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Public Hearing

STATE WATER RESOURCES CONTROL BOARD

DIVISION OF WATER RIGHTS

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Subject: Phase I of the BAY-DELTA ESTUARY HEARING	Impacts of Freshwater Inflow on San Francisco Bay

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Held in
Contra Costa Water District Board Room
Concord, California

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Wednesday, December 9, 1987
8:00 a.m.

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W I T N E S S E S

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INDEX OF EXHIBITS

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WEDNESDAY, DECEMBER 9, 1987, 8:00 A.M.

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MR. MAUGHAN: We can continue with the hearing commenced in Concord on the Bay-Delta, impacts of freshwater inflow on San Francisco Bay.

What we have left, according to my recollection and my notes, is Romberg Tiburon Center for Environmental Studies.

Dr. Herz, I understand that Dr. Rozengurt is ill today and will not be available, and whoever else they have they would like to submit -- Mr. Thomas, I think you are the attorney involved here. Any time you are ready to go, Mr. Thomas, proceed.

MR. THOMAS: Good morning, Mr. Chairman. I am Gregory Thomas, appearing for the Romberg Tiburon Center for Environmental Studies.

As I make introductions, Mr. Chairman, perhaps I can have one of our associates pass out an errata sheet reflecting changes in the Romberg Tiburon Center Exhibit No. 20.

I regret to inform you, as you have noted, that Dr. Rozengurt, the principal author of our Exhibit No. 20 was taken rather seriously ill on Monday night and is in the hospital and won't be able to appear today.

We will be somewhat handicapped in responding in detail, perhaps, to some of the methodological features of the report, but we will do the best we can and we are certainly prepared to permit additional detail for the record, if requested on cross-examination.

MR. MAUGHAN: I think Dr. Herz knows all that's in there.

DR. HERZ: Thank you.

MR. THOMAS: That remains to be seen. I hope you are right.

Then, let me have you state your name for the record.

DR. LEOPOLD: Luna Leopold. I am professor emeritus of Geology and professor emeritus of Landscape Architecture at the University of California, Berkeley.

DR. HERZ: Michael J. Herz, Senior Research Scientiest, Romberg Tiburon Center for Environmental Studies, San Francisco State University.

MR. MAUGHAN: Have these witnesses been sworn?

MR. THOMAS: I believe they have not.

(Thereupon Romberg Tiburon Center witnesses were sworn.)

MR. THOMAS: At the same time, Mr. Chairman, let me just briefly introduce both of these witnesses by giving a brief resume of their professional qualifications.

Dr. Leopold, as you know, is professor emeritus of

Geology and professor emeritus of Landscape Architecture at the University of California at Berkeley. Dr. Leopold's experience in hydrology spans a good many years, including over ten years as the chief hydrologist for the U. S. Geological Survey in Washington, D. C. From 1956 to 1966, Dr. Leopold remained in the U. S. Geological Survey as the senior research hydrologist until 1972, when he joined the faculty at the University of California at Berkeley. Dr. Leopold holds numerous degrees. He has a Bachelor of Science in Civil Engineering from the University of Wisconsin, a Master's Degree in Physics Metereology from UCLA, and a Ph.D. from Harvard in Geology.

In addition, many honorary degrees have been conferred upon Dr. Leopold for his published work on water and the general field of geomorphology. These honorary degrees include Doctor of Science Degree from the University of Wisconsin, Doctor of Geography from the University of Ottawa in Canada, an honorary Doctorate from the University of Mercia in Spain, and an honorary Doctorate of Science Degree from the University of St. Andrews in Scotland.

Dr. Leopold is also an elected member of several of the most prestigious scientific societies in the world, including the National Academy of Sciences. Notably no other hydrologist is a member of the National Academy of

Sciences.

He is also a Fellow of the American Academy of Arts and Science, where again, he is the only hydrologist.

He is a member of the American Philosophical Society which was founded by Benjamin Franklin. It has only 500 scholars, and again, Dr. Leopold is the only hydrologist.

He has served as the President of the Geological Society of America.

There are many other honors and awards that have been conferred upon Dr. Leopold, and they are listed in the Tiburon Center Exhibit 27, as are most of the publications he has authored in his field of expertise. These number five books in the field of hydrology and 145 published scientific papers.

Dr. Herz obtained his doctorate from the University of Southern California. He is a specialist in environmental management and public policy of coastal and estuarine resources. He is currently the Director of the Bay-Delta project for the Tiburon Center for Environmental Studies of the San Francisco State University.

He serves on numerous Boards of Directors and advisory committees as listed in Tiburon Center Exhibit No. 25. A list of nearly 100 publications and presentations by Dr. Herz are also listed in that exhibit.

MICHAEL HERZ,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. THOMAS:

Q Dr. Herz, speaking of the Romberg Tiburon Center, I wonder if you could just state briefly what the Tiburon Center is and what it does?

A The Romberg Tiburon Center is a research facility of the San Francisco State University and as such it is the only research and teaching facility in the bay area that is dedicated to looking at the health of San Francisco Bay.

Q Dr. Herz, are you one of the principal authors of the Romberg Tiburon Center Exhibit No. 20 entitled "The Role of Water Diversions in the Decline of Fisheries of the Delta San Francisco Bay and other estuaries?

A I am.

Q Would you please, briefly, describe the purpose and design of that study?

A The basic purpose was to investigate the relationship between levels of freshwater outflow from the Delta and populations of fish that we know to be good indicators of the overall biological health of the estuary.

The study was designed to identify the most significant correlations for further examination to develop why changes in the inflow affect fish populations. We did not attempt to exhaustively examine these mechanisms. Our purpose was to identify flow levels that need to be maintained in order to assure that fishery resources of the estuary are protected while more definitive studies of these mechanisms are conducted.

LUNA LEOPOLD,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. THOMAS:

Q Dr. Leopold, are you familiar with the analysis of hydrologic conditions and year-to-year changes that are presented in the Romberg Tiburon Center Exhibit No. 1?

A I have studied it.

Q And you prepared a re-analysis of the data contained in that report?

A Yes, I did.

Q Is your re-analysis Romberg Tiburon Center Exhibit No. 22 entitled "Sacramento-Delta Water Supply and Review of the Tiburon Report"?

A Yes.

Q And is your re-analysis in fundamental agreement with the conclusions contained in the Tiburon report?

A Yes.

Q Would you explain in simple terms what about this report and about the general problem are your main impressions?

A When you are dealing with water supply problems, one is most interested in the years that are dry and, therefore, one must go beyond the question of average values, and the Tiburon report is correct in that it is wise to examine the range of values particularly when one deals with runoff, and, therefore, with regard to the general approach that Tiburon report took in arranging the values in the order of magnitude and plotted them as a frequency diagram, I think that is a correct way to do it.

I, therefore, felt that I wanted to see other kinds of relationships, so I took the annual values of runoff from the Riburon report and re-analyzed them in my own way, but again, using the general procedure that the Riburon report used, which was to deal with the data as a frequency analysis.

Q All right. I believe that analysis is presented in Figure 1 from your report. Perhaps we could display that at this time.

Dr. Leopold, would you explain what this figure shows?

A Yes. Plotted on probability paper, I plotted the four types of data. They represent annual values of runoff to and from the Sacramento Delta and the four sets of data include, first, the computed natural inflow and the natural outflow for the whole period of record, and then, the data were tabulated for the regulated inflow and the regulated outflow. These data for each of the four sets of data, in this type of analysis one arranges them in the order of magnitude and plots them as a probability statement.

Now, note in these kinds of plots, if one looks on the bottom scale at the No. 50, this means that 50 percent of the points are larger and 50 percent of the points are smaller.

In the case of these particular data, the median represented by the 50 percent point also happens to be very close to the average, the arithmetic average. This is not always true.

Now, the thing that is striking about this set of data is that under natural conditions inflow to the Delta and outflow from the Delta are practically the same, but after the regulation upstream and the diversions within the Delta occurred, then the numbers are much smaller.

For example, looking at the 50 percent point here, we see that the natural inflow and outflow in this period of record averaged about 25 million and the 50 percent point under regulated conditions has dropped down to something like 18.5 million.

Now, the water supply problem that we are concerned with is what happens in the dry years, and one must note that if you look at the 80 percent point even under natural conditions, 20 percent, which is 80 from 100, 20 percent of the annual values, 20 percent of the years had a flow under original conditions of approximately 15 million; in other words, very much smaller.

So, that the reason that the probability curve is useful is because you can see how many years out of 100 or how many years out of 10 the result of the flows are smaller and those are the flows that we are interested in.

And so, the conclusion is reached that we have already 20 percent of the years under regulated conditions where the outflow is less than or approximately equal to or less than only 10 million acre-feet.

Q To be clear, what do you mean when you use the term "natural unimpaired flow"?

A Those were simply the data that were presented to me, the data which were tabulated in the Tiburon report, which I understand came from the Department of Water Resources.

Q These are flow levels that were experienced without the operation of the state and federal water projects?

A That's my understanding, yes.

Q And when you refer to regulated outflow or regulated inflow, you are speaking of flow levels experienced with the operation of those projects?

A That is correct.

Q Okay. Now, what are the implications of this analysis if you were to extend it to the year 2020, Dr. Leopold?

A Well, there is a set of data recently furnished to me that represent the estimate of what the annual values would be at the year 2020. The one that I analyzed is the outflow from the Delta for the year 2000, not 2020. Those data show that the estimated outflow from the Delta at the year 2000 -- note, first, that the average outflow comes out to be in the order of 12.5 million, but notice that the 50 percent point is different than that. Fifty percent of the years estimated at 2020 would have a flow of less than 9 million acre-feet. So that in this case, the average value looks much larger and appears to give you more water than when you look at the 50 percent point, meaning that at the year 2000 it is estimated that 50 percent of the years would have an outflow from the Delta of less than about 8 million acre-feet. When you start looking at what you call critically dry years, then you find that the critically dry years are a large part of the total number of years if the estimates of 2020 conditions are fulfilled.

MS. LEIDIGH: Mr. Thomas, is this overhead in the exhibits that we have?

MR. THOMAS: I believe that this is not in the report. We simply produce it as a way of illustrating how this analysis would look if it were extended beyond the period reflected in the figures.

MS. LEIDIGH: Would it be possible to provide us all, including the audience, with copies?

MR. THOMAS: Did we bring copies of that with us? I suspect that we can do so at the next break, and in the meantime, if you would like, we can have it marked for identification with an exhibit number.

MS. LEIDIGH: Yes.

MR. TAMBLYN: That would be No. 31.

MR. THOMAS: Let the record show this is Tiburon Exhibit No. 31 for the record.

(Romberg Tiburon Center Exhibit No. 31, overhead graph, was marked for identification.)

MR. THOMAS: Q Dr. Leopold, you were alluding to the increasing frequency of dry and critically dry years with the operation of the project. I wonder if that can be more easily understood if we look at it in tabular form?

A I would prefer to do that because it is clearer.

Q Let me have you refer to Figure 2 from the report.

A Using the definitions of years which might be called wet, abnormal, subnormal, dry and very dry that actually was presented by the Department of Water Resources, I have compared using the frequency data the unimpaired inflow to the Delta shown in this column here, unimpaired, compared with regulated flow. Now, this represents the percentage of years and that means how many years out of 100. The ones that we are worried about in water supply problems are the dry years, so the most important line here is the change in the number of critically dry years as a result of regulation.

Now, this chart represents the total record available to me, 1921 to 1982. It is not a forecast of what is going to be in the future. Already the regulated flow has increased the percentage of years which would be called critically dry from 14 to 39; in other words, that the number of critically dry years at the present time has already been doubled as a result of the control, the diversions and the controls upstream, and since it's a water supply problem, it's the change in the number of dry years that is of greatest importance.

Q The classification that you reflected in that chart, Dr. Leopold, did that come from the Department of Water Resources Bulletin 23-62 and 130-70?

A Yes, but then, I made a comparison of that particular classification with the classification that was actually in the water rights Decision 1485 in which, as I understand, the classification of years into various categories is based on flows from the Sacramento River alone; in other words, Sacramento valley, rather than the total watershed area.

But when you compare the relationship between the classification in 1485, in Decision 1485, and the one that is used here in this classification, they come out to be practically identical, and particularly, they are the same with regard to the critical years. They differ slightly in the definition of subnormal, normal and high, but no matter which of the definitions you use, the Department of Water Resources or the water rights Decision 1485, the definition of the critically dry years is identical.

Q Dr. Leopold, your report seems to indicate that this change in the frequency of low flow periods as experienced in the bay itself has been progressive through the year. Could you explain that referring to page 6 of your report?

A Yes. When you tabulate the difference between inflow and outflow; in other words, the diversions, the natural outflow less the regulated outflow; in other words, the change you find, and that could be an estimate of the depletion -- in other words, the difference between the regulated and the natural outflow values from the Delta, the depletion started in the decade 1921 to 1929 to be a depletion of only about 3.7 million acre-feet approximately, increasing until the period 1980 to 1982 that it jumped from 3 million to nearly 13 million, so in other words, there has been a progressive change in this value of depletion defined by natural outflow from the Delta less the regulated outflow.

This is a depletion figure, increased each decade starting in the 1920s with the value of about three to four million and increasing to more than 12 million in the last decade.

Q Having made this effort to demonstrate the gradual withdrawal of water and as a consequence a decrease in outflow from the Delta, apparently you think this is important. Could you explain what the importance of this analysis is?

A Well, if there has been a progressive depletion, what we have done, and any further depletion will further the tendency to increase the number of years which are critically dry, no matter how defined.

