the six key factors that we're talking about I'll just list and then give some brief comments about them. The six factors are weather, habitat quality, nesting density, food supply, disease and parasites, and predation. Dr. Winkler will touch on some of these topics, so I won't deal with all of these in detail, but just some of the major events that have

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happened at the lake that were thought to be associated with these various factors.

For weather, 1981 there was a major die off of California gulls, virtually the whole cohort of young gulls died at the lake, and it was thought that this was a combination of heat wave and possibly food supply.

The second factor is habitat quality. There's been quite a controversy over whether habitat on Negit Island, which has a lot of grease wood scrub, is preferred or is better habitat than that on some of the islands that they're currently nesting on where they're mostly nesting on white rock habitat. In my opinion, there are good reasons to believe that the habitat of Negit could provide significant benefits to the gull, but there have been no studies at the lake to compare these habitats directly and compare reproductive success. There's really no way currently to evaluate that data, whether these habitats are preferred or are not preferred.

Nesting density. There's been one paper published by Dr. Jehl which suggests that the adult mortality is higher at higher densities on the nesting island.

Regarding food supply, again, in 1981, Dr. Winkler thought that the low food supply of brine shrimp was a

contributing factor to the major die off of gulls in that year.

Disease and parasites. The main parasite out at Mono Lake is a tick which is endemic not only to Mono Lake -- not only to California gulls but also to the Mono Lake, California, gull population. Our studies have shown a correlation between the amount of tick infestation and mortality of chicks at the lake, but we've not shown any major effect during any given year which contributed to a significant amount of mortality of the population. The one year at little Norway we were convinced that adults abandoned that island because the tick infestation was so high that year. So there really wasn't any direct evidence of a relationship between gull nesting densities and levels of tick infestations on the islands.

Then we get down to the last factor and this is predation, and there have been several predators on California gulls at Mono Lake. Great horned owls and golden eagles and prairie falcons have been shown to prey on adults and young at the lake, but these have contributed only minor amounts of mortality in any given year colony wide. But I think the key factor of all these is predation by coyotes on the California gull. And it's the only one of these six major factors

that I'm discussing that is demonstrated to have a clear and major effect on reproductive success at the lake, and it also has shown a consistent relationship to lake level as well. That's not to say that other factors aren't influenced by lake level, but this is the one factor that's had a major effect on reproductive success. It's clearly linked to lake level. It's detailed in Exhibit A of my written testimony.

Since 1979, there have been five major instances where coyotes have crossed over to nesting islands and have caused abandonment of those islands and total reproductive failure of the colonies. The first of those was -- Dr. Winkler is doing work at the lake in 1979 when the land bridge was formed and coyotes crossed to Negit Island and displaced 33,000 California gulls from that colony causing total reproductive failure in that year.

The other really major event was in 1982 when Twain and Job Islands were visited by coyotes and at least 30 percent of the population was displaced and abandoned that year and was thought to have further effects beyond that on the reproductive success on Mono Lake. The other instances of abandonment have not been of this magnitude. They've been smaller populations.

But I think the information that we could draw from them is quite important towards viewing what kind of security the California gulls need at Mono Lake and which lake level should be identified to protect California gull colonies.

And we've heard in testimony before the Board that coyotes can swim. We all know that is true, and that

there's really no guarantee of security for these nesting islands, and I think theoretically that's true. These coyotes can reach all the islands if they so desire, but I think what common sense and historical record of the last 18 years show is that that is not really the case. There's definitely a relationship of lake level to the access of these coyotes to the islands. In fact, every single instance where coyotes have crossed to these islands and caused a reproductive failure of the nesting birds has been when there's either a direct physical land bridge to these islands or very close to that situation where the coyotes can cross over, wade or swim or walk through very narrow -- or very shallow water in a relatively narrow stretch to get to these islands.

One island in particular, Java, I think, is most instructive of the recent history. Back in '82, Java and also Twain were abandoned with a lake level of 6372

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feet causing total failure of those islands. And hence after that, predictions in the EIR were that at 6373 feet, a foot higher than that lake level, that these islands would be protected. Well, in 1992, what happened was at an elevation of 6374 the coyotes crossed over to Java Island and caused reduction in reproductive success there. And the following year, the lake rose another foot, in 1993, to an elevation of 6374 and during that period, the coyotes crossed over again. And at that lake level, they caused a total reproductive failure of that colony.

So the bottom line is we don't really know what level will protect these islands. If the lake were to rise another foot next year, we don't know for sure whether the coyotes will get across, you know, to Java. The importance of Java is linked closely to Twain. Twain Island, based on the 1982 information, is susceptible to access by coyotes at roughly the same elevation. And currently, Twain Island holds -- has been for quite a while, holding half of the California gulls breeding at Mono Lake. Hence, half of these gulls are potentially susceptible to predation at the exact same level at Java Island which, last year, was at 6375. So, you know, you could argue that 6376 would protect these islands, but it seems like the coyotes

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either haven't read the predictions or are blatantly ignoring these predictions, so we don't really know what's going to happen. So it doesn't give me really high hopes that 6376 is for sure going to keep coyotes off of these two nesting islands.

So we look at the lake level alternatives that we've been discussing. The 6377 foot alternative, the lake -- under this alternative, the lake would drop to 6373 feet, so all of these islands, Twain and Java and Negit Island and Pancake Island are all susceptible to predation that the level.

Twain and Java currently are holding over 50 percent of the population. Back in the mid 70s when Dr. Winkler was doing his work, the islands that we're discussing were holding about 70 percent or more of the

population of California gulls. So at that lake level alternative, all those colonies were susceptible to coyote predation.

If we move up to the next lake level alternative of 6383.5, this should protect Twain and Java, but I consider it really the absolute minimum that might protect Negit Island and Pancake Island. There's several factors that go into that. At 6383.5, the lake could drop to about 6378 feet. The Draft EIR states that at 6376, they're not convinced or not sure that

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the coyotes won't cross over that lake level, and then we have the recent history at Java Island where, in fact, the predictions were incorrect and at a minimum of two feet higher than the predictions, the coyotes could get across. So at that 6383.5 level, it's still possible at the low end that coyotes could get across to Negit Island.

And if we look at the Los Angeles Department of Water and Power's plan for managing the lake level, which as I understand the lake would vary between 6374.6 and 6385.3, it sort of has, I think has been pointed out, sort of worst of both worlds for California gulls, at the high end of this lake level alternative. And we know that during long periods, the lake will vary up and down through this range of lake levels. At the high level, the Paoha Islands will be lost to nesting for the California gulls, and if we go down to the lower lake level, Negit Island will be affected and probably the other two islands which are currently holding half of the gull population. So it seems if the lake were to be managed that the lake level, the gulls would be concentrated on very, very few islands and would undoubtedly not be able to support anywhere near the population it's held today.

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So given the above information, I think that realistically, the 6390 foot alternative is the only alternative that will provide the maximum amount of both potential gull nesting habitat and secure nesting habitat from predation. And if you go beyond the 6390 level, you're still going to have the security. You would lose, you know, some more of Negit Island and some of the Negit Islets, but you still have a huge amount of habitat out there that could accommodate very large numbers of gulls, anything that has been seen at the lake in historical times.

Regarding these predation events, these things are not just a one-time thing that just happens. It's not like turning the faucet on and taking it off again. If you look at this exhibit up here, this is Exhibit B from my testimony. If you look at various islands and what has happened to their population size, these are various years across the top. These are the islands, and these are columns going -- these rows going across are the number of nests that were counted on each of these islands. If we look at Negit Island, Negit island was recolonized again in 1985, and here we are '85, '86, '87, '88, '89 it was still increasing. At

this point, coyotes got across this island and it was subsequently abandoned in this year, 1991.

So if the lake were to vary back and forth near level that the coyotes can get across to Negit Island, you can have a period here five years where the population is growing at a very small rate. This is less than 20 percent of the historical numbers on the island and then again, if it's land bridged again, it is also recolonized again, you could have a period of 10 or 15 years where the gulls were really not using that island. So the key point is that they're not -- it's not just the gulls abandoning. Immediately they go back to the nesting island and can use these islands again.

MR. HERRERA: Mr. Dodge, that's 20 minutes.

MR. DODGE: Mr. Del Piero, I believe that Mr. Shuford can finish in about five minutes; is that right?

MR. SHUFORD: I can try.

MR. DODGE: If we were to apply for ten minutes?

MR. SHUFORD: There's two other islands here that similar effects have been shown. Pancake Island, which is right here, and the numbers here when it was recolonized again, increased over quite a period of time, again coyotes got on the island this year. Numbers were reduced the following year and abandoned this year.

Twain Island was visited by coyotes in '81-82, abandoned in 82. It took it through, like, four years to regain its size. And this was a very unusual event in that Twain Island was connected one year and then we had a huge rise in the lake level the following year making it difficult for coyotes to get there. There's been discussion that Paoha Island could provide alternative habitat for the gulls if, in fact, these other islands were lost. And I think the historical record argues otherwise in this case.

Number One, the gulls have not nested successfully on Paoha Island for approximately 60 years. After humans left the island early in the century and that, you know, possible disturbance was removed, the gulls did not expand on Paoha, in fact, they abandoned Paoha. And during the period of greatest expansion of the colony, the birds were nesting and increasing in number on Negit Island. And currently Paoha Island supports a resident coyote population, and it's able to do this for two factors. It has a base out there which coyotes can subsist on year round. It also has a freshwater source. None of the other islands have these two factors in combination.

In 1985, a Forest Service trapper did extensive efforts to remove coyotes from Paoha Island and to the best of his knowledge, he was successful. He killed one coyote and found the remains of two others and felt there were no other coyotes there. Coyotes have subsequently returned to Paoha Island and remain there today. And this is a major deterrent to nesting on

that island, and they would likely return again if the coyotes were removed once more.

So basically, my judgment is that there's extremely little likelihood that Paoha Island would support any large number of nesting California gulls in the foreseeable future in the lake level elevation ranges we're talking about.

There's also been some talk that concentration -- at the higher lake levels that the gulls are concentrated on Negit and a few of the smaller islands, that this could be detrimental to the California gulls. First of all, there will be quite -- at 6383.5 or 6390, there'll still be considerable nesting habitat on the Negit Islands. Negit Island is a very large nesting island which will be available at that lake elevation.

And the point on Negit Island is the density of California gulls on that island will not increase above what are on these other islands. It's a huge island. Actually, the density of California gulls per acre of

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nesting habitat throughout the lake would actually decrease under that scenario. And these factors such as disease and parasites are thought to be density dependent. In other words, the -- as the density of the population of gulls increases, there would be a greater effect on the California gulls. As I've stated, the density of the gulls would be spread out over these islands and there would actually be a decrease in density.

As far as predation and concentrating the birds on these islands, I don't think that's a major factor either. Currently, we have 50 percent of the colony on one island, and it is very susceptible to predation at current lake levels. And at much higher lake levels, even at 50 percent or more of the colonies on Negit, I think with a higher lake level, it would be very unlikely that coyotes would get to that island.

So the summary on the gull issue, I think my professional judgment is that 6390 feet or higher would be the preferred alternative providing the most amount of habitat for California gulls at Mono Lake and also the most secure habitat for California gulls at Mono Lake.

Q BY MR. DODGE: Mr. Shuford, could you go up to the board there and -- you talked about the size of the

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various islands and you also talked about Negit, Java, and Twain. Can you just point out those islands to the Hearing Officer?

A BY MR. SHUFORD: Right here is Negit Island, the largest island close to the mainland on north shore. Twain Island is the largest of the smaller Negit Islands, right here. And Java's right here, right close to Negit. Those are the key islands I talked about as well as Paoha, which is the largest island.

Q Did you mention there was a coyote invasion on Java in the 1993?

A Yes, there was.

Q And I think you may have misspoke. What was the

lake elevation in 1993?
A It was 6375.
Q Thank you.
Next we'll call David Winkler. Good afternoon, Professor Winkler.
A BY DR. WINKLER: Good afternoon.
Q Do you have a copy of National Audubon Society and Mono Lake Committee Exhibit 1-A-E?
A I do not, but I just looked at your copy before this hearing.
MR. HERRERA: Both those microphones work.
DR. WINKLER: So this is fine? Thanks very much.

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Q BY MR. DODGE: I should note that my copy of that exhibit is labeled National Audubon Society and Mono Lake Committee Exhibit A-E, so on some of them, there may be a one missing. In any event, is that your written testimony, Sir?
A Yes, it is.
Q And do you have any -- do you have any corrections to make?
A No corrections, no.
Q Would you summarize your testimony, please?
A Yes. I'm a professional ornithologist and ecologist presently serving as assistant professor and curator of birds in section of ecology and systematics in Cornell University. I received a Ph.D. in zoology from the University of California at Berkeley in 1983, and I conducted post-doctoral research at the University of Gottenburg in Sweden, at Oxford University in the UK, and at Cornell University with support from the Fulbright Commission, the American Scandinavian Foundation, NATO, and the National Science Foundation. I joined the full-time faculty at Cornell in 1988.
My current teaching duties include graduate seminars as well as advances courses in ornithology and population and evaluation and ecology, and I've also

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taught introductory courses in evolution and ecology. In addition to my Cornell teaching and research seminars presented throughout the U.S. and abroad, I've also served as visiting professor for the first international course in desert ecology at Ben Vareen University of Madeb in Israel where I taught theoretical ecology to an international group of students.
I'm also on the advisory committees for the bird populations studies and National Science experiment panels at the Cornell laboratory of ornithology. I have published or have impressed 25 research papers in peer reviewed scientific journals as well as 20 other reviews, reports, and book chapters, and I'm working on a book on life histories of birds for Oxford University Press. I've studied the ecology and behavior of birds throughout North America, much of Central America, northern Europe, southern Africa and Australia.
My research at Mono Lake began in 1976 when I helped organize the first ecosystem-wide study of the lake funded by the National Science Foundation on the

ecological effects of its changing lake levels. My published papers in professional journals on the birds of Mono Lake include ones on the history of the gull colony there, on the determination of clutch sizes of

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gulls, on the thermal and osmo-regulatory physiology of gull chicks, and on the breeding biology of plovers and the foraging ecology of the breed.

As a result of my experience and expertise, I've served as an information source to the Corey and NAS studies of Mono Lake, offering a technical appendix for the Corey report on populations of gulls and plovers at the lake. At the request of staff at Jones and Stokes Associates, I have reviewed and commented on previous drafts of the wildlife section of the Draft EIR for Mono Basin water rights, and I have reviewed the written testimonies of Dr. Melack and Dr. Jehl, and Dr. Jehl's oral testimonies to these hearings.

Having myself reviewed the work on bird populations at Mono Lake, I know how controversial many aspects of this topic are and, in general, I commend the staff at Jones and Stokes for creating a Draft EIR that distills the important biological conclusions from often contradictory sources of information. Ecology and history are both inexact disciplines where true replication of conditions is rarely, if ever, possible, and I would reinforce David Shuford's statement about our understanding of the California gull population at Mono Lake in that it is typical that the more we study any bird population, the more questions we generate and

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that a proliferation of questions should not be taken as a poor level of understanding relative to or bird populations.

Critics using the language and standards all too seldom uniformly applied of laboratory based science, can always fault ecological and historical findings as being, quote, anecdotal, end quote. However, the lack of replication inherent in historical and ecological data is unvoidable and often the only alternative to making decisions based on such flawed data is to make decisions based on no information at all.

In its most significant addition to our knowledge of the bird populations of Mono Lake, the Draft EIR synthesizes a considerable body of new historical information on numbers of waterfowl visiting Mono Lake during migration in historical times. I have been impressed with the manner in which this material has been gathered and presented, and I am convinced by it that waterfowl populations supported by the lake were much larger than previously suspected and that the lake comprised a waterfowl stopover of broad regional significance.

Despite the overall scholarship and judgment displayed by the Draft EIR, however, there are several points in the ecology of birds at Mono Lake that I

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think should be clarified and on which I will concentrate the rest of my comments today. The first point I'd like to address is that of historical numbers

of gulls. David Shuford and I did work on the history of the gull population at Mono Lake as has Dr. Jehl and some of his colleagues, and I have to emphasize from the start that this historical record is extremely imprecise and by any modern scientific standards, is very suspect. But the one picture that does emerge from that review of that historical record is that there seemed to have been large gull populations at Mono Lake in the late 19th century, that a decrease in those gull populations appears to be associated in time with large scale egging operations, harvesting those eggs for food supply in nearby mining towns, and that the most parsimonious interpretation of what has happened at the lake since then is that the gull population has been engaged in a slow rebound from that depression in population levels.

The reason I raise this at this point is that in several points in previous testimony, it's been implied that we could use the gull populations that were interpreted to be present, say, in 1940 at the beginning of diversions, as some indication of what the normal lake or pristine lake would support. And I

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think if we look at the historical record, the pristine lake, if you will, probably supported many more gulls than were nesting here in 1940. So I don't think 1940 serves as a very good benchmark for comparison.

I'd also mention in passing that I was entertained by Dr. Jehl's testimony that one graduate student's opinion is as good as another when looking at gull population size estimates. This arose in reference to a presentation that Dr. Jehl made about Dr. David Johnston's work at the lake in the early 1950s, and I just want to point out that Dr. Johnston never interpreted his results and never wished to have his results interpreted as any kind of census of the colony. David Shuford and I corresponded with Dr. Johnston when we were preparing our article on the history of gull population at Mono Lake, and he was very loathe to have those estimates that he made be used as a census in any way. In fact, I think he was a bit perturbed with me that his estimates, which we took from his field notes, actually appeared in our paper at all because he didn't want them to be used in the way that they were actually starting to be used here in these hearings.

One other point I wanted to mention in passing is the history of the Caspian tern colony at the lake. I

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just say, first of all, that it's a very small population of birds. Dr. Jehl has given you the numbers. I don't think we could interpret it as being a very broad regional importance, but I just wanted to clarify where they did nest in 1976 when we found them during my first visits to the lake. I guess I can point this out on some of the photos we have up here. Unfortunately, they're pretty -- oh, good. There's a larger one here. This is Twain Island -- I'm sorry. What is this? I cannot see. I don't know what this exhibit number is. This is blowup of this part of

Exhibit 142; is it not?
DR. STINE: Taken at slightly different times.
MR. DODGE: We'd better give it a new exhibit number, and we'll try to make copies for everybody. It will be National Audubon Society Exhibit 230.
DR. WINKLER: Okay. So here's Twain Island, and Twain has a promontory here made of white rocky substrate, but much of the central portion of the island is gravely substrate, and the terns were nesting here to the north of that rocky outcrop on this gravel plateau before the gravel area takes a steep drop off to the north and east. So --
Q BY MR. DODGE: Now, we can all see where you're pointing, but unfortunately, that won't necessarily

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appear in the record.
Can you describe it with as much specificity as you can as to where on the island you found the terns?
A I would prefer to just say that if you took the northeastern quadrant of the island and went out from the center of the island, the rocky promontory in the center, that the terns were nesting on a flat area. Near the edge of the flat area, that is near the northeastern edge of the flat area, before that flat area definitely changes in slope and drops off to a lower plateau area on the island, and I'm interpreting that -- this photograph to indicate that that's approximately halfway along a transect from the central rocky promontory on the island going north northwest to the island's edge.
Q All right. Thank you.
A Now, to return to the gulls. I led the first systematic census of the birds at Mono Lake in 1976 and that included California gulls, and in that summer of 1976, in excess of 33,000 gulls were nesting on Negit Island. This was approximately 65 percent of the lake's breeding gulls, and the majority of these Negit nesting gulls were nesting on a grease wood vegetative plateau on the island's eastern half.
To minimize disturbance to the nesting birds, we

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delayed the census of gulls until the 4th of July and the numbers of chicks that we counted on Negit and the Negit Islands during this July census yielded as estimate of total nesting gulls for the lake of at least 51,162 birds. This 1976 estimate is based on a late season count of chicks and it does not include any correction for the number of adults that had begun nesting in the spring of 1976, but had ceased breeding; that is, they had lost all their eggs and/or chicks by the time of the 1976 census.
This point has not been adequately appreciated in the Draft EIR. The population increases of gulls censused at Mono Lake in the late 1980s have, at best, likely returned their numbers to levels near where they were in 1976.
Dr. Shof -- I mean, David Shuford has summarized the events in 1979 with the land bridging of Negit Island and interests of time, I won't go through those in detail. I've talked about them in my written

testimony. But in 1981, approximately 96 percent of the gull chicks on Negit Island perished before fledgling. At the time, I attributed this extremely high mortality to a combination of reduced food supply and unusually high air temperatures for chicks being raised on rocky islands with no substantial source of

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shade.

The Draft EIR fails to incorporate information on food availability in 1981 that I presented in my appendix to the Corey report. Specifically, although total brine shrimp production for 1981 was not depressed relative to earlier years, the timing of the availability of shrimp was shifted approximately one month later than in other years. Whereas gulls normally begin feeding on shrimp in early June when high shrimp densities appear in surface waters, these high densities did not materialize in 1981 until early July. Although the Draft EIR notes that brine shrimp numbers were similarly delayed in 1982, it incorrectly concludes that, quote, brine shrimp appeared to be sufficiently abundant do sustain the nesting gulls, end quote.

The 1982 season was the only year in the 13 years that the gulls have been intensely studied that they are known to have eaten large numbers of cicadas and without knowing how the gulls would have fared without the emergency of this unpredictable and uncommon food source, it is impossible to conclude how delayed food supplies as in 1982, can be expected to affect the gulls. The low chick productivity of 1981 was followed by another season of the nest side abandonment as

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coyotes reached Twain and Java Islands early in 1982 and caused the abandonment of nesting areas that, in the previous year, had supported approximately 30 percent of the lake's total nesting population.

In addition to this loss of a large segment of the breeding population, the productivity of those gull pairs that persisted in nesting was further depressed from expected levels. In my opinion, this further depression in offspring productivity appears to have been to a, quote, snowballing, unquote, effect whereby adults from Twain and Java having been usurped from their breeding areas by coyotes turned to eating the eggs and chicks of nesting gulls on other islets. Once adults on these other islets had their breeding thus foiled, some of them in turn became predators on other gull's eggs and chicks leading to a spreading of the disruption of Twain and Java Islands throughout the colonies on the Negit Islets.

Contrary to previous testimony describing this effect as a Mono Lake, quote, fairy tale, end quote, this effect has been described in detail in a published paper based on a gull colony in Great Britain to which I've referred when I've mentioned my hypothesized implication of this effect at Mono Lake. The Draft EIR acknowledges the possibility of this snowballing

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effect, but in its summary of the impacts of

intermittent land bridging of gull nesting areas that would occur in the 6377 foot and lower alternatives, it does not mention how this effect could magnify the disturbance of land bridging spreading the reduction of reproductive success to many gulls on still isolated islets.

Next, I would like to turn to some comments on previous testimony in which it's often been claimed on the basis of numbers of adults nesting at the lake or attempting to nest at the lake that there's every indication that the Mono lake ecosystem is healthy. If we consider a long-lived bird like the California gull --

MS. GOLDSMITH: I have an objection at this point. I don't believe this was in the witness' written testimony.

HEARING OFFICER DEL PIERO: Mr. Dodge?

MR. DODGE: I think that, in a very technical sense, that's a right, but we are trying to bring Dr. Winkler out only once. It's expensive for us to do this. I asked him to comment on certain of Dr. Jehl's testimony. Certainly, we have ample precedent in this proceeding for that in terms of expansion of the direct examination. I know Mr. Kuebler did it. I know Dr.

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Beschta did it. I know Mr. Gewe did it for free, and this will be a very minor expansion which hopefully will obviate the need to bring Dr. Winkler back.

HEARING OFFICER DEL PIERO: Thank you very much. I'm going to overrule objection. Proceed.

DR. WINKLER: If we consider a long-lived bird like the California gull and we want to look at year-to-year variations in the health of the Mono Lake ecosystem, looking at total numbers of adults is a poor indicator because by their long-lived nature, the variation in the numbers in their population would be damped out by the fact that adults live through more than one year, and so that they will keep coming back to the lake regardless of what ecological conditions might have been in any given year. With this sort of possibility in mind, I think a better indication of ecological conditions of the lake is to look at something like chick productivity. That is, how many chicks' parents manage to fledge per pair in each given year.

And when we start looking at data that way, the next natural step is to calculate what the expected population growth rates would be for any given level of chick productivity, and Dr. Jehl in his previous testimony referred to a graph which I haven't seen but

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in which he refers to a critical chick productivity of 0.6 chicks per pair as being a chick productivity that would lead to a positive population growth rate; i.e., a very healthy Mono Lake ecosystem. That -- there's no explicit justification in Dr. Jehl's testimony for that, and I don't think such justification exists. It's apparently based on work that I did in the Corey report, and it's extremely unlikely that a production of 0.6 offspring per pair would lead to a positive

growth rate. Even if we were to take it as production of 0.6 offspring per individual, I think you'd have to make very, very liberal assumptions about the survivorship of birds at Mono Lake to produce a population growth rate that's positive.

One last point I'd like to make on the so-called life table analyses and the estimation of population growth rates therefrom is that the variability that we see in chick productivity at Mono Lake, if you look at the record that Dr. Jehl has looked at and Mr. Shuford has looked at from '83 forward or, even more importantly, if you go back to 1979 when my data were first collected, there's been a great deal of variability in the chick productivity at Mono Lake, and it's a well-known principle of population ecology that when you have variability in fecundity, it has a very

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large effect on the population growth rates, specifically small numbers in terms of small years of fecundity have a very disproportional effect on expected population growth rates.

And if we were to convert the data from Mono Lake into an expected long-term population growth rate, those years of low productivity would have a very large effect and would result in a mean that's much lower than a simple arithmetic average that you might draw across the years.

Finally, it becomes important, given any kind of projection of what population growth rate at Mono Lake has been, it becomes important to try to justify or to map those estimated population growth rates on what we actually see in terms of numbers of adults breeding at the lake. And I think that we have enough evidence now from the demographic work that has been done to indicate that the birds at Mono Lake are not a self-contained population, that clearly there are movements of birds to and from the Mono Lake colony, and that it's also very likely that birds are changing the probability in any given year that they will skip breeding. There are probably birds out that there that are deciding not to breed or to breed based on ecological conditions which may be varying from year to

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year and based on cues of which we have no direct knowledge.

Okay. I wanted to move from that one little area to yet another, the debate over the relative suitability of different island habitats for gull nesting. As most of you are aware, in 1981 and '82, I engaged with some colleagues in some physiological measurements in various nesting habitats, and we discovered that gulls nesting in open areas faced substantially higher risks overheating for their chicks than gulls nesting in shaded habitats. Other researchers have followed up this work and found that proximity to water can be another important factor ameliorating the risk of chick overheating. But these authors seldom point out that nesting near water carries risks of its own from flooding and wave action.

Furthermore, it's been suggested in previous testimony that chicks can cool themselves by swimming in the lake, but those claims have neglected to mention that getting to the lake is a very large challenge and that chicks walking to the lake from their native nests, face considerable risk of injury and mortality if they must travel any considerable distance to reach water.

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MR. HERRERA: Mr. Dodge, that's 20 minutes.

MR. DODGE: We would apply for an additional 20 minutes, Mr. Del Piero, and hopefully, we will not need that much time.

HEARING OFFICER DEL PIERO: I hope that's true, Mr. Dodge, because you will have gone well over an hour by that time.

DR. WINKLER: I think I can wrap it up in ten minutes.

MR. DODGE: Thank you. I would point out that we put this panel together --

HEARING OFFICER DEL PIERO: I understand, and I'm granting you the time.

MR. DODGE: Thank you.

DR. WINKLER: It has often been argued on the basis of gull nesting on unvegetated islands elsewhere in their range, that California gulls prefer not to nest in scrub. But these inferences are invalidated because they are based on observations from sites where gulls do not have a choice between vegetative and non-vegetative islands. When given the choice between unvegetated islands and vegetated mainland areas with predators, gulls will always chose islands, and rightly so. High temperatures can be a real threat in certain years, but terrestrial predators are always a threat.

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No number of slides of gulls on vegetated islands can substitute for a scientific study with careful measures of habitat availability, the history of that habitat availability, and the gulls' usage of that habitat.

Now, to a specific habitat question, that having to do with the habitat on Paoha Island, which at first sight appears to offer large areas of available shaded nesting habitat, but has not been used as a significant site of gull nesting since the early 1900s. Even when the island was used by gulls, it appears that their occupation was limited to small areas of lava and the northeast shore in the vicinity of the hot springs on the south shore, and when he nested in those areas, historical accounts indicate that they often nested in and around shrubs. In fact, the historical accounts indicate that they actually sought shrubbery as a source of shade.

The island has been avoided, however, throughout the large expansion of gull populations on the islets just off its western shore during the 1980s. This avoidance of Paoha could be due to many factors but the most likely appear to be that the island has a year-round source of fresh water and a coyote population that is very difficult to eradicate once established. And the second possibility is that away

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from the hot springs and lava areas referred to above, the island soils are composed almost entirely of extremely fine-grained lake bottom sediments that are easily blown around and gulls may be loathe to nest where their chicks would be covered periodically with drifts of this lake bottom dust.

I mentioned that historically we have indications that the gulls on Paoha used shrubbery. I wanted to introduce into the record a few photographs of gulls nesting on Negit Island. There will be four of these, and I don't know how to proceed in terms of numbers. The first of these will be along -- I should just say these all taken by Frasier's (phonetic) photos in 1928 on Negit Island.

The first of these has been Xeroxed, and I have a copy here, but this is an enlargement of the same photograph. And this is a photograph taken from the eastern side of Negit Island on the eastern slope of the minor cone on the island looking to the south and southwest toward the main spine of the Sierra and the Mono Craters and Paoha Island in the background.

MR. DODGE: Did you identify the number of that?

DR. WINKLER: I don't have a number. I don't know what number to give it.

MR. DODGE: It will be National Audubon Society

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Exhibit 231.

HEARING OFFICER DEL PIERO: I want to know how Mr. Frasier (phonetic) got these birds to pose like this.

(Laughter.)

DR. WINKLER: I would point out that there's shrubbery scattered throughout this area. It's not particularly dense shrubbery, but the gulls are certainly standing amidst the shrubbery, and in some of the other photographs, you'll see that they are actually sitting and nesting beneath the shrubbery.

The next photograph is a detailed photograph taken from the vicinity of this rock here. This is -- this one has the notation on it Mono Craters and high Sierras from Negit Island, Mono Lake, California. And this is taken from this area here again looking south and southwest.

MR. DODGE: This is Exhibit 232.

DR. WINKLER: Now, there are two other photographs -- these both bear the legend nesting time gulls on Negit Island, Mono Lake, California. The first of these is still on the eastern side of the lake -- pardon me, the eastern side of Negit Island with a -- two large rocks on the left. This time we're looking north and northeast toward the Bodie Hills that

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are barely visible in the background. Again, scattered shrubbery, gulls in and around the shrubbery.

MR. DODGE: That will be Exhibit 233.

DR. WINKLER: And finally, we have a photograph taken from the same general area again looking north and northeast with the Bodie Hills in the background and several small islets just visible in the lake

beyond Negit Island. I believe that that's Little Norway on the right and just the tip of Little Tahiti sticking up on the left.

And that's number --

MR. DODGE: 234, I believe.

DR. WINKLER: Now, I wanted to point out that especially in 234 you can see several gulls sitting in the shade of shrubbery, and I just wanted to make the point that of all the people you will have heard from in this testimony, I believe I'm the only ornithologist that actually saw the Negit colony in 1976 when it was actually nesting in grease wood, and I can tell you that the density of shrubbery they were nesting in was higher than this and, as several people have pointed out, the shrubs were actually also higher, individual shrubs. And I would be happy to provide some photographs of the colony area at that time if the committee is interested.

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The points I want to make about this is that indeed there are indications that the gulls were nesting in the vicinity of the rather deep shade cast by these shrubs, and that previous testimony indicating that they avoid shrubbery, I think, ignores not only some of this historical evidence, but also evidence of nesting in grease wood areas of similar density to these photographs in Great Salt Lake where I did my thesis. If you consider my thesis and look at a map I provide in that thesis, there's a detailed map of the Morton Salt Plant where I did much of my work on gulls at Great Salt Lake, and those birds were nesting in and around grease wood. And again, I could provide photographs of birds raising young in the shade of these grease wood shrubs.

The final point I want to make is that I agree with Dr. Jehl's previous testimony that much of the shrubbery that birds -- that gulls do nest in association with elsewhere in their range is much shorter and sparser than that present on the plateau at Negit Island. But I would point out that the Mono Lake colony is the southern most and one of the highest gull colonies in this species range, and it may well be that the heat loads imposed by this southern locality and high altitude actually place a premium on shade

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requirements for these birds.

And I'd further point out that I don't see any reason why the height of the shrub should matter to the birds because even shrubs that are shin high, as Dr. Jehl described them, are high enough that gulls can't see over them. And once they can't see over them, they can't see over them, and it doesn't seem to me that it should matter very much how high they are.

To return to the specific issues of habitat and habitat availability at Mono Lake, I think the Draft EIR does not make sufficiently clear the fact that Negit Island is the only historical nesting area on Mono Lake that provides proven shaded habitat in areas large enough to support a large segment of the lake's gull population. Given the very large number of birds

known to nest there in 1976, Negit Island would appear to be the single most important area of habitat to preserve if the lake's gull population is ever to be maintained in a stable manner at or above its previous size.

Preservation of the quality and quantity of gull habitat on Negit Island by maintaining a sufficient water barrier around it urge strongly for at least the 6383.5 lake level or higher.

Finally, I'd like to close with a few comments on

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the food supply that Mono Lake provides to all the birds that feed there. Studies of gull diet choice at Great Salt Lake, Ebert (phonetic) Lake, and South San Francisco Bay, all localities where several prey types are available, indicate that brine shrimp are the least preferred prey taken. Margaret Grubegas' (phonetic) recent studies suggest that shrimp are poor prey for phalaropes as well. Earlier testimony attempting to dismiss Grubegas' (phonetic) work as a laboratory study with little relevance to the real world field situation is misunderstanding at best of her work. Chapter Three of her thesis includes a considerable quantity of field data on true densities and phalarope foraging behavior and her work is a model of the integration of laboratory studies with feed situations.

Many of the arguments -- pardon me. This recent work underlines the importance of considering the distinction between food abundance and food profitability. There may be enormous amounts of food available to birds in principle, but if the food is not sufficiently nutritious and dense for the birds to maintain themselves and fuel reproduction, moult, or migration, then the food source must be seen as being less than adequate.

My dissertation research suggested that the brine

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shrimp food supply for gulls in 1981 and 1982 was considerably less than, quote, super abundant, end quote, and the recent research by Grubegas (phonetic) indicates that fly densities in the field at Mono Lake are associated with foraging at less than 40 percent of their potential foraging efficiency.

Now, many of the arguments in previous testimony as to the health of the Mono Lake ecosystem have been based on what I consider to be uncritical or incomplete analysis, and I would like to just give two examples of how these analyses could be improved if we are to get a better indication of the health of the Mono Lake ecosystem.

The first would be to talk briefly about some of Dr. Melack's work on productivity and its relation to years. And I think we're going to get a little piece of paper here. So I want to emphasize here I'm not going to be specific about details and the data, I just want to get across a general point about the way the data have been analyzed to date.

MR. HERRERA: Could you use the microphone, please?

MR. WINKLER: Yes.

We could put on the vertical axis here any measure of ecosystem health, but let's say it's productivity of some trophic level. And this is year here. And let's say we just had data that produced a trend like this. Now, you stand back there and clearly that indicates a nice hump-shaped function, a very clean fit. But if we do, as analyses so far have done, and fit these with a simple correlation which assumes a straight line relationship through the data, we would get a correlation of zero. A flat horizontal line. Even though, looking at it from standing back, there's a very strong relationship.

And if we happen to have the same sorts of data that show that lake level did this, again, we would see a nice U-shaped function but, again, if we did a correlation, we'd see no correlation. Now, my point is that if you were an economist and asked to look at inflation or -- pardon me, if you were asked to look at some index of gross domestic product or whatever and you started doing your analyses by just looking at the year for gross domestic product, I think you probably wouldn't have a job as an economist very long because clearly there are -- we have theories about how different things affect gross domestic product, things like inflation rate or unemployment rate, what have you. And you would probably want to try to draw correlations between those predictive factors, not between year but predictive factors and the index of the health of the economy.

Likewise, here, if we actually, then, did the correlation between lake level and productivity, we would see a nice positive regression, with a very tight fit, and we would have a very different conclusion about the effects of -- sorry. This is lake level now. And this is, let's say, productivity. So my point is that this sort of analysis based on year is a very uncritical analysis and not very likely to produce any kind of indication of an impact of ecologic conditions at the lake.

MR. DODGE: We would mark that as Exhibit 235.

MR. WINKLER: Okay. One other example is if we look at Dr. Jehl's presentation of data on phalarope masses as an indication of how well phalaropes are doing at Mono Lake, in his Figure Five of his written testimony, he has a bunch of data on phalarope masses with regression lines through them.

MS. GOLDSMITH: Mr. Del Piero, I would like to note for the record my objection. There is no way that I can adequately prepare a cross-examination of this material which is wholly new, has not been provided before. I realize that it's desirable to have Dr. Winkler come up once,but I think this is very unfair.

MR. DODGE: Well, it's the same issue we had before,but I would -- she added this time that it's wholly new. It's not wholly new. It relates to Dr. Jehl's testimony which Ms. Goldsmith helped him

repair.
MS. GOLDSMITH: This is rebuttal testimony.
MR. DODGE: I believe Dr. Jehl is here somewhere. There he is. She's able to prepare for this.
HEARING OFFICER DEL PIERO: The --
MS. GOLDSMITH: Mr. Del Piero, it's my understanding that Dr. Winkler has to be gone this evening which leaves me no time to prepare to cross-examine him.
HEARING OFFICER DEL PIERO: During the course of -- during the course of this process, a number of witnesses have been available and some witnesses have not been available in a timely fashion. Also during the course this process, I granted those parties who were presenting evidence tremendous amounts of latitude, all parties that have presented evidence, a tremendous amount of latitude in terms of introducing as much information in evidence into this process as possible in order to afford the State Board a maximum opportunity and maximum information possible upon which

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to predicate and develop and ultimately adopt a decision on this matter.
I pointed this out before, but it's probably appropriate for me to point this out again. One of the reasons the State Board is not bound by the rules of evidence is expressly so that the State Board has available to it as much information as possible. I'm not particularly interested in seeing a disservice done to your client. I would be particularly concerned in the event that this matter were going to be concluded by the 22nd of December, however, inasmuch as the process has taken a tremendous amount of time, far longer than I think it probably should have taken, even though I've been as accommodating to all parties, particularly the Los Angeles Department of Water and Power in terms of presentation of their case and the time they've taken to cross-examine witnesses, I'm inclined to allow the testimony to be presented today.
In the event that you are not capable of concluding your cross-examination of this witness today, as I indicated yesterday, this hearing is going to go on. I had hoped to be able to get all of the direct testimony taken care of prior to Christmas. If that is, in fact, not possible, then I will make accommodations the second week January and I will

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attempt to do what I can to arrange to have Dr. Winkler return for that purpose.
However, I have to point something out. This is not unlike several situations that have presented themselves during course of this proceeding in which objections were made, both on the record as well as off the record, to the character and nature of the evidence being presented by the Los Angeles Department of Water and Power. I've attempted to be as fair to all parties as possible, and I will continue to do that. All of the counsel for all of the parties have the obligation of doing the very best they can to represent their clients. This information, as well as the information

that is being responded to now that resulted from direct testimony by L.A. Department of Water and Power is not new. This is not a big surprise. I'm not surprised at all that these issues are coming up. And whether Mr. Dodge chooses to put this on as rebuttal or whether he chooses to have it presented in this fashion, at this point, it's up to him because the stage was set during the course of the initial presentation of the case by the Los Angeles Department of Water and Power.

So with that, I'm going to overrule your objection, and I want you to recognize that I'll make

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accommodations for you in terms of pursuing cross-examination of this witness in the event that you are not capable of concluding today.

Proceed.

MS. GOLDSMITH: Thank you.

DR. WINKLER: So if we look at Figure Five of Dr. Jehl's written testimony, we see there are some -- what appear to be regression lines. The details on those are not provided, but I assume they're linear regressions fit through those data and what those are is collections of data points from birds apparently that were collected at the various sites and weighed. And we see that the weights of the birds at all sites appear to increase with date and that none of the points seem to be wildly off range with others.

I would make just two general points, there, however. One is that there's no statistical confidence limit at all indicated for the regression line and using my experience in dealing with statistics, I think that many of these point probably do lie outside the confidence limits of the regression line indicated.

Secondly, these points require some large assumptions that are not substantiated in this work or any other work that I know of by Dr. Jehl. Those assumptions are one, that these relationships of weight

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to date would exist in individual birds followed over time. The assumption when you collect a sample of individuals is that all the individuals in the population are following the same trajectory and weight over time, and I would, especially in this case, like to propose an alternative interpretation which needs to be rejected before we can accept the interpretation that's been offered.

That alternative interpretation is that birds are arriving at Mono Lake and all of these other sites from other sites north of there in the fall migration, and that the differences that we see in the weights of birds collected at different dates are due to differences in the weights at which they left the breeding grounds. And that the indications of actual mass gain have to be substantiated by an indication that the birds actually stayed at the lake at which the birds were collected, that other birds in that population stayed at the lake and increased weight at that site. Without that information we can see that we could interpret these as just indicating that birds in

the North American population from farther north in the source areas are actually increasing weight over the migration season, and they just happen to be arriving at these sites at heavier weights and then leaving very soon thereafter.

This same kind of problem arises when we consider some of the criticisms that have been made of Grubegas' estimates of foraging profitability where it was pointed out that if the birds were food stressed, they wouldn't be sitting around resting on rocks but rather they'd be out there feeding all the time.

What I would suggest as an alternative interpretation, again, I don't say this is true, but it needs to be rejected before we can adopt the interpretation that's been presented. The alternative interpretation is that these birds, as Grubegas suggests, would indeed lose weight trying to forage on the shrimp, especially at Mono Lake, and that they would actually be better off resting, sitting down on a rock and putting their head under their wing, if you will, but resting until the following night when they will leave the lake rather than to sit and try to forage and actually burn up metabolic energy chasing food that does not reward them with a net benefit in terms of weight gain.

Okay. Finally, I would like to just close by saying that I discussed with David Herbst his extensive research on the alkali fly and brine shrimp populations on Mono Lake. It appears very likely that historical alkali fly densities were higher than they have been recently and that fly populations are very likely to increase with increasing lake levels at least up to 6400 feet. Herbst's work further indicates that the productivity flies and shrimp as well as their individual body sizes and at least for flies their fat composition will all increase if lake levels are increased and the lake salinity is decreased. Thus increases in lake level are projected to increase the profitability of avian foraging in Mono Lake by increasing both the density and the food value per individual of the bird's two major prey species.

I believe the Draft EIR should have made a stronger case that invertebrate production appears to have been reduced by past reductions in lake levels, that this reduction could be reversed at least in part by returning the lake to higher levels, and that the foraging profitability for birds at Mono Lake would be increased as a result.

Q BY MR. DODGE: Dr. Winkler, just a couple of cleanup points. You may have testified to this, but these four photos that you referred to, National Audubon Society Exhibits 231 through 234, do you know when those were taken?

A 1928.

Q And in an effort to avoid making multiple and expensive copies of National Audubon Society Exhibit 230, it's been pointed out to me that DFG Exhibit 101

also has a good picture of Twain Islet.
Can you -- referring to DFG Exhibit 101, can you describe where you found the Caspian tern on Twain?
A Well, basically, I would describe it the same way. I would orient the photograph so that it was facing north/south and then once -- would you like me to actually -- I can pencil it in on this one. I mean, I would estimate that it's right here where I'm drawing a circle.
Q All right. Well, hearing no objection from the Department of Fish and Game, I will conform our copies to DFG Exhibit 101.
Dr. Winkler has drawn in pencil his best estimate as to where in 1976 he found the Caspian terns. And that completes the direct examination, and I appreciate your giving me the extra time.
HEARING OFFICER DEL PIERO: Thank you very much. Mr. Dodge.
Ms. Goldsmith? I'm sorry.
MR. MOSKOVITZ: Mr. Hearing Officer, if this is time for cross-examination on behalf of the Department of Water and Power, I will lead off --

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HEARING OFFICER DEL PIERO: Please proceed.
MR. MOSKOVITZ: -- with Dr. Herbst. May I have a moment to arrange some exhibits?
HEARING OFFICER DEL PIERO: Of course.
MR. MOSKOVITZ: During the course of my cross-examination, I will be referring to some additional documents that have not been put into evidence or marked for identification.
HEARING OFFICER DEL PIERO: Do you intend to introduce them as evidence, Sir?
MR. MOSKOVITZ: I may very well introduce them as evidence, but I will be questioning Dr. Herbst with respect to them. Would it be desirable to have them marked before I commence?
HEARING OFFICER DEL PIERO: Actually, no. What I'd prefer you to do, depending upon whether you intend to do them or not, and I assume that's going to be a decision you make during the course of the presentation, depending on whether he's ever seen them before, it would be appropriate during the course of your presentation, we won't discount time from you to ask they be identified individually.
MR. MOSKOVITZ: Yes. And I do have copies for all concerns, so as I go forward, I'll be getting them.
HEARING OFFICER DEL PIERO: That's fine.

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Mr. Moskovitz, we're going to break, just so you know, because I anticipate you going longer than 20 minutes. We're going to break right at 2:15 which ought to be right at the end of your first 20 minute increment just so you can plan. We'll take a ten-minute break then.
MR. MOSKOVITZ: I've just been informed by somebody who's been here throughout and this is my first day up here, that I should be referring to you as Mr. Del Piero rather than Mr. Hearing Officer, and I apologize.

HEARING OFFICER DEL PIERO: You don't have to apologize. That's fine.

MR. DODGE: With all due respect to Mr. Moskovitz, I've been here for a lot longer than that, and I didn't know that.

HEARING OFFICER DEL PIERO: You two graduated from Stanford; is that not correct? Where is Flinn? Where is he?

MR. DODGE: You know what my standard answer to that question is?

HEARING OFFICER DEL PIERO: I know what your standard answer is.

CROSS-EXAMINATION BY MR. MOSKOVITZ

Q Dr. Herbst, we've had some exchanges before in court, have we not?

A BY DR. HERBST: We have indeed.

Q It's nice to see you again.

A It's good to see you, too, Adolph.

Q Dr. Herbst, I first want to go briefly into your relationship with the Mono Lake Committee.

A My relationship with the Mono Lake Committee.

Q And I have some specific questions that I want to put to you about that. Is it true that you've been a member of the Mono Lake Committee since about 1979?

A Yes.

Q And is it also true that you were at that time an undergraduate student at UC Davis?

A No.

Q What were you then?

A I was a graduate student at Oregon State University.

Q In 1979?

A (Witness nods head.)

Q In 1979? I see. Okay.

And have you been an advisor to the Mono Lake Committee throughout the years since then?

A In certain capacities, yes.

Q And did you do research for the Mono Lake Committee?

A I've done research for the Mono Lake Committee.

Q And you've taught classes for the Mono Lake Committee?

A For the Mono Lake Foundation.

Q And is that associated with the Mono Lake Committee?

A Yes. It's the non-profit branch of the Mono Lake Committee. That's right.

Q And you've written articles for the Mono Lake Committee publications?

A That's true.

Q And when you were in Oregon, were you the southern Oregon representative of the Mono Lake Committee?

A I don't believe that's the exact term. I was an Oregon representative, but yes, that's true.

Q And you've done other work in helping the Mono Lake Committee in its activities like filling mail orders on various things; is that right?

A No.

Q You didn't fill mail orders for merchandise?
A Not that I remember.
Q You don't recall telling me that in court a couple of years ago?
A No. You'll have to refresh my memory if I said that.

Q I won't take the time right now.
Is it true that you also have helped the Mono Lake Committee in some work respecting facilities in their office?
A That's true, yes.
Q And is it fair to characterize your relationship with the Mono Lake Committee over the years as one of close and continuing support?
A Yes.
Q I'm going to be using the written summary of testimony of David B. Herbst which is marked as NAS and MLC 1-G for purposes of getting into the various topics that I want to question you about. Do you have a copy available to you?
A Those are my testimony notes.
MR. FLINN: If I gave you my copy, Adolph would have me at a disadvantage, and he already has enough.
HEARING OFFICER DEL PIERO: What are you looking for?
MR. FLINN: His written testimony.
HEARING OFFICER DEL PIERO: Do we have an extra copy of 1-G?
DR. HERBST: I got it.
HEARING OFFICER DEL PIERO: You have it available to you now, Dr. Herbst?

DR. HERBST: Yes, it is.
Q BY MR. MOSKOVITZ: On Page 2, Paragraph 4, you say, and I'll read it to you directly, this is a portion of Paragraph 4, "First, I believe that the Mono Lake ecosystem has been significantly and measurably degraded as a result of the drop of the lake level from pre-diversion levels to current levels. My opinion is based on the effect of the following lake level on alkali flies, brine shrimp, and algae, all which of are discussed in more detail below."
And on Page 3, Paragraph 5, you say, "Second, I believe that any lake level below 6390 will result in long-term degradation of the aquatic productivity of Mono Lake as compared to pre-diversion levels of productivity."
Now, do you agree that the direct effects of changing lake levels from pre-diversion to current conditions relative to alkali flies are the effects of, in your opinion, increasing salinity and the effects of the amount of physical habitat?
A Yes.
Q Now, I want to direct your attention to one of your exhibits that goes along with your testimony and that's Exhibit 64. And when I refer to an exhibit number, unless I otherwise indicate, I mean an NAS/MLC

exhibit.

A Okay. I don't know which one that is because I don't have it with me.
Q It has the -- the heading of Mono Basin EIR aquatic productivity evaluation of models experiments and new data and has your name and address at the top.
MR. FLINN: For the record, those are Dr. Herbst's Draft EIR comments. Do you have a copy?
DR. HERBST: Those are the Draft EIR comments? That sounds to me like the auxiliary report.
HEARING OFFICER DEL PIERO: Dr. Herbst, it's got an August 20th, 1993, date that again, considerable time and effort have gone into completing the Mono Basin EIR?
DR. HERBST: Okay. Got ya.
MR. FLINN: Do you have a copy?
DR. HERBST: Yeah.
MR. MOSKOVITZ: If you could get a copy that would be helpful.
Q BY MR. MOSKOVITZ: You did identify that document on Page 1 of your testimony Exhibit 1-G at the bottom of the page?
A Okay.
Q Now, I want to refer to you Page 6 of that exhibit.

A Um-hum.
Q And that page shows a number of graphs, does it not?
A That's true, yes.
Q Let's look at the two middle graphs.
A Okay.
Q Now, those two graphs, one is entitled Kimmerer-Herbst model habitat area times salinity, and the other Jones and Stokes model habitat area and salinity. Now, those two graphs basically are bell shaped; that is, the curves on those graphs are basically bell shaped?
A That's correct.
Q And they reflect the direct effects of salinity and habitat on flies according to the two models identified; is that not so?
A According to the two models, that's correct.
Q According to the two models. Now, one of those models is a model that you participated in preparing, I gather, the left-hand one, and the right-hand one is the Jones and Stokes revision of that model that they used for the Draft EIR?
A That's also right.
Q Now, first, let's look at the left hand of those two graphs, the one depicting the Kimmerer-Herbst

model. And what I want to do is test out what you said in the quotations from your direct testimony, the summary of your testimony that I read to you earlier about the comparison between pre-diversion conditions and current conditions.
A Okay.
Q And would you agree that pre-diversion conditions are characterized by the 6415 elevation in that left-hand model?

A According to that model.
Q Yes. And that present conditions or current conditions would be characterized roughly by the 6375 --
A That's correct.
Q -- elevation? Now, what does that curve show to be the relationship between the 6415 condition, that is the far left, and the condition with the elevation 6375?
A It shows that the percent of what you'd expect the productivity to be would be lower at 6415 than at 6375.
Q Does that support what you said earlier about the reduction from pre-diversion conditions to current?
A No, it doesn't. But --
Q All right.
A -- the conditions that I specified went into this

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model, as I explained earlier, I believe were conservative in the sense that they underestimated the beneficial effects that would occur at high lake levels and the adverse effects that would occur at low lake levels because of, Number One, the lack of incorporation of submerged vegetation as alternative latoral habitat for the attachment of their larval and pupal stages and, Number Two, and most importantly, it underestimates vastly the beneficial effects of low salinity at high lake levels which were dramatically demonstrated in the microcosm experiments that I also discussed in my direct testimony.
Q We'll get to those qualifications in a while.
A Okay.
Q Now, looking at the right-hand model, the Jones and Stokes model, does it show any deteriorating or declining relationship or effect at current lake levels as compared to pre-diversion lake levels?
A I'm sorry. Would you repeat that?
Q Does the right-hand graph, which --
A Um-hum.
Q -- depicts the Jones and Stokes model show that there is a decline from pre-diversion levels to current levels?
A It shows that they're about the same by that

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model.
Q I want to refer you to the two bottom graphs on that same page, and those graphs, as I understand it, depict the amount of total literal zone habitat area in the left-hand graph and literal zone hard substrate habitat area in the right-hand graph. Is that so?
A That's correct.
Q And what do -- what does the left-hand graph show as the relationship between the total literal zone habitat area pre-diversion at elevation 6415 as compared with 6375?
A There's a -- they're about the same. There's a plateau that's between 6400 and 6380 where habitat is at its maximum and it declines on either side, either at higher lake elevations or lower lake elevations.
Q But comparing pre-diversion to current, there's no decline, is there?