Q What are the consequences of that, in your judgment, for the salinity levels experienced in the estuary?

A Well, quite clearly the position of the null zone is dependent in part on the outflow from the Delta and, therefore, as the outflow of freshwater from the Delta progressively increases, you can expect salinity is going to gradually move upstream, the null zone is going to move upstream and salinity values will probably also increase with time.

Q Do you view average measures of salinity as being the salient measure, or are you more concerned with the salinity levels experienced during these low-flow periods?

A Well, I think that you have to recognize that average values are useful, but not the whole story. One of the things that you can say about salinity data is that the variance is very large, large changes occur from season to season and from year to year, and therefore, one has to study the variance, if you like, of salinity values.

More than that, let me say that the way we measure salinity or tend to generally measure salinity is taking samples out of the surface or the upper part of the flow, and that is not necessarily what we want to know, because the intrusion of saltwater tends to move along the bed of the river and, therefore, it would actually be better if we had measures of the variation of salinity from the surface down to the bed.

Q Are there reasons related to the biological health of the bay that lead you to believe that these changes in flow levels are important?

A Well, it's quite clear that the biological diversity of the whole ecosystem in such an estuary has developed over a long period of time under conditions of natural inflow and outflow, and quite clearly that equilibrium is going to be disturbed in one way or another when one deprives the system progressively of the freshwater under which it developed.

Q I gather that after you finished your report, you were shown reports that had been prepared by the experts of the State Water Contractors, one of which had to do with the computation of the so-called natural flows to the Delta? Did your review of that report call into question the data that you have just been describing on the levels of historic flows and how they have changed with water development?

A Well, that report ended up by giving simply an average value. That average value computed indicated that the changes wrought by man have increased the total flow into the Delta by twice. I think that that figure is so out of line with all the data that we have in hydrologic direct measurements of the effect of water yield from altering the vegetation, that it appears unreasonable to me.

Q Now, one of those direct measurements was made by the U.S. Forest Service in their experiments with reducing vegetation.

A Well, you see what this report purports to show is that the change of vegetation can increase the water yield. The U.S. Foresty Service has had for a long time experiments trying to demonstrate that changing the nature of the forest would increase the water yield, and so, large amounts of data from varous parts of the country are available. These data show again and again that the largest increase in water yield ever obtained by the change of forest cover was in the order of 15 to 20 percent, and that increase did not last more than five years, and that's the reason that I believe that a computation shows that the water yield has doubled is unreasonable in relation to the data available to us.

Q You are also familiar with experiments conducted in the State of Arizona by the U. S. Geological Survey; are you not?

A As a matter of fact, I started that investigation. The water users in Arizona had felt that by cutting down the vegetation on the pinon juniper zone in the mid elevations of Arizona, they could increase the water yield for the irrigation of water supply.

The Geological Survey set up an experiment under conditions of actual change; in other words, where the vegetation was actually being changed. The result was that the data are so varied that it was estimated that it would take nearly a century of experimentation to find out whether, indeed, the water yield had increased. In other words, you could not increase the water yield by changing the vegetation under those conditions.

Q Let me turn now to Dr. Herz, and ask you in comparing the Delta outflows that have been described by Dr. Leopold with fishery populations, did you use the same hydrologic data that Dr. Leopold has been describing?

DR. HERZ: A Yes, we did.

Q What was the source of those data?

A Those came from the Department of Water Resources. They were their data on the period of record for regulated and natural Delta outflow and a few years added after 1978, a few years before the 1921 period which is the starting period of record.

Q Do these Delta outflow values reflect the water diversions due to the operation of the state and federal water projects and other consumptive users?

A They do.

Q Are these the diversions that are displayed in Figures 3-2 through 3-14, and Tables 3-1 through 3-3 of your Exhibit 20?

A That's right.

Q Before we refer to this figure, just to be clear, Dr. Leopold has been describing his analysis of annual flow data, I take it you also made use of monthly flow data in your analysis; didn't you?

A That's right. We did an analysis on an annual basis, and also, on a seasonal, and particularly the spring period.

Q Now, let me have you refer to this Figure 3-2 which we have displayed here in the hearing room, and, Mr. Chairman, that follows page 51 of the report for those that can't see the display here.

What does this photograph tell us about the effects of water diversions on Delta outflows before and after construction of the state and federal water projects?

A This figure illustrates the changes in the amount of water diverted from the system during the period of record. It shows that during the early part of the century and up until the beginning of the projects, until the forties when the Shasta Dam was completed, three and a half to four million acre-feet per year was diverted from the system and as various components of the water project were completed the average amount of water diverted for a five-year period has increased up to approximately 11.5

million acre-feet per year.
Q All right. This figure displays mean annual flow levels. You also analyzed the monthly changes in flow; didn't you, from these diversions in Figures 3-6 through 3-11 on page 51 of the report?
A Yes. And, although I don't think we have an overhead on it, I will call attention to the spring months April, May and June, particularly, Figure 3-9, 3-10 and 3-11, and in this case particularly 3-10, which is for May, which shows a similar trend of increasing diversions, and I wanted to call attention to the springtime because the springtime is the period when flow to the estuary is most important in terms of the needs for fish and wildlife, or fish migration, spawning and so on.
Q Very good. Now let us turn to Figure 6-11.
MR. MAUGHAN: Just for the record, our copies show these charts between pages 46 and 47. I don't know, you have been referring to page 51.
MR. THOMAS: It may be just my error. Let me check.
MR. MAUGHAN: There are tables at page 51, not charts.
MR. THOMAS: Do you have a stapled or bound version?
MR. MAUGHAN: Stapled.

MR. THOMAS: Okay. There was delivered to the board about a month after the first version was delivered on the deadline, a revised version.

MR. MAUGHAN: So, you are referring to the revised version?

MR. THOMAS: Yes, we are referring to the revised version.

To facilitate your following the testimony, perhaps we can provide you with that revised version. In fact, perhaps you can provide a copy for each of the board members.

MR. MAUGHAN: Does the errata sheet refer to the revised report or --

MR. THOMAS: I believe that is correct.

A Yes.

MS. RUIZ: They referred to the revised report or they are, in fact, changes that are already in that revised report?

A No, they are changes -- they are subsequent to the revised report.

MS. RUIZ: So, they are revisions to the revisions?

A That's right.

MR. THOMAS: We were speaking of this Figure 6-11 which follows -- we were about to speak of Figure 6-11 which follows page 115 in the report.

Q What does that figure indicate, Dr. Herz?

A This shows deviations in flow for spring months which means the upper line, the dashed line across the mid point of the graph represents the average natural Delta flow, mean natural Delta flow for the period of record, 1921 to 1978, and the dashed line, that wiggles back and forth across, is the mean spring outflow for that set of information, that set of years. And what it shows is that the natural Delta outflow varied around the mean of that period throughout the entire duration of what we portray there within plus or minus about 25 percent of that mean.

However, if you look at the lower line, line 2, which shows the mean regulated Delta outflow, you see that it diverges markedly from the mean for the natural, particularly in the period following the beginning of the completion of various components of the projects in the forties and toward the end of the period of record shown there the percent of deviations is as much as 60 percent of the natural flow.

Q Dr. Herz, after analyzing the changes in Delta outflows over this period of time, you compared these data to populations of certain fish species in the estuary some years later; is that correct?

A That's right.

Q Now, I would like to ask you some questions regarding your choice of data in investigating these correlations. First, for which species did you examine population data and why did you use those particular species?

A What we looked at were salmon, striped bass and shad, and our choice of those particular species was that they are anadromous fish, fish that spend part of their life in the ocean, but return to the estuary to spawn and, therefore, are very much dependent on the conditions in the estuary, and particularly those conditions that are established by freshwater inflow.

Q Now, we have had some previous testimony about fish population levels and their relation to flows presented by the State Water Contractors in their Exhibit No. 263. They presented information on the abundance of Pacific herring and other saltwater species. Why didn't you choose these species for your investigation?

A The basic reason was that if you are looking for changes that are dependent upon freshwater flow, you want to use species that are, as I indicated, dependent upon that freshwater outflow and the pelagic species that spend most of their lives in the ocean such as herring and other species that were used in those reports, since they spend almost their entire lives in the ocean, are not particularly influenced by the level of freshwater flow.

Q Would you say that they are not good indicator species for showing the biological health of the estuary?
A Well, not only are they not good indicators for showing the biological health of the estuary, they are not good indicators of impacts of freshwater flow on the system.
Q What indices of fish abundance did you use in your investigation?
A We used a variety of different indices. First, we looked at the commercial catch during the period early in the century, approximately 1915 to the 1930s. We chose that particular period because that was when the system was working relatively naturally. The level of freshwater diversions from the system was quite low and the system was quite productive in terms of species of interest, so we thought that it would make a great deal of sense to look at the relationship between flow and abundance during the periods that were relatively unaffected by major projects.

Then, too, we used later in this century and more currently we used some other measures. We used the party-boat catch which is data collected by the State Department of Fish and Game, and we used several measures of abundance which are independent of catch. One is the salmon run or the return of salmon to the Red Bluff Dam, and the other is some analyses we did utilizing some modifications of the striped bass index, all of which -- or both of which are indicators of population abundance, as I said, and not dependent upon catch information.

Q Now, appreciating that you used several indices of abundance beyond fish catch data, let me ask with regard to the fish catch data, did you take any steps to assure that this data was not affected by the variability and the level of effort over the time series that you used?

A Yes and no. For the early part of the century the commercial fish catch records were taken pretty much as they were reported. Skinner, who is considered by many to be one of the best sources of information on the fisheries in the system the early part of the century, indicated that the level of effort was relatively constant during that period, and the more recent data, striped bass data, there is a level of effort calculated in those figures and our other two measures of abundance do not depend on fishing data, so it is a moot point.

Q Why did you use running averages of Delta outflow and lag times of several years in investigating the relationship between levels of outflow and fish landings?

A Well, there was a biological basis for using these running-year averages. We reasoned that fish are affected by freshwater outflows, especially for the first several years of their life before they mature and are caught.

Some people argue that the conditions preceding the years they spawn also should be taken into account because the water system is an accumulative system. We wanted to take account of the average conditions over the critical phases of their life cycles. We found the strongest correlations between outflow in salmon populations, for example, when outflow was averaged for three running years, and for striped bass when it was averaged for five running years.

This corresponds in some degree to what we know about the susceptibility of these species to various kinds of environmental stress during the early periods of their life.

MR. WALSH: I'm sorry, I should have asked this question when you were on the fish catch data. Is there anything different in the practices between the turn of the century and presently? There was some discussion on fish catch data not too long ago as it relates to more recent catches. Today fish-catch data could reflect anything from Monterey Bay to Gualala or Fort Bragg in terms of catch, and where they are landed, a much larger range.

At the turn of the century, would you sometimes have that range?

A The data that we used were not the ocean-trawl catch. We were using during the period of commercial fishing the period from 1950 to the 1930s, data reported on catch in San Francisco Bay because that was the period before commercial fishing for all three of these species was ended. So, we don't have that complicating factor of ocean catch and not knowing where it was caught and having some confusion about the fact that you could be having Bodega Bay and Monterey Bay landings reported if you are using the ocean-catch data, but since it was bay catch, that difficulty, we feel, is not a true one.

MR. WALSH: Okay.

MR. THOMAS: Q Now, I would like you to explain the correlations that were discovered, Dr. Herz. Let's turn, first, to Figure 5-9, if that could be displayed. That appears after page 87 in the report. What does this figure indicate about the relationship between regulated Delta outflow and commercial salmon catch in the Sacramento and San Joquin Rivers?

A Well, first of all, what this figure shows is the relationship between regulated Delta outflow for the spring months, April, May and June, compared with commercial salmon catch in the Sacramento and San Joquin Rivers.

MR. WALSH: Which page are we on?

MR. THOMAS: This figure appears after page 87, if you have the revised bound version.

MR. WALSH: I have the bound copy.

MS. LEIDIGH: Top of page 89 in the stapled version.

A Thank you. In any case, it is the commercial salmon catch for that period and what is shown there is based on a two-year lag between flow and catch, and we showed this figure because it shows a relatively high degree of coincidence between flow and catch when that lag is put in there, and it also includes both the pre-and-post-project periods.

MR. THOMAS: Q Was there a particular reason why you chose to utilize data from the spring months, April, May and June?

A As I have indicated earlier, because many of our correlations are strongest for the spring period -- let me back up. We have reasoned that because freshwater inflow to this estuary, or any other estuary at least in the Northern hemisphere, is very dependent on flows during the spring period, that we would expect that spring flows would play a major role in fish production and that's why we have shown these data in this way.

Q Can you explain why the fish catch data on this chart end as of 1957?

A 1957 was the last year of commercial salmon catch in San Francisco Bay. Since then, commercial salmon fishing has been only in the ocean.

Q Now, to understand the correlation lying behind the coincidence of curves, perhaps we can turn to Figure 5-10 that appears after page 88 of the report, and we have that on the overhead project as well.

Dr. Herz, what does this figure tell us about the correlation between the springtime Delta outflow and annual salmon catch for the years between 1916 and 1930.

A Well, this figure shows a quite close relationship between catch and flow, in this case a three-year running average of flow and a two-year lag, and in this case, the correlation between flow and catch plotted in this way is exceedingly high. It is .97, a perfect correlation being 1.0. And this means that approximately 94 percent of the variance in the relationship between these two factors is accounted for by this correlation coefficient.

Q Why does this figure contain data only for the years 1916 to 1930?

A Well, again, this is the period we chose because it marked a time when the estuary was operating relatively naturally. Diversions were relatively low. According to Skinner, the level of effort for the commercial catch was relatively constant and we thought this was a period that reflected the healthy functioning of this estuary.

Q What was the justification for using a three-year running average for Delta outflow figures and a two-year lag between this data and the annual catch data?

A These time periods, the three-to-five years corresponds to the time between hatching and returning to the Delta to spawn, and we felt it made biological sense for that reason.

Q Now, did you continue this analysis beyond 1930 in order to capture the post-project period after commercial fishing ended?

A Well, not precisely because of the fact, as I indicated, we moved to a different type of analysis and in the next --

Q Perhaps we can display Figure 5-22, which comes in the report before page 94. Could you explain this figure?

A In this figure we showed the relationship between the five-year running mean of the fall salmon run with no lag, and what we have is a correlation between those two factors of .89, which accounts for about 80 percent of the variance between the two factors, a fairly high degree of agreement.

I should point out this is not fish catch data, this is based on the relationship between flow and the number of salmon returning to spawn at Red Bluff Dam, so it is a measure of abundance rather than catch data.

Q Now, let's turn to Figure 6-7, which follows page 113. This shows the relationship between the striped bass index of abundance and regulated Delta outflow for the period 1959 to date.

Do we have that one for display? This is Figure 6-7, which follows page 113.

A I think we don't have an overhead for that. Let's move on past that one, skip that one for the moment because we don't have it up.

Q I can find it for you.

A Wait a minute. This figure again deals with deviations rather than raw data. The relationship shows the deviations, in this case, five-year running means of two different striped bass indices, the total and the Delta compared with the deviations of regulated Delta outflow for the 1959-to-1985 period.

It should be noted that the water deviation, the regulated Delta outflow line represents the deviation of regulated Delta outflow from the natural Delta outflow mean which is for the period 27.3 million acre-feet.

MR. WALSH: It's still pretty early. Can you explain what you are doing there to me again, please? I didn't follow you. Maybe it was my fault.

A The zero line in the figure, this one takes a bit of getting used to because deviation is not the easiest concept. I have trouble with it and I have only had my first cup of coffee.

The zero line represents the mean for all three measures in this case, the two striped bass indices and the natural Delta outflow line.

MR. WALSH: The mean of all three for Delta outflow?

A No, the zero line represents the mean. There are three lines shown there. One is the total Delta striped bass index, the zero line for them represents the average for the entire period covered there. The zero line for the water flow information represents deviations of regulated Delta outflow from the mean for the period.

MR. WALSH: Okay.

A And what it shows is that as the deviation flow increase in a negative direction, that means more water is being diverted, an increasing amount of water is being diverted from the system, the measures of striped bass abundance, these two striped bass indices, also shows increasing deviation away from their average, indicating a decline in those species.

MR. THOMAS: Q Will you explain why you use a five-year running average for the striped bass index?

A In the use of the striped bass index, there have been some serious problems encountered particularly since the drought years. We have found that if we use five-year averages of striped bass index, that it continues to remain a good predicter of abundance of bass, and if we use May flows rather than June and July flows, which were orginally used as the flow by Fish and Game in their original use of it --

MR. MAUGHAN: I would like to get into the record right here, if I can have an interruption. I asked last week about the fact that in 1977 we had a beautiful correlation between striped bass abundance and Delta inflow sufficient that the people who were there and testified thought, this looks like a great relationship, so it was incorporated, and those standards have been met since that time for ten years, but unfortunately, that correlation has not worked the last ten years.

What I am getting at, is that I have seen over my time, that in advance things like they are going to work and then you apply them, and they may or may not work.

Do you have any comments to make on why that relationship, that good correlation, with the high percentage of correlation that appeared to be there, didn't work?

A Well, the only insight that I can offer, and it is somewhat speculative, is that if one views the water in the system and the system is an accumulative system that is not just influenced by the flow of one year, and look rather at the influence of multiple years, then it appears as if this index does work.

MR. MAUGHAN: It wasn't just one year. It was the fifties up to the seventies.

A No, what I am saying is if you use means of a number of years rather than using individual years to compare with the striped bass abundance --

MR. MAUGHAN: I think if you will do that and stop in the middle of the seventies, you will think you have a good correlation. If you continue beyond that, I think you will find that you don't.

A As you will see in some of the figures that follow, some of these striped bass index flow relationships that we presented in our report do go up into the period past the drought years. That's the whole point, that we feel that the modifications that we have used do seem to make it a better predicter.

MR. MAUGHAN: It seemed to, that's my point. Until we have some experience, I'm not so sure just how much competence anyone can place in some of these correlations because I could name others, but they get outside of this particular area, which I have observed in the past and I have seen some that do work, but I have seen a lot that don't work, so we have to look at them with some degree of concern and care.

Q Well, I think again, we rely on the fact that we are not looking at one measure, we are looking -- we have six different measures of fish abundance. We have commercial fish catch and we have two measures of abundance that are not based on catch, and all of these, as you will see as we go through the testimony, seem to predict a requirement for the same amount of water. We feel relatively confident that the relationships that we are showing are not just chance ones and do make some sense.

MR. MAUGHAN: Just one last comment. On the striped bass index they spent a considerable amount of money and they have probably the best data. Some of this data that you now have are sort of indirect and I'm just throwing it in there to see if you had any further comments, and you have already made your comments, so proceed.

MR. THOMAS: We will have some further reflections on that, too, as we go through the testimony, Mr. Chairman.

Q Let's move now to Figure 6-26 that appears after page 119 in the exhibit, and I believe we have that information displayed here in the hearing room.

Dr. Herz, does this figure display the correlation for the relationship that you were describing between spring Delta outflow and the five-year running mean striped bass index?

A It does. This is with spring monthly flows, five-year averages with no lag, we find a relatively high correlation of .82, which accounts for about two-thirds of the variance between these two factors.

Q Could you explain why you used the period 1959 to 1981?

A Well, we were particularly interested in spanning a period that was, first of all, using a period that was post-project; and secondly, to see whether this relationship held up after the drought years, and as I was discussing with Mr. Maughan, it appears from these correlations that that relationship does, in fact, hold up.

Q Let's now display Figure 7-1 that follows page 129 of the report. This is for the shad fishery. This figure shows the correlation between annual shad catch and the two year running mean annual regulated Delta outflow; does it not?

A That's correct.

Q In this case, the running mean for outflow is two years and the lag time between flows and catch is one year. Can you explain those choices of data?

A In this case, I think we used -- we are showing this because this gives us our strongest correlation for shad. It also makes some biological sense in that the first returning shad come back to the system to spawn after three years.

Q And why was data chosen from the years 1916 to 1931 only?

A Well, again, the same response as with our previous salmon and striped bass, that was the period when the system was working well and commercial fishing was at its high point, and the system was very productive and we wanted to see under relatively natural conditions before large exports how the system worked and what the relationships were apt to be.

Q Now, for salmon and striped bass, we were looking at the correlation between fish abundance and spring flows. Perhaps just for the sake of consistency, we can look at the spring flow correlations for shad as well. We do have a figure displaying that, which is unnumbered?

A Actually, in the errata sheet it does have a new number of 7.5. I don't know whether you want to use that or whether you want to assign it a new exhibit number for testimony purposes.

MR. THOMAS: This particular chart does not appear except in the errata, as I understand, so we might for convenience just designate it for the record as Tiburon Center Exhibit No. 32.

MR. TAMBLYN: Just incorporate it in the errata.

MS. LEIDIGH: Why don't we just include it as part of the errata sheet and designate the errata sheet Exhibit No. -- say, 20A.

MR. THOMAS: Is 20A appropriate?

MR. TAMBLYN: 20A. Let the record show that this figure comes from Tiburon Center Exhibit No. 20A, which is the errata sheet.

(Errata Sheet was marked Romberg Tiburon Center Exhibit No. 20A for identification.)

MR. THOMAS: Q Could you explain what this figure shows, Dr. Herz?

A Well, in this case we are looking at the relationship between annual shad catch and mean spring regulated Delta outflow, in this case, a two-year average, two years previous and a one-year lag, a total of three years.

For example, the catch of 1916 is based on the outflow of 1914 and 1915, the mean of those two. In this case we get again a rather high correlation of.89, which accounts for about 80 percent of the variance between the two.

Q Having found all of these highly significant correlations between freshwater inflows in the San Francisco estuary and populations of indicator species, is there any evidence to show that these are not mere coincidents?

A Well, as I indicated, one of the reasons that we feel that it is not just coincidence, is that we find these relationships across three species of fish during two different eras of the history of the system, one the contemporary period and the other the historic period, so both pre and post, and we also have some independent measures of fish abundance that are not dependent on catch, which also show the same relationship, so that would require an unusually high degree of coincidence to have those things all come together by chance and show these relationships.

Q Have you analyzed data from other estuaries to see whether or not the same correlations can be found in other natural systems?

A Well, this is really Dr. Rozengurt's area of expertise since he spent much of the last 25 years doing these comparisons, but yes, in fact, the relationships that we find here seem quite consistent with what had been observed not only in some of the Soviet estuaries that are discussed in a couple of other exhibits that we submitted, but also, estuaries of other continents of the world, so these relationships between declining freshwater flows and deterioration of the system, the first signs of deterioration being fisheries catch or fish catch and fisheries abundance measures starting to decline.

Q For the record, in Dr. Rozengurt's absence, since he is not available to testify in detail on these estuaries, his study of the Sea of Azov and other estuaries described in the Tiburon Center Exhibits 23 and 24, I might have you just in summary fashion, Dr. Herz, indicate for the Sea of Azov what was found.

We can do that by referring to Figure 6A.

A Well, first of all, I should say a few things, I guess, about the Sea of Azov. It's my understanding, I have been told --

MR. WALSH: Where is the Sea of Azov? I am trying to get a picture in my mind of the map.

A If you look at -- there's a map in Tiburon Center Exhibit 23 that shows where it is in the Soviet Union. It's connected to the Black Sea.

MR. WALSH: It's near the Caspian?

A It's connected to the Black Sea. One of the points I wanted to make was that in someone's discussion earlier on the inflow to San Francisco Bay, I am told there was some objection made to comparing the Sea of Azov with San

Francisco Bay because they said it did not communicate with an ocean or sea and was shallow or much shallower than San Francisco Bay.

I think if Dr. Rozengurt were here, he would be jumping up and down and saying that was not the case, that it was, in fact, communicating with the Black Sea, and that it does have depths that are not as deep as the deepest spots in San Francisco Bay, but it is not an entirely shallow sea and it does contain some fish species that are not dissimilar to some of the anadromous species that we have here in San Francisco Bay, and particularly sturgeon.

MR. WALSH: Okay. Mr. Thomas, have you got the map there -- I've got it. Let's go on.

A In any case, what this Figure 6A shows is similar data to what we have been showing for the Sea of Azov, which shows -- the first line 1 is regulated combined river inflow to the Sea of Azov and commercial fish catch of a number of anadromous species; line 2 is sturgeon and a couple of other anadromous species, and what it shows is this same kind of paralleling trend of flow and fish catch.

MR. WALSH: So you have three major river systems going into the Sea of Azov?

A Yes.

MR. THOMAS: Q I don't know whether you will be in a position to answer this, Dr. Herz, in Dr. Rozengurt's absence, but it would be illuminating what this study of the Sea of Azov shows regarding the effectiveness of hatchery stocks to mitigate the natural fishery losses that were experienced in that Russian estuary.

A Well, there are some fairly astounding numbers. As the freshwater diversions from the river leading to the Sea of Azov began to increase and get up above 50, and then, I think 60 percent, the result was near collapse of their anadromous fish species. They attempted to mitigate this by building huge numbers of hatcheries, and I think the number of hatcheries approached 100 hatcheries, and even in their peak year of dumping something in the order of six billion fry of one species into the system, they could not reverse the declining trend, and ultimately this area, which was one of the richest fisheries in the world, is now producing one or two percent of what it did before these diversions began.

Q To just sum up, all of the data from the several other estuaries that were analyzed in the report, what conclusions can be drawn that bear upon the freshwater needs for San Francisco Bay by looking at these other estuaries?

A Well, the similarity that results from a examination of all these estuaries all over the world indicates that freshwater inflow serves a number of different functions and once you start radically -- well, not even radically, once you start diverting, according to Dr. Rozengurt, over a quarter or a third of the historic inflow to these systems, you start seeing problems with these functions, and the functions are freshwaters repelling the intrusion of seawater, its ability to provide nutrients for the system, its ability to create the conditions necessary for migration of anadromous fish species, both in and out of the system, the creation of a null or entrapment zone which is needed for production of food at the base of the food chain, it's providing of flushing and mixing needed to -- in the case of most of these estuaries, entrain and flush out to sea various pollutants, and finally, creating an equilibrium in the salinity system.

Some or all of these things have been identified by one or more people for this estuary, and unfortunately, some of the data that would be useful to have for describing what has happened as we have diverted an increasing amount of water, we do not have in the kind of detail we should have.

Finally, I would like to say that in addition to this 25 to 30 percent threshold, if you divert more than

25 to 30 percent of the historic inflow, you begin to see the deteriorating conditions ultimately reflected in fish productivity, and at the other end, if you go beyond about two-thirds or three quarters of the historic flow, if you divert more than two-thirds or three-quarters, it appears that these changes may be irreversible.