A That's right.
Q And that's also true, or is it true even more so with respect to the total -- to the literal zone hard substrate habitat area on the right-hand graph?
A That's correct.
Q That is, the conditions are better at current than they were historically?
A That's correct.

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Q Now, you mentioned that an offsetting factor in your opinion is that at higher lake levels, there would be additional habitat area created by submerged vegetation; is that correct?
A That's correct.
Q Now, I think that Mr. Del Piero asked you about would vegetation tend to deteriorate after being submerged with rising lake levels, and you said it would last for a while, maybe up to ten years, was your opinion. After that time, the vegetation would no longer afford substrate for flies, would it?
A That's true.
Q So if you assume that the lake is going to increase in elevation from its current elevation, for a while there would be some vegetation that would be available at substrate, but then in the longer term, it would no longer be available, right?
A Only if you assume -- no, only if you assume that the lake level remains constant. The lake level in any given year is dynamic, even within a single year, the lake level drops in the summer and rises in the spring with fluctuations in the runoff cycle and the evaporation cycle. So in any given year during a period that's, quote unquote, called stable, you have elevations going up and down. You have a dynamic lake

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level. And during that time, you also have cycles of colonization and of recolonization and inundation of vegetation right along the shoreline. This will especially be true at high lake levels where there's going to be much more fresh water seepage along that shoreline zone and there's going to be much more vegetation that's going to be growing along that shoreline zone.
Q You began to get into a subject that I wanted to inquire into. It is the fluctuation of the lake level that you visualize as creating a continuing supply; that is, as the lake level goes down, terrestrial vegetation along the shore can grow.
A Um-hum.
Q And if the lake fluctuates up, that will be submerged and be available as habitat.
A That's right.
Q Now, would that not been the case at any range -- at any lake level around which you have a range of fluctuations?
A That's true.
Q Do you have any information, that is, any measurements, any other hard data, to indicate how much substrate from submerged vegetation you would have at any lake level?

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A You mean in relative areas?
Q Yes. Areas.
A Yeah, I do, as a matter of fact. There's a paper I published -- in fact, two papers. One which was published in Hydrobiologia in 1988, and another which was published in a symposium proceedings of -- a symposium at White Mountain. I believe the publication date for that would be last year. And in that, I document the different densities of flies that occur on vegetation in addition to rocky substrate and compare those two. The rocky substrate densities are just lightly higher than you would find on the vegetation, but nonetheless, vegetation ranks second above all other substrate. And in the first paper that I mentioned, I describe in that the proportion of sites sampled in which vegetation was present. For actual areas of vegetation present, I haven't personally published any of that data, but from what I understand in discussions of this with some of the consultants with Jones and Stokes, there is information on the amount of vegetation that's in and around the lake, both that could be inundated and that's around the lake.
And in addition, one more source of information, I believe, is data on the distribution of vegetation in
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studies that were done by Paul Little, Stewart Robert, and Tim Bradley. I believe they also document the distribution of vegetation on that paper.
One more thing. There's also a page in the National Academy of Sciences' publication that describes the presence of submerged mats of attached pupae in Mono Lake and the distribution of them at a couple of different locations and describes them as being widespread high density mats of pupae attached to the submerged vegetation. And that would have been at a time when the lake level was at a maximum, so there would have been a lot of submerged vegetation during that period of time, but those were studies that were done, the bathometric studies done by --
Q Paul Lagoes (phonetic)?
A Paul Lagoes (phonetic), yeah.
Q Now, is there any information to indicate that there would be greater areas of submerged vegetation due to fluctuation at pre-diversion lake levels as compared to current lake levels?
A Not that I'm aware of, except that during this period of time, as the lake levels -- or during the past 50 years, as the lake levels have been going down, there's been no vegetation to be inundated. So when perfectly -- there's nothing to be submerged, whereas
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during rising lake phases, there is vegetation to be submerged.
Q But the fact is that once you reach stability at any lake level the fluctuation around that lake level would have the same general effect of making substrate available in vegetation as a lake rises after a fall, and you don't have any information as to the fact that

there would be more of that vegetation available at a higher lake level than at a lower lake level. Isn't that right?
A I personally don't have that information.
Q All right. And you can't quote any such information from any our source, can you?
A Not that I'm aware of.
Q All right.
MR. FLINN: I don't know what the rules are with regard to our hybrid panel here, but I don't know if the rules allow any other panel member who has something to say can volunteer or not. I don't know.
HEARING OFFICER DEL PIERO: If there are other individuals who can lend information in regards to this, they're afforded the same opportunity as past panels that were presented by the Los Angeles Department of Water and Power and also the panels presented in the last couple of days. If any of you

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have information that bears on the question being asked and the person to who the question is directed is incapable of answering because he or should does not that have information and others of you do, you're fully requested by me to respond so that we can get as full an evidentiary record as possible.
DR. STINE: I would like to respond in that case, if that's okay. A couple of points here. First of all, there's 900-year-old vegetation out there in many places around the lake and that 900-year-old vegetation has been providing substrate as long as it's been under water for almost a millenium now, so there's pretty good evidence that at least in some cases, and I would say that it's fairly widespread, old vegetation, very old vegetation, continues to provide hard substrate. And if I could refer for a second to Exhibit 142. On 142, it's obvious --
MR. BIRMINGHAM: Excuse me, Mr. Del Piero, I'm now sitting in the back as a member of the audience, and I wonder if Dr. Stine could be afforded leave of having to carry the microphone --
HEARING OFFICER DEL PIERO: You know, I can't satisfy any of you people. Half of you want him to talk in the mike and half of you don't. I don't understand.

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DR. STINE: On Exhibit 142 I think it's very clear that until you get up to a lake level of approximately 6390 feet, you're doing very little inundation of vegetation because at these lower lake levels like this, you're encountering greater salinities and more and more alkali around most of the lake. So as the lake goes up and fluctuates within a high level, it's going to tend to inundate far more vegetation per foot rise than it will at the lower lake levels, and I think it's quite apparent on here on the photographs as it is on the ground when you're out there that there's precious little vegetation around most of the lake at these low lake levels.
Q BY MR. MOSKOVITZ: Dr. Herbst, a little while ago, you cited some information as to what the -- what the

proportion of density of flies on vegetation substrate was compared to hard substrate. You made some sort of percentage. What did you say?
A BY DR. HERBST: I don't remember exactly that the percentage is. It's something like -- it's between 50 to 75 percent. Something like that.
Q Isn't it just 50 percent?
A No.
Q It's more?
A Something like that. Yeah. 50 to 75 percent.

Q Is it 50 to 75 or is it 50, which is it?
A Have you got some data you can show me?
MR. FLINN: Objection. Asked and answered.
DR. HERBST: I think it's between 50 to 75 percent.
Q BY MR. MOSKOVITZ: Do you have a copy of the auxiliary report that you wrote for Jones and Stokes? Number 8? I think it's referred to in your direct testimony --
A BY DR. HERBST: Yep. Got it.
Q -- on Page 1.
MR. HERRERA: Mr. Moskovitz, your time has elapsed.
MR. MOSKOVITZ: Mr. Del Piero, I'll requesting an additional period of time.
HEARING OFFICER DEL PIERO: I'll be happy to grant you an additional 20 minutes after we break.
MR. MOSKOVITZ: Thank you.
(Whereupon a short recess was taken.)
HEARING OFFICER DEL PIERO: Ladies and Gentlemen, this hearing will again come to order.
Mr. Moskovitz, you can begin your second 20-minute increment.
MR. MOSKOVITZ: Thank you.
Q BY MR. MOSKOVITZ: Dr. Stine, I want to follow up on

the information you gave in aid of Dr. Herbst. You mentioned that you had found very, very old trees that were still intact and could be or were substrate for flies. Is that what you were testifying to?
A BY DR. STINE: That's correct.
Q Did you see flies on them?
A Yes, I have seen flies on them. I've seen Tufa, I think, forming on them as well immediately off shore in 1982, right off the Lee Vining Creek delta.
Q And what was the elevation of the lake at that time?
A Very close to its low stand, approximately 6372 point -- perhaps four feet? Something like that.
Q And what was the elevation of the trees you found?
A It was in approximately one foot or so of water. This is a rooted stump now, though. This in a tree stump. That is shrub stump.
Q And so this was found at an elevation that would be inundated at current lake levels?
A That's correct.
Q And so it would be available at current lake levels?
A Yes. Although, many of them -- this is now one

stump. Most of the stumps that I've seen have been above the lake and they lie between approximately 6372

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to 6371 feet on the low side up to about 6401 feet.
Q And do you have any information as to how extensive those trees are?
A Well, I've found -- I thought I had found them all, and then Dave Carl of the State Park system and I were out the other day, and I found another one. But there are probably, roughly, 100 stumps that I've now found that are protruding above ground enough to be conspicuous, enough to be obvious.
Q A very, very small area compared with the area of hard substrate, right?
A That's true. And I was not -- I was not trying to make the point that this constitutes some monumental amount of hard substrate. The question arose as to how long vegetation would persist, and I brought up these stumps simply to speak to that point, that it is something that simply lasts a short period of time, which I believe was the implication that was perhaps left, that this is something that can persist for a much longer period of time.
Q And these are tree stumps, right?
A These are tree stumps, though I have found in the record in stream cuts grass mats as well and things that I've described in the literature where I've described these things, as graminoid vegetation where

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we can still find the shoots of grass in the record as well. And those likewise dated -- I forget if the date was 600 years old or 900 years old, but in any case, they were centuries old.
Q And are you saying that those are substrate for flies?
A I wouldn't say that, but that wasn't the question. The question was how long will vegetation persist, and that's what I was -- was addressing.
Q I see. Dr. Herbst, the kind of vegetation that you were referring to that could provide the substrate for flies is basically grasses of some kind, salt grass; in that so?
A Anything will work.
Q Anything will work?
A You could take some kind of artificial material as well, something like fishing line or anything that is a stringy kind of substance that plant tissue is made of, let's say, and fly pupae would indeed attach to that.
Q And what you were referring to and what your exhibits, those two pictures, depicted, Exhibits --
MR. SMITH: 49 and 50.
Q BY MR. MOSKOVITZ: -- 49 and 50 was basically salt grass; in that right?
A BY DR. HERBST: Those are salt grass, that's right.

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However, in the sense that I've referring to inundation of latoral -- inundation of terrestrial vegetation and latoral as alternative habitat, it doesn't have to be just grasses. And at these high lake elevations as you get into more arbuscular vegetation, more of this

brushy vegetation along the shore, that would provide more surface area and a better place for attachment.
In fact, to elaborate a bit on what Scott was talking about, here is a phenomenon whereby much of the wood substrate that's in the lake could become rock substrate by a process that's in some ways akin to how petrified wood forms. Are you familiar with the phenomenon of Tufa formation from gaylussite?
Q I'm asking questions and you're answering them, so don't ask me questions.
A Okay. Well, I was just going to elaborate on this particular process. There is a mineral called gaylussite that forms on any kind of substrate that are in the lake, whether it be vegetation or rock substrate or beer cans that are on the lake bottom, and as it forms, it transforms into Tufa. And so a lot of that vegetation that might otherwise decompose after being submerged at those higher lake elevations, could well, itself, be transformed into a rocky substrate. And I've got many examples in my laboratory, of that very

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kind of that transformation of woody material into rock substrate.
Q Do you have any information as to how much that is and as to what extent it would actually be available?
A I don't have it, but Jones and Stokes Associates have done extensive mapping of the upland vegetation.
Q I asked whether you knew about it?
A I do know about it, and I have it.
Q Do you have any information as to the quantity?
A Not the off top of my head.
Q Dr. Herbst, I show you a picture taken -- that is in the Department of Fish and Game Exhibit 99 on Page 9. It's an article from Condor Magazine --
A It just so happens I have a copy of it right here.
Q Very good.
Mr. Del Piero, do you have one available?
HEARING OFFICER DEL PIERO: I've got several copies of it. I think I have one autographed by the original --
Q BY MR. MOSKOVITZ: Do you have idea what lake elevation that was taken at?
A BY DR. HERBST: I think this was taken in 1908; is that correct?
Q 1902.
A 1902? I believe it's somewhere in the vicinity of

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6410 to 20, although I can't put my finger exactly on what it it would be.
Q Now, does this show the vegetation you had in mind?
A No, it doesn't.
Q Doesn't show any vegetation?
A No.
Q Now, getting back to a question that was pending for you, and that concerns the relationship between the available habitat on submerged vegetation and on hard substrate. Do you have a copy of your auxiliary report available to you?
A Yes.

Q All right. Would you look at Page 13?
A Okay.
Q And would you -- let me read to you what I want to call your attention to. In the second paragraph, before the numbered items, it says, "The information that is yet to be incorporated," and that's in the -- in the model, "includes," and then you go down to 2-B, "addition to the area of the vegetation zones inundated with rising lake level to account for new habitat that becomes available as substrate or the attachment of pupae and sediment stabilization," citing Jones and Stokes Associates, and it says, "Densities on submerged

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vegetation habitat are about 50 percent of those on rock substrate habitat," Herbst 1990.
A That's correct.
Q Does that refresh your recollection as to what the proper percentage is?
A It does, but, you know, if you look at the actual data, I think the numbers really are closer to 75 percent. I think I just used that as a way of being conservative to try to evaluate some of these extra factors. It's probably always best to err on the side of being conservative, and so I think I used 50 percent rather than 75.
Q You mean it was closer to 75 but you used 50 percent?
A That's right.
Q Is that what a scientist is supposed to do?
A Absolutely. If you're going to be doing things where you're adding new elements to models and you have the opportunity to err on the conservative side rather than overestimate particular factors, that would be the recommended procedure to follow.
Q Even if it's closer to 75 percent?
A That's right.
Q Would you look at Exhibit 64 again and --
A What was that?

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Q That is -- that's your comments on the Draft EIR.
A Okay.
Q And look at Page 5. I want to direct your attention to the graph on the upper left.
A Okay.
Q It talks about alkali fly growth and development. The horizontal axis talks about salinity, and that's, I guess, in grams per liter; is that right?
A That's correct.
Q What is the lake elevation at which you'd find 50 grams per liter that is on the far left of that axis?
A 6415.
Q So that's pre-diversion conditions, essentially?
A That's correct.
Q And and what is the lake level at which you find 100?
A 6373.
Q So current conditions would be a little to the left of the hundred mark?
A That's correct.
Q Now, what does that graph show as to the

relationship in pupa size between pre-diversion conditions and present conditions?
A It shows that there is a decrease from about 40 percent of the body size.

Q 40 percent. Now, what data is that graph based upon?
A I think that particular graph is based on laboratory studies.
Q So-called microcosm studies?
A No. The microcosm studies was a separate data set.
Q What studies, then, is it based on?
A Laboratory studies.
Q That you did?
A That I did.
Q And are those studies reported anywhere?
A Yes. They're published in that White Mountain Symposium volume I referred to earlier.
Q That's a journal of some kind?
A It's a referee publication put out by the White Mountain Research Station every, I think it's two, three years, and they hold a symposium there. And I believe it was two years ago in the fall they held a symposium there on the history of and ecology of water issues in the eastern Sierra, and it was at that symposium that I presented a paper. And they always published the series of papers that result from that symposium, and that's where it appears.
Q Do you have a copy of it here?

A No.
Q And the information that is -- the numbers or the description of the experiment, you don't have any place in the material that you assembled for this hearing; is that right?
A No, I didn't. The graphs that you see here that you're referring to in my comments are, as it says on the previous page, a way of summarizing basically everything I've done, or nearly everything I've done to date so that we have a way of looking at that all in terms of percentage of the maximum response variable, and that's why you see all these things in terms of percent like we talked about with the nitrogen fixation work, so that it would be easier to compare one value to another in terms of the percent change.
So this particular experiment represents laboratory work that I did that once again is published in this other symposium volume.
Q Are you aware of any other laboratory experiments on this subject; that is, the effect of salinity on the size of alkali fly pupa?
A Yeah. There is data from the microcosm experiments as well.
Q Was that subject covered in your Ph.D. dissertation?

A No. The microcosm experiments were only done in 1991.
Q No. Did you have some studies of the relationship

between salinity and pupa size?
A Yes.
Q And what did those studies show?
A Those studies showed basically the same results.
HEARING OFFICER DEL PIERO: While you're away from the microphone, Mr. Moskovitz, Ladies and Gentlemen, let me -- we're going to break about -- between 4:30 and five for about 15 minutes, and then we're going to call it a day at seven o'clock because Mr. Dodge is yawning. Okay?
MR. MOSKOVITZ: You said we're going to break at about --
HEARING OFFICER DEL PIERO: We'll break between 4:30 and five for about 15 minutes, and then we'll call it a day around seven o'clock.
MR. DODGE: With the understanding that Dr. Winkler can be on his airplane.
HEARING OFFICER DEL PIERO: That's why I'm breaking at seven o'clock because it doesn't behoove you or me to be here if Dr. Winkler's gone and, obviously, Mr. Moskovitz or his firm is interested in cross-examining him, so he may have to come back at a

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later date if they aren't successful in completing the process in the next four hours.
Additionally, today is the last day of Hanukkah. There are some people that might want to go home and be with their families.
MR. DODGE: We obviously have no objection to Dr. Winkler testifying for as many days as the Hearing Board wants to hear him. I just don't want to spend the money to bring him back because we don't have it.
HEARING OFFICER DEL PIERO: Are you going to keep him until tomorrow?
MR. DODGE: No. I just don't want him to make a second trip. I'd like the examination of him to be completed in this session. It's my fervent hope that I don't have to bring him back in the rebuttal case.
HEARING OFFICER DEL PIERO: You indicated he's leaving at seven o'clock; is that correct?
MR. DODGE: He can stay 'til eight.
HEARING OFFICER DEL PIERO: Let's see how it goes.
MR. DODGE: If push comes to shove, we can ask all parties to cross-examine Dr. Winkler and the others are here.
HEARING OFFICER DEL PIERO: That might be -- let's take a look at that around four-ish and see how it

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goes.
I'm sorry, Mr. Moskovitz. Proceed.
MR. HERRERA: You have five minutes remaining.
MR. MOSKOVITZ: I'm told there are five minutes remaining and --
HEARING OFFICER DEL PIERO: You're welcome to take -- if you wish additional time, you can make a request at the time.
MR. MOSKOVITZ: I have an exhibit that I'd like to have marked. Shall I give some copies to --
HEARING OFFICER DEL PIERO: A copy to the Staff

and a copy to the other attorneys.
Dr. Winkler, where does he not want to bring you back from?
DR. WINKLER: It's Ithaca, New York.
MR. DODGE: It's not that I don't want to bring Dr. Winkler back. It's that I don't want my clients to have to spend the money to bring Mr. Winkler back.
HEARING OFFICER DEL PIERO: Dr. Winkler, have you ever noticed that Mr. Dodge tends to take bait very easily?
(Laughter.)
MR. MOSKOVITZ: Mr. Del Piero, I've had distributed to the witness and to others and to you, I gather, a one-sheet exhibit entitled Table 5.1 Salinity

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Effects on Size at Maturity of Athedrahine (phonetic), and I don't have a number for it yet. I'm waiting for it.
HEARING OFFICER DEL PIERO: Can we have an exhibit number?
MR. SMITH: Momentarily. Actually, Tom has them. He has my records. He's making notes on it. Proceed. We'll get the number.
HEARING OFFICER DEL PIERO: Mr. Birmingham, we need those numbers.
MR. MOSKOVITZ: Shall I proceed before we have the number?
HEARING OFFICER DEL PIERO: Go ahead.
Q BY MR. MOSKOVITZ: Dr. Herbst, a moment ago you told me that your Ph.D. dissertation study came to the same conclusion that there would be a 40 percent drop in the size between pre-diversion and current conditions; is that right?
A BY DR. HERBST: That's right.
Q Would you please take a look that the Table 5-1 and tell me whether that comes from your -- from your Ph.D. dissertation?
A It does come from my Ph.D. dissertation.
Q All right. Would you look at the lower part of that graph headed -- I mean, that table headed Mono

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Lake Larvae?
A Um-hum.
Q And would you read the size in millimeters, that's the width, at 50 grams per liter?
A 1.81.
Q And would you read the size at 100?
A 1.82.
Q Does that show a 40 percent drop?
A No, it doesn't. And -- I recalled wrong. You're right. You're right.
Q All right. And would you look -- would you look at the next --
MR. FLINN: Mr. Del Piero, before we go on, I don't know if the witness wanted to explain his answer. I don't know whether the Hearing Officer wants an explanation or not, but the witness clearly had something more he wanted to say.
DR. HERBST: Yeah. Yeah.
MR. MOSKOVITZ: Go right ahead.

DR. HERBST: This particular kind of experiment is one I have done many times over. I suppose, like many graduate students, I shouldn't be ashamed to say that there's certain experiments that I've done that I no longer trust the results of and this happens to be one of them. I don't know if we talked about this before

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when I testified in Judge Finney's court, but this particular experiment was done with individuals reared individually, and one of the things that happens when you rear individuals of the alkali fly is that there is fungal growth that occurs and culture stagnation that occurs under low salinity conditions. And if you rear animals together where they have a chance to graze algae off each other and graze fungus off each other, you don't get the kind of mortality and repeated development that you see in these cultures right here.

So any of the work that I've published subsequent to the work that I did for my dissertation, I corrected this experimental problem in, so I was really looking at a salinity effect rather than a fungal contamination effect, which is what this experiment reflects by this absence of a body size effect.

HEARING OFFICER DEL PIERO: Mr. Moskovitz, excuse me for one second.

Dr. Herbst, what year was your dissertation completed?

DR. HERBST: 1986.

HEARING OFFICER DEL PIERO: Please proceed, Sir.

Q BY MR. MOSKOVITZ: Looking at the second -- the third column headed Adult Mass, does that compare the sizes

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of adult flies as contrasted with pupa size in the second column?

A BY DR. HERBST: That's correct.

Q And what does it show for Mono Lake larvae at 50 grams per liter?

A 1.023.

Q And what does it show at 100?

A 1.327.

Q And does that show a 40 percent decline?

A No, it does not.

Q It shows an increase in size, doesn't it?

A That's right.

Q And do you want to say something about that?

A Well, I account for it in the very same way. These are all from the same experiment. When you have that kind of a bias in an experiment, you can't really trust if it's the salinity effect that you're looking at. And, indeed, in experiments where these things are reared in groups where you don't have that fungal contamination problem, you don't see these results.

Q So this particular table, 5.1, is in error?

A That's correct.

Q Were you aware of it when it was published?

A It was not published.

Q When it was submitted?

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A Yeah.

Q And is there a note to that effect?
A I think there's somewhere in the text of my dissertation where I do discuss that. That's right.
Q And identify this particular exhibit as being questionable?
A I don't know if I identify this particular exhibit, but I do discuss that effect.
Q There's another document I want to distribute.
MR. SMITH: Mr. Moskovitz, the first table, first thing you distributed, that's L.A. DWP 99 and the next one you are going distribute will be 100.
MR. MOSKOVITZ: Thank you.
May I proceed?
HEARING OFFICER DEL PIERO: Certainly, Sir.
Q BY MR. MOSKOVITZ: Dr. Herbst, do you have a copy of L.A. DWP Exhibit 100?
A BY DR. HERBST: I guess if that's what this is, salinity bioassays.
Q Yes.
A Yes.
Q Have you ever seen this document before?
A I think Dr. Bradley (phonetic) may have sent me a copy of this before. I don't recall looking at this in great detail.

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Q That exhibit, L.A. DWP 100, is referred to, in it, in your report with Dr. Bradley (phonetic) called An Analysis of the Growth and Survival of Larvae of the Alkali Fly on Munal (phonetic) Algal Culture?
A Um-hum.
Q You cite it, don't you?
A Dr. Bradley (phonetic) is the first author on that paper.
Q But you're familiar with it, you helped to write it?
A Of course.
MR. FLINN: That question was ambiguous. The "it," I don't know whether you're familiar with "it." Whether the "it" was Exhibit 100 or the "it" was the paper Dr. Bradley (phonetic) co-authored.
HEARING OFFICER DEL PIERO: Excuse me, Mr. Flinn. If you want to object, I'll sustain your objection. But it's Mr. Moskovitz' prerogative to rephrase the question.
MR. FLINN: I'm sorry.
Q BY MR. MOSKOVITZ: Dr. Herbst, you're familiar with L.A. DWP 100, aren't you?
A Yes, I am.
Q Would you look at Page 3?
A Okay.

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Q And the next to the last paragraph. The first sentence reads, "All the parameters measured in life stages after the larval-pupal mote were unaffected by the salinity of the larva in the rearing medium." Do you see that?
A Yes.
Q Now, does that summarize the results of experiments by Dr. Bradley (phonetic) made very recently regarding the relationship between salinity

and size of adults?
MR. FLINN: I'm going to object to that on the grounds of lack of foundation. Obviously, the witness is as capable as anyone of reading the document, but unless Dr. Herbst was actually there doing the experiments, all he could know is what Dr. Bradley (phonetic) may not have told him.
HEARING OFFICER DEL PIERO: Mr. Moskovitz?
MR. MOSKOVITZ: He said he's familiar with the paper, it seems to me that as a scientist, he could answer the question.
MR. FLINN: I agree with the paper, but the question didn't go what did the paper say? The question was are these results of experiments concluded at a particular time? And again, unless he was there or had some knowledge of it, he would only be

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guessing.
HEARING OFFICER DEL PIERO: Ms. Anglin, would you be kind enough to read the question back for me?
(Whereupon the record was read as requested.)
DR. HERBST: That summarizes --
HEARING OFFICER DEL PIERO: Wait. Wait. Wait.
DR. HERBST: I'm sorry.
HEARING OFFICER DEL PIERO: I'm going to sustain the objection. You can get to where you want to go, Mr. Moskovitz, just restate the question. Okay?
Q BY MR. MOSKOVITZ: Are you aware that Dr. Bradley (phonetic) made studies or experiments regarding the relationship between salinity and the size of adult flies?
A BY DR. HERBST: I am.
Q And was not the result of those experiments that he found no effect on the size of adult flies as a result of increasing salinity?
A That would have been his conclusion, but I do differ with Tim's opinion on this. If you'd like me to, I can point out a couple of figures to you in the text where the results are fairly ambiguous. Would you like me to do that?
Q You agree that Dr. Bradley (phonetic) did come to the conclusion that there was no difference?