So, what this says for San Francisco Bay is that we have not yet reached the level that appears to be irreversible in other estuaries, and this is one of the reasons that we were so interested in performing these correlations and coming up with a recommendation that we will discuss in a moment.

Q Let me now ask you some questions about the conclusions and recommendations that you drew from your analysis, Dr. Herz. Let's turn to Figure 8-1 which follows page 146 of Exhibit 20. Let me ask you to explain what this shows regarding the freshwater inflow standards for the spring months that are necessary to maintain the health of the San Francisco Bay fishery.

A In this case, we are saying that the various correlations that we have performed require on the order of a total of 6.9 to 7.5 million acre-feet each spring averaged over two to three years in order to insure that we get production of fish in the system.

And what this further shows is that in the pre-project period spring runoff was, in fact, above these levels that we say are necessary for successful catches.

In the current era of the post-project period, we are slightly on an average over one, one and a quarter, one and a half million acre-feet a year, which is far below what we think is necessary and the projected year 2000 drops it down even further, so this is one of the reasons that we feel that there has been a deterioration in abundance of some of these species.

Q Now, when you say that your conclusions led you to the recommendation of 6.9 to 7.5 million acre-feet, you are talking about a level of flow over the entire three-month period?

A During April, May and June, an equivalence of 38 to 42 thousand cubic feet per second at Chipps Island.

Q The figure before us here actually shows those outflow requirements as a monthly requirement; does it not?

A Yes, that's right.

Q How is that flow requirement that you have been describing derived from the correlation data for salmon, striped bass and shad that you were testifying about?

A Well, what we have done with these correlations is to identify the range of water flows within which we found that the majority of the data points are near the mid-point of the regression, and then, we have averaged this range for all of the regressions that are presented in the report, and that leaves us with these numbers that we are proposing or recommending.

Q So, the numbers that you were giving of 6.9 to 7.5 million acre-feet for the spring are computed, are they not, from the correlations?

A That is right.

Q They are not simply estimates?

A That's right.

Q You say that you derived the inflow recommendations by considering the mid-point of the correlations rather than the levels of flow that optimize the fishery populations; is that correct?

A That's right. We chose a level that we feel recognizes the competing demands for what everybody sees as a limited water supply, but at the same time, would maintain the fishery. We want it to be noted that this recommendation should be considered to be the bear minimum that's needed to protect fisheries, and that's because in biological investigations of this type the error band can be on the order of as much as plus or minus 20 percent, which means that the flow recommendations may actually be 20 percent less, that our recommendation may be 20 percent less than what is necessary to maintain the fishery at a mid-range level, and as you can see from the graphs or the regression lines that we have presented, there is not a large margin of error in the flows. Fifty percent of our recommendations result in very very little, if any, fish catch, so that flows of 50 percent of our recommendations would have a catastrophic effect on fish abundance.

Given the inherent and certainties, there's not much margin for error and, therefore, our recommendations usually the mid-point of these ranges should be seen as fairly conservative.

Q To put these recommendations into context, what percentage of the minimum unimpaired runoff is required to meet the proposed spring flow standards?

A Approximately 64 to 70 percent.

Q Now, for the recommendation on annual flows, let's turn to Figure 8-2. This annual flow recommendation of 17 to 19 million acre-feet is to be attained each year, not averaged; is that right?

A That's correct.

Q What percentage of mean annual unimpaired runoff is required to meet that proposed standard?

A About 63 to 70 percent.

Q And how is that flow requirement derived from the correlation data for salmon, striped bass and shad that you have described?

A In much the same way as it was done for the monthly or springtime. We identified the range of flows around which the majority of data points in the mid-range of the regressions appear, and then, we averaged this range for all of the regressions that we presented.

Q And again, the recommendation for annual flows is based upon the average of the mid-range flows shown in the regressions, not the level of flow that optimize the fishery populations; is that right?

A That's right.

Q Dr. Leopold, if I could, at this time I would like to ask you whether you have had an opportunity to examine the Decision 1485 standards and whether you have an opinion as to whether they would be or they are adequate to provide the required level of inflow to protect the San Francisco fishery resources?

DR. LEOPOLD: A The standards that are set up in the water right decision are extemely complicated, and I would imagine that the control board might want to look at the whole question of what data are needed in the long term to satisfy any standards. Not only does Decision 1485 call for a large number of different kinds of measures, but particularly with some of them we are not sure that is really what we ought to be measuring.

I spoke before about the question of salinity, and

I think one of the things that the control board might want to think about is how the data collection affect over the long term in the future, how it should be defined and how it can be made uniformly consistent over a long period of time. This is not easy to do.

Q All right. Dr. Herz, what are the implications of the recommendations that you have stated for the operation of the state and federal water projects?

DR. HERZ: A Well, what it says at a minimum is there should not be any increase in the levels of diversions out of the estuary. It certainly seems to indicate there is a need for larger springtime releases and probably the most difficult thing is that there is going to be a requirement for a more equitable sharing of the shortfall of water during the dry and critical years.

Q How do the Tiburon Center recommendations compare to those recommended by the Fish and Wildlife Service?

A It's my understanding that they are relatively similar because the Fish and Wildlife Service recommendations for salmon are in the neighborhood of 30,000 cubic feet per second from the Sacramento and 12,000 from the San Joaquin during the springtime, which seems to compare quite favorably with the numbers we are recommending.

Q And those Fish and Wildlife Service standards were recommendations that were for the purpose of protecting the fishery as well, were they not?

A For the salmon, yes.

Q Are there further questions regarding the level of freshwater needed to maintain the San Francisco fishery resources that merit investigation?

A Well, based on our research here and on the information from other estuaries, things are going downhill. We can't let the resource totally collapse. There must be at least interim standards to leave enough water for the resource while further information is gathered to determine what should be the final standards.

Q What is your view on how and who should perform these additional investigations?

A Well, I think that the state board should require that the consumptive users fund some sort of studies to evaluate the damage that's already resulted. I think the very large amounts of information that have been generated by these hearings will also need to be evaluated by an objective independent entity that has a broad perspective and it would seem to me that the National Academy of Sciences National Research Council kind of approach which has been used in the last year or so with Kesterson, Mono Lake and Lake Tahoe, is a good way to evaluate this mass of information.

There has been a tremendous amount of information that we have been discussing, even just in the inflow hearing, and that needs to be evaluated with some level of expertise and objectivity, and may require expertise that is beyond the scope of people in California.

And finally, I would suggest that perhaps the state board should request the Environmental Protection Agency estuarine program to be involved in helping to design studies and in-perpetuity monitoring of the system so we can keep track of what's going on and what the relationships are between flow and the resources.

Q That's suggests a final question for Dr. Leopold. Dr. Leopold, having evaluated the D-1485 standards and being a member of the National Academy of Sciences, do you see merit in referring the problem of standard setting to this expert body?

DR. LEOPOLD: A In the case of Lake Tahoe, only recently did both Nevada and California decide that they needed to have their basic data collection effort looked at by an independent agency, they turned to the Water Science and Technology Board of the National Research Council and asked that an independent exhibit be set up to study the question of basic data requirements.

D-1485 envisions a data-collection system, not only so extensive and so complicated that I cannot imagine that over a period of the next 30 or 40 years that we could continue to carry out the data collection system that's envisioned in 1485, I think it ought to be made simpler and I think that the main idea in data collection for a long term must relate to the processes by which the eco-system operates, and therefore, as I see it, the control board must visualize a review and extensive review of the data-collection system itself, and the initiation of studies that relate the data which are to be collected to a better understanding of how the eco-system operates.

With all the material that we hear about the relation of salinity, fisheries and water discharge measured in different places at different times, we still don't understand as much as we should about the processes, about how this interaction works in the eco-system. And, for that reason, I think that it would be well to consider asking an independent organization like the National Research Council to consider the matter of what data should be collected and how a simplified data-collection scheme is intimately related to what we presently understand and what we should understand about the natural processes within the eco-system itself.

MR. MAUGHAN: Mr. Thomas, Dr. Leopold referred to data collection. I thought you said something about referring to standard setting which I wondered if that was properly the question you had in mind, or was it data?

MR. THOMAS: Well, let me just ask both of our experts here to reflect further on that, if you would care to

Q Is it simply the collection of data that you would recommend be referred or the actual recommendation of standards to protect the estuary?

DR. LEOPOLD: Standards depend on data. No matter how you set the standards, and the standards are going to be of such a nature that if flows or chemical or biological data show certain things, then certain steps must follow and, therefore, the question of setting standards is very closely related to the basic data-collection effort itself.

Regardless of how the standards are to be written, they are all going to be related to data collection; in other words, data availability, and that's why I say the two have to be considered together.

MR. THOMAS: We recognize, of course, Mr. Chairman, that the statutory responsibility for setting protective standards for the estuary lies with this board and not with the National Academy of Sciences.

MR. MAUGHAN: I was wondering if you were suggesting switching. I am quite serious.

MR. THOMAS: That's not a part of the suggestion.

I believe that the suggestion is that not only the collection of data, but also, the interpretation and translation into public policy is a matter on which the National Academy might beneficially register, and what you would receive would be recommendations, certainly not the final and binding standards from such a body.

DR. HERZ: In fact, if you take the Mono Lake, the recent National Research Council study of Mono Lake, they essentially identified issues and set up a set of flow levels or lake levels and discussed potential impacts of these different flow levels or lake levels on these, if you will, beneficial uses, so it is very comparable.

They did not, and I think they would probably balk at being asked to make recommendations.

MR. MAUGHAN: I wanted the record to be clear what the distinction is.

DR. HERZ: What I was envisioning was something like that, would be to outline the critical questions and to make some evaluation of the large mass of data. I mean, I think that's the basic place where a group like the National Research Council could be of great assistance.

MR. MAUGHAN: All right.

MR. THOMAS: Mr. Chairman, that completes our direct testimony.

MR. MAUGHAN: Well, I think this is an appropriate time to take a 15-minute break.

(Recess)

MR. MAUGHAN: Let's go ahead.

MR. SMAAGE: Denis Smaage, Department of Fish and Game.

At the cross-examination in Concord, the Department of Fish and Game was asked to add additional data to Exhibit 60A concering the abundance of bay shrimp historically, and we have done that by adding six years of data from 1980 to 1986, and I would like to offer that exhibit in evidence at this time.

MR. MAUGHAN: That was requested so I don't know that there is any objection. Hearing none, that completes that, Mr. Smaage?

MR. SMAAGE: Thank you.

MR. SCHULZ: Mr. Chairman, while Mr. Smaage was talking, during the cross-examination of their witnesses on striped bass, I asked for some information from Fish and Game as to the raw data that they used to correlate various indices as to how many fish equaled what index.

MR. MAUGHAN: And they haven't done so?

MR. SCHULZ: They haven't done so.

MR. SMAAGE: Do you know which witness you asked that of?

MR. SCHULZ: Striped bass -- Stevens.

MR. MAUGHAN: All right, that's in the record. I would just request Mr. Smaage to remind Mr. Stevens.

MR. SMAAGE: Thank you for reminding me.

MR. MAUGHAN: All right, Mr. Littleworth, I think you are first up to bat.

MR. LITTLEWORTH: I have just a few questions for Dr. Leopold. The contractors, in order to try to facilitate things, are going to defer the major cross-examination to Mr. Somach, but I have a few questions.

MR. THOMAS: Before Mr. Littleworth begins, I wonder if I might clarify for the record the source of one of the exhibits which I apparently failed to identify during direct examination.

Referring to the chart that Dr. Leopold testified to entitled "Inflow to Delta, 1921 to 1982, Percentage of Years of Different Supplies of Water," that is Figure 2 from the Tiburon Center Exhibit No. 22.

MR. MAUGHAN: All right, I think that's clear enough.

Does staff find that clear?

MR. TAMBLYN: Yes.

MR. MAUGHAN: All right. You may proceed, Mr. Littleworth.

CROSS-EXAMINATION

by MR. LITTLEWORTH:

Q Dr. Leopold, your analysis was based on data which you were furnished. You didn't do any original data-collection work?

Dr. Leopold A No, I didn't, sir.

Q And did I understand also that when you were using the term "natural flow," that that, in fact, was what the Department of Water Resources had described as unimpaired flow?

A That is correct.

Q And that's the calculations that showed an average of 28 million acre-feet annually?

A Yes, sir.

Q And then, to get the regulated flow what you did was to subtract the depletions, as you called, exports and so forth, from that unimpaired flow?

A That was my definition of depletions, the subtraction.

Q Well, you, in fact, took the 28, the unimpaired flow figures and then you subtracted the upstream uses and by exports?

A No, I took the difference between the two sets of data that were furnished to me, the outflow data tabulated by years and the unimpaired data year by year, and subtracted them.

Q So, you got the depletions in a backward way then?

A Yes, sir.

Q By comparing the unimpaired flow and --

A The tabulated --

Q The tabulated outflow?

A Yes.

Q I take it you didn't look then at the State Water Contractors' Exhibit 260A which purported to show actual Delta outflow over this period?

A I was not shown that at the time I wrote my report.

Q Now, are you aware, Dr. Leopold, that the conditions under which, or the assumptions under which the unimpaired flow was calculated included no storage and upstream reservoirs, no use by agriculture or cities, but that it did include the present-day levees and channelization?

A I understood that, yes.

Q And you would agree that that, in fact, was not what the state looked like in, say, around the 1800s or early 1900s?

A Yes. I was having to use the data that were furnished to me.

Q Are you aware that the Department of Water Resources in reaching that unimpaired flow average of 29 million acre-feet annually used a consumptive use of two acre-feet per acre for everything except in the Delta itself?

A Actually, I did not know how that computation was made. I was only furnished -- since I came in very late in this; in other words, I never saw the report until the summer. I did not have a chance to evaluate how that tabulation was actually arrived at.

Q I appreciate you are in a pinch-hitter role here today. Are you aware that in the natural condition of the state if you used natural conditions, say, for the year 1800, that there were very large tule marsh areas and large riparian forest areas?

A Yes, I'm aware of that.

Q And do you understand what the State Contractors did was simply to adjust the consumptive use figures which the Department of Water Resources had used by the estimated use of the tule marshes and by the riparian forests?

A Yes. I could see how it was done. The problem that I have is that the net result is so much larger than anything that has been measured directly in any previous investigation, but I am not able to say what part of the analysis I would have questioned.

Q You would agree, I am sure, that a tule marsh area

or large riparian forest would actually have a consumptive use higher than two acre-feet per year; wouldn't it?
A Yes, under conditions where potential evaporation was possible.
Q And if the consumptive use figures used by the Department of Water Resources to reach their unimpaired flows were, in fact, larger, then the 28 million acre-feet average inflow would be something less; wouldn't it?
A Yes, but as I say, I did not know the assumptions that they made.
Q I understand. I just want to get, if, in fact, the consumptive use figures used by the Department of Water Resources underestimated the actual consumptive use, you would, in fact, get a lower flow than the 28 million acre-feet; wouldn't you?
A Presumably their computation could be redone using another set of assumptions, yes.
Q And if, in fact, the 28 million acre-feet were something less than that number, then, in fact, that would change virtually all of the tables and exhibits and so forth which you used; wouldn't it?
A I don't think so. It might change the numbers but the main thing that the frequency analysis brings out is the distribution around the mean, above and below the mean, so the shape of the curve would not necessarily have

changed regardless of the fact that the average value has been made different.

Q But it would change all the numbers themselves?

A They would be changed in proportion, yes.

MR. LITTLEWORTH: Thank you.

MR. MAUGHAN: All right. Mr. Turner, do you represent anyone that would like to cross-examine, like the Geological Survey, Fish and Wildlife Service or the Bureau of Reclamation, any of those agencies?

MR. TURNER: As a matter of fact, Mr. Chairman, I am representing all three today as well, and I just had a couple of questions, if I could.

MR. MAUGHAN: Sure, you are next.

CROSS-EXAMINATION

by MR. TURNER:

Q I have, like I said, a couple of questions for clarification. I was wondering if I might get you, Dr. Herz, to turn to -- I don't have the revised version of Exhibit 20, but I presume it is probably still on the final page, the recommendations in Exhibit 20, and I presume your final report, larger report, says the same thing as this.

You are proposing the establishment of criteria of annual flows of no less than 17 million acre-feet, and then going on, for a period of at least two to three consecutive years. I am trying to get a little bit more clarification as to how we would compute the actual time period during which those annual flows were to be maintained.

DR. HERZ: Well, I think in terms of a criterion or a standard, that would be more binding and more measurable. Our spring flow numbers are the ones that we proposed as playing that role, providing that function.

The annual figures are less, I didn't want to say, enforceable, but not as much -- well, I just think from our perspective what we have recommended, the spring flows are the ones that we feel are the most useful in terms of protecting the system and that the annual flows are -- I certainly don't want to say less precise, but because they are stated in annual flow, million acre-feet, not cubic feet per second during specific seasons, it is more difficult to utilize them as a regulatory kind of number.

Q So, I take it then that these annual flows, the maintenance of that annual flow is not tied to the type of water year or it is not tied to which percentile that particular type of water year fits?

The reason, obviously, that I am concerned, we are trying to figure out what amount of acre-feet of water is going to be necessary during the historic and projected Water year conditions to actually satisfy these recommended criteria of 17 to 19 million acre-feet in various years.

I was wanting to try to get what kind of guidance I could to analyze what the impacts would be.

DR. HERZ: Well, you are correct in your statement that we have not tied those numbers to any year-type classification, and I am not prepared to go into greater detail. I am not the hydrologist and I think, if necessary, it may become necessary to submit questions on the hydrology to Dr. Rozengurt in writing, and I don't know whether it is permissible to have that become part of the record or what, but we did not plan to have Dr. Rozengurt in the hospital during this proceeding.

Q Okay. Well, that would be fine. I would appreciate it.

Let me move on to the spring flows. I wanted to make sure that you were talking about mean monthly flows of at least 2 to 2.5 million acre-feet, and that's in your direct testimony. I just wanted to make sure you were saying that would equate to this approximately 6.9 to 7.5 million acre-feet during the spring period, so these are monthly flows, not seasonal flows?

DR. HERZ: That is right, 2.3 to 2.5 for each of the three spring months, April, May and June, 2.3 to 2.5 times 3.

Q And then, again, maybe I will get the same response, but are you talking about maintaining those particular flows in each of those months again during those two to three consecutive year periods, or is this supposed to be all across the board?

DR. HERZ: No, for a period of at least two to three years.

Q It is not tied to any specific type of year?

DR. HERZ: It is not tied to any particular type of year, that is correct.

Q Okay. The only other thing I wanted to ask is I believe I heard you say on your direct testimony that one of the things that you felt showed that the relationship between flows and the fish populations was not just a matter of coincidence, but that you were showing some kind of similar relationships with respect to the salmon, striped bass, shad, more than just one species; is that correct?

DR. HERZ: That is correct.

Q What I was concerned about, as I understood it, were you not using different criteria or different measuring devices to determine the relationship between flow in each of those individual species? For example, using running means or running averages of certain periods for one species, but using a different running average of flows for another species, using one lag time for one species, but using a different lag time for another species?

DR. HERZ: If you look closely at the figures that we presented, I think you will find that the three-to-five-year period that we talk about is represented in the means and lags combined, so that our recommendations of this two-to-three consecutive years is based on the fact that we consistently found that you got optimum relationships between flows and these various measures of fish abundance with lags or periods of three to five years, and the periods include both the period that is averaged, the number of running years of flow and the lag between the end of that period on the catch, so three to five years predominates.

Q Okay. I guess I was trying to be as up front as possible. It seemed to me you are saying, for example, that you get a high correlation between flows and salmon catch or salmon populations when you are using a three-year running average of flows, and you are using -- what was it, a two-year lag time between the time of the flow and the population count. You then say you use a five-year running average for striped bass and you use a two-year running average for shad.

It seemed if you were going to vary the periods of averaging flows and the lag times, are you not just saying if you pick up some arbitrary flow period, some arbitrary lag period, you can always show there is going to be a relationship when you change the criteria you are using to establish the relationship?

DR. HERZ: No, not at all. I think what we are saying, as I tried to make clear in the direct testimony, is that the system is an accumulative one, that it's not the effects of this season's water only on this year's fish, that there are cumulative effects, and we did not take an infinite array of possible mean periods and lags. We zeroed in on this three-to-five-year period as the period that we felt made the most sense because most of these species when they are mature and when they return to the estuary to spawn are in that age range and, therefore, we thought that it made biological sense and was not at all arbitrary, and I would also like to add that what we found with other scatter plots and correlations that we attempted correlating was a given year's flow with given year's catch was that those relationships did not hold up.

That is why we went to these lags, and I should also point out that these means and lags are used successfully as a description of relationships between flow and productivity in a variety of estuaries.

I have a list with me of about 20 to 25

publications on flow-productivity or flow-fish abundance or flow-catch relationships in other estuaries in this country and abroad that showed the same thing.

So, this is not at all an arbitrary procedure that we are using. It's one which has been used in a variety of biological systems by a variety of fisheries biologists and fishery statisticians as a technique for showing relationships between flow and production.

Q Well, let me, I guess, complete by asking one last question. If you look, for example, just to take Figure 5-11 in Exhibit 20, one of the figures you made reference to in your testimony and in that particular exhibit you say the catch is lagged by three years, and you have versus the mean spring regulated Delta outflow -- was that a running average or was this the actual flow in each of those years?

DR. HERZ: It's a mean of the three running years for the spring period with a three-year lag.

Q That's what I thought. Now, my question would be, did you utilize that same criteria using the same running average and the two-year lag and try to graph how that affected striped bass, shad? This relates to salmon. If we use the same kind of running average and use the same lag period, what kind of correlations do you get between those flows and the striped bass and shad populations?

Did you try using those figures?

DR. HERZ: I'm sure we did. It would take me some time to go through to find the appropriate comparison. Again, the bottom line is that although we did not use precisely a three-year running mean and a two-year lag, or a two-year running mean and a three-year lag every time, the three-to-five-year period was the one we focused on because we felt it made biological sense, and that is the place where we seem to have gotten our strongest associations.

MR. TURNER: I would have no further questions.

MR. MAUGHAN: Mr. Smaage, do you have any questions?

MR. SMAAGE: We have no questions.

MR. MAUGHAN: Mr. Anderson?

CROSS-EXAMINATION

by MR. ANDERSON:

Q The first question that I have relates to a statement that Dr. Herz made regarding some nameless testimony that was given at the bay inflow hearings in Concord. I believe you are probably referring to testimony by Ed Huntley of the Department of Water Resources. I assume that's the case.

DR. HERZ: It was nameless because it was just reported to me second or third-hand.

Q My recollection is that the only testimony about the Sea of Azov was given by Mr. Huntley and since you characterize his testimony a certain way, I would like to clarify that.

If you look at the record, you find Mr. Huntley did not say the Sea of Azov does not communicate with the Black Sea. He very specifically said, in speaking of the great differences between those two bodies of water, the Sea of Azov and the San Francisco Bay and the comparability of the two. He pointed out the great differences in the average depth of the Strait of Kerch and the Golden Gate, the former being 20 feet on average and the latter being 200 feet on average.

He also pointed out the great difference in area of the Sea of Azov being 14,700 square miles and the bay being 400 square miles.

He also pointed out the maximum tide range of the Sea of Azov being .7 feet and the bay being 11 feet.

Do you agree or disagree with those statements?

DR. HERZ: Of the authors of this report I am not the most knowledgeable about the Sea of Azov. I suspect that those figures are probably correct.

I would ask a couple of additional questions, for example, in addition to the depth of the straits, the breadth of the straits and the area that is under the influence of the tide is of extreme importance as well.

Q I wanted to go on. I note that in the qualifications neither you nor Dr. Rozengurt are fishery biologists; is that correct?

DR. HERZ: That's true, although Dr. Rozengurt in his training in the Soviet Union took a large number of courses in fishery biology, fish physiology. He, in addition, collaborated with people in the institute in the Soviet Union who were doing research on fisheries questions for some 20 or 21 years. He did this work and was working closely with them and published something on the order of somewhat over 40 publications on the relationship between oceanography, hydrology and fisheries problems.

Q Let me ask you this: Did you have the material on fisheries that you presented here reviewed by fisheries people, bypassing the question of whether Dr. Rozengurt is an expert, other fisheries experts to determine its technical accuracy?

DR. HERZ: Certainly, we had this report reviewed.

MR. MAUGHAN: Dr. Herz, would you sort of speak into the microphone.

DR. HERZ: I said, certainly, we did have it reviewed.

MR. ANDERSON: Q How would you characterize the comments that you received? Were they favorable, were they incorporated into your report?

DR. HERZ: We had a range of responses. Whenever you submit something to peer review, that's what you get, and the purpose of a peer review process is to get as much constructive criticism as you can to strengthen what you have. We got a variety of comments. Some said that they felt that the procedure used on the results that we came up with were quite consistent with what they thought they should be. There were several who felt that the techiques were quite appropriate and indicated that they were not unlike the techniques used in other systems, as I have already indicated.

In addition, we did receive a number of comments and suggestions about different statistical techniques, very specific things that we might have done, some of which we integrated into the final report, some we chose not to, that we felt we could answer their criticisms without making a major change.

Q In your analysis of fisheries, did you try to separate the bay effects from the upstream effects?

DR. HERZ: Can you expand a little on the question?

Q When you take a look at the state of the fisheries over historical periods, some defects might be some of the environmental impacts or the outside impacts on the populations might be occurring in the bay, and some of them might be occurring upstream of the bay.

DR. HERZ: We were looking primarily at the effects of Delta outflow on levels of catch and levels of production of the species. So, to the degree that what was happening upstream of the bay was influencing Delta outflow, yes, we did consider that.

Q If what was happening upstream was happening at the same time that changes in outflow were occurring, then you might not be able to distinguish which factor was the cause of any change that you discerned.

DR. HERZ: We were looking at the relationship between modifications in flow and levels of productivity. We did not choose to do anything other than that to make any interpretation of the results that you want, but what we were looking at was the effects of changes in management and outflow as a result of management on the number of fish in the system.

Q You would agree the distinction between factors occurring upstream and factors occurring in the bay downstream would be important to make?

DR. HERZ: If you are talking about the amount of water that is there to influence the resources, I don't think it makes much difference where that change takes place.

Q I am talking about things other than the amount of water, the state of the habitat upstream, perhaps degradation may have occurred concomitantly with some of the effects you observed.

DR. HERZ: If I understand the changes in habitat correctly, most of those changes had occurred before the more contemporary period of analysis that we performed and, therefore, can't be attributed as being responsible for the flow-productivity relationships that we demonstrate for the contemporary post-project period.

Q Let's move on. Are you aware of the problems the Department of Fish and Game mentioned previously regarding the use of catch data in analyzing abundance trends?

DR. HERZ: Since time immemorial, people have been aware of problems with fish-catch data, yes.