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A That's right.
Q And you disagree with him?
A I disagree with that.
Q You worked extensively with Dr. Bradley (phonetic); is that so?
A I do.
Q And you respect him as a scientist?
A Absolutely.
Q I want to turn now to what you said in your summary regarding your microcosm experiments. Looking at Page 8, the bottom of Page 8, the top of Page 9, you refer to Exhibit 52, that's Paragraph 20. And then you say, "These data illustrate the above points by showing that the overall number and individual size and fat content of flies are vastly greater at lower salinities." The bottom of Page 8, top of Page 9; is that right?

Q Um-hum.
A Yes.
Q Now, the data mentioned here, do they include the microcosm experiments?
A They are the microcosm.
Q They are the microcosm. Very good. Now, are the results of the microcosm experiments shown in your Exhibit 52?

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MR. FLINN: To revisit the record, Exhibit 52-A is a compendium and a clear version of Exhibit 52.
DR. HERBST: That's correct.
Q BY MR. MOSKOVITZ: And are the results also shown in Exhibit 64? That, again, is the comments you made on the Draft EIR? Page 5? In the middle of the page? Right-hand graph? The one that's entitled Adult Fly Body Size, Field Microcosms?
A That's right.
Q Now, on what data do you base that graph on Page 5 of Exhibit 64?
A If you look in auxiliary report Number Eight, I'm not sure if that has an exhibit number attached to it.
Q It does not, but you did refer to it in your testimony.
A If you look at the graph on Figure 33?
Q Figure 33. All right. Let's take a look at Figure 33 of your auxiliary report.
Do you have a copy, Mr. Del Piero?
HEARING OFFICER DEL PIERO: No, I don't.
MR. MOSKOVITZ: I did not make copies of that because I felt it was in the records, in the Board's exhibits, is it not?
HEARING OFFICER DEL PIERO: The auxiliary --
MR. CANADAY: It's a Staff exhibit.

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HEARING OFFICER DEL PIERO: It's a Staff exhibit.
MR. FRINK: Yes, it is a Staff exhibit. We don't have all the exhibits here right now, but we can get one momentarily.
MR. MOSKOVITZ: I think it would be helpful if --
MR. FRINK: I will give the number of it in a minute, too, when we get it down here.
MR. MOSKOVITZ: Perhaps I can continue with the question --
HEARING OFFICER DEL PIERO: Why don't you go ahead with the question, Mr. Moskovitz, and we'll try and catch up when Mr. Canaday returns?
Were we able to get a number for that additional exhibit?
MR. FRINK: We will have a number in a minute. The auxiliary report?
HEARING OFFICER DEL PIERO: Yes. No. Not the auxiliary report.
MR. SMITH: The two submittals? They are 99 and 100.
HEARING OFFICER DEL PIERO: Okay. Fine.
Q BY MR. MOSKOVITZ: Dr. Herbst, looking at Figure 33 in that auxiliary report, it's entitled Adult Body Size and Salinity, Microcosm Experiments, right?
A BY DR. HERBST: That's right.

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Q And in the vertical axis it shows body length in millimeters, right?
A Yes.
Q And in the horizontal axis it shows salinity level, and it shows 50, 75, 100, and 125 grams per liter, right?
A That's right.
Q Now, looking at that -- at that graph, what does it show to be the body length at 50 grams per liter?
A 4.73 approximately.
Q I would say 4.72 if you scale it out. Now, what does it show the body length to be at 100 grams per liter?
A Oh, I guess I'd eyeball that at about 4.41.
Q And what would it be at the current salinity?
A The current salinity conditions?
Q Yes.
A Body size is -- it varies seasonally.
Q On the graph, where would it fall?
A 4.41.
Q That's for 100?
A That's right.
Q And the current salinity is not 100, is it?
A Well, close to it.
Q Close to it. For purposes of our discussion.
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A Yes.
Q What is the difference in percentage?
A It's 40 percent because it's scaled to the minimum body size of flies in the field. For example, if you were to take this data and just on an absolute scale, calculate what the percent of reduction is, you could say that a fly that has 100 percent reduction body size would be zero, and obviously, a fly that measures zero millimeters in length doesn't exist. So what you need to do is scale any kind of estimates to changes in body size to that minimum body size observed in nature or in laboratory experiments below which it's impossible for a fly to emerge and survive and live. So it's scaled to the minimum body size observed in nature in the field, which is 3.75 millimeters.
Q Looking at this graph, don't you compare the size 4.73 at 50 grams per liter with 4. -- approximately four at 100? Don't you compare those two sizes?
A Scaled to --
MR. FLINN: I do have to object. "This graph" is ambiguous whether you're talking about this graph in the auxiliary report.
MR. MOSKOVITZ: It's the one I've been examining on, Figure 33.
MR. FLINN: With that statement, I withdraw the
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objection.
HEARING OFFICER DEL PIERO: Please answer the question.
DR. HERBST: Yes. You do compare those two values, but rather than using zero as a baseline, because a fly that measures zero millimeters doesn't exist, you scale it to that minimum body size that

exists in nature. You scale it to the minimum body size that is possible for a fly to achieve before it can no longer get any smaller, it's effectively dead. So you use that as your baseline value for comparison.
Q BY MR. MOSKOVITZ: So you're saying that the difference between 4.72 and 4.4 is 40 percent?
A About a 40 percent reduction.
Q That's not what I get when I make the calculations.
A Do you understand what I told you, though?
HEARING OFFICER DEL PIERO: Wait. Wait. Wait. Wait. Wait. Wait. Wait. That's not acceptable.
DR. HERBST: Okay.
HEARING OFFICER DEL PIERO: That's not an acceptable response. Mr. Moskovitz -- do you have an objection?
MR. FLINN: No, I'm sorry.
HEARING OFFICER DEL PIERO: Mr. Moskovitz, you

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want to restate your question, Sir?
MR. MOSKOVITZ: Yes. I'll restate the question.
Q BY MR. MOSKOVITZ: If you take the difference between 4.4 and 4.72, what do you come up with?
A .32 or so.
Q .32. And what is the percent that that is -- that difference of the 4.72?
A When scaled to the minimum body size achievable in nature by a fly that's about a 40 percent reduction.
MR. FRINK: In order that our record's clear, we do have an identification number for that exhibit. It's Staff Exhibit 13-H, Herbst 1992, Mono Lake benthic ecosystem research.
HEARING OFFICER DEL PIERO: Dr. Herbst, what page are you on in that report?
DR. HERBST: It doesn't even have a page. It's Figure 33.
HEARING OFFICER DEL PIERO: 33? Thank you. How much more time does Mr. Moskovitz have?
MR. HERRERA: He has a little over four minutes.
MR. MOSKOVITZ: I will be asking for more time.
HEARING OFFICER DEL PIERO: I assume you will. I point out we sort of have an unwritten rule here that the longest amount of time granted was to Mr. Birmingham, it was an hour and ten minutes, for the

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cross-examination of any given panel. As I recall, we're working on 40 now for you, so I'm making you aware of that.
MR. MOSKOVITZ: And I'll do my best to finish within --
HEARING OFFICER DEL PIERO: Thank you.
MR. MOSKOVITZ: -- that outside limit, if not --
MR. DODGE: In all fairness on the ground rules, that was stated to be the outside limit for the panel.
HEARING OFFICER DEL PIERO: Yes, I understand that, Mr. Dodge.
MR. DODGE: And Mr. Moskovitz may have understood that was his outside limit.
MR. MOSKOVITZ: I certainly don't want to use up the time that Mrs. Goldsmith may need.

HEARING OFFICER DEL PIERO: Is Ms. Goldsmith here?
MR. MOSKOVITZ: She's in the building, and she'll be coming back when it's time to cross-examine the people on the birds.
HEARING OFFICER DEL PIERO: If you can move along in terms of your cross-examination, Mr. Moskovitz, we'll try and get this this matter done before everyone turns into a pumpkin tonight at seven or eight o'clock.
MR. HERRERA: Is then -- excuse me, is that a

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granting for an additional 20?
HEARING OFFICER DEL PIERO: Let's wait until his time is up, and we'll see where he is.
MR. MOSKOVITZ: I have another sheet that I want to distribute for an exhibit. By the way, may I have the first two exhibits received in evidence? That is --
HEARING OFFICER DEL PIERO: Usually, what we do is we hold off on that, Mr. Moskovitz, until --
MR. MOSKOVITZ: Until the end?
MR. SMITH: Mr. Moskovitz, that will be L.A. DWP 101.
Q BY MR. MOSKOVITZ: Dr. Herbst, you have a copy of L.A. DWP Exhibit 101?
A BY DR. HERBST: I do.
Q What is it?
A It's a graph of seasonal changes in body size of adult alkali flies.
Q And it covers what period of time?
A June 1982 to October 1984.
Q Is this from your Ph.D. dissertation?
A That's correct.
Q And are the body sizes shown here sizes that you yourself measured?
A Yes.

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Q And this was -- it starts in July 1982 approximately?
A That's right.
Q And goes through September 1984 approximately?
A That's right.
Q And what was the salinity in July 1992?
A 1992?
Q Excuse me. 1982.
A In July of 1982, let me be sure. Let's see, the elevation was 6374, so the salinity was probably right around 95.
Q Right around 95. What does it show to be the average size of adult flies?
A 4.7.
Q 4.7. How does that compare with the body size that your microcosm exhibits showed you'd achieved at 50 grams per liter?
A About the same, a little bit over 4.7.
Q So in nature, you had body size at a salinity of 95 grams per liter. The same as your microcosm exhibits showed for 50.
A That's right. But you really can't compare those two particular ways of evaluating body size because one

is flies that were collected in the field and the other is flies that were exposed to a completely different

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kind of environmental regime in these microcosm experiment tanks. So the only way you can really evaluate any kind of experiment is with reference to the different treatments to which the experimental groups were exposed. So in that kind of a context, you can't compare the body sizes of flies that come from those experimental microcosms to flies that are in the field because they experience completely different kinds of environments.

Q Well, then the microcosms really don't reflect reality, do they?

A They reflect reality better than laboratory experiments.

Q But not as good as reality in the field?

A That's right. That's true.

Q And in the field, you've got flies of the same size, you actually measured out in the lake?

A Um-hum.

Q As your experiments showed you would get at 50 grams per liter?

A That's right.

Q Now, taking a look at that -- a further look at that graph, L.A. DWP Exhibit 101, what does it show to be the average size of flies in July of 1983, the year after the one we just talked about?

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A Pretty close to the same.

Q Pretty close to the same. And was there a change in lake elevation between 1982 and 1983?

A There was.

Q And what happened?

A The lake elevation rose.

Q Would you expect that there would be a larger fly with the increasing lake elevation and decreasing salinity?

A I'm sorry. Can you repeat that question?

Q Yes. Would you expect that there would be an increase in the size of the flies with increasing lake elevation and therefore decreasing salinity?

A Indeed you would, and there is evidence of that if you look over a long-term historical record. In fact, the only way of really doing that, since I don't have data that dates back myself, in my own collection, is earlier than about 1980, is to look at historical records of flies that have been collected in museums. And, in fact, I have gone back and done that, looked in several museums where entomologists have deposited collections of flies from years past, in fact, from as long as as 1911 when Mono Lake was first visited and the flies were first described from the habitat. There's number of intervening years that I was able to

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collect information on those flies and, in fact, what we see is that there's a decrease in the body size of flies to -- at current elevations, current salinities, compared to those earlier records of flies from museum collections that were collected under high lake level,

low salinity conditions. And indeed those historical records suggest that there is indeed a decline in body size with increasing salinity.

MR. BIRMINGHAM: Excuse me, Mr. Del Piero. I hate to interrupt. Mr. Moskovitz hasn't been here, and he's not aware of the problem that we have had with witnesses who have gone well beyond the question in the response. In listening to many of the answers that Dr. Herbst has given to Mr. Moskovitz' questions, he has gone well beyond the scope of the question, and Mr. Moskovitz has been very polite with allowing him to do that.

Mr. Dodge, I know, is very concerned about getting this panel out of here, and I wonder if we could have an instruction to the witnesses to respond only to the questions as opposed to going beyond the scope of the questions.

HEARING OFFICER DEL PIERO: I'll be happy to give that instruction to the witnesses.

MR. HERRERA: Also, Mr. Moskovitz, your time has

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expired.

HEARING OFFICER DEL PIERO: How much more time do you think you'll need, Mr. Moskovitz? And Mr. Birmingham, you may want to get Ms. Goldsmith in here.

MR. MOSKOVITZ: I would say I would need about another 20 minutes. But I don't want to deprive Ms. Goldsmith of her time, but I believe that what I'm inquiring into is quite relevant with respect to the credibility of this witness' testimony.

HEARING OFFICER DEL PIERO: Mr. Moskovitz, that will have put you -- that will have put Los Angeles Department of Water and Power -- how much time would that --

MR. HERRERA: 40 minutes so far plus an additional 20 would be an hour.

HEARING OFFICER DEL PIERO: Do you have an appreciation for the amount of time Ms. Goldsmith is going need for her cross-examination?

MR. MOSKOVITZ: She told me she felt it would be considerably less than what I expect.

HEARING OFFICER DEL PIERO: Fine. I'll grant you the additional 20 minutes and -- recognizing -- perhaps Mr. Pollack can inform them as to how much time is left so they're aware of it. Thank you very much.

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Please proceed, Sir.

Q BY MR. MOSKOVITZ: What I want to have is a direct answer to my question as to whether -- this is perhaps a question I hadn't yet put. Does your exhibit or your Table 4-4, which is L.A. DWP Exhibit 101, show any increase in size between 1982 and 1984 during a time when lake elevation rose from the low 1982 to considerably higher?

A BY DR. HERBST: No.

Q I want to turn now to the subject of food for flies, and in your summary of your testimony on Page 11 -- excuse me, Page 5, Paragraph 11.

HEARING OFFICER DEL PIERO: Mr. Dodge, I don't

mind. I don't think he has a hat for you, though, so why don't you go take your seat? You're interrupting Mr. Moskovitz' cross-examination.
Please proceed, Sir.
Q BY MR. MOSKOVITZ: On Paragraph 11, Page 5, you say in part that the increasing salinity of the water generally reduces the algal food supply available to the flies.
A BY DR. HERBST: Yes.
Q Looking at Exhibit 64, Page 5 again, that's your comments on the EIR. Looking at the upper right-hand graph on that page.

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A Okay.
Q Looking at mixed algae. Does that support your statement?
A That does not support my statement. However, that particular experiment --
Q That's all I'm asking.
MR. FLINN: Madam Reporter, would you mark that part of the tape, please?
Q BY MR. MOSKOVITZ: Now, looking at that same graph, the next line shown purports to be what happened to algae called Tintoclatus (phonetic).
A That's correct.
Q Is that right?
A Yes.
Q And it shows it to drop?
A That's right.
Q With increasing salinity?
A That's right.
Q And I believe you talked about Tintoclatus in your summary of your testimony in Paragraph 23 on Page 10, right? That's in your section called the effect of decreasing lake level on nutrient supply and algae?
A Yep.
Q As a matter of fact, that's only the algae you really address in your summary, right?

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A I don't think that's actually correct. I think that there are references to the fact that there's lower algal production in the microcosm tanks as well. I don't refer to a particular species in there because it's a study of all the algae that occurs in the tanks.
Q The only species that you identify and talk about specifically is Tintoclatus, right?
A That's right.
Q Now, isn't it true that Tintoclatus was shown in your own studies to be an inferior food for flies?
A That's true.
Q What is the significance of talking about the fact that Tintoclatus volume or numbers declines with increasing salinity when it's an inferior food?
A Because even though it's an inferior food, it tends to be in many habitats the only food available, and so because it's the only food available and it's what you typically find in the guts of flies, one has to consider it an important food source.
Q Is it the only food available on Mono Lake?
A No.

Q In fact, didn't your research with Dr. Bradley (phonetic) show that other foods were the ones that were more important?
A Correct.

Q And was there any discussion about that in your summary?
A Yes. The --
Q No. The fact that other foods, other kinds of algae foods were better foods?
MR. FLINN: Objection. It was asked and answered.
HEARING OFFICER DEL PIERO: Sustained. It was asked and answered. He said yes.
Q BY MR. MOSKOVITZ: And your answer was no.
MR. FLINN: I believe the answer was yes.
HEARING OFFICER DEL PIERO: The answer was yes. Wait, Mr. Moskovitz, so we can with clarify,
Mrs. Anglin, would you read the answer back?
THE REPORTER: It was yes.
Q BY MR. MOSKOVITZ: Can you point out to me where you discuss the fact that other foods are referred to? Other algae?
A In Page 8, Paragraph 20, on the top of Page 9, I say that both reduced salt stress and enhanced food availability of 50 grams per liter combine to increase fly production to levels at approximately 100 grams per liter. And that, I believe, addresses your question about the stimulation of food sources by low salinity.
Q Could you direct me to the page again, please?
A Page 9, top of Page 9.

Q Does that say that there are other algaes than Tintoclatus which are the better food sources for flies?
A I didn't understand that to be your question.
Q That was the question I asked. Where in your summary did you point out Tintoclatus is not a good food source and other algae are?
A I didn't point that out. I didn't understand that question.
Q Now, looking at that same graph in Exhibit 64, you graph -- your microcosm -- the results of your microcosm experiments with algal growth, right?
A That's right.
Q It shows a drastic reduction in the production of algae, right?
A That's right.
Q With increasing salinity.
A That's right.
Q Now, is that a reflection of the standing crop of algae?
A Yes.
Q And does that not include the reduction in Tintoclatus in large measure?
A It includes all algae that are in the benthic part of the ecosystem because I didn't measure specifically

those different algae. I can't say that it was due mainly to Tintoclatus. It reflects the total abundance of algae in the benthic community.

Q Now, is net photosynthesis a direct measure of algal growth?
A Yes.
Q Did you make any measurements of direct photosyn -- net photosynthesis in your work?
A I did, in the microcosms.
Q Is that in the auxiliary report?
A I believe it is.
Q Figure 36.
A Yes.
MR. MOSKOVITZ: I'm distributing, Mr. Del Piero, Figure 36 from that auxiliary report which I'd like to have marked called Microcosms Metabiology.
HEARING OFFICER DEL PIERO: Next number, Mr. Smith?
MR. SMITH: L.A. DWP 102.
MR. FLINN: Although, this is already in the record, I assume, as part of auxiliary report Number Eight?
HEARING OFFICER DEL PIERO: You want it numbered by a separate number?
MR. MOSKOVITZ: I would like to have it numbered

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because I passed it out as a separate sheet, and I want to ask questions about it.
HEARING OFFICER DEL PIERO: Okay.
Q BY MR. MOSKOVITZ: Now, Dr. Herbst, do you have a copy of that?
A BY DR. HERBST: Yes.
Q And are you familiar with it?
A Yes.
Q And does it relate photosynthesis to salinity?
A Yes.
Q And what does it show with respect to the effect on photosynthesis, which direction a measure of algal growth is relative to changes in salinity?
A There's a step-wise decrease in net photosynthesis from 50 grams per liter to 100 grams per liter after which it pretty much stabilizes.
Q All right. Let me ask you what percentage decrease is there between 50 and 75?
A Probably around 30, 25. 25 percent.
Q About 25 percent?
A Yeah.
Q And what would be the lake elevation at 75 percent, 75 grams per liter, excuse me?
A 6389.
Q Approximately 6390. And is there any

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statistically significant change from 75 grams per liter going on all the way up?
A To 100 grams per liter? Probably not a statistically significant effect there, but measured by the absolute difference between the two, maybe something on the order of 10 to 15 percent reduction.
Q That's 10 to 15 percent reduction?
A Something like that, yeah.
Q But not statistically significant?
A Probably not.
Q And similarly, when you go above 100?

A That's right. No change.
Q So that shows that from elevation 6389 to current elevations, to even higher elevations, the photosynthesis which is a measure of the algal growth, does not decline with increasing salinity; isn't that right?
A That's right. This can't be related directly to those algal standing crops.
Q Algal standing crops are related to the amount of grazing; isn't that right?
A No. It's the total amount of growth that has occurred over a period of time, and you're looking at the -- at the abundance of algae at a particular instant in time so that the total biomass of algae at

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that particular instant in time, and in the context of these experiments here, the biomass of algae that we see in these tanks is after they've been growing for a two-month period of time whereas the photosynthesis studies that you're referring to right here refers to only a single 24-hour period. And it's the accumulation of those 24-hour periods of photosynthesis that will eventually result in the kind of biomass that you get after a two-month period of growth which is what you see in these chlorophyll standing crop measurements.
Q What is the significance of the photosynthesis relationship, then?
A It's to give us an idea of the relative amount of photosynthesis and respiration that goes on in a tank in a single day. So it's another way of our evaluating the relative production of the different tanks.
Q And it shows that the relative production between 75 grams per liter and 100 is about the same, right?
A Yeah.
Q I want to briefly touch on nitrogen fixation, and I want to look at your summary again on Page 11, Paragraph 25. And the third sentence says, "The data show that nitrogen fixation rates at current salinities are only one-half those at pre-diversion salinities."

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I'd like you to look at your Exhibit 65. Do you have a copy of it?
A Can you tell me what exhibit it is?
Q Exhibit 65 is a document entitled Salinity Limits Nitrogen Fixation and sediments from Mono Lake, California, by you, Mr. Culbertson (phonetic) and Mr. Armenlin (phonetic).
A Okay.
Q I want you to look at Table 1 and compare nitrogen fixation at 6415, which is pre-diversion. That's the last column, and at 6375, current conditions.
A 10.9 at 6415.
Q Yes.
A And 8.1 at 6375.
Q And what is the difference between those two? In percentage?
A About 20 percent.
Q It's not double between -- or to put it another way, at 63 -- 6375, it's not half of what it was

pre-diversion, 6415?
A This in the rate function, though. The rate function -- if you look back in that same document, or look ahead in that same document, rather, on Figure 1 or Figure 2, whenever you please, Figure 1, is unacclimated sediments. Figure 2 is acclimated

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sediments, so that we're looking at both kinds of conditions of exposure to the algal mass community, and what you see is at 50 grams per liter, that light or total activity is indeed twice what it is at 100 grams per liter. And if you look ahead to Figure 2, the rate function for 50 grams per liter, which is the circles --
Q I don't know what you're referring to?
A Same document.
HEARING OFFICER DEL PIERO: You want to give a page reference? You're talking off of --
DR. HERBST: These are figures on that same figure.
HEARING OFFICER DEL PIERO: They're immediately behind your Table 1? One's entitled Nitrogen Fixation in Mono Lake and Effective Salinities, the first one, and the second one is Cultured Sediments?
DR. HERBST: That's right.
HEARING OFFICER DEL PIERO: Last two pages, Mr. Moskovitz.
Q BY MR. MOSKOVITZ: All right. And what you repeat again?
A Those are the rate functions. If you're just looking at this effect of salinity per se, the activity of the nitrogenous enzyme, which is what fixes

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the nitrogen, then you can see that at 50 grams per liter in the first figure, we'll be looking at the unacclimated sediments, this is just straight sediments from the lake, you see that the activity at 50 grams per liter is twice as high as 100 grams per liter, that's the white bars. That's the total activity, light activity.
If you look at the next figure, which is --
Q I see two bars for each of the salinities. Which are you talking about, the left-hand bar?
A Left-hand bar, the white bar, which is total activity.
Q So you're comparing the bar that says -- that is opposite the number 30 vertical axis?
A That's right.
Q With the bar that's opposite approximately 17 or so?
A Yeah. Yeah.
And then on the next figure, with the acclimated sediments, You can see that both 50 and 75 do about equally well in terms of how rapid the fixation rate of the enzyme is. Whereas you go down to the squares, which is the rate at 100 grams per liter, you can see it's about half that of the rates that you find at 50 or 75 grams per liter. So the rates indeed are half.

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The thing that affects the table that you're

referring to earlier is the smaller latoral area at high lake elevations. So it's not related to the rate, itself.
Q So the latoral area is very important in the total amount of nitrogen fixation. It's not just the rate, but how much --
A Sure.
Q -- bottom you have?
A Sure.
Q And 6415 you have much less bottom area than at 6375, so in it important to take that into account in the conclusion as to what the difference is between those two lake elevations as far as nitrogen fixation is concerned?
A It is, and I do.
Q And when you take that into account, then you don't have a halving or only a 50 percent of 6415 when you go down to 6375.
A No, you don't. But you do at 75 grams per liter, 6390, where I showed you earlier.
Q But comparing pre-diversion to today.
A That's right.
Q You can't say it's only half?
A That's right.

MR. HERRERA: Excuse me, Mr. Moskovitz --
DR. HERBST: But that's not a rate function.
MR. HERRERA: Excuse me. Your time has expired.
MR. MOSKOVITZ: I think at this point I'll just withdraw so there'll be sufficient time for Ms. Goldsmith. Thank you very much.
HEARING OFFICER DEL PIERO: Ms. Goldsmith? Ms. Goldsmith? Good afternoon.
MS. GOLDSMITH: Good afternoon.
HEARING OFFICER DEL PIERO: Ms. Niebauer -- I'm sorry, Ms. Goldsmith. Did you have questions?
MS. NIEBAUER: I don't have any.
HEARING OFFICER DEL PIERO: Did I miss anybody else? Mr. Haselton? You guys?
MR. FRINK: Mr. Gipsman is also here.
MR. VALENTINE: I should point out that Mr. Gipsman is here representing the United States Forest Service.
HEARING OFFICER DEL PIERO: Do you have any questions?
MR. GIPSMAN: No.
HEARING OFFICER DEL PIERO: Well, I'll ask you again after we get done here. It's nice of you all to join us today.