Q So, you are aware of those also when you offer them to the board with those necessary qualifications.

DR. HERZ: Yes. I should point out, however, that I am told that the Department of Fish and Game spends 1.1 million dollars a year to collect fish-catch statistics, and it is stated throughout the literature there are statements such as despite their limitations, fish-catch statistics are of extreme value in terms of estimating changes in productivity of systems and in many places throughout the world there is nothing other than fish-catch data to use as a research management tool. We fully accept the fact that they are not the absolute best data, they are not as carefully collected as research data, but fishery people throughout the world use them with that knowledge.

Q Just to round this out, wouldn't you agree that when the Department of Fish and Game spends over a million dollars on acquiring catch data, it is not to use it exclusively? It supplements and adds to other data that are acquired, and that's not an indication they, therefore, believe that strict reliance on catch data is a good measure of abundance?

DR. HERZ: Absolutely, that is one of the reasons that we use a variety of different measures of abundance of fish in the system and didn't just restrict our analysis to catch data.

Q I hope this isn't repetitive of an answer you gave to Mr. Turner, but this has to do with the biological justification for an analysis which uses various life periods of catch abundance. For example, chinook salmon, could you tell me what lag periods and what averaging interval you would use with chinook salmon, and can you tell me how this works biologically with a species that at most spends only a few months and usually only a few days in the bay?

DR. HERZ: What we used for most of our averages and most of our analyses, the ones that showed the strongest relationship again were the three-to-five-year range, if you combine both the flow period averaged and the lag.

I think the important part of your question is, though, that regardless of how much time fish spend in the system, and I think it is usually considerably more than several days, the conditions that they encounter while they are in the system are not simply the flow of that few days or weeks or months, but according to the basic premise on which our work rests, it is a cumulative system and the conditions are established by flow conditions that are preceded by as much as a number of years, and that's why we used the procedure we did.

Q Do you know how many races of chinook salmon are found in the Central Valley system?

DR. HERZ: I'm not a fisheries biologist. That's not one of the areas that I can comment on.

Q Well, let me suggest to you that prior testimony indicates there are four, and my question, perhaps without knowing the precise number, you could still be able to answer, I don't know. Given that there are several races of salmon, do you know if they all migrate through the system up and down at the same time?

DR. HERZ: Of course, they do not. They are named by the different runs, the different time of the year that they make their migrations.

Q Would you expect similar flow-abundance relationships with all four races?

DR. HERZ: I can't really address that question because we really only looked at the fall run and it should be noted the fall run migrate out of the system during the spring season, so there's reason to believe spring-flow conditions are relevant to their life cycle.

Q But may not be relevant at all to the life cycles of other races?

DR. HERZ: If the system functions the way we believe it does, as an accumulative averaging system, then it should influence all races, but we have been led to believe that only the fall run is a large, significant part of the salmon fishery -- I mean the salmon production in the system. Therefore, we focused our attention on that race, and also, because there were data available on the return migrations for that race.

Q Do I understand correctly it is your testimony that some cumulative or long-term average of slows in the springtime can be used as an indicator of survival of other than fall-run salmon; is that what you just said?

DR. HERZ: I didn't say survival.

Q Health, abundance?

DR. HERZ: Abundance, yes.

Q Were you present when the Department of Fish and Game and Fish and Wildlife Service presented their testimony on striped bass, salmon and shad?

DR. HERZ: I was not.

Q Notwithstanding that, would you be able to tell the board whether your analysis is intended to supplement or replace the fish agencies' testimony?

DR. HERZ: I don't know that we view it as doing either of those things. We view it as an independent analysis, an analysis using techniques that were somewhat different than what anybody else was using?

Q Have you discussed your analysis with the Department of Fish and Game?

DR. HERZ: Yes.

Q What has been their reaction?

DR. HERZ: Various reactions from various people at various times.

Q Dr. Leopold, I have some questions for you. I have some questions on the four-basin index. In your testimony, your written testimony, on the first page, and perhaps a little bit you might have to answer for Dr. Rozengurt, if you are able to, in some of his presentations -- you say that the data base was reviewed in some detail. It appears that during the planning and construction stages of water development and diversions in the Sacramento system, two data compilations were used, the four-basin index and a previous one, which I guess is referred to as the Shasta index; isn't that correct, that these two, Shasta and four-basin index, were essentially developed, promulgated in about 1965 for the Shasta, and around 1976 for the four-basin index?

DR. LEOPOLD: That's my understanding.

Q Can you tell me if you have knowledge of this, in what fashion these indexes were, in fact, used in the planning and construction of the project?

DR. LEOPOLD: I can't answer that from the Tiburon report.

Q I see.

DR. LEOPOLD: That's the reason that I went on to make my own analysis of the data.

Q Is this statement in the Tiburon report -- I'm referring actually back to, I guess it is their Exhibit No. 1, page 1.39, that says: It is interesting to note that despite this obvious inconsistency, the Shasta flow year-type classification was used as the environmental background during one of the most important periods of California's water development when the major water facilities were built and numerous contract obligations were adopted.

Do you recall if that's the reference that you relied upon?

DR. LEOPOLD: Yes, I think that's the reference.

Q That really doesn't say that the projects either in planning or construction relied upon them, it says they occurred at the same time.

DR. LEOPOLD: Yes, that's correct.

Q So, getting right down to the four-basin index itself, I want to refer to Exhibit 21. It's No. 8. I think this is the central point that is being made here. I am also going to be referring to Exhibit 20 in these references.

I would like you to keep in mind Figure 31 following page 8 in Exhibit 21, and it follows page 43 in Exhibit 20. This is the same figure used in both and it is referred to in these quotes, and these are quotes -- item 8 from page 7 of Exhibit 21, and I would like to get your reaction to this in total: Current decsions, including D-1485 regarding water distribution in California, are based on water-type classification system, four-river index, which excludes 25 percent of the Sacramento-San Joaquin rivershed. As a result, the normal long-term mean four-river index runoff, and it has Q=17.2 million acre-feet, in 1921-1978 account for only 61 percent of the normal Sacramento-San Joaquin River inflow to the Delta originating from 100 percent of the basin, and it shows a Q average of 20.2 million acre-feet.

Therefore, evaluation of wetness of the years, residual runoff and consequent planning for water diversions based on the four-river index overestimates the level of water availability in a manner incompatible with the relatively meager natural levels of runoff, and I understand natural to be unimpaired.

And then a similar conclusion that is made with reference to this same point in this same graph, Exhibit 20, is that -- and this is underlined, this illustrates, however, the four-river index water year type classification system on which D-1485 is based biases potential decision making by classifying dry years as normal or wet, thereby minimizing the significance of alarmingly low outflows to the bay over the last decade and promoting an erroneous conclusion about the existence of water surpluses when there are none.

Isn't what is happening here, a comparison of the two figures, and let's refer to Figure 31, is that they are taking 17.2 million on the left, which is classified in the four-river-basin index as a normal year, and a line is being drawn over to that same point on the right which is the unimpaired inflow to the Delta, and saying this is really not a normal year, this is really a subnormal year or a day year?

Is that your understanding of the point that's being made here?

DR. LEOPOLD: Yes, and that's the reason I made my own independent analysis of this because I did not attempt to determine whether the four-river index was comparable to using a whole record, so that what I did was I made a comparison of the Department of Water Resources historical inflow from the Sacramento valley, that's DWR Table 7, for the 45-year period 1922 to 1966. For that, in the board's Decision 1485, a definition was set up of dry, below normal, wet, based on the Sacramento valley data. Then, I wanted to compare that with the previously designated year classification of critical, below normal, above normal, and I found the following thing which I said in my previous testimony .

MR. THOMAS: Excuse me, if you are referring to a figure there, I believe we can display it here in the hearing room.

DR. LEOPOLD: Yes, it's this one here. What I wanted to know was if you --

MS. LEIDIGH: Can you use the figure number or something?

DR. HERZ: Exhibit 31.

MR. THOMAS: This exhibit has not been previously referred to. We can give an exhibit number for the record, if you wish.

MR. ANDERSON: Actually, if I could, Mr. Chairman, and I appreciate your forthcoming explanation, but I did understand your testimony. I believe I understand how it does differ from Dr. Rozengurt's, so I really don't need a clarification of that, so I don't think we do need to go into it.

Q So, what I want to ask you is, do you understand the four-river-basin index to be an index of water availability in the system?

DR. LEOPOLD: I presume, but as I say, I did not study the actual data for the four-river-basin index. I understood it was an index, a surrogate for the total flow.

Q And is it your testimony that you have any reason to believe it's not a good index or not a good surrogate?

DR. LEOPOLD: No, I cannot tell you that because I did not make a comparison in my own writing.

Q So, you are not able to agree or disagree with the criticism that I just made?

DR. LEOPOLD: No, I am not.

Q Dr. Herz, are you able to shed some light on this?

DR. HERZ: It's my understanding that the basic purpose of this figure, and basic criticism of the difference between the two systems, is to point out the fact that it is not only an index, but in determining how much water is diverted each year, the four-river index numbers are used to determine what year type classification each year is, and there is a bias built into that which ends up resulting in the bay getting less water than it would if the whole system were used.

Q I believe that's absolutely incorrect. You have evidence that it is used as an index of diversion or depletion of historic inflow to the Delta? It is, in fact, an index of total water availability; is it not?

DR. HERZ: But that is not the issue I am addressing here. What I am addressing is my understanding that the Department of Water Resources each year makes a determination as to the year type we are experiencing and based on that determination a decision or a set of decisions is made regarding how much water can be exported from the system, and it is my understanding from my collaboration with Dr. Rozengurt that the point he is trying to make here is that this system of using only the four-river index rather than the full basin as a basis upon which this determination or classification is made ends up creating a bias such that more water can be diverted out of the system in drier years than would be possible if the classification system were based on the entire watershed.

Q So, you don't think any index would be adequate, you would demand the full measurement of -- I don't know how you do that -- full measurement of available water than using an index of available water?

DR. HERZ: Certainly, in terms of establishing the year-type classification, because that's the principal point that we were addressing at this point, and the four-river index may be a reasonable surrogate, as Dr. Leopold would say, but that's not the issue.

Q Would you agree, Dr. Herz, that the issue is for the board to in some fashion determine what water is totally available, and then, to determine what uses ought to get that water in some sense, and in that sense, the fact that the Department of Water Resources or the Bureau of Reclamation, or anyone else, relies upon that determination through an index is absolutely proper and approporiate?

DR. HERZ: But we are talking applies and oranges here. I am talking purely and simply about the basis upon which a classification calling a year dry, critically dry, average, whatever, and you are talking about a measurement tool, and I am not addressing the measurement aspect. I am addressing the degree to which the classification system is biased because it uses the smaller four-river index in terms of what kind of a year we have.

Q But an index, the fact that numbers in an index are smaller than the actual flow is no bias; is it?

DR. HERZ: That's what an index is, one number that stands for another.

MR. MAUGHAN: I'm not sure we are going to make much more progress.

MR. ANDERSON: I have no more questions then.

MR. MAUGHAN: All right, Mr. Anderson.

Mr. Sanger, do you have any questions?

CROSS-EXAMINATION

by MR. SANGER:

Q Dr. Herz, just following up on the last question, isn't it true that there is a table in a prior submission by Romberg Tiburon Center in its earlier testimony on hydrology that actually shows that years that were classified by the Department of Water Resources as being years of a certain type in accordance with the four-river index turned out to not have the same frequency distribution when total flows were used as the basis for classification?

DR. HERZ: Yes, that is right, and that is what produces the bias to which I was speaking a moment ago, and ends up with a higher proportion of dry years and a higher amount of water being permitted to be diverted out of the system because the year-type classification is different with the four-river index than it would be if the full basin --

Q You mean a higher proportion of dry years if total flows are taken into account?

DR. HERZ: Yes.

MR. SANGER: Thank you.

MR. THOMAS: For the record, the reference that Mr. Sanger made is to Figure 3-1; isn't it?

DR. HERZ: Yes, that is correct, 3-1, Exhibit 20.

MR. MAUGHAN: Mr. Nakagawa, do you have questions?

MR. NAKAGAWA: No questions, Mr. Chairman.

MR. MAUGHAN: Mr. Dawdy, do you have any questions?

MR. DAWDY: I would like to ask a couple of questions.

MR. MAUGHAN: Then, Mr. Somach will be next.

CROSS-EXAMINATION

by MR. DAWDY:

Q I am representing David Dawdy.

I would like to ask Dr. Leopold a couple of questions. There was a question about the unimpaired flow index that the Department of Water Resources used two acre-feet per acre in their adjustment to obtain that figure. It was intimated that perhaps those figures could be wrong. It was intimated they may be too low and, therefore, the results might be wrong. Based on the Department of Water Resources' results, I would ask two questions: Do you have any reason to think that the Department of Water Resources has repudiated its computations of the unimpaired flow index?

DR. LEOPOLD: Not to my knowledge.

Q If they were wrong, could they be wrong large or wrong small?

DR. LEOPOLD: Presumably you would have to make a study of both the area covered by different vegetation types and a careful comparison of the tule evapotranspiration loss for each type by season; in other words, because it cannot be assumed that the evapotranspiration is uniform either from year to year or through seasons.

I'm not sure that answers your question.

Q I think it does.

And don't you think that's probably what the Department of Water Resources did in deriving their figures?

DR. LEOPOLD: One presumes so because that's about how you would go about it.

MR. MAUGHAN: This is speculation, Dr. Leopold. It doesn't help the record.