CROSS-EXAMINATION BY MS. GOLDSMITH

Q Mr. Shuford, I believe you testified that coyotes can swim, that there's no guarantee of security, and we don't know what level will protect the nesting island. Is that right?
A BY MR. SHUFORD: I testified there was no absolute security, but I did list some lake levels that I thought would provide reasonable security for the nesting islands.
Q I believe I got your quote to be pretty accurate,

actually. Given the fact that there is no guarantee that coyotes can be kept off any island of the lake, isn't it a better strategy to disburse, to have the nesting gulls disbursed among the number of islands rather than concentrated on a single island or a couple of islands?
A Well, if that were the case. As I said, I don't believe -- I think there's a high degree of security at higher lake levels.
Q Well, then, let's turn to -- to where coyotes can get to. You testified that you thought there was a relationship between coyote access and lake levels; is that right?
A Yes, that's right.
Q You also testified that there have been coyotes on Paoha Island; is that right?

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A That's correct.
Q And that there are currently coyotes on Paoha Island; is that right?
A That's right.
Q How far the Paoha from the nearest land?
A From the Negit Channel, I couldn't tell you exactly. I think the easiest way to get across there is perhaps a half mile, quarter mile. Dr. Stine could probably address that a little more accurately.
A BY DR. STINE: That's close to correct.
Q In 1985 would that have been correct, Dr. Stine?
A Yes. There's not much change in the width of straits with changing elevation.
Q Mr. Shuford, you've testified that a trapper would move the coyotes in 1985 and they returned the following year; is that right?
A BY MR. SHUFORD: I don't know about the following year, but they have returned.
Q Concerning the effect of predation on these islands, you testified that there were a number of predation events, and I probably won't get them all right, but putting together your testimony and Dr. Winkler's testimony, there was predation in 1979 and about 65 percent of the gulls were ousted from Negit Island; is that right?

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A All of the gulls from Negit Island were ousted.
Q And how much of the population did that amount to at that time?
A That's approximately right, about two-thirds of the population.
Q And then in 1982 about 30 percent of the population was dislocated from Twain; is that right?
A That's correct.
Q And then there have been three other population events, one, again, involving Negit with about 2 or 3,000 nests or -- I don't know if it's nests or birds?
A It's nests.
Q About 4 to 5,000 birds, I guess. And a couple of other incidents involving Java; is that right?
A That's correct, as well as Pancake.
Q And that pretty much sums up, as far as I gathered from your testimony, the history of predation in the

last, say, 14 years at Mono Lake?
A That's correct.
Q And during that -- during that time period, the adult gull population has remained relatively stable or increased; isn't that right?
A Yes, that is correct.
Q Now, the Corey report concluded that land bridging of islands for brief periods of time would not unduly

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affect the nesting colony; isn't that right?
A It depends on which islets you're talking about.
Q I believe in the Corey report, it was talking about Negit and Twain.
A That would be correct. I don't remember the exact -- the wording of the Corey report, but I know that they did consider Negit occasionally was land bridged, that would be a problem. That's what their conclusion was.
Q Wouldn't you say that's consistent with the history that we've seen in the last 14 years?
A Consistent with what?
Q The stable population over the last 14 years despite predation?
A The population has remained stable. Whether that has been totally a reflection of the predation is another thing. Population increased dramatically in 1990 and that, as far as I can tell from looking at evidence from the Great Salt Lake in particular, that seems not to have had a direct effect -- have been a direct effect of what was going on at Mono Lake. So there's many compounding factors that could influence size of the population at Mono Lake.
Q Are you testifying that there was a substantial immigration from the Great Salt Lake in 1990?

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A No, I'm not. What I'm saying is in 1990, the Mono Lake population went in the previous year from less than 50,000 to over 60,000 one year. In the exact same year, the Great Salt Lake went from the high 70,000 in its population to over 130,000 which indicates, to my mind, that there was probably some broad regional inputs going on perhaps related to the drought.
Q Let's move on. Now, you testified that you believe that there's a marauding effect when the gulls are displaced by a terrestrial predator from the nesting island; is that right?
A I testified to the effect that that had been reported at Mono Lake, and Dr. Winkler could expand more about that. But I do believe that that can happen at Mono Lake.
Q And you weren't there in 1979 or in 1982 to observe that, right?
A No, I wasn't. So Dr. Winkler would be the best one to address that question.
Q Now, Dr. Winkler, you weren't at Mono Lake when Twain was invaded by predators, were you?
A BY DR. WINKLER: Not in 1982, no. Maybe I should add that I have seen predators on Twain. It was late in the summer of 1981.
Q But when the gulls were dislocated in 1982, you

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weren't there to observe any marauding effects?
A That's correct. I was in Great Salt Lake and one of my senior field assistants was managing the Mono Lake operation.
Q Isn't it true that your senior field assistant was Virginia Norris (phonetic) at the time?
A That's correct.
Q And she went out to Twain Island with Dr. Jehl to investigate; is that right?
A I think they made a trip out there at some point. I don't remember the timing of the trip.
Q Do you remember the results of the trip?
A I can't remember -- I would have review my notes and her notes. I remember they discovered that there were no -- or very few gulls nesting there and that that surprised them.
Q That's not consistent with your marauding scenario, is it?
A Could you tell me why?
Q Well, if there aren't any gulls nesting there, then there's no gulls marauding shifts and --
A Oh, the very reason that surprised Virginia is that there were gulls standing there, but they weren't nesting. So those gulls had been disturbed apparently, and those would be the very same birds that would have
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started this whole snowballing marauding effect going.
Q And they were just standing there?
A Well, they were roosting there and that's where they were resting. So that from Krakatoa (phonetic) without having visited the island before, that it looked like there were gulls on Twain.
Q Gulls stand all around Mono Lake on the shore lines, don't they, when they aren't nesting?
A Yes.
Q Now, Mr. Shuford, you testified that you found a correlation between the degree of particular infestation and chick mortality at the lake; is that right?
A BY MR. SHUFORD: That's correct.
Q And I have it here if you'd like to see it, but I'd like to read you from the Corey appendix which is written by you, Dr. Winkler.
A BY DR. WINKLER: That's correct.
Q "The substrate type of newly exposed island can be important to the gulls in that the ticks appear reliant on bits of loose stone and debris beneath which they spend the winter months and daylight hours. The lack of such shelters on such islands like bottom sediments will probably insure low or negligible tick infestations there."
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The ticks that we have been talking about, the ticks that infest the Mono Lake gulls are ticks that are found on the rocky islands; is that right?
A BY MR. SHUFORD: That's where I've observe them on the Negit Islands where I've done my studies.
Q Are you aware of any reports of tick infestation on the Paoha Islands?

A I think there have been some observations of ticks there. I don't think there's any major activity there. I also understand there's been some observations of ticks on Negit.
Q On Negit Island?
A That's correct.
Q And Negit Island's a rocky island?
A Parts of it are rocky and parts of it are sandy as well.
Q All other things being equal, wouldn't you agree with me that it's not a good idea to concentrate gulls on an island which is conducive to tick infestations?
A If you were to concentrate them -- are you just talking about one island?
Q On any island. One island, primarily.
A Well, theoretically --
Q We've been testifying about gulls on Negit and the EIR process has identified Negit as a primary site of

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gull expansion, I guess. And so that's the context in which I'm asking the question.
A In my earlier testimony, what I testified to is generally diseases of parasites. Parasites are density dependent factor and what I mean by that is as the population increases and the density of the gulls increase on an island, they would be more likely to be affected by disease and easier for transmission and so forth. If birds are on Negit Island, the density of gulls would actually be lower because the size of the island is so great and they could expand over more area. In that respect, they'd probably be less likely to be affected by ticks.
Q So gulls don't distribute themselves evenly over island, do they?
A Not necessarily evenly, but there's a huge amount of area that's in the map in the Corey report and on the Draft EIR that show where the gulls are nesting.
Q And they concentrate themselves in little sites on the island?
A I think Dr. Winkler could better answer that question. I've never actually observed, you know, large-scale nesting on Negit Island.
Q Let me ask you about the Paoha Islands. Isn't it true that in the densest colony on the Paoha Islands,

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the gulls occupy only a portion of the islands?
A They do on that island. On the islets I've observed, the Paoha Islets, I've made a number of trips around those islands. They do concentrate on what's been termed rugose or rough substrate.
Q So just by knowing that Negit is a large island doesn't allow us to predict gull density on that island, does it?
A No, it doesn't. But we have a historical record of mapping where these birds were and the approximate size and populations in the various areas that Dr. Winkler has mapped.
Q Is there any guarantee they'd go back to those particular areas?
A There's no guarantee. I think there's high

likelihood that they would, you know, choose areas of habitat that they preferred in the past.
Q When they recolonized Negit after being dislocated from Negit, they didn't go back, did they?
A Some of the birds did.
Q A large proportion or a small proportion?
A A small proportion.
Q And, of course, the predation effect on an island is independent of the density on that island, in it?
A I don't think you could say that. When Negit was

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reinvaded by coyotes, I believe in '89, it took quite awhile for the gulls to abandon that island. And I -- my suspicion, the reason for that, is that at that time, there were several small colonies that were widely separated on Negit Island so that they would be infrequently exposed to coyotes compared to if the coyotes had gone to a small island or an island was continuously occupied across the whole range of that island.
Q I'd like to turn to the history of the Mono Basin colony. In your written testimony, Mr. Shuford, as I understand it, you argued that there is a need to protect Negit Island because it was so important to the gulls historically and because it was, quote, the island of choice during the period of greatest population increase in the 20th century. Those are historical reasons for protecting Negit; is that right?
A That's correct.
Q But in it more reasonable to base your decisions about the gull colony and its future and its protection on actual needs rather than historical accidents?
MR. DODGE: Objection, unintelligible.
HEARING OFFICER DEL PIERO: Did you understand the question?
MR. SHUFORD: I'm not sure exactly what the

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question was.
HEARING OFFICER DEL PIERO: Why don't you restate the question, Ms. Goldsmith?
Q BY MS. GOLDSMITH: The question was premised by my understanding of your statement in your testimony that the reason to protect Negit is because it was important historically and the gulls chose it during the 20th century. My question to you is isn't it more reasonable to select -- to base management decisions on what the gulls need now and in the future rather than on historical precedent?
A BY MR. SHUFORD: Well, I guess you should take all factors into consideration and if you do, the lake is raised to 6385, 6383.5 feet or above, Negit Island will provide the largest amount of gull habitat, suitable gull habitat to the gulls.
Q If it were possible to protect the island so that you had a broad panoply of habitat types and numbers of islands, wouldn't that been the most ideal situation?
A Did you say the Paoha Islets or the Paoha Islands?
Q Paoha Islets, and I'm asking you to assume that they could be protected.
A Well, it still would not provide the maximum

amount of habitat even if they could be protected from erosion. Negit Island is by far much greater in size than those islets combined.

Q Do the islands that currently exist at the lake provide an adequate amount of habitat for the gulls that are there now?

MR. DODGE: Objection. Calls for a conclusion, ambiguous as to, quote, adequate, end quote.

MS. GOLDSMITH: Mr. Shuford is holding himself out as an expert. There are gulls at Mono Lake now, and I believe he's qualified to render an opinion as to whether or not he thinks the existing habitat is adequate --

HEARING OFFICER DEL PIERO: I'm going to overrule the objection. Go ahead and answer the question.

MR. SHUFORD: I'm still a little unclear on what adequate is. The birds that are there are nesting there so in that term, it is adequate. It provides an excellent amount of habitat which I would say is adequate for supporting the gulls as the gulls would like to do, I would say the higher amount of habitat. And currently, we were right at a lake level right now where a large part of the population is threatened.

Q BY MS. GOLDSMITH: Focusing on the existing gull population rather than some potential expansion of the gull population and assuming that Twain and Java were adequately protected, wouldn't it be better to provide a natural number of islands than to raise the lake and limit the habitat that's available to them?

A BY MR. SHUFORD: Raising the lake would not limit the habitat, it would increase the habitat if Negit becomes an island again.

Q It would limit the habitat choices.

A Well, habitat -- it would actually increase their choices. They would have the choices of grease wood scrub where two-thirds of birds were nesting back in 1976.

Q Would they have the choice of the Paoha Islets?

A If we were at the upper levels, the 6385 alternative, the Paoha Islets would be lost.

Q Part of your testimony included the notion that Negit was the island of choice during the period of greatest population increase, the 20th century. But the Paoha Islets weren't available at all during that time, were they?

A No. They weren't, to my knowledge.

Q And Paoha Island had a goat farm on it, didn't it?

A It did for a short period and then it was abandoned.

Q Leaving predators aside, do you think it's reasonable to suppose that the gulls left Paoha Island for reasons related to habitat?

A I really can't speak to that issue with any knowledge. All I know is, you know, what the historical record shows and during that period after the goat farm was no longer in operation, that the gulls increased dramatically at Mono Lake and they

increased largely on Negit Island. Exactly why they left Paoha Island is unclear, but it would suggest that other factors being equal, that Negit was preferred over Paoha.
Q Now, you have written, together with Dr. Winkler, a historical paper on the gull colony at Mono Lake; isn't that right?
A That's correct.
Q And I believe that you relied in part on J. Ross Brown and described him as a highly respected observer; is that right?
A That's what was reported in another reference.
Q And J. Ross Brown (phonetic) reported, "In some parts of the main island, the open spaces between the rocks are so thickly covered with eggs that the pedestrian is at a loss to find a vacant spot." He was talking about Paoha Island, wasn't he?
A I believe so.
Q And this was in the 1860s; is that right?
A That's correct.

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Q And Dr. Jehl has written the initial -- in the earliest report colonization by California gulls at Mono Lake was on Paoha Island, wasn't it?
A I can take a second and refer to his table. I can't remember the exact thing there.
MR. HERRERA: Ms. Goldsmith, excuse me, your time has elapsed.
MS. GOLDSMITH: I would ask for another 20 minutes.
HEARING OFFICER DEL PIERO: Has anyone advised you of how much time Mr. Moskovitz had?
MS. GOLDSMITH: No. I was in there awhile. I think in light --
HEARING OFFICER DEL PIERO: I'm inclined -- before you -- I'm inclined to grant you an additional 20 minutes. Let me point out that at the end of that 20 minutes, however, the total amount of time elapsed in terms of examination of this panel will be what?
MR. HERRERA: One hour and 40 minutes.
HEARING OFFICER DEL PIERO: One hour and 40 minutes. Which is the longest amount of time with any panel by any single party. Proceed.
Ladies and Gentlemen, let me point out that at the end of this 20 minutes, we're going take a break.
Q BY MS. GOLDSMITH: Have you refreshed your

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recollection?
A BY DR. HERBST: I have refreshed my recollection. This paper I'm referring to is called the History of the California Gull, Mono Lake, California, by Joseph R. Jehl, Jr., David E. Badd (phonetic), Dennis M. Power and was published in Colonial Water Birds in 1984, and the table I'm referring to is Table One. And what it says in Table One, there really -- there is a location identified for the Brown sightings. In 1880, the next one, there's no doubt Paoha Island -- there's a large colony on Negit Island.
Q I'm going to hand you an excerpt from the Brown article so that you can perhaps refresh your

recollection.
A I've read this passage.
Q Would you agree with me that it describes the gull colony that I read citation about on Paoha Island?
A It's talking about these paragraphs at the bottom of Column One and going on Column Two. I don't see any reference to the particular island except for it mentions -- it says these smaller islands and evidently an extinct crater which I assume refers to Negit Island.
Q Negit Island is smaller that Paoha.
A Yes. Considerably.

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Q And it has a crater on it?
A That's correct. Nowhere in here does it identify where these gulls were actually observed.
Q It says they were on the larger island, doesn't it?
A No. I don't see that here.
Q It refers to the main island?
A In what context?
Q Does it refer to the main island?
A It does say on some parts of the main island the open spaces were covered with eggs, et cetera.
Q Assuming that it does refer to Paoha Island --
A Well, the previous paragraph says -- the first part it says in some parts of the main island, the open spaces between the rocks are so thickly covered with eggs, et cetera. And in -- the paragraph at the end of the first page, of this page, the first column it says. immense swarms of gulls visit these islands, which I assume means more than one island.
Q Isn't it entirely possible that Paoha Island was the island of choice for the gulls originally?
A I don't see anywhere in the historical record that that's implied or stated.
MS. GOLDSMITH: I'd like to have this marked and I'd like to offer it as L.A. DWP next in order, and

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I'll provide copies tomorrow.
HEARING OFFICER DEL PIERO: Fine. 10 --
MR. SMITH: 103.
HEARING OFFICER DEL PIERO: 103.
MR. HERRERA: Mrs. Goldsmith, could you give us a reference on that?
MS. GOLDSMITH: Sure.
Q BY MS. GOLDSMITH: Dr. Winkler, you criticized Dr. Jehl's slide show concerning habitat preferences of the gulls. And you offered your opinion that his conclusions were based on observations where gulls don't have a choice between nesting on islands and nesting on the mainland; is that right?
A BY DR. WINKLER: Yes. I don't think I criticized it as a slide show, though.
Q A matter of interpretation, perhaps.
I'd like to show you L.A. DWP Exhibit 81.
A Great, you have the pictures?
Q I have the pictures.
A Oh, good, I'd like to see them.
Q These are the pictures we made from the slides.

HEARING OFFICER DEL PIERO: Mr. Pollack advised me there's no trout habitat here.
MS. GOLDSMITH: Oh, I don't know.
HEARING OFFICER DEL PIERO: What are these good

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for?
MS. GOLDSMITH: Would you pull out 81-A?
DR. WINKLER: Where are they labeled, please?
Q BY MS. GOLDSMITH: On the back. 81-O, which looks like this. 81-P, which is the next one. Actually, if I could have some assistance -- Dr. Jehl, could you come help me here?
HEARING OFFICER DEL PIERO: Ms. Goldsmith, who was the photographer of these?
MS. GOLDSMITH: I believe it was Dr. Jehl. They aren't fuzzy, are they?
HEARING OFFICER DEL PIERO: No. They're actually very nice. I was going to compliment whoever did it.
MR. DODGE: I invited Dr. Jehl to join the panel this afternoon despite Mr. Frink's admonition that they didn't want to go issue by issue.
HEARING OFFICER DEL PIERO: What else, Ms. Goldsmith? Is that it?
MS. GOLDSMITH: I'm working on it.
HEARING OFFICER DEL PIERO: I'm sorry. I don't mean to rush you. 81-Y, 81-JJ.
HEARING OFFICER DEL PIERO: Mt. St. Helens was going off when this was taken; is that right?
MS. GOLDSMITH: I guess so.
HEARING OFFICER DEL PIERO: These are the same

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photos that were presented during the course of your direct?
MS. GOLDSMITH: Yes. And 81-C.
Q BY MS. GOLDSMITH: And if you'll pull out NAS 233.
Now, I'm going to ask you to assume that these are all islands, which is what Dr. Jehl testified to earlier. 81-Q is labeled Gunnison Island, Great Salt Lake, and the legend says, "The large spots are pelicans, the small spots are gulls." Do you see the small spots?
A BY DR. WINKLER: Yes, I do.
Q Are they nesting in scrub?
A Well, I'd have to be convinced, first of all, that they're nesting, and if they are nesting there, they're not nesting in very dense scrub, no.
Q And there is vegetation available on that island, in there?
A Yes. And when I was on Gunnison Island, they were nesting in that vegetation.
Q Okay. I would like you to look at L.A. DWP 81-W, which is Honey Lake, California?
A I'm sorry. I didn't hear the letter.
Q W.
A W. That one we didn't pull. Okay.
Q Where are the bulk of the birds? Are they in the

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vegetation or out on the sand bar?
A Well, certainly the pelicans are out on the sand bar. I don't know exactly what you mean by the -- by

the sand bar, you mean the --
Q Okay. Look at it in conjunction with 81-A, which shows Brushy Island.
A And you're representing these are the same place?
Q I believe so.
A They don't look like the same place.
Q Well, they're different parts of the same island. One part is brushy and the other part in, and where are the gulls?
A BY MR. SHUFORD: Can I answer this question?
My experience at Honey Lake -- and the one that is really scrubby which the marked A is the traditional site of the black ground and snowy egret colony. And this island is the one that is most frequently connected to the land. I've been on this island in the mid eighties and saw the -- there were dead chicks all over -- there was coyote -- not all the nests, but quite a few of the nests. So that might be a reason why the gulls wouldn't want to nest on that island.
A BY DR. WINKLER: I'd also like to add that -- let's see. This is W. The vegetation in W -- I'm not sure what it is, but it looks like the plant salsola

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(phonetic) to me, which is tumbleweed, which I've never seen gulls nesting in because if I were a gull, I wouldn't nest in it because it grows throughout the summer, and by the time the chicks fledge, it would choke them out. So I don't think that's the kind of habitat I have in mind when I'm talking about scrubby habitat.
Q Let's look at 81-JJ. Pyramid Lake, Nevada. Again, assuming that's an island.
A Which, if this is Anapo (phonetic), it has not always been.
MR. DODGE: I'm sorry, Counsel. What number ?
MS. GOLDSMITH: JJ.
HEARING OFFICER DEL PIERO: I didn't hear the last response.
DR. WINKLER: I said if this is Anapo Island, it has not always been an island.
Q BY MS. GOLDSMITH: And there are gulls on it?
A BY DR. WINKLER: Yes, they are.
Q And they're not in the scrub.
A Yeah. I'm not even sure they're nesting. I would point out if they were in the scrub, you wouldn't see them in this picture.
Q Although, we have seen them in other pictures where they've been nesting in scrub, at least according

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to the exhibits that were offered earlier.
A Yeah. But those pictures were much closer and taken from a different angle. This is taken from up above. If we'd been down below, I mean, at the level of the gulls, I think the likelihood -- and looking up into the scrub, the likelihood of seeing any birds that were in the scrub were much higher. I don't mean to say that if Jehl was there and looking at this, I don't question it if they weren't in this scrub. I'm just saying from this photograph, I can't tell.
Q Lastly -- let's skip P and O. Lastly, I'd like

you to compare NAS 233. Do you have that Mr. Del Piero?
HEARING OFFICER DEL PIERO: Which one is NAS -- turn around and show it to me. Is that one of ones that was submitted earlier?
MS. GOLDSMITH: Yes.
MR. DODGE: Earlier today.
HEARING OFFICER DEL PIERO: I've got it.
MS. GOLDSMITH: And 81-C.
DR. WINKLER: Yes. I'm comparing these two.
Q BY MS. GOLDSMITH: Now, in 1928 the scrub was much lower, wasn't it?
MR. DODGE: Objection, unintelligible.
HEARING OFFICER DEL PIERO: Wait. Wait. Wait. It can't be unintelligible unless I know what she's

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asking about.
MR. DODGE: I don't agree with that.
HEARING OFFICER DEL PIERO: Somehow, Mr. Dodge, I don't find that difficult to believe that you wouldn't agree with that. Hold on for one second. 81-C? Is that where you are now?
MS. GOLDSMITH: 81-C and NAS 233.
HEARING OFFICER DEL PIERO: 81-C.
MR. SHUFORD: It's possible --
HEARING OFFICER DEL PIERO: What was the unintelligible thing that Mr. Dodge had difficulty understanding?
MS. GOLDSMITH: I asked the witness whether or not in the 1928 photo -- perhaps the comparison is what you missed. Compared to the 81-C photo --
MR. DODGE: The reason I missed it is it wasn't in the question.
Q BY MS. GOLDSMITH: Wasn't the scrub in 81-C much denser, higher than in 1928?
A BY DR. WINKLER: You know, I'm getting a whole big deja vu all over again. We went through this in Finney's court, and the trouble here is you're giving me two pictures with the same rocks in it, much to your credit, but the picture's taken from a different spot on the island. Look at 233 -- is that what that is?

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So look up slope from those big two rocks that are the good landmark and can you see that there's an area of sparse vegetation in front of the rocks and as you move up slope, it gets denser. And we don't know what's going on as we go downslope from there.
I would maintain that from the angle that the picture's taken in C, we're closer to the lake by evidence of being able to see the islets in the background, and we're looking at those rocks from a different angle. So I can't take this as any kind of evidence of the relative density of shrubs now as opposed to 1928. And if you look up slope in this Exhibit C, you'll see we actually get an area of lower density there, so you could show me all kinds of pictures and have any kind of impression of changes in density and shrubbery, unless you show me two pictures that are absolutely taken from the same place, I can't take it as evidence either way.
I'd be very interested to know whether the density

is different, but I just don't think this is evidence one way or the other.
Q They do show different heights in density of vegetation?
A From different angles and different places of taking the photograph, that's right.

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Q And seagulls are site faithful in their nesting; isn't that right?
A Most gulls are site faithful.
Q California gulls. Sorry. They're not seagulls.
A I'm trying get my two-year-old daughter to start saying gulls, not seagulls, it's one of my pet peeves.
Q I'm delighted to hear that.
Didn't you testify, Dr. Winkler, in South Lake Tahoe, speaking of deja vu, in 1990 that the advantage of scrub is fairly minor in most of these?
A Yeah. It's likely to be as long as the temperatures are not really high and other things being equal, I think relative to something like predation, it is a minor thing.
Q Are you aware of any year at Mono Lake where heat was the cause of chick mortality?
A To use the word "cause" is pretty strong and we try to avoid that, "we" being scientists, but I can tell you that there's a very strong correlation between air temperatures and daily mortality rates for chicks in 1981.
Q Well, was the cause of death in 1981 the heat or was it the low food supply?
A I can't say that. I think it's actually a combination of the two, is the most likely explanation.

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Q Other than 1981, is there any other year in which you are aware that heat has caused chick mortality?
A I know of no other year in which the evidence is so strong.
Q In the past 13 years, the gulls at Mono Lake have not nested in scrub, have they?
A The past 13 years. I think some have nested on Negit, some have nested in scrub.
A BY MR. SHUFORD: I'll jump in here. There's also been birds nesting on Twain Island and Tahiti Island that were nesting in small patches of scrub that have grown up on those islands.
Q Is that scrub similar to the type that's found on the lake?
A Yes, it is, it's grease wood scrub.
Q Now, just a couple of minor -- minor points.
Dr. Winkler, I believe you testified that Johnston's 1940 population numbers should not be used in considering baseline or pre-diversion figures for Mono Lake; is that right?
A BY DR. WINKLER: They weren't 1940 numbers. They were numbers from the early fifties, and I think Dr. Jehl corrected that in his testimony. I -- it was our impression from talking to Dr. Johnson he'd rather not see them used in that way.

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Q What numbers were they?

A I can't remember. All I remember is that when you look at his field notes and when we talked to him, they were numbers that differed by -- he bracketed them by a factor of two, and I believe the number was 5 to 10,000, but it may have been 2500 or 5,000. I could look in the notes.
Q You yourself wrote that the gull population was as few as 1500 nesting adults as late at 1951 in a paper you published in 1993 with Dr. Zink (phonetic); is that right?
A I said that I think I cited the work of Young at that time, and those are the numbers he reported.
Q And you accepted those numbers?
A I accepted those numbers -- well, I used those numbers -- I cited those numbers. So I guess you could say --
Q Didn't you use that number as the basis for your conclusion there's a genetic bottleneck at Mono Lake?
A If you'll remember, we were looking for a genetic bottleneck and found precious little evidence for it. I used those numbers as an indication that the population had been much smaller in the past.
MR. HERRERA: Ms. Goldsmith, your time has elapsed.

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MS. GOLDSMITH: All right.
MR. MOSKOVITZ: Mr. Del Piero, I forgot to offer again exhibits that we marked during my cross-examination in evidence. Could I do that now to get it out of the way?
HEARING OFFICER DEL PIERO: Mr. Flinn, you bounced up very quickly, why?
MR. FLINN: Because I have an objection to one of them.
HEARING OFFICER DEL PIERO: Let's hear about it.
MR. FLINN: It's the Bradley report Exhibit 100, and my objection basically turns on the Water Board's view of out-of-court hearsay opinions by scientists. Dr. Bradley (phonetic) has, from time to time, been funded by the Department of Water and Power to do work and to give testimony, but for some reason they didn't see fit to bring him here and have him testified and be cross-examined on this report, so I don't think it ought to come into evidence. The only thing it was used for was this witness had seen it and disagreed with it.
HEARING OFFICER DEL PIERO: Mr. Moskovitz?
MR. MOSKOVITZ: Mr. Del Piero, this is a report that is cited in a report that Mr. -- Dr. Herbst co-authored with Dr. Bradley (phonetic), and he cited

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it, and he mentioned that particular point in his own report. It seems to me that that makes it appropriate to have it received. As an expert, he cited it.
MR. FLINN: If I could --
HEARING OFFICER DEL PIERO: Mr. Flinn?
MR. FLINN: just a brief response to that. The paper itself that was cited has not been introduced into evidence. The paper itself is not something that has -- was used for any purpose in this proceeding

other than to bootstrap this Bradley (phonetic) paper. The fundamental point to this is we will be deprived of the right to cross-examine this witness, which there's absolutely no reason why, if they wanted to call him, they could have, to testify here, if they wanted the actual truth of the matter to be asserted. I have no problem with what's already been in the record, that he disagrees with Dr. Bradley (phonetic) on this point, that he disagrees with one of the scientists in one of the thousands of articles he cited in his history as a scientist. That's already in. We don't need the report itself.

MR. FRINK: Mr. Del Piero?

HEARING OFFICER DEL PIERO: Mr. Frink? Come up here.

I'm inclined to allow the document to be admitted

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into the record, recognizing that the witness disagreed with the assertion that was made by Counsel for Los Angeles Department of Water and Power in terms of its proposal. I'm not particularly motivated by the justification, however, that because Dr. Herbst participated in the preparation of the report, that somehow that constitutes justification for the incorporation of this into the record.

I am motivated, as I have been regularly motivated, by affording all of the parties the maximum opportunity to introduce as much evidence, albeit hearsay evidence into this record so that the five members of the State Water Resources Control Board are afforded the absolute maximum amount of information regardless of its condition so that we can fully deliberate and ultimately decide this issue that is of tremendous importance, not only to the parties here, but obviously, to all of the citizens of the State of California.

So I'm going to allow it into the record, recognizing that its hearsay, and I think, if everyone checks back over the course of the record to date, that that is not a change in position for this Hearing Officer nor do I intend to change that position in the future.

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We will be on break for about ten minutes and then we will come back and try to move through this as quickly as possible.

(L.A. DWP Exhibits Nos. 99, 100, 101, 102, were admitted into evidence.)

(Whereupon a short recess was taken.)

HEARING OFFICER DEL PIERO: Ladies and Gentlemen, if you take your seats, we can start anew.

Ms. Cahill?

CROSS-EXAMINATION BY MS. CAHILL

Q Good afternoon. I'm Virginia Cahill. I'm attorney for the California Department of Fish and Game, and I have just a few questions for Dr. Herbst. I'll let you catch your breath and get the microphone.

Do you believe that the Mono Lake ecosystem is in a degraded state with regard to biological diversity

as compared to its pre-diversion condition?
A BY DR. HERBST: Yes. If you define "degraded" as a loss of species compared to that earlier condition.
Q And what do you base your conclusion on?
A The fact that there are organisms that were collected in the past at Mono Lake that apparently were fairly common, a couple of species of rotifers, and there were apparently others as we begin to examine

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more of the fossil evidence that may make it clear that there were more species in the lake at higher lake levels than we find under current conditions.
Q Given a sufficient reduction in Mono Lake salinity, do you believe it's physically feasible that some of those extirpated species might reestablish themselves in Mono Lake?
A Certainly.
Q And what would be the mechanism for them to rearrive at the lake?
A Well, there's several mechanisms of recolonization that organisms like the rotifers have or that certain kinds of algae have that may, in fact, be an important and rich contributor to Mono Lake, and that is through resting stages that can be deposited in the old lake sediments that are up on higher lake shores that can be reactivated, much like seeds when they get watered. They regerminate once they're exposed to water and favorable conditions for growth.
Moreover, there are other habitats that are around the lake basin that still support waters at lower salinities where these organisms still occur, and they could, by the action of wind, also be removed from those kinds of basins, either in this resting stage form or in living forms in water spray and be carried

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into the lake. We're talking about micro organisms here.
Q Yes. And do you have a recommended Mono Lake level which might allow the restoration of biodiversity at Mono Lake?
A Not specifically that's a recommendation for biodiversity, but my recommendation for the optimization of productivity at the lake would be elevations between 6390 and 6400. At those conditions, it should permit restoration of conditions that would allow the growth of many of these organisms that no longer exist in the lake.
MS. GOLDSMITH: Thank you very much, Dr. Herbst. I have no questions of the rest of the panel.
HEARING OFFICER DEL PIERO: Thank you very much. Mr. Roos-Collins is gone. We have no one here on his behalf.
Mr. Valentine?
MR. VALENTINE: Thank you. I take it Mr. Roos-Collins is taking my example that I tried to set last night.
HEARING OFFICER DEL PIERO: I think so.
CROSS-EXAMINATION BY MR. VALENTINE
Q Thank you, Gentlemen. My name's Mike Valentine, and I'm the Staff Counsel of the State Lands

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Commission. I have a series of questions which I believe still will be under 20 minutes.
Dr. Winkler, you were asked by Ms. Goldsmith a series of questions on historic suitability -- maybe it was Mr. Shuford who was asked, historic suitability of Paoha Island and whether or not at some point in the past that was the main colony. I don't want to get into that. I don't care about it.
My question is at the present time is Paoha suitable or not as a significant site for a gull colony?
A BY DR. WINKLER: All I can do is use the gulls as an indication of that and basically restate what Mr. Shuford said earlier and that is, if you look at the record in the 20th century, the big expansion of the Mono Lake colonies happened on the Negit Island and not on Paoha where at least for a large part of that time, it's been available in what would appear to be very similar to its present state.
Q And there are significant potential benefits, I take it, for the gulls for Negit Island over Paoha? If you agree with that question, I think you do, could you summarize them for us, please?
A I do think Negit is preferable habitat and as -- trying to interpret what the gulls see in that island
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that they may not see in Paoha, the biggest difference that I can see has been the presence on Paoha of coyotes over the years off and on and maybe more importantly or at least more constantly the fact that the substrate there is much lighter and much more easily moved around by wind than it is on Negit Island.
Q I believe you were also asked, Dr. Winkler, wouldn't it -- by Ms. Goldsmith, wouldn't it be better to provide gull habitat to manage the lake for gull habitat according to what the gulls need? Do you recall that series of questions?
A I think those were questions directed to Mr. Shuford, but I remember those questions, yes.
Q From what I interpret your last response to mean, rather than managing the lake according to what the gulls need, we should let them tell us; is that correct?
A Well, I think in the absence of anything else, the gulls are certainly the best judge.
Q Would either you, Dr. Winkler or Mr. Shuford, describe to the Hearing Officer the behavior of a coyote when it invades a colony to eat? What I'm getting at, does it go seize a prey, take it somewhere and eat it, or is there something more destructive that happens?
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A I guess -- I don't know if David's seen coyotes enter the colonies. I've seen them enter colonies at Mono Lake and Great Salt Lake. The typical behavior is that they come into the colony and if there are chicks about, they'll start grabbing and killing chicks and running about in the colony creating a great deal of disturbance around where they are. And basically,

then, eventually carrying a chick off, but oftentimes leaving other dead chicks behind.
Q Could you explain for us -- I think you have in several different points in your testimony, but could you -- and in one breath or two, explain to us the potential benefits that you believe scrub habitat has over other habitats at Mono Lake?
A I think that the scrub habitat in hot years provides the birds with shade and the developing chicks with shade, thus freeing up some of the adult's time to be out foraging. I should add that it's probably good for the adults as well to have a source of shade.
Q Mr. Shuford, you were asked questions about ticks and tick infestations. Is it true that some habitats at Mono Lake are more infested with ticks than others?
A BY MR. SHUFORD: Yes, that's correct.
Q Do you know where these are?
A Well, it can vary from year to year. There are

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certain islands on the Negit Island where we do our studies that traditionally have higher ticks, tick levels than other islands, Norway in particular is one.
Q And it has higher tick counts, I take it, than Negit?
A I couldn't answer that directly because I haven't measured the ticks loadings on chicks on Negit.
Q Ms. Goldsmith began her series of questions of you, Mr. Shuford, by asking about the relative security which can be afforded gulls or nesting gulls from coyotes. Do you recall those series of questions?
A Yes, I do.
Q I think your answer was while there was no absolute security, there's relative levels of security. Is that fairly reasonably accurate?
A I think that's correct. You can liken it to a castle. You pull up the drawbridge and build a big moot around it, and you're going to be a lot safer from attack than if you don't do that.
Q And the deeper and/or wider the moot is, the more security is provided?
A I think that's correct.
Q At some point, even though it's not physically impossible for coyotes to go there, the cost benefit analysis for them doesn't pencil out?

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A I think that is a driving factor. In other words, if the coyotes are going to go to an island, presumably they're going to do it for a good reason. And it is energy, you know, expensive to be swimming long distances in a lake and then getting back, particularly if that island can't support you for a long time.
Q In other words, if there's no fresh water on there, they can't stay?
A That's right. Most of the islands except for Paoha do not have a fresh water source.
Q So in that respect Paoha Island at the present time is not comparable to the other islands, is it?
A No. It's not and for that reason the coyote population has been able to maintain itself there.
Q Dr. Herbst, I have only a couple of short

questions for you. With regard to the Bradley (phonetic) report that was just recently admitted into evidence, as I understand that, based -- admittedly, and I think I understand on the most cursory readings of the summary, as I understand it, there is a difference between you and Dr. Bradley (phonetic) as expressed in that paper on the effects of salinity on body size. Did I get the gist of this right?
A BY DR. HERBST: That's right.
Q Despite that difference -- I direct to you Pages

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13 and 14 of the Bradley (phonetic) report. Do you have that there? If not, I have it.
A Yeah, I do. Okay.
Q Even though there is this difference to which I've alluded, Dr. Bradley (phonetic) obviously denotes that salinity has seriously negative effects on the life stages of the fly; is that correct?
A That's correct.
Q Back to you, I think, Mr. Shuford. It's been established, I think, that until 1982, the Caspian terns population at the lake at Mono Lake lived at Twain Island; is that accurate?
A BY MR. SHUFORD: I believe it is. Dr. Winkler can speak to that question better than I can.
Q Thank you. Please, Dr. Winkler.
A BY DR. WINKLER: Could you repeat the question just about the dates?
Q In 1982 where the Caspian terns nesting at Twain Island?
A If they were nesting anywhere, they were nesting on Twain. I just can't remember precisely where they nested.
Q Actually, I think I misspoke. They actually nested up to 1982. My belief is that they nested there until 1981.

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A I would have guessed that they would have left the island when the gulls did in 1982.
Q And the gulls left in 1982 because of?
A Well, the presumption is because the island had already been visited by coyotes late in the breeding season of 1981, and we think it was visited again by coyotes in the spring of '82.
Q I realize I'm hopping around here inexcusably. I apologize for it, but here I go. As to the heat loading for chicks, there were some questions about in certain years, scrub habitat would be important to gulls, especially hot years. Is that your testimony?
A That's correct.
Q Is it also true that during particular seasons of years, there would be more need for scrub habitat than in early seasons?
A Basically, before the eggs hatch, there's one bird incubating the eggs and that bird basically takes care of all the thermal regulatory needs of the developing embryos. Once the chicks hatch, that's when they begin to get out in the open world a little bit, and that's when shade starts to get important.
Q And the gulls don't all breed and nest and lay

their eggs and hatch their eggs in unison, do they?
A No. Mr. Shuford could probably comment more

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directly on what the span of egg laying is. My recollection is a lot cloudier than his, if you want those numbers.
Q Could you briefly -- I don't think we need hours and days, but --
A BY MR. SHUFORD: Well, I mean, the total period of egg laying is rather extended because some birds lay their eggs and then lose them. But most of the eggs in the colony are laid within a relatively short period of two to three weeks, and they usually start laying at the latter part of April. And then, you know, we would do nest counts during the later part of incubation, which would be in late May, so at that point, there are sort of more eggs being lost to predation and so forth than are being laid.
Q Would either or both of you agree with me that the importance of scrub habitat within an individual year would be more important for late breeders than it would be for early breeders?
A BY DR. WINKLER: I would say that's true if it is true that temperatures continue to increase from, say, late June up through late July. At Mono Lake, most of the birds are fledging around mid to late July, so it's only the late birds that would experience temperatures much later than late July.

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Q Thank you. Again, I jump back to coyotes. I apologize again. If -- Mr. Shuford, if a water barrier to islands is maintained and -- for a substantial period; that is, a period of years, would the number of coyotes attempting to visit that island tend to drop off?
A BY MR. SHUFORD: That's a hard question to answer. I've never really studied coyotes, per se. But I think, you know, it's a wide barrier in a long -- I think there's not a lot of likelihood they'll get to these islands. The record has really shown that the islands were close to shore and close to the land bridge, were the ones that the coyotes have visited.
Q The last question, even though it was a poor one, had in its background two assumptions which I would like you to agree or disagree with. And one is as distances -- water distances go up that the coyotes must travel, so does energy expended?
A That's definitely true. The longer distance you have to travel, the more energy you have to spend, muscle power and so forth.
Q And a second assumption. And even if a coyote had learned to travel to those islands at low lake levels, eventually it or its descendants would stop visiting the island at higher lake levels. Do you agree or

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disagree that?
A I think that would be very, very likely.
Q If we were going to compare brushy and white rock habitat, Dr. Winkler, wouldn't it be necessary for the -- both types of habitats to appear at the same

distance from the surface of the water?
A BY DR. WINKLER: Yes. That's certainly one of the variables that you'd want to control for.
Q So the slide show, Ms. Goldsmith's term, not mine, is not necessarily relevant to an actual comparison of what kind of habitat particular gull colonies would choose if given the choice and all other things were equal?
A That's exactly right and that's what I was trying to say in my first statement today is really to evaluate habitat choice, we need a very controlled set of comparisons where we look not only at distance from water, but also recency of the land bridge.
Q And also I believe you mentioned earlier that you had some photographs taken in 1976 of nesting gulls?
A Yes, I do.
Q At the risk of incurring the rath of Mr. Dodge, would you care to share those with us at this time?
A I don't have them with me. It wasn't until I was on the airplane yesterday that I realized that slides

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were being shown, so I could provide those through mail, if you'd like. I must say in anticipation of doing that, that I don't think I have any pictures of gulls on the ground at nests that are similar to what we've seen. What I do have is actual shots of the habitat area with gulls flying over it, and you'll just have to take my word for it that that's where they were nesting.
Q All right. Thank you.
Dr. Stine, I have one question for you. It was mention earlier that at certain lake levels, including one of the ranges we're studying here, the 6383.5 level, the Paoha Islets would disappear, would be under water and perhaps lower?
A BY DR. STINE: As the lake rises, the Paoha Islets, unlike the Negit Islets, will not only be submerged, but they'll be beveled back. So this is due to the fact that they are of a very soft easily erodible nature and so a rise in lake level not only submerges them, but more to the point, bevels them off. That is correct.
At the 6383.5 foot alternative, they would be completely lost. Indeed, as Dr. Jehl pointed out, at the DWP offered alternative, they would also be beveled off. It looks like we should probably not count on

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them in the long-term.
Q Can you tell me --
HEARING OFFICER DEL PIERO: Wait a second. I need to ask Dr. Stine a question that keeps coming to mind every time the Paoha Islets comes up. Is there a substrate in the area of either the Negit Islets or the Paoha Islets that provides habitat for brine fly?
DR. STINE: Yes. And Dr. Herbst and I have talked about this. The brine fly larvae and pupae, and he may want to -- may want to correct me there, but I believe it's both the larvae and the pupae, attach themselves to hardrock, including Tufa surfaces. Tufa forms and indeed rings the hardrock of Negit Island. Most of

Negit Island is indeed hardrock, so most of it is Tufa covered, and so we do have a fairly large amount of hard substrate there on Negit Island.

The substrate that we find on most of Paoha Island and indeed on the Paoha Islets, is more properly characterized as mud stone, and in the report that I did for the DEIR on substrate types, I conferred with Dr. Herbst and his sense was, therefore it shows up in the report, that mud stone is better than shifting sand substrate like sand that would move around in waves, but --

HEARING OFFICER DEL PIERO: But --

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DR. STINE: -- not as good as the hardrock substrate or hard substrate as we've chosen to call them.

DR. HERBST: Can I add something?

HEARING OFFICER DEL PIERO: Sure.

DR. HERBST: In the same sense that other submerged objects in the lake, like dead vegetation, particularly branches and woody material, can collect this mineral gaylussite and that will become transformed into calcium carbonate Tufa, in addition that there also happen to a certain extent on mud stone. I've seen some of the products around the shores of -- where you have this kind of crust, if you will, of tufaceous deposits that have obviously formed over some of these relatively solid, quote unquote, mud stone deposits. So they may also serve as a site for the formation of this mineral.

HEARING OFFICER DEL PIERO: Last question. The island, Dr. Stine, the unnamed island that you characterized as being exposed as part of the bridge in between Negit and the mainland, what's the material that that island is made of?

DR. STINE: That, too, is composed of up-arched lake sediments, but that has been beveled numerous times by previous, by natural fluctuations of the

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lake. Paoha Island is very young and therefore it hasn't undergone as much beveling, but that has been beveled down. It's the same sort of material, and it has some pumpice blocks on it. These pumpice blocks that floated to place about 1700 years ago, there's Tufa on the pumpice blocks but not on the substrate surface, itself.

HEARING OFFICER DEL PIERO: Does this exposure, because of the lower lake level, reduce the amount of habitat available for brine flies in that area?

DR. STINE: Yes, although --

HEARING OFFICER DEL PIERO: Okay. The answer is yes. Is it significant?

DR. STINE: It would -- it is not significant in that there are a number of other things happening around the lake as the lake drops, so it would be -- what's going on right there on the land bridge, itself, would not be significant.

HEARING OFFICER DEL PIERO: I'm sorry, Mr. Valentine. Please proceed.

MR. VALENTINE: No problem.

Q BY MR. VALENTINE: Dr. Stine, you mentioned that the Paoha Islets are fairly new. Can you give me a feeling for how long the Paoha Islets have been in existence this time?

A BY DR. STINE: Well, this is, indeed, the only time they have been in existence. The island formed approximately 300 years ago --
Q The main Paoha Island, the big island.
A That's the big island, and the islets themselves are sort of a chaos of material that slid off the flank of the island as the main island was coming in and not -- I guess it was approximately 1960 is when the Paoha Islets began to emerge from the falling lake. So they -- the Paoha Islets have been in existence since 1960, though the sediment has been there for about 300 years.
MR. VALENTINE: That's all the questions I have.
HEARING OFFICER DEL PIERO: Thank you very much. Mr. Valentine.
Mr. Gipsman, are you still here? Ms. Niebauer's gone. She indicated she had no questions. Mr. Haselton's gone.
Redirect?
MR. DODGE: Staff?
HEARING OFFICER DEL PIERO: I'm sorry. Mr. Frink, forgive me.
MR. FRINK: I've got a thick skin. Don't worry.
HEARING OFFICER DEL PIERO: I know. You have to around here.

MR. FRINK: I really just have a couple of questions.

CROSS-EXAMINATION BY THE STAFF

Q Mr. Shuford, you testified that it is likely the coyotes would return to Paoha Island if they were completely removed; is that correct?
A BY MR. SHUFORD: That's correct. That's based on the evidence in 1985. They were removed and they have returned.
Q Are you certain -- thank you. Are you certain that they were removed in 1985, or is it possible that some stragglers survived?
A I guess that's possible. The trapper that was out there made multiple visits to that island in 1985, told me they were gone.
Q Assuming that they could be --
HEARING OFFICER DEL PIERO: Was he paid?
MR. SHUFORD: He was employed by the U.S. Forest Service in Lee Vining.
Q BY MR. FRINK: Assuming that the coyotes could be completely removed and that the land bridging was not a problem, because it has not been any land bridging with Paoha Island, wouldn't it be less likely to have the coyotes return to Paoha Island than to Negit or one of the other islands that's near shore?

A BY MR. SHUFORD: It would be less likely in terms of getting there and how far it is to swim, but as far as getting there and staying there, they'd be much more

likely to stay on Paoha because of a combination of a prey sources other than gulls to keep them there year round, plus the water source.
Q And what's the first record of coyotes on Paoha Island?
A I really don't know the answer to that question.
HEARING OFFICER DEL PIERO: Does anybody?
DR. WINKLER: I remember a coyote seen on one of the early -- what do you call those? Multiple agency censuses. Might have been 1980, and I believe that was the first that people had recorded.
Q BY MR. FRINK: In view of the gull nesting that occurred on Paoha Island, would you assume that historically, prior to that time, that coyotes were not a problem?
A BY MR. SHUFORD: I think --
MR. DODGE: Objection. Vague as to time.
Q BY MR. FRINK: Prior to 1980 or prior to the first sighting of the coyotes when the gulls were on the island.
A BY DR. WINKLER: I think that it's pretty safe to say that when the McPhersons (phonetic) were on the island,

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that is the goat ranch, that if there'd been coyotes there then, they would have known about it. In between that time, I don't think we have any -- in between when the McPhersons (phonetic) left and when -- if I'm correct in thinking that 1980 or so was the first sighting, I don't think we had much information at all about what was on the island. But I think if there were -- if we take it as face value and it looks like -- if, indeed, in 1980 those were the first coyotes on the island, then, yes, if the coyotes were the problem, I would have expected them to have nested on Paoha in the interim.
MR. FRINK: That's all the questions I have. Thank you.
HEARING OFFICER DEL PIERO: Mr. Smith?
Q BY MR. SMITH: Thank you. I have a couple of questions about -- I don't want to beat this poor coyote to death like in the cartoons, but just a question of reference. Were there any times when Paoha and the Negit were both islands and you had coyotes on both?
A BY DR. WINKLER: Certainly not in my recollection.
Q No? Okay. This is a very simple question. This Board is charged with certain lake levels and certain restoration measures, if any, and are trying to balance

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some of the uses. At a level of 83.5 as you --
A Mr. Smith, I just want to clarify my last answer. In that answer, I assume that when you say when they were both islands, that they had a water barrier around them, that we would all accept as a sufficient water barrier to dissuade coyotes.
Q That was my assumption, too, thank you.
At levels of 83.5 or 6390 or 6400, would you expect the avocets to return? We heard some anecdotal testimony that there were avocets back in 1964 approximately.

A Well, there were avocets in the mid seventies as well, though their nesting status, my recollection is pretty fuzzy on that. But the only -- I'd have to look at what the shorelines were at those lake levels.
A BY MR. SHUFORD: The avocets do breed around the lake currently. Around the lake shore, itself.
Q Okay. But -- that answers that question, but there was a question and no one knew the answer to it at that meeting.
We've heard some testimony about formation of lagoons, I can't see -- I can't see Scott back over there, but I take it you still stand by that testimony, formation of lagoons at approximately 6400, 6405. These would be important for migration. We've also

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heard testimony that the higher lake levels would reduce the nesting habitat for snowy plovers.
Do any of you have an opinion in your pro -- a professional opinion about is this an even trade off?
A BY MR. SHUFORD: Well, I think it is correct that the amount of habitat for the snowy plover would be decreased at higher lake levels but at the levels were you talking about, 6383.5 and 6390, I don't think there'd be any decrease in the size of the snowy plover population. I've discussed this with Gary Page, who is, I believe, an expert on snowy plovers. I've also surveyed snowy plovers at Mono Lake, so I don't think at those lake levels there would be a reduction.
Q I'm sorry to break in, but I wasn't referring to those particular levels. That question was about 6400, 6405. We're assuming that we got to that level and assuming that the lagoons, important lagoons we've heard for migration were formed, but you lost a lot of, quote, a lot of snowy plover habitat. Would that be an even trade off, do you think, in your professional opinion?
A I think it would in terms of I think there would be a loss of habitat, but I think up until you really get to the no-diversion alterative, there would be enough habitat to support the current size of the snowy

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plover population.
A BY DR. WINKLER: To my mind, there's no trade off. But I'd like to be clarified. NAS 159, what lake level is that?
MR. STINE: That's 6419, 6420.
MR. WINKLER: If it got up that high, in my judgment, you'd probably have a diminution of habitat, but there's lots of habitat even at that level. There's still the basaltic ridges to the northeast. They're not the alkali flats, but they'll nest there. It's hard to say even at that level that we'd be out of habitat for the birds.
Q BY MR. SMITH: So your testimony is that you would have both excellent migratory habitat and we could still retain some --
A BY DR. WINKLER: A good chunk of habitat.
Q BY MR. SMITH: Habitat for the snowy plover. Thank you very much.
A By the way, I think -- I won't speak for him. But

Dr. Jehl said in his testimony, that pre-diversion levels there's plenty of habitat for snowy plover as well. So I don't think there's much debate on that.