MR. DAWDY: Q I'm trying to elicit here the fact they might be wrong does not mean that the conclusion is in favor of one side or the other, but it could be a random error.

DR. LEOPOLD: I agree with that statement, yes.

MR. MAUGHAN: Mr. Somach. He represents the Central Valley Project Water Association, et al.

CROSS-EXAMINATION

by MR. SOMACH:

Q Is the revised Exhibit 20 substantively the same as the prior Exhibit 20?

DR. HERZ: Yes.

Q What types of modifications were made in the revised Exhibit 20?

MR. THOMAS: I think I can speak about that. They were numerous and both documents, I assume, have been made available to Mr. Somach.

MR. SOMACH: Both documents have not been made available to Mr. Somach, only the original one.

Was there a subsequent submission of some kind?

MR. THOMAS: The unbound copy was submitted initially for the record. A number of errors were found that made us decide to just reproduce it again in a bound version, and that's the version that we have referred to.

MR. MAUGHAN: How was it distributed after you did that?

MR. THOMAS: We sent the record number to the staff for distribution to the exhibit centers.

MR. MAUGHAN: I see some shaking of heads.

MR. THOMAS: We have the correspondence that will confirm that and we also provided the revised version to adequate parties.

MR. JOHNSTON: No.

MS. LEIDIGH: I asked Mr. Beringer about this and he had his staff check and so far as he can tell, he can't find the revised version was ever submitted to the board staff.

DR. HERZ: The only light I can shed on this is that day before yesterday I authorized payment of a bill for $97 or so for the shipment of those 38 copies up here to the board, and that was done in early October, I believe, and I think that it's true that we did not distribute any copies other than the 38 copies that we originally distributed to the board. We did not distribute any other copies of anything other than the revised version to people who requested them.

We have here with us, in fact, the gentleman who physically transmitted those copies, who can verify they were, in fact, sent to the state board under a cover letter that explained what I just stated, that they were in substitution for the original copy.

MR. MAUGHAN: Just one last comment. I don't think we are going to resolve anything here. It is still the staff's viewpoint that they have not received them. Is that what you are saying?

MS. LEIDIGH: Yes.

MR. BERINGER: Yes, we checked our mail log for that period and there was no entry of incoming documents, and we also checked with participants, other participants if they had received copies, which they had not.

DR. HERZ: It was the understanding that 38 copies were sent up here to the board for distribution to whatever your list is. I don't know how you distribute the copies that are sent to the board, but that was where they were to be sent.

MS. LEIDIGH: Those are for distribution to a lot of locations. In addition to that, the parties request copies from you or whomever provides the copies, and they pay you for them. If there are changes after that, I would expect you would tell them about the changes after they acquired copies from you.

DR. HERZ: As I indicated, it is my understanding we distributed, other than the 38 copies that were distributed to the board on the submission date to meet the requirements. There was no distribution of anything other than the revised copy to people requesting copies from us directly.

MR. NAKAGAWA: Mr. Chairman --

MR. MAUGHAN: There is not much point in going on very long here.

MR. NAKAGAWA: Just so I cover a little clarification about what we are talking about, I have a copy of the bound edition which I sent away for to the Romberg Tiburon Center. Am I correct that this is the revised version?

MR. MAUGHAN: That is the revised version, the bound copy. The difficulty that presents itself in the record is that a lot of the other people received the early one and there was not any real indication what the changes were, and you just heard Mr. Thomas say they were numerous and there's a dispute over whether other people received them, including our staff.

I don't think we can resolve it right now, so let's go on.

DR. HERZ: Number one, it is my recollection that Mr. Nakagawa's request was the first outside request that we serviced and received, and it was serviced with the revised edition.

MR. MAUGHAN: But other people may not have known it was even available.

DR. HERZ: The only other way that people would have gotten copies of the original report would have been through your distribution here, the 38 copies, because we did not distribute them.

MR. SOMACH: As a practical matter, many people obtain copies of exhibits, and that's through going to one of the sites where the documents are lodged to obtain Xerox copies of those exhibits, so if the board hasn't been given a revised copy, then none of the parties who relied upon the board's record would ever get them.

DR. HERZ: We are not going to be able to untangle this. From our perspective, we shipped the requisite revised copy to the board. We have the bill from the shipping service.

MR. MAUGHAN: Board Member Ruiz has a question, but I really would like to go on.

MS. RUIZ: I would like to go on, but I want to understand clearly from the parties if this bound copy has changes in it from the one that was stapled and mailed to the board, and it was received by the board; is that correct?

DR. HERZ: That is correct. As we go through the questioning, however, I think it might be useful if there are problems to compare the two texts because the changes, for the most part, were not substitute changes. They were --

MR. THOMAS: Mostly typographical changes.

MS. RUIZ: You did not prepare an errata sheet or delineate what changes were made from the original which was lodged with the board?

MR. THOMAS: That's correct.

MS. RUIZ: Thank you.

MS. OTSEA: What is this errata sheet?

MR. MAUGHAN: That has nothing to do with it. Let's go on. The record shows there is confusion.

MR. SOMACH: Q Is the preface to Exhibit 20 a part of Exhibit 20 in terms of what is stated there?

DR. HERZ: It was submitted as part of Exhibit 20, yes.

Q Okay. Is it a basic assumption in your work and in the report, and I quote from the preface, "In basic environmental conditions, however, estuaries are very similar all over the world." And I believe that that quote can be related to pages 1 through 17 as well as pages 18 through 28 where there is a great deal of discussion and charts and graphs with respect to estuaries all over the world. Is that a basic premise -- as a statement, is that accurate in terms of the premise that much of the report is based upon?

DR. HERZ: Yes.

Q If the basic assumptions were not true, if all estuaries throughout the world also had some basic differences which would result in different answers to fundamental questions, your report would have an analytical flaw in it; isn't that true?

DR. HERZ: No, not at all. One does not expect all estuaries all over the world to be absolutely identical. They have certain similarities and they have certain differences, and the goal of the scientific investigation is to develop logs that hold for as general a set of conditions as possible. So, what we have stressed is the similarities among estuaries and, in fact, it is quite striking that the flow-productivity relationships that we see in San Francisco Bay exist in other estuaries. The numbers, the values of the flows that produce a given value of catch, will not be identical in any two estuaries, but the basic phenomenon of flow and productivity or flow and fish abundance should be similar in all estuaries.

Q I am looking for a yes or no answer to this question. Is it your opinion that in spite of the fact that there may be some basic and fundamental differences in estuaries throughout the world, that nonetheless, information obtained from studies in other estuaries throughout the world can be utilized on a one-to-one basis in terms of analyzing these estuaries?

DR. HERZ: I can't answer that with a yes or no because I don't understand what you mean one-to-one relationship with this estuary.

Q Let me back up and ask you the first question perhaps. How do you define the term estuary?

DR. HERZ: Estuary is a variety of definitions.

Q I want to know the definition that you and Dr. Rozengurt utilized in this report in the context of the comparison that you are making with other estuaries throughout the world.

DR. HERZ: I think we used the definition that says something to the effect that an estuary is a semi-closed body of water which is a meeting place between fresh and saltwater.

Q Okay. Differentiate for me, if you would, the difference between the estuary that you just defined and San Francisco Bay?

DR. HERZ: There isn't any difference.

Q So that they are synonymous terms that one can utilize the term estuary interchangeably with the word San Francisco Bay?

DR. HERZ: No, the San Francisco Bay estuary is the river, Delta, bay and adjacent coastal zone. The bay is those hunks of the system that are between the Delta and the Golden Gate.

Q So then, your first answer to my question in terms of defining an estuary was an inaccurate definition; is that correct?

DR. HERZ: No.

Q You didn't define estuary the same way you just defined, or you didn't define the bay in relation to the estuary in the same way you just defined estuary?

DR. HERZ: I define an estuary as a meeting place and the system that I described is, in fact, a meeting place of fresh and saltwater, and those are basic components that I think anybody who studies estuaries throughout the world accepts as the definition of an estuary.

Q Well, humor me, because I am not anyone around the world who studies estuaries. I understood your original definition of an estuary to be a self-contained embayment, and I asked you whether or not that was synonymous with San Francisco Bay and you essentially said, yes; and then, I went further and asked you an additional question and you now define estuary as not just the embayment, but all the other rivers and systems feeding into the bay.

Which is accurate?

DR. HERZ: They are both accurate. I think we are engaging in a semantic argument here, and perhaps if you would go on to try to make the point that you are trying to make by differentiating between the two, it might help me, but as far as you have gone now, I don't see the point of the question.

Q Your seeing the point of the question isn't particularly relevant. I am merely looking for a definition that you utilized in terms of writing this report and referrring to estuaries all over the world. I asked you for a definition of estuaries. I have several on the record now. Can you give me one more time what your definition of an estuary is, the one that when we look back at the record we should refer to is your definitive answer, and if you don't know, you can also obviously say you don't know what an estuary is.

DR. HERZ: I think I won't choose the latter. I would like to differentiate between the classical definition of an estuary which is the first definition I gave you, a semi-enclosed body of water which is the meeting place of fresh and saltwater, and if it will clarify things at all, to refer to the second thing that I defined as an estuarine system, which includes the rivers, the Delta, bay and adjacent coastal zones.

Q Which definition is the operative definition for the report, Exhibit 20? Is it the classical definition or is it the modified estuarine system definition?

DR. HERZ: I am hard pressed to choose because I think we are probably not 100 percent consistent throughout our exhibit and because if you were to look at the authors who study estuaries, you would probably find as many definitions of estuaries as there are estuarine scientists, each one having a slightly different variation in the wording they use to define estuary.

Q Well, if that's the case, isn't it true then that reliance upon studies of estuarine systems or estuaries that are based upon a different definition as you have indicated may be the case all over the world with every different scientist, have limitations in terms of relating to this particular, whichever way you define it, estuary or estuarine system. Don't we need a common definition to make common or basic assumptions? Yes or no, and then explain.

DR. HERZ: Yes, we need a common understanding and I believe that we have it. I believe that the nuances that I referred to in terms of the definitions that differ among estuarine scientists do not change -- the basic components of an estuary or estuarine system are agreed upon. There is quite a high level of agreement among scientists around the world and I think there would be very little disagreement. There might be, you know, one percent of the estuaries around the world in which some people might prefer to not call it an estuary, but I think in general, there is a high level of agreement of what is considered to be the subject matter when you are talking about estuaries.

Q In your preface, and I quote, is a statement that says: San Francisco Bay is a classic example of colonization by foreign species.

In a situation of conflict, should flow requirements be managed for natural or foreign species?

DR. HERZ: Yes.

Q I don't think that particular question, if you were listening, elicited a yes or no response.

DR. HERZ: A yes means, from my perspective, both. When you look at the introduced species that we are dealing with, particularly in our report, these are introduced species that have been in the estuary for so long, the two introduced species, shad and striped bass, have been in the estuary for over a hundred years, and according to most people are quite well adapted to this system.

Q You have to listen to the question. I said in a situation of conflict between the natural and foreign species, should flow requirements be managed for natural or foreign species?

DR. HERZ: I don't know that we have a conflict.

Q The question was hypothetically speaking, if there is a conflict, what is your view? If you have none, just say you don't have a view, you haven't thought about it or it's not an important question.

DR. HERZ: I have thought a lot about it. I haven't thought about it in a hypothetical conflict situation. I see the State Department of Fish and Game placed in a position by the large number of striped bass fishermen, they are placed in a position of having to manage that species as one of the principal species that derives income for their department and they, therefore, spend a tremendous amount of time and energy managing that species because there are on the order of over 100,000 fishermen a year who fish for that species.

MR. MAUGHAN: Dr. Herz and Mr. Somach, I appreciate these are very important matters to each side, but I think what is being sought is your opinion on these things, and if you have one, you can say it; and if you don't have one, just say you don't have one, and then we can move on.

DR. HERZ: I have no opinion.

MR. SOMACH: Q Fine. I quote again from the preface and I quote: The basic reason for this greater impact is that the system of bays and rivers is small, yet we are trying to use its freshwater to produce a mesophysic agricultural environment in a near-desert region.

Is it your view that the agricultural economy of the Central Valley does not exist and that, rather, we are trying to do this, trying to establish that type of agricultural economy?

DR. HERZ: The principal focus of our research was not to consider the agricultural economy, it was to consider the needs, the resource needs, fishery needs of this system.

Q So, do you disavow the statement in the preface I just quoted?

DR. HERZ: Do I disavow it? First of all, I didn't make it but it is part of our exhibit, and could you reiterate the question? I have lost the train of the question.

Q The question is, focus on the word trying, and the quote assumes somehow that we are trying, and this may be an impossible task, and the question I am asking then to rephrase it or to restate is whether or not you consider the agricultural economy of the Central Valley to be still in the state of trying, or rather, it is in existence today?

DR. HERZ: I don't feel prepared to address that question.

Q On page 19 of the exhibit is this statement:

Today, largely as a result of massive diversion of river water, up to 85 percent of total flow during the critical spring season in some years for irrigation.

I believe that may not be an exact quote, but essentially what you are talking about there is a reduction of flows due to diversions of river water and tying those diversions into irrigation. What about diversions from municipal and industrial purposes, doesn't that contribute to the reduction in flows to the Bay-Delta?

DR. HERZ: I'm sure it does, but my understanding is that 85 percent of the state's water is used for agriculture.

Q I am asking whether or not municipal and industrial diversions contribute to the reduction of inflow-outflow with respect to the bay.

DR. HERZ: Yes.