HEARING OFFICER DEL PIERO: Mr. Herrera? Mr. Canaday?

Q BY MR. CANADAY: First for Dr. Stine. National

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Audubon Society/Mono Lake Committee Exhibit 200, which is the topographic profile Number Three entitled Negit, Java, and Twain,

A BY DR. STINE: Yes.

Q At what lake level does Java -- is Java covered by water? At 6377 it's not and at 6383.5 it is. Do you know approximately what lake level it does go under?

A It's a little bit more complicated than that because this, Mr. Canaday, would simply be the level at which Java is inundated along this transect. And in fact, Java Islet, if I may put up this exhibit NAS/MLC 230, Java Islet has a considerable range of elevations. On its western side, as I'm pointing to it on the exhibit here, we have a relatively low -- I do this.

Q Now, you can use that one so I can see it.

A Okay. That might be better. On Java Islet here, we have a fairly low protuberance over here so that this area of Java is under water -- in fact, the whitish portion that you see under here would all be under water at about 6383 feet, something like that. And then, of course, the higher portion of Java goes up to an elevation that I don't remember but I can get for you, if you're interested.

Q Yes, I am.

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A If you'll bear with me for one second.

Q While you're looking for that number, Mr. Shuford, what percentage of the gulls, at least in the past, have nested on Java Island? Do you have any records on that?

A BY MR. SHUFORD: Yeah. We definitely have records of what's nested on each island each year. The percentage in most years is not great. I couldn't give you an exact percentage.

Q Well, ballpark.

A The total population for the lake?

Q Yes, please.

A Well, the last time when there was a high count there, there were about a thousand nests, so that's 2,000 birds relative to the 60,000.

Q Okay. Dr. Stine?

A BY DR. STINE: Approximately, the high point there would be somewhat over 6420 feet. With the roughly a quarter of the island area shown on Exhibit 230, roughly a quarter of the island being over 6410 feet.

Q Thank you.

Dr. Winkler, as I understand your testimony earlier today, you were, in fact, the -- one of the researchers who discovered terns nesting in the Mono Basin?

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A BY DR. WINKLER: Yes. I can't remember if it was I

who actually saw them first. Certainly, somebody in my crew.
Q And that was in what year?
A 1976.
Q '76? And they were nesting on which island?
A May I just back up? I think Dr. Jehl introduced a citation of an account in 1963. I'm not sure where that came from, and I think he referred to terns in that, so that may be the earliest. But as far as we've known, traditionally '76 was when he found them.
Q And they were nesting where, Sir?
A On Twain Island. The place I indicated.
Q And it's your opinion that the lake levels that this Board is considering that the -- the terns will not be impacted, or if they're nesting habitat is, that there are other places in the basin that they will have available to nest?
A I don't think there are other places in the basin, the Mono Basin, that they could nest, other than island in the lake. My impression is that as long as there is gull nesting habitat on Twain, there will still be habitat -- yeah.
A BY DR. STINE: A point that might clarify that, Mr. Canaday, is that where Dr. Winkler pointed out

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terns nesting, stands at an elevation of about 6415 feet. So they're aways up there and presumably that's tern habitat.
MR. CANADAY: That's all.
HEARING OFFICER DEL PIERO: Thank you. Redirect? Will you keep him under control?
MR. FLINN: I do my best, but I think we left his medication at home.
REDIRECT EXAMINATION BY MR. FLINN
Q Dr. Herbst, Mr. Moskovitz asked you questions about the membership of the Mono Lake Committee and are you now or have you ever been one. Which came first, your scientific study of Mono Lake or the Mono Lake Committee as an institution?
A BY DR. HERBST: My scientific studies.
Q And did the things that you observed in the field influence you with respect to whether or not you would provide information to the Mono Lake Committee?
A Yes.
Q Could you explain why?
A The fact that I had been collecting information at the lake and it was clear to me at the time that there were things that were relevant in terms of my desire to have them be public knowledge and my desire for that information to be shared with people who were

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interested in Mono Lake; that is, I would say the main thing that influenced me to interact with the Mono Lake Committee and provide information to them when they were interested in a particular natural phenomenon that they were observing at the lake, whether it had something to do with lower lake level or higher lake level or the ecology of insects or what have you, and I was only to happy to be able to provide that kind of information to them.

Q Now, at the time you began your study in the 1970s, was DWP funding scientific research in Mono Lake at the levels they did in the 1980s; to your knowledge?
A No.
Q In fact, wasn't it correct that during this time, all of the research done at Mono Lake was done by independent non-funded scientists like yourself?
A That's right.
Q Now --
A BY DR. WINKLER: Could I just -- we were funded by NSF, but it was independent of L.A.
A BY DR. HERBST: But after that, Dave --
Q In fact, didn't there come a time that you yourself were funded by somebody else in this room besides the Mono Lake Committee?
A That's right. There were several years the

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Department of Water and Power funded me.
Q And you did those studies under your contract with the Department of Water and Power?
A That's correct.
Q Did there come a time at which that funding stopped?
A Yep.
Q Approximately, when was that?
A Well, I last conducted research for the Department of Water and Power in 1991 through the auspices of the work that was done for State Water Board.
Q Apart from the Water Board process --
A For the EIR.
Q Leaving that aside --
A I believe it was 1989 or '90. Possibly '90.
Q And when did you first testify in court as a witness called by the National Audubon Society and the Mono Lake Committee?
A That was the fall of 1990.
Q Now, Mr. Moskovitz showed you, and it was introduced into the record, L.A. DWP Exhibit 100, a Bradley (phonetic) report. I noticed in there that the most recent study cited in terms of the references is like a 1988 or 1989 study. Is it your recollection that this document dates from the 1989-1990 era?

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A That sounds about right, yeah.
Q Now, at the time, Dr. Bradley (phonetic) was one of the scientists DWP was funding; is that right?
A That's right.
Q Is Dr. Bradley (phonetic) -- has he ever told you whether or not he still gets funded by the Department of Water and Power?
A From what he has told me, he no longer gets funded from the Department of Water and Power.
Q Now, Mr. Valentine pointed out a couple of things, but if you could just follow along with me on Page 10 on this 19 -- Exhibit 100, do you have that? The Bradley (phonetic) report?
A Okay. Page 10.
Q And what may well have been one of Dr. Bradley's (phonetic) last works for the Department of Water and Power, does he not conclude that there was a

significant negative effect of salinity on hatching success? Starting at Page 10, results?
A That's right.
Q The same thing for larval growth?
A That's correct.
Q The same thing for pupation success?
A That's correct.
Q The same thing for pupal weight?

A That's correct.
Q The same thing for survival to the adult stage? Looking at Page 11?
A That's correct.
Q Now, I want to turn, briefly, to the subject of vegetation, and this is perhaps both to Dr. Stine and Dr. Herbst. Mr. Moskovitz, at some length and with some degree of interest, seemed to wonder whether or not, Dr. Herbst, you sitting here had at your fingertips data available on vegetation levels at various lake levels. Do you recall that, Sir?
A I recall that.
Q Now, do either of you have any information as to whether or not that data is, in fact, available to the Water Board?
A BY DR. STINE: Yes. I helped doctor, I believe, Dr. Joquerst (phonetic), James Joquerst (phonetic) of Jones and Stokes prepare maps of the shore land vegetation that surrounds Mono Lake as part of the DEIR, and my auxiliary report was then the basis for a comparison that he did between previously existing vegetation and present day vegetation surrounding the lake. So it is in there. I believe he not only includes that information in the DEIR, but I believe he, if I remember correctly, he wrote an additional

auxiliary report on that question of modern day distribution of vegetation around the lake.
Q And from this data, can one provide the specific quantification, the facts and the figures that Mr. Moskovitz was interested in?
A I think one could. I have not done that. I don't have it at my fingertips either. But it is in the DEIR, in any case.
I do have at my fingerstips, a map that was produced and is actually included in the Corey report, but it was produced by the State of California, State Lands Commission, and State Attorney General's office as part of the State of California versus U.S. Federal Government over ownership of land surrounding Mono Lake back in 19 -- I think it was '83, '84, and that, too, was looked at by -- by Mr. Joquerst (phonetic) and that provides me with some basis for estimating, in a general way, where the vegetation is and in and approximate densities as they existed in 1982.
MR. FLINN: What we would like to do, Mr. Del Piero, is that's our only copy right now. If it would be agreeable to the present parties, is to mark that an our exhibit next in order, but we will make copies and have them distributed tomorrow.
HEARING OFFICER DEL PIERO: That's fine. What

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number?
MR. SMITH: Should be 231.
HEARING OFFICER DEL PIERO: Sounds right?
MR. FLINN: Yes, that does.
MR. HERRERA: Would you identify that specifically for our records?
DR. STINE: That is map with the title Vegetation of the Exposed Lake Bed --
MR. DODGE: Excuse me. The 231 is already marked. I have marked my own -- for my own purposes, something as 236, so I would --
MR. SMITH: 236. Excuse me. You're right.
MR. FLINN: We'll give that 237 because Mr. Dodge has a 236.
MR. SMITH: 237.
HEARING OFFICER DEL PIERO: The map will be 237. Please identify it, Dr. Stine.
DR. STINE: The map is titled Vegetation of the Exposed Lake Bed and Adjacent Lands of Mono Lake. I included it as Page 98 of my report to the Corey panel, Community Organization and Research Institute panel, and the name of this thing is Geomorphic and Geohydrographic Aspects of the Mono Lake Committee. I'm the author, and it's dated August 1987.
Q BY MR. FLINN: Thank you.
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A BY DR. STINE: I said of the Mono Lake Committee?
Q What does it really say? Does conspiracy reach higher and farther than we all can possibly imagine?
A I can say proudly that I've never been a member.
Geomorphic and geohydrographic of the Mono Lake controversy, excuse me.
HEARING OFFICER DEL PIERO: Same here.
DR. STINE: We're both non-joiners, I suspect.
Q BY MR. FLINN: Also on the subject of vegetation, is there photographic evidence, I think either you, Dr. Herbst, or Dr. Stine, that you've got relating to the existence of the kind of vegetation that could be inundated as the lake fluctuates at higher levels?
A BY DR. HERBST: Yes, I do have a photograph that would bear on that.
A BY DR. STINE: I have many, and they're not along.
Q You've got the one there in front of you. If you don't, we'll move on. All right. We'll move on.
Let me ask you, Dr. Stine, Dr. Herbst was shown exhibit, I think Fish and Game, 99 that picture from the Condor article. Dr. Herbst was asked if he saw any vegetation in that particular article. Could you give me an estimate, Dr. Stine, from what you can actually see in that what percentage of the total Mono Lake shoreline is depicted in that one single photograph?
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Would it be fair to characterize it as a fairly small percentage?
A BY DR. STINE: I would say order of magnitude a 10th of a percent, perhaps.
Q Would it be particularly reasonable to draw conclusions about the availability of the vegetation lake wide from the tiny bit of Mono Lake shore shown in

that photograph?
A No. Not only because of the small amount of lake shore shown, but very clearly, in this photograph, the lake is rising and it's cutting a cliff, so it's undercutting whatever vegetation is there as the lake rises. So this would not be a good place to find -- it's not a place where you would expect to find any vegetation being preserved on the lake.
Q One final question on the subject of vegetation, Dr. Herbst, is what makes it tough for these plants to live in Mono Lake the salinity?
A BY DR. HERBST: That's correct.
Q And at salinities of approximately 50 grams per liter, are there plants that will survive at that level?
A There are some macrophytes that could survive. Ruppia martima is an aquatic macrophyte that I found growing in Big Soda Lake out in Nevada which has a very

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similar chemistry as an alkaline water lake to Mono Lake, and under those kinds of salinity conditions, Ruppia grows quite well and, in fact, serves as a substrate for the attachment of alkali flies at that lake.
Q Dr. Herbst, could you spell that for the record?
A Capital R-U-P-P-I-A and specia maritima, M-A -- small M-A-R-T-I-M-A.
Q Now, Dr. Herbst, I want to turn the subject to Exhibit -- L.A. DWP Exhibit 101, which is this chart from your Ph.D. thesis, and I want to put back up Exhibit 52-A because Mr. Moskovitz drew some comparisons between microcosm studies and this and then some internal comparisons.
Let's talk about the validity of drawing comparisons from the microcosm studies and the sampling data. First of all, are the microcosm studies. Was what you were interested in the microcosm studies the absolute body size figures or the relative changes in body size figures depending upon salinity?
A Certainly, the relative body size figures is what I was most interested in. The only valid way really of comparing experimental effects in this kind of a situation is one to another, not conditions in the experiment to conditions someplace else in nature or in

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other experiments. Really, it's the relative effects within the context of the treatments in that particular experiment that tells you how things respond.
Q Now, some of your predecessors sitting at this table have drawn -- with some emphasis the importance of research scientists as opposed to other kinds of scientists. I want to focus on the methodology of science briefly.
Is the concept of control -- controlling for variables something that is involved in the scientific method?
A I'm sorry. Can you run that past me one more time?
Q Is the concept of controlling for variations, if you're trying to study salinity effects, for example,

you want to control for everything else so that you know what you're observing is the effect of salinity?
A That's right.
Q And does the need to control for all these other effects involved in why you would not, as a matter of fundamental scientific methodology, not compare field data subject to all kinds of variations with the microcosm data?
A That's correct. Although, if you did want to make that comparison, if you do want to make a comparison

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that Mr. Moskovitz was trying to get at here, looking at this field data for the flies, really what you need to do to make a valid comparison is to make a valid seasonal comparison. As you can see, the main thing that's going on in this graph here -- I'm not sure everybody's got it at hand here.
HEARING OFFICER DEL PIERO: We've all got it.
DR. HERBST: -- shows that there is a very significant variation in body size from being at a maximum during spring months to being about at a moderate level during the mid summer months, and then towards the late summer and the -- into the late summer period, the body size decreases to a minimum size and, in fact, the flies that emerged from the microcosm tanks emerged from flies that were developing during month of all August. So if we want to compare flies that are emerging from the microcosm tanks during month of August, we should go to the field and look at flies that are emerging from the field conditions that developed during the month of August as well.
When we do that, what we ought to be looking at in that particular graph is those flies that emerged during the month of September, and when we do that, we see that the data for Mono Lake shows that in September of 1983 and September of 1984, the body size of those

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flies was 4.4 millimeters which, at the field conditions of 100 grams per liter, is identical to the 4.4 millimeters we observed in the flies emerging from the tanks at 100 grams per liter. The only exception to there being a minimum body size in that particular month for both lakes was the one observation of flies from Mono Lake in September of 1982, and I can only chalk that up to the fact that sometimes data is variable, but the rest of the data are very consistent with regard to the body size being minimum in that month.
Q BY MR. FLINN: Now, the other thing Mr. Moskovitz I understood was asking you about was the apparent absence of a trend from 1982 to 1984 as the lake level rose and salinity decreased. Do you recall that testimony?
A BY DR. HERBST: Um-hum.
Q Now, a predecessor of yours sitting at this table, a research scientist, I understand, testified on November 9th, 1993, at Page 22, quote, he testified as to, quote, a very strong warning to any management decision that you can't make a decision based on 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's changing." The

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scientist, research scientist,, who gave this opinion who is Dr. Melack. Would you agree with Dr. Melack that one should not draw any kind of trend conclusions from your 1982 to 1984 data?

A Not a year-to-year comparison. Some of it is -- but not for yearly comparisons, correct.

Q Now, finally, if you could get out exhibit -- I hope this in too confusing, 64, your comments on the Draft EIR on Page 5 had your graphs.

A Which page?

Q Page 5.

A Okay.

Q And I believe Mr. Moskovitz asked you about the upper right-hand graph that had algal growth and there are three bars, three sets of data, mixed Tintoclatus and the microcosm data?

A Yes.

Q And then he asked you very briefly something about the mixed algae conference -- mixed algae data, and there was

that I had the Reporter mark and would could find it. But I got the impression you wanted to say something more about that. Do you recall that particular line of questioning?

A Yes, I do.

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Q And did you want say something more about that?

A I did want to elaborate a bit.

Q Please do.

A Is there anything I can draw on here?

Q There is, and Dr. Winkler trod the path for you. In fact, we'll even steal his blue pen.

HEARING OFFICER DEL PIERO: We have a green one there. It's more seasonal.

DR. HERBST: This is perhaps a little lesson in how to do experiments with algae. When I did the experiments that are documented in that upper -- upper right-hand figure that shows --

MR. MOSKOVITZ: Excuse me. Could that be marked, please, as an exhibit?

MR. FLINN: Let's mark this as 238?

MR. SMITH: Yes. I'm on the right page now. 238.

DR. HERBST: When you're doing experiments with algae, what's really desirable to do is get an idea of what the full growth curve is. So if this is the total amount of algae that's in any particular culture and this is the time over which it's growing and these are different treatments, in this situation, my suspicion is that the curves on this end are lower salinity and the curves on this end of graph are higher salinity,

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that, in fact, what you need to do is have data from the early part of this time series to ascertain whether or not there are differences in growth rates that are affected by these different low or high salinity treatments.

In fact, what I did in these experiments is harvest algae after a prolonged period of growth, after things had reached a stationary growth period. And I don't want to discount the fact that the algae did eventually achieve the same level of growth. They did achieve the same biomass. Nonetheless, when you have a natural system, rather than a laboratory system where things become limited because they they deplete nutrients, which does in the necessarily happen in nature, that what, in fact, you want to know about the intrinsic growth rate of the algae relates to these parts of the growth rate functions, and that's data that I didn't collect in those particular experiments and which I am in the process of collecting right now in further experiments with benthic algae from Mono Lake.

So I just want to clarify that my interpretation of those results being fairly flat over the salinities has to do more with when I harvested the algae than it being true differences between the intrinsic rates of

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growth under different salinities.

Q In that experiment, which is depicted on Exhibit 238, did you, in fact, include the effects on the algae of the grazing by other organisms and a larger ecosystem?

A These experiments?

Q Yes.

A No.

Q Did you do that in the microcosm experiments depicted on 52-A?

A Yes.

Q And could you tell us that once you included the effects of the other organisms, what did that do, then, when you included the effects of algae?

A The overall community effects show that with increasing salinity, there is a dramatic decline in the overall biomass of algae, which includes a variety of different species as you increase the salinity, and that occurs despite the fact that no doubt there's much higher rates of grazing going on in these low salinity tanks because you can see there's a legion of flies emerging from this particular treatment. There's no question about the fact that the larvae have to be grazing a lot harder at this salinity than they were in these other tanks where there's very much reduced

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survival and very fewer flies were able to emerge.

MR. HERRERA: Mr. Flinn, your time has expired.

MR. FLINN: If you could have five more minutes?

HEARING OFFICER DEL PIERO: Go ahead. Fine.

Q BY MR. FLINN: Finally, I wanted to ask some questions about nitrogen fixation and the interrelationship between that and what might happen at the higher lake levels. Now, Dr. Stine, as I recall, you've already testified, I believe, about marsh lands and brackish waters that might exist at the higher lake levels above 64 -- 6405?

A BY DR. STINE: I wouldn't say "might." I'm quite confident that they will. The topography is there, and

there's going to be water in it.
Q Now, Dr. Herbst, assuming the existence of the ponds and marshes and lagoons that Dr. Stine is sure will be there, could you tell us how that might affect nitrogen fixation?
A BY DR. HERBST: Yes. Recent reviews done of the ecology of the nitrogen fixation in aquatic ecosytems, one in particular done by a scientist named Hans Pearl (phonetic) suggest that the most important habitats for nitrogen fixation really worldwide are in these kinds of latoral edge marine, latoral edge lake situations where there's extensive marshland, where there is a

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great deal of decomposition of vegetation, stagnant water situations, anoxic sediment buildup, and under those conditions, you have the perfect set up for these anoxic requiring nitrogen fixing organisms to grow and, in fact, that's where you find a great deal of nitrogen fixation occurring. And it's from these coastal, if you will, whether it's on a lake or on an ocean, sources that there's a huge contribution of external nitrogen to those kinds of aquatic ecosystems. Very true in estuarian situations and certainly true in fresh water lakes that have marshland borders.
Q We don't have to -- I don't want to go to the trouble of putting up the overhead again. You recall Exhibit 65-A which is your graph of nitrogen fixation of salinity and area effects?
A Yes.
Q I notice that there's no data point between 6390 and 6420 but there is a decline drawn on that chart. I'll give you another one.
A I've got it.
Q Is that solely due to an area effect as opposed to a salinity effect?
A Primarily.
Q Okay. And is that because you have -- a change in the slope of the lake affects the latoral area; is that

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right?
A The break-in slope in the lake occurs above 6400. I'm sure Scott --
Q You're two steps ahead of me. All I'm saying is the reason for this decline, this area effect, is as you get to higher lake levels, there is a change in lake slope that gives you less latoral area?
A That's right.
Q You don't have any data points between 6390 and 6420. If you did, where would this graph peak?
A The reason I didn't do that was I had experiments running at 75 and 50, so I simply graphed those. But yes, if you assume that the rates of nitrogen fixation are equal between 50 and 75, and they are equal at 50 and 75, so I think that's a fairly safe assumption, then you can redraw the curve as follows. I'll just flip this up here quickly.
If you redraw the curve, it would look something more like this where because of the fact that you don't get that inflection to stabler lake shores until you get to about 6400. In fact, this maximization of the

potential for nitrogen fixation lake wide should, in fact, be more or less a plateau between 6390 and 6400.
Q If you could take your seat again, you can leave that up there.

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One final set of questions and this is really addressed, perhaps, to the panel as a whole because it crosses disciplines. But it starts with this vegetation and assuming that these -- at higher lake elevations, you have the cycle of inundation and exposure of vegetated areas and the vegetation used as a substrate.
First of all, Dr. Herbst, am I right that the pupae fare more likely to be dislodged from this waiving vegetation substrate as opposed to the hardrock?
A That's correct. The drag forces associated with that are such that there's a much higher likelihood that it would be dislodged.
Q What would you expect to happen to these pupae if they're dislodged?
A Well, typically what happens is that they float up to the water surface once they're off their point of attachment, and they typically then form large floating rafts and then move in towards the lake shore with any kind of wave activity. And once they're cast up on the shore, there's a high probability of mortality, but in addition of that effect on the pupae, themselves, those large floating rafts of pupae are an important food source to birds, from my understanding from talking

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with Dr. Vega (phonetic).
Q Now, let me move on to the bird folks briefly. Assuming that instead of these fly pupae with their arms firmly wrapped around the rock substrates, instead floating around in these wind droves and these large mats, do either of you have an opinion as whether the food source would be more accessible to birds at these higher lake levels?
A BY MR. SHUFORD: I think it definitely would. I've spent numerous days around the lake and observed gulls on really windy days concentrating on the shoreline right where the waves are washing in -- given these higher lake levels, there's less at attachment of these pupae that would be washed in and birds would definitely take advantage of a situation like that.
Q The final question to Dr. Herbst. Is there historical evidence that there were larger masses of these flies and wind droves that are no longer present today?
A BY DR. HERBST: I have recently seen a photograph that I believe indicates that.
Q Is that in your stack in front of you?
A It is indeed.
Q Would you identify it, please?
A This is NAS and MLC 39, Nellie Carter on the north

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shore of Mono Lake collecting alkali fly pupae. Lake elevation 6398.4. And this color photograph shows Jessie Durant (phonetic) along the shore apparently in

an area where there's a lot of Tufa formations and right down next to the shore are very large black masses. And in her basket, she has a pile of these black masses.

One thought that crossed my mind is well, those black masses could be adult flies and frankly, this Seahart Miriam (phonetic) photograph that has been passed around so frequently here, I'm not very impressed by because we don't know if those are pupae or adults. There are places along the lake shore where today you could photograph adults with those kind of densities, and so I find it unimpressive.

However, the pupa wind droves, I find very convincing, very compelling. She could not be holding a basket full of live flies like that. They'd obviously fly away.

HEARING OFFICER DEL PIERO: It depends on what else is in the basket.

DR. HERBST : Looks like pupae to me.

Q BY MR. FLINN: The conditions depicted on that Exhibit No. 39.

HEARING OFFICER DEL PIERO: She is Mr. August

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Hess' grandmother, I understand.

Q BY MR. FLINN: Yes, and that document was admitted into evidence at her testimony. Do those exist currently at the lake today?

A BY DR. HERBST: I've never never seen pupa wind droves at Mono Lake like that.

MR. FLINN: Thank you, Sir.

MR. DODGE: I'll try to go through this as quickly as I can. I'm basically going to follow Ms. Goldsmith's questions.

RE DIRECT EXAMINATION BY MR. DODGE

Q Doctor -- excuse me. Mr. Shuford, you told us that in 1979, Negit Island was basically abandoned by gulls due to coyotes, correct?

A BY MR. SHUFORD: That's correct.

Q And that represented approximately two-thirds of the population at that time, correct?

A That's right, two-thirds of the entire population of gulls at Mono Lake.

Q Then you gave what I thought was a little bit -- at least you responded to what I thought was an ambiguous question, but it didn't occur to me until a couple of questions later. You said that in 1982, 30 percent of the gulls were dislocated to Twain. What did you mean by that?

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A Well, 30 percent of the entire lake's population was displaced when coyotes got across to Twain and Java.

Q But 100 percent of gulls left Twain, correct?

A Correct.

Q Now, Dr. Winkler, you gave some testimony about the marauding aspects of that abandonment of Twain. Can you -- and you were asked questions about the gulls just standing there on Twain. Can you expand on how that relates to marauding?

A BY DR. WINKLER: Well, the reason I didn't see why it

related directly to marauding is that under this marauding hypothesis, the gulls may well roost or stand around on an island that doesn't have nesting gulls on it. All the marauding hypothesis requires is that when they are foraging, they're foraging in other parts of the colony on eggs and chicks, and we did see chicks being eaten by gulls in other parts of the colony during that year. And that's consistent with this hypothesis of this snowballing effect.
Q So the marauding doesn't take place on the island that's been evacuated, correct?
A That's the source of the marauders that start this whole thing going.
Q They maraud on other island where gulls are

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attempting to nest but there are no coyotes.
A Right. I mean, if you put yourself in the webs of one of those gulls standing on Twain Island, going to get the eggs and chicks of another gull is not an activity without risk and so they can't just roost in the middle of colony. So if they're not actively foraging, one wouldn't expect them to be roosting actually in the active colony.
Q Back to you, Mr. Shuford. Ms. Goldsmith next asked you a question which elicited from you an opinion that in 1990, there was a large increase in the gull population at Mono Lake. But you said, and I believe I'm pretty close to quoting you, it was not an effect of what happened at Mono Lake. Can you expand on that?
A BY MR. SHUFORD: Well, since we've started our studies of really accurate counts of nests at Mono Lake in 1983, population was quite stable varying from about 45,000 to 49,000 adults from '83 to '89. And all of a sudden in 1990, the population in one year shot up to over 60,000 birds. And there is no clear reason at Mono Lake why that would have happened in that year, and the corresponding increase in the Great Salt Lake in that exact same year from a population of approximately like 78,000 to over 130,000 in that year

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indicates to me that there's something going on, you know, in a broader region that might have affected the gulls and the increase in the population.
And this was during this six-year drought, some of these birds could have been moving from other colonies to these sites. So I think there are other factors.
There's always other factors going on that affect these populations at this particular site. I see no reason to explain that increase by any phenomenon that happened at Mono Lake.
Q Dr. Shuford was asked questions about ticks, and there was some testimony about Negit Island being large and therefore densities being potentially lower. Let me ask you, first, Dr. Winkler, in terms of Negit Island as you observed it in 1976, how broadly disbursed were the gulls?
A BY DR. WINKLER: Well, it's certainly clear that the densities were much lower than they have been on some of the Negit Islets recently, especially Twain. And

so, you know, it's clear that it was much lower density than we've seen in recent years.
Q Can you characterize where you found the birds on Negit Island in 1976?
A Yes. I believe there are some maps somewhere that have to be somewhere in the testimony of other

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witnesses having to -- or sorry. It's in the Draft EIR. The birds were basically in two habitats. They were up on the grease wood, in the grease wood, or in what we call white rock, the Tufa cemented rocks and gravel along the shore that had recently been exposed. The great bulk of the birds was up in this grease wood habitat.
Q Assuming just on an order of magnitude, I'm not asking you to assume unless you know it, that Negit Island in 1976 consisted of 250 plus acres, can you give the Hearing Board any estimate as to how many acres contained nesting gulls?
A Somewhere between 20 -- somewhere around 30 percent, probably 30 to 40 percent. That's just a guess. Scott probably has --
A BY DR. STINE: You don't remember this, but I asked you that question, and you sent me a map. And so I plotted it out and planimetered, and it was about 50 percent of the island.
Q Let me ask you that, Dr. Stine, we've had some questions about the comparative size of Negit Island versus the Paoha Islets. Let me ask you, let's take a lake elevation because I understand the sizes change with different elevations. Let's take 6380. Approximately how large is Negit Island?

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A Negit Island would be about 250 acres at that point.
Q And at that same elevation, how many acres do the Paoha Islets consist of?
A I think it's about 30, but I'd like to check that, if I could. Give me a second here. I'm sorry. I'm off. It's about 12 acres. About 12 acres total. So 12 versus 250.
Q At elevation is 6380?
A Yes.
Q And then the Paoha Islets disappear at approximately what elevation?
A The Paoha --
Q Islets.
A Disappear. Well, at what level would they be completely beveled off? I believe it's at about 6388 feet they would be completely beveled at that point. The lake would -- if the lake did drop then, these now flattened beveled features would re-emerge, but I think that Dr. Jehl even made it quite clear that they would not be reoccupied. I think he used the word either irrelevant recoverable or irreparable changes or something like that.
Q Now, going back to you, Dr. Winkler, in focusing in on 1976 and Negit Island was still an island then,

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correct?

A BY DR. WINKLER: Yes.
Q And there were a lot of birds there?
A Yes. A very impressive sight.
Q You counted approximately 33,000 nesting birds?
A At the time of the census, yes, that's what we estimated.
Q And can you tell us approximately what percentage of those were in the scrub habitat as opposed to the rock habitat?
A I could double-check, but I think it was something -- well, certainly over half were in the scrub. To be any more accurate than that, I'd have to -- I'd like to double-check the estimates. Actually, I could probably get it from -- is it in here? It's about two-thirds.
Q And did you also look at the Negit Islets in 1976?
A Yes, we did.
Q Now, let me ask you to assume that Negit Island becomes a secure island on a long-term basis. Do you have an opinion as to whether large populations of California gulls would return to Negit Island for nesting?
A Yes. I think they would return there and eventually build up to numbers that rivaled their

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previous numbers.
Q You mentioned that there was a heat problem in 1981 that basically killed the -- almost all of the chicks. Was Negit Island an island used by gulls in 1981?
A No, it was not.
Q It had been land bridged at that time?
A Yes, it was land bridged in '79.
Q Now, you gave testimony in response to Ms. Goldsmith. She was asking you about brushy versus white rock habitat -- and it might have been Mr. Valentine. I'm not sure which. It was Mr. Valentine according to my notes -- and how you would make an assessment, and he asked you about distance from water, and you mentioned also the recency of land bridging was important. Can you expand on that?
A Right. I'm glad you asked that because it didn't come out when we were looking at Joe's pictures earlier, but I would be surprised if you showed me an island where -- which had been land bridged and had scrub on it where the birds immediately recolonized the scrub once the island had been reisolated.
I certainly agree with Joe and with David that these birds feel insecure when they can't see around

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themselves, and they're in a new habitat. And I think -- my own opinion, and I think we probably have disagreement right here at the table, but my own opinion and interpretation of what I've seen says that these birds need to gradually feel secure in these habitats in order to start colonizing habitats in which there's limited visibility around them.
My point is once they come to occupy those habitats, that they will then be better off if they

encounter one of these years of high temperatures and they may well be better off in all years.

The frustration in all of this discussion of habitat quality for all these years is that we've never been able to make a comparison between how well birds do in scrub habitat at Mono Lake and how well they do in open habitats at Mono Lake, and not until Negit island is reisolated and isolated for quite some time do I think -- will we ever be able to make that comparison.

Q And, in fact, during the four or five years in the 1980s where Negit Island provided some breeding habitat for gulls, as you said, relatively few returned to the scrub habitat, correct?

A That's right, and it does not surprise me.

Q And the reason for that is?

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A Again, I think that in order for them to occupy those habitats, they would have to be -- put in it anthropomorphic terms, they would have to feel secure enough, having nested there in past years, that there wasn't going to be a predator coming around the corner to interrupt their nesting or endanger them, for that matter.

Q Just a couple more questions. In response to a question by Dr. Smith, there was some questions about Paoha and coyotes first spotted there in 1980? Do you recall that Dr. Winkler?

A Yes, I recall those questions yes.

Q I have just a simple question. According to your historical research, when did California gulls last nest on Paoha?

A Well, they've nested a nest or two intermittently while we've been studying the birds intensively, but in terms of successful nesting of any numbers of birds, I believe the last was in 1919.

Q 1919?

A I believe that's correct.

Q And do you have an approximation as to when the -- when the goat farm was taken off the island?

A I've only the vaguest recollection, maybe David can help me out, but I thought it was in the late

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twenties.

A BY MR. SHUFORD: I don't know the exact date. The McPhersons (phonetic) were only out there a very short period of time. I would think it was in the early twenties.

Q And there was a period of time from the time the McPhersons (phonetic) left until 1980 where no substantial nesting took place on Paoha. Do you have an opinion as to why that is so?

A BY DR. WINKLER: Again, my personal opinion, and again it may not even be the unanimous opinion at the table, but it seems to me that the different substrate types have a big effect. Let me just say that if you live out on Krakatoa for long periods of time in the summer, which I've done and David's done, whenever the wind comes up, you see a big dust cloud coming off of Paoha and you see no dust coming off of Negit. They

definitely are different substrates and the wind definitely affects them differently. And I can't imagine that gull parents are enthusiastic about having their chicks buried in dust.
Q Is there a difference of opinion at the table?
A BY MR. SHUFORD: Not regarding that factor, no. I think that would definitely influence the suitability of Paoha for nesting.

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Q Is it also possible that were coyotes out there from the 1920s to 1980?
A The historical record is so limited I don't think we really know for sure. I mean, surely it's possible, but the McPhersons (phonetic) did not observe coyotes out there while they were there.
Q Okay. Now, with all due apologies to everyone and particularly you, Dr. Winkler, I previously identified -- misidentified DFG Exhibit 101, so I have now marked what I thought was DFG Exhibit 101 as National Audubon Society and Mono Lake Committee Exhibit 236, which has your pencil marking as to where you found Caspian terns in 1976. Does everyone have a copy of Exhibit 236? Mr. Cane is handing those out. It is the same as what I previously misidentified as DFG Exhibit 101.
Dr. Stine, you've seen this pencil mark and you stand by the proposition that Dr. Winkler found the Caspian tern in 1976 at approximately 6415 feet?
A BY DR. STINE: That's correct, yes. And I say approximately 6415 plus or minus two or three feet. Sure.
A BY DR. WINKLER: Can I clarify one thing?
Q Sure.
A I have to emphasize that 1976 is a lot of field

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seasons ago for me, and as I said earlier I wasn't even -- I can't even recollect whether it was I or one of my crew who actually found the terns nesting there. What I based that circle on is most vividly based on recollection of the 1979 field season, so I am extrapolating because at the time I didn't think that the birds had moved. So that's all that I wanted to do to clarify.
Q So let me ask you hypothetically, Dr. Winkler, assuming that Dr. Stine is right, that the Caspian terns in 1976 were found at an elevation in excess of 6400 feet, let's just take that, in excess of 6400 feet, Dr. Jehl has testified that at elevations in excess of 6386, the Paoha Islets will be inundated and the Caspian tern will no longer have any habitat at Mono Lake in excess of -- that is, at elevations in excess of 6386. Do you agree with that?
A I can't agree with that. No.
Q Would you expect the Caspian tern to simply return to Twain?
A I would. I certainly agree with Dr. Jehl that the gulls give the terns a hard time and that if gull densities were sufficiently high, that terns may not nest there. But certainly the physical habitat is there, and I would expect them to at least try nesting

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there.
Q Okay. Final question to either of the ornithologists. Assume that -- assume that Dr. Herbst is correct, that higher -- as I understand his testimony, it's very much a layman's understanding, but as I understood Dr. Herbst's testimony, that higher lake levels, you're going to have lower salinities and bigger, fatter alkali flies, although I go back far enough so that I always call them brine flies, and I get chided for that. But assuming that they're bigger and fatter, can you -- at higher lake elevations and lower salinities, can you tell us how that would affect them as food for birds?
A I think there's little question that if they were bigger and fatter, they would be better prey and that the birds would have a higher rate of return per unit of investment in foraging.
Q Mr. Shuford?
A BY MR. SHUFORD: I would agree with that characterization. The more -- the better food out there, the more it's going to benefit all the birds that depend on those food supplies.
MR. DODGE: No further questions. Thank you.
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Dodge. Mr. Moskovitz?
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MR. MOSKOVITZ: Are we going to be taking a break so we can remove our cars from the garage and not have them kept there overnight?
HEARING OFFICER DEL PIERO: Yes. The garage closes at seven, and I had anticipated breaking right around ten to seven. Frankly, it's going to depend on the nature of your recross and you, Ms. Goldsmith. Do you have an idea as to how much time you all are going to take?
MR. MOSKOVITZ: I would be finished before ten to seven.
MR. DODGE: Could I ask that any party having questions for Dr. Winkler pose them so that he can make his departure?
HEARING OFFICER DEL PIERO: Is there anyone else besides Ms. Goldsmith who has questions for Dr. Winkler?
MR. MOSKOVITZ: Mrs. Goldsmith tells me she will not have any.
HEARING OFFICER DEL PIERO: You have no questions for Dr. Winkler and Mr. Moskovitz has none for him.
MR. MOSKOVITZ: Not for Dr. Winkler.
HEARING OFFICER DEL PIERO: Anyone else have questions for Dr. Winkler? Mr. Canaday?
MR. CANADAY: All it is is a clarification. This
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Exhibit 236 that Mr. Dodge willingly took the blame for misrepresenting was, in fact, my fault. I misrepresented it to him as DFG 101.
MR. SMITH: And I did it to Mr. Canaday.
HEARING OFFICER DEL PIERO: We've all confessed.
MR. DODGE: I wasn't aware of any of that. All I know is Mr. Cane gave it to me, and it was wrong.

HEARING OFFICER DEL PIERO: We've all confessed now, except for Mr. Cane.

Okay. Well, Dr. Winkler, why don't you just sit tight until you have to leave? In the meantime, Mr. Moskovitz, why don't you begin, Sir?

MR. MOSKOVITZ: I wonder whether it would be permissible to have the short break that you had in mind for ten minutes to seven and take it right now, and then we'll return and we can get our cars out and so forth.

HEARING OFFICER DEL PIERO: That's fine. That's more than adequate. So we'll take a break for ten minutes then.

(Whereupon a short recess was taken.)

HEARING OFFICER DEL PIERO: This hearing will again come to order. Mr. Moskovitz, if you'd be kind enough to indulge me for just a moment, I have to tell you all a story because this story was related to me

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this afternoon during the course of the proceeding.

Mrs. Forster who, as a number of you noted, was here this afternoon, and she came in, One, to participate to the hearing to the extent that she had time available today, and Two, to pass some information on to me. As a number of you know, I called about the temperature in the room here this morning and the people I called were actual -- I actually called the Executive Director of the State Water Resources Control Board and happened to get the Chairman of the Board and the exec in the same office at the same time and told them about the problem with the temperature in the room over the speaker phone.

So the Chairman of the Board, who is a career state employee and who handles state employees better than anybody on the face of the planet, apparently called the Assistant Director of General Services for State of California and advised him that there was a room full of very cold attorneys in a hearing room all of who were going to sue the state for not providing them with an appropriate hearing room in which to conduct business, and as a result they were going to allege that their cases had been compromised, and that the primary witness on their behalf was going to be the Hearing Officer.

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Witness three different repairmen who have come through here during the course of the last five or six hours in order to make sure that the Department of General Services didn't get sued. So some things work in strange and mysterious ways.

Mr. Moskovitz, I don't think we'll be cold anymore in this room.

RECROSS EXAMINATION BY MR. MOSKOVITZ

Q Dr. Herbst, I wanted to ask you some questions about your relationship with the Department of Water and Power, the funding of studies. First of all, your funding was for short-term projects. Is that not so?

A BY DR. HERBST: That's correct. It was not for longer than a single year, I don't believe.

Q And you completed those projects, did you not?

A That's correct.
Q Now, did DWP ever put any limitations on you as to how you could use the results?
A No.
Q Are you familiar with the work that Dr. Melack's team did regarding shrimp bioassays?
A Yes.
Q And did that work not show that there were some salinity effects on shrimp?
A Yes, they did.

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Q And was there any limitation, as far as you're aware, put on Dr. Melack and his team from using the results of those studies?
A Certainly not.
Q And Dr. Melack's funding continues for his long-term project; is that not so?
A At the present time, as far as I know.
Q Do you know whether any limits were placed upon Dr. Bradley (phonetic) for the results that he had?
A I'm sure they were not.
Q Now, with respect to Exhibit 64, that's the comments on the -- comments on the Draft EIR that you prepared, and turning again to Page 5 where we have these graphs.
A Okay.
Q And in particular, the one about algal growth.
A Okay.
Q Upper right-hand corner.
A Okay.
Q Now, the mixed algae portion of that graph shows in effect that between 50 and 150 there was no change.
A That's correct.
Q Now, I think you said that -- on redirect, if I'm correct, you correct me if I'm wrong, that yes, at the conclusion, there was no change because eventually,

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this algae all was able to grow but the growth rate was different.
A That's correct.
Q Now, do you have any information about what the growth rate is after a certain period of time with respect to algal growth at different salinities?
A Yes, I have done some experiments. In fact, the experiments from which this particular result is derived did have some studies done with it in which I did try to do harvests at earlier time intervals than the point at which they achieved that stationary growth phase.
Q And didn't those studies show that after -- after three days, the growth rate for algae at 50 grams per liter was faster than the growth rate for algae at 100 grams per liter?
A Yeah. They do show that. Although, those results, I think, are fairly difficult to interpret because of sample size.
Q But they did show that for a short period of time --
A Yeah.
Q -- the growth rate as 50 was more rapid than at

100?
A That's correct.

Q But didn't that work also show that after five days, the growth rate at 100 caught up and exceeded the growth rate at 50?
A Um-hum. That's also true.
Q So both in terms of growth rate and in terms of the eventual biomass, the algae at 100 did as well except for the first three days or better than the algae at 50?
A That's true.
Q Now, looking at the curves on Page 6 of that same exhibit, I think you said a number of times that salinity is the most important factor in affecting what happens to alkali flies.
A That's correct.
Q Now, if salinity is the most important factor, how is it that, for example, looking at the -- at the result of the two models in the middle of Page 6, that you have a decline as you get to higher elevations where the salinity goes down?
A Um-hum. Well, as I stated in my direct testimony, I think what is the case for those particular models, those central models you're pointing out, the Kimmerer-Herbst model and the Jones and Stokes model, is that they're conservative with regards to their estimating the impact of salinity at those high lake

level, low salinig conditions because of a number of important different factors which were not incorporated into the model.
Q One of the factors you mentioned was vegetation you thought would provide more habitat.
A That was physical habitat. I'm speaking specifically with regard to the effect of salinity on growth rates and life history characteristics. The beneficial effect or stimulatory effect, if you will, of low salinity conditions on the growth rates of the larvae, their size of maturity at pupae and adults, and their reproductive success. That information was not incorporated in the fashion that I think it needs to be incorporated into this kind of a model, thereby I think this model errs on the conservative side in terms of underestimating the potential stimulation at high lake levels and low salinities, the physiological effects of salinity.
Q Are you saying that if those were plugged in, you would no longer have this bell-shaped curve, and it would no longer show the bell shape and -- well, no longer show the fact that at higher lake elevations, you approached the same kind of effect as at the low elevations that you presently have?
A Well, since we don't really have that data to

generate such a model, we don't -- we haven't done that kind of a simulation. I don't know if that would be the case, but I suspect that it would be case. You'd no longer see the bell-shaped distribution.
Q This is just something that you have speculated on

and don't have anything to pin it to in terms of specifics?
A Sure. I can pin it to the results that come out of those microcosm experiments. I think those are very compelling results which suggest that the effect of salinity can override just about any other factor.
Q Now, turning to your microcosm of experiments that you just referred to, did they not show in terms of size of flies at 50 grams per liter, flies that were certainly no larger than the flies in nature that you found when you gathered flies in the 1980s?
A That's right.
Q Does that cause you -- should that not cause you some concern about the design of and the reliability of those microcosm studies? Why should they not reflect nature?
A Can I take a moment to look at the results of the studies?
Q All right.
A What I was doing here was just trying to check and

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see whether or not, in fact, the predictions from the microcosm tanks indeed show that there is a larger body size in the experiments at 50 grams per liter compared to those at a hundred grams per liter from the field. And indeed, there's really no direct trend here. It's difficult to say, although if we look at the August development times for the flies that are emerging from the tanks, once again, compared to the August development times for flies that are emerging from the field conditions, indeed, the flies from the tanks are quite a bit larger, 4.75 compared to 4.4. So indeed they match the prediction that we would expect there.
Once again, I'd really still like to emphasize that the interpretation of these microcosm experiments as with the interpretation of any experiments really needs to be done in the relative context of the influence within those sets of treatments. Because we have no way of making the absolute comparison between tanks and nature.
So indeed, we don't know that we're getting an exact replication there, but I would add that this is about the most realistic kind of experimental manipulation you can get for ecological systems, and it's a very widely used technique now for trying to simulate conditions in nature without going out and

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actually experimental manipulating with the environment itself.
Q Were there some confounding effects that came out of the microcosm exhibits --
A Indeed there were.
Q -- experiments?
A Indeed there were. Would you like me to explain?
Q If there are confounding effects, don't they indicate that there may be something questionable about the reliability or the usefulness?
A Not at all. In fact, the factors that were confounding and the microcosm experiments were indeed that there were higher concentrations of nutrients at

the higher salinities. And as a confounding factor, it makes it difficult to say whether or not -- well, was it high salinity or high nutrients that had the impact in terms of reduced productivity which you see in terms of flies and algae over here. I suspect there would not be very many people, very many ecologists that would be willing to say that it was higher nutrient concentration, and by "higher," we're talking about micromolar concentrations of ammonia, that could have had that sort of toxic effect.

What instead, I think, these confounding effects do is reinforce the conclusion that indeed there are

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stimulatory effects by low salinities and adverse effects at high salinities because if those nutrients, which were confounding, having high concentration, had been able to offset things by stimulating productivity, they would have done so. Instead they didn't. So instead of the confounding nutrients being a problem, they, in fact, reinforce our conclusion that even with higher nutrient concentration at the higher salinities, they still have a very debilitating effect on productivity of the flies and algae.

Q Let me look at L.A. DWP Exhibit 101. That's the one you referred to that was produced in your Ph.D. dissertation?

A That's right.

Q That showed what you collected during the early 1980s?

A Right.

Q Shows the sizes. Are you saying that the field collected flies were about 4.7?

A Field collected flies for the same --

Q 4.4? Excuse me? 4.4?

A For the same period of time that flies are emerging from the experimental microcosm tanks and you have to take into consideration that seasonal correction because you can see here there's pronounced

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variation seasonally. You have to look at those flies that are emerging in the month of September. If you look across the bottom of the chart, you see the two 30s, the one that's sort of on the middle, the ones that's sort of on the right, the lowest down on the chart? Do you see those?

Q Yes.

A Those are the bottom ends of the ranges of the two groups of field-emerging flies that would be emerging at the same period of time, the same month that flies would be emerging from the microcosm tanks. So what I'm telling you is that the microcosm tanks at 50 grams per liter have the salinity that we see under these circumstances here. Those flies emerging at the same period of time were 4.73, 75 millimeters in length, so substantially larger than those flies that are emerging from the field under conditions of 100 grams per liter.

Q And then don't you have to ignore the size that you collected in September of 1900 and '82 to come to that conclusion?

A I must agree with that. That's an anomalous

point.
Q Well, is it anomalous or are the others anomalous?
A I believe that's the anomalous point because if you look at the Aberglec (phonetic) data, just above it

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you can see that the minimum body size also coincides in each year with the month of September. So I can only conclude that in those five out of six data points in which I have that information, that one data point there is an anomalous bit of information.
MR. MOSKOVITZ: I have no further questions.
HEARING OFFICER DEL PIERO: Thank you very much, Mr. Moskovitz. Ms. Cahill?
MS. CAHILL: No questions.
HEARING OFFICER DEL PIERO: No kidding. Mr. Valentine --
MR. VALENTINE: Should we express our appreciation, Mr. Del Piero?
HEARING OFFICER DEL PIERO: Don't express it yet. You have no questions, Mr. Valentine?
MR. VALENTINE: It depends on what I hear in response to the questions Mrs. Goldsmith asks.
HEARING OFFICER DEL PIERO: Mrs. Goldsmith's done.
MR. VALENTINE: I'm done, too, then.
HEARING OFFICER DEL PIERO: Mr. Frink?
MR. FRINK: No questions.
HEARING OFFICER DEL PIERO: Mr. Smith? Mr. Herrera, we aren't going to wait until Canaday gets back.

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You Gentlemen are excused with my greatest appreciation. Thank you very much.
Any cleanup?
MR. FLINN: Yes, we have some exhibits. I don't know about Mr. Dodge, but I'm ready to offer my exhibits. We would offer Exhibit 1-G, the summary testimony of Dr. Herbst or his written testimony. We would offer 49 and 50, 52, 52-A, 64, 65, 65-A, 66, 66-A and 66-B, 201, 201-A, 202, and 203.
HEARING OFFICER DEL PIERO: Okay. Do I hear any objections to the introduction of those exhibits? None? So ordered. Mr. Dodge?
(NAS/MLC Exhibits Nos. 1-G, 49, 50, 52, 52-A, 64, 65, 65-A, 66, 66-A, 66-B, 201, 201-A, 202, 203, were admitted into evidence.)
MR. DODGE: I have an offer also which I can state faster than Mr. Flinn's.
MR. SMITH: Please don't.
MR. DODGE: Through Dr. Winkler, I offer our exhibits and these are all our Exhibits 1-A-E, 231 through 236. Through Dr. Stine, 1-U, 198 to 200. 142-A and B. Through Mr. Shuford, 1-P and Exhibits A and B thereto.

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HEARING OFFICER DEL PIERO: Any objections to those? Mr. Flinn, you aren't allowed to object.
MR. FLINN: I wasn't going to object.

HEARING OFFICER DEL PIERO: Hearing none, those will be ordered into the record. What do you have there, Sir?

(NAS/MLC Exhibits Nos. 1-A-E, 231 through 236, 1-U, 198, 200, 142-A, 142-B, 1-P, 1-P-A, 1-P-B, were admitted into evidence.)

MR. FLINN: Exhibit 238 from Dr. Herbst we will provide eight and a half by 11 copies for record for every one of the exhibits that were drawn on. 235 and 238.

HEARING OFFICER DEL PIERO: Any objections to those? Those are ordered into the record.

(NAS/MLC Exhibits Nos. 235, 238 were admitted into evidence.)

HEARING OFFICER DEL PIERO: I already admitted into the record all of those submissions by Mr. Moskovitz, right? I didn't miss any, right? Did we get them all, Andy? Mr. Frink?

MR. FRINK: Yes. I have a little bit of

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scheduling information that everyone may be interested in. Assuming that they arrive, the first witness tomorrow will be Dr. Quinn of MWD. And then, we were also planning on having a panel on Tufa issues and state land issues. Mr. Dodge and Ms. Scoonover are jointly working on it, I believe, in the afternoon.

MR. DODGE: And I believe the Forest Service is planning to join that panel.

HEARING OFFICER DEL PIERO: And I think that you may -- I can't be positive of this now because of the things going on with the Sacramento/San Joaquin/Bay/Delta issue and the listing of the winter run salmon as endangered as of today as opposed to threatened, but it was my understanding, with the exception of Mr. Brown, who's been very ill, that all of the other Board members were to be here tomorrow to listen to the testimony in regards to Tufa, also. So it's going to be a full house here tomorrow afternoon, I think. Although, that remains to be seen given the fact that EPA has now listed winter run as endangered.

8:30 in the morning. Anything else?

Have a nice evening, Ladies and Gentlemen. This hearing is recessed until tomorrow morning at 8:30.

(Whereupon the proceedings were adjourned.)

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REPORTER'S CERTIFICATE

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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 378 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: December 15, 1993

IN WITNESS WHEREOF, I have subscribed this certificate at Sacramento, California, on this 4th day of January, 1994.

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