Q And what about flood control and the operation of the various upstream facilities that contribute, perhaps not in the reduction, but a shifting in terms of when that water is available to the Bay-Delta; isn't that correct?

DR. HERZ: Yes.

Q Now, I believe with respect to someone else's testimony you indicated that none of the preparers of this report are fishery biologists, although you attempted to qualify Dr. Rozengurt simply because of his involvement with the other reports similar to this one as being capable to talk in terms of fishery biology matters; is that accurate?

DR. HERZ: That's right.

Q If we assume for a moment that fishery biology is an expertise that requires extensive training and involvement in the various nuances of it, would it be accurate to state then that the fishery information that is involved within Exhibit 20, rather than being presented from a biological perspective; that is, presented by a biologist based upon studies that a biologist had conducted, is rather a cataloguing of statistical information with respect to catch and other fishery issues?

DR. HERZ: I think most of the analyses that are done of the estuarine system reflect a data base that very often is not collected by the people who actually perform the analysis.

So, in this case, our first submission, our hydrology report, was not based upon research that we conducted, but rather, Department of Water Resources data. Similarly, this report is based upon our analysis of data collected by other agencies.

Q So, that's a yes to the fundamental question that I asked; isn't that correct?

DR. HERZ: Restate the question.

Q No, I'm not going to restate the question.

DR. HERZ: I am not going to give you a yes or no --

MR. SOMACH: Can we have the question reread?

MR. MAUGHAN: We can, but we have had this trouble before and we shouldn't keep repeating questions and re-answering questions, but go ahead.

(The reporter read the question: If we assume for a moment that fishery biology is an expertise that requires extensive training and involvement in the various nuances of it, would it be accurate to state then that the fishery information that is involved within Exhibit 20, rather than being presented from a biological perspective; that is, presented by a biologist based upon studies that a biologist had conducted, is rather a cataloguing of statistical information with respect to catch and other fishery issues?)

DR. HERZ: I can't answer that question.

MR. SOMACH: Q Let me restate it. Isn't it true that the biological information within Exhibit 20 is merely a cataloguing of statistical and other information collected from other sources rather than data developed by biologists specifically for this report?

DR. HERZ: Yes.

Q Okay.

MR. MAUGHAN: That took five minutes and I really think I understood it to begin with.

MR. SOMACH: Q I note on pages 37 through 39 of the report there is a cataloguing of facts and figures. Those are accurate; are they not?

DR. HERZ: Which one are you referring to?

Q I assume they are if they are in the report. I am talking about all of them and I assume they are all accurate, and I am just trying to get confirmation.

DR. HERZ: If they are in the report, I would hope that they are accurate.

MR. THOMAS: We may have a problem if you are referring to the unrevised copy.

MR. SOMACH: And I am.

MR. THOMAS: We don't know what you are talking about.

MR. SOMACH: Let me refer you to the revised report, page 36 where it begins: Water diversion, economics and environment," through page 39.

MS. RUIZ: Would it be possible to have another copy of that, please, the revised version? Does anyone have a copy of the revised version I can look at?

MS. LEIDIGH: Mr. Thomas, do you have an extra copy of the revised version?

MR. SAMANIEGO: We have one up here.

MR. THOMAS: Apparently, all of the additional copies that were brought have been distributed.

MR. SOMACH: Q Referring to the revised edition, page 38, there is a statement which I will quote for the board's assistance. It states: Because of this water, meaning the water developed and delivered primarily by the State Water Project and the Central Valley Project, and some additional from the Colorado River, California is the largest agricultural manufacturer in the nation.

It goes on to say: It produces over 200 commercial varieties of crops and livestock with a value of 10.5 billion dollars in 1979.

Is that a correct reading of that statement?

DR. HERZ: It would appear to be, yes.

Q Now, on page 39 there is this statement: The striped bass, shad and Dungeness Crab have experienced almost the same level of decline. Since 1957 up to 1986, losses sustained by the recreational fishery, and actually, this is the portion of the quote I am interested in, since 1957 up to 1986, losses sustained by the recreational fishery account for 1.5 billion dollars.

Is that an accurate reading?

DR. HERZ: Yes.

Q So, if a comparison were made between the 10.5 billion dollars related to irrigated agrciulture in the valley in one year versus a loss of 1.5 billion dollars over a 29-year period -- let me restate that so you understand me.

In order to make a comparison between the productivity or the economic benefits since we are talking about economic figures here that you presented, would it be fair to make a comparison that would include the comparison of the 10.5 billion dollars related to irrigated agriculture in a one-year period as opposed to a loss of only 1.5 billion dollars over a 29-year period?

DR. HERZ: No, I don't think it would be fair because it doesn't include the value of other aspects of the fishery in San Francisco Bay and the coastal zone, and this only refers to the losses sustained. So, if you are going to compare value of agriculture, you have got to compare value of agrciulture with value of fisheries, and if you look at some of the figures, it is my understanding we have got -- I have seen a 2 billion dollar a year figure placed on California fisheries.

Q Where does that information come from? Does that come from Meyer?

DR. HERZ: That comes from Pacific Coast Federation of Fishermen's Association. I have heard it used on multiple occasions, so I mean, we were not doing an exhaustive economic study, and I don't think any economics comparison that you make from things in our report are really valid because we are addressing fisheries biology and not economics.

Q If I could point you to page 54, there you state that cumulative losses of such magnitude are believed to be one of the major factors responsible for salt intrusion and salinization of the Delta and bay. Is that an accurate quote?

DR. HERZ: Yes.

Q I don't know that it's on page 44, to think of it.

Now, indeed, there is no evidence that the Delta is being salinized; is there?

DR. HERZ: I think that there does not currently exist accurate data to evaluate the degree to which there have been changes.

Q Is that a yes or no answer? Do you want me to repeat that question

DR. HERZ: The basis of that statement was that based on calculations relating flow values to levels of salinity, there is reason to conclude that there have been these changes. However, the data that are necessary to validate that statement do not exist currently.

Q Are you familiar with DWR Exhibit 60?

DR. HERZ: Why don't you refresh my memory of it.

Q The exhibit deals with various issues dealing with salinity, and I believe that -- let's assume for a moment that DWR Exhibit 60 establishes that salinity intrustion in the Delta has been substantially less under post-project conditions than under pre-project conditions. Assuming that that is an accurate statement with respect to what DWR 60 states, would that be the type of evidence that you are referring to that is lacking in terms of making a definitive determination?

DR. HERZ: State that again for me, would you, the quote?

Q Let's take a hypothetical situation and in this hypothetical situation, DWR Exhibit 60 says or shows that salinity intrusion in the Delta has been substantially less under post-project conditions than under pre-project conditions.

Do you understand the hypothetical I am posing to you.

DR. HERZ: Yes.

Q Assume that is correct, wouldn't that be evidence contrary to what your intuitive feeling is with respect to salinization of the Delta?

DR. HERZ: No. What I am saying is that my recollection of Exhibit 60 is that it did not contain information that anything other than surface salinity and that's not how you adequately measure salt intrusion in the estuary.

Q Okay. So, at the most then, what you are saying is there simply is no evidence, no data?

DR. HERZ: That's right.

Q Okay. There has been no evidence that the bay is being salinized -- there is no evidence that the bay is being salinized; is there?

DR. HERZ: There are no data.

Q The same response, there are no data to show that.

Have you taken a look at State Water Contractors' Exhibit No. 266 --

MR. SAMANIEGO: Mr. Somach, what you read was on page 54 in starting your salinization question?

MR. SOMACH: Yes.

MR. SAMANIEGO: Page 54 of the earlier submission?

MR. SOMACH: Yes, let me go to it.

MS. RUIZ: Is that one of the revisions in the revised draft? Was that revised out?

DR. HERZ: I can't answer that without having the two pieces, the two copies in front of me.

MS. OTSEA: It's on page 52 at the bottom of the new report.

MR. SOMACH: I only have the old one. Yes, it's on the bottom of page 52 of the bound volume.

MR. MAUGHAN: It has not been changed.

DR. HERZ: No, it looks to be identical.

MR. SAMANIEGO: We were trying to follow in the revised and could not find it on page 54.

MR. SOMACH: Q So, just to summarize, then the response to those questions, your statement is that that statement that we are referring to is made in the report, but there is no data to support it; isn't that correct?

DR. HERZ: Yes.

Q Now, starting on page 54 again, and I think this is 53, after conclusions, for those of you that are following in the new and revised one. You have listed a number of factors that characterize water development in the Sacramento River basin and the San Joaquin River basin; isn't that correct?

Do you want to go ahead and take a look at it? It's on page 53.

DR. HERZ: Yes.

Q It says: Pre-project period, 1915 through 1943, then a post-project period, 1944 through 1983.

DR. HERZ: Yes.

Q Have you compared the population of California during the 1915 through 1943 period versus the 1944-through-1985 period?

DR. HERZ: No.
Q So, you have done no comparison of those figures?
DR. HERZ: No.
Q Isn't it true that California's growing population also establishes and characterizes the water development during each of the two periods in question?
DR. HERZ: Yes.
Q On page 55 of the old version and in the middle of page 54 on the revised version, you refer to the current and future of the Delta-Bay eco-system. When you make that reference, you are talking about the Bay-Delta eco-system status or health; are you not? Is that what that statement deals with?
DR. HERZ: The current annual diversions result in 35 to 55 percent reductions -- is that --
Q No. The statements starts with "Given," in fact, in the new version we have underlines here, so I guess it is very important, and what I am reading is the last phrase there, that is what I am referring to because I think it modifies the sentence when it says current and future of the Delta eco-system is in question. We are talking about the health, the status of that system; is that correct?
DR. HERZ: Yes, that is true.
Q In your opinion, is the bay in a state of crisis if

one were to exclude issues surrounding the striped bass?

DR. HERZ: There is reason to believe that the bay has suffered a fair amount of deterioration as is reflected from the status of some fishery stocks, yes.

Q Could you define your understanding of the word "crisis"? Tell me what that word means and I will use your definition of crisis.

DR. HERZ: Crisis reflects a critical period, a period that relfects some major change that is likely to have an impact.

Q As differentiated from the situation where the impact has occurred, how would you differentiate the two, just so I understand how you are using the word "crisis".

DR. HERZ: I would perhaps have said pre-crisis to define what you said, but I want to make sure I understand how you are defining it.

Q How would you define that situation once the event has happened? In other words, once the inevitability of the crash, this cataclysmic event that is implied in your definition happens.

DR. HERZ: I don't think that I subscribe to the cataclysmic crash school, that there is an event that will be likely emptying of San Francisco Bay or any major single event that will mark going from non-crisis to a crisis situation. It is a process of deterioration to which we were referring. It was not crisis versus non-crisis.

Q Okay, using your definition then, in a period of deterioration, let's assume a sliding scale of deterioration, one being the beginning of the deterioration and ten being the bottom end of that deterioration; does the definition of crisis you are using include the entire scale or a portion of the scale?

DR. HERZ: I, frankly, don't know.

Q Okay, in your opinion, is the bay toward the one end of my scale, that is just beginning to deteriorate, or is it toward the ten where it is just about there, if it is not already at the bottom of what could happen to it?

DR. HERZ: I think the statement that I made here reflects the fact that we are currently diverting on an annual basis about 60 percent of the annual flow in some years, and as we have indicated and as you have quoted, as high as 85 percent during some springs. That means we are in a range that in other estuaries has resulted in very serious decline and in some cases total dispersion of some commercially important species from that system.

So, we are someplace beyond the mid-point, and I suppose the diversion figure is the easiest way to conceive of it. We are 60 percent of the way across that continuum.

Q If I understand the rest of your statement, that conclusion is based upon the assumption that diversions of water as have occurred in this system is basically based upon that observation of amount of diversions from the system?

DR. HERZ: It's based on that in relation to a bunch of other estuaries around the world that have been looked at similarly in terms of the proportion of change in diversions over time in the past.

Q But nothing based upon this particular estuary other than the flow issue?

DR. HERZ: Is what?

Q You said that your conclusion, where we were in terms of scale of crisis, was dependent upon two things; diversions, the amount of diversions in comparison with the second thing, what's happened in other estuaries throughout the world.

DR. HERZ: There are two parts. It has to do with the amount of diversions, number one; and number two, the responses of the system to those increases in diversion which appear in some of the other lists that you have referred to here, having to do with changes in fisheries production, things which you have pointed out are speculative, like saltwater intrusion, and loss of nutrients and other influences which we ascribe to diversions increasing.

Q But the hard data that you are referring to is from other estuaries, that's why the report spends so much time dealing with the other estuaries; isn't that correct?

DR. HERZ: Well, the hard data in this report, the purpose of this report, although we spent some time discussing the theoretical underpinning, is to analyze the changes in flow and the resulting fish abundance measures that we talk about in the report itself.

Q Okay. I am going to go on to another line of questioning here. On page 63 of the old report which begins Chapter 4, 61 of the new report, you indicate essentially that the 1915-through-1931 period used in your data analysis may be too short for a valid statistical analysis; isn't that correct?

DR. HERZ: I don't see that statement.

Q Well, let me ask you, in your opinion, is the 1915-through 1931 period adequate for a valid statistical analysis?

DR. HERZ: I don't think we would have used it if we didn't think it was adequate.

Q So, your testimony is that is an adequate period for a valid analysis; is that correct?

DR. HERZ: Yes.

Q Following page 115, Figure 6-8, can you explain for me what that figure is attempting to show?

DR. HERZ: It is showing a number of different things. It shows over the period 1960 to the mid 1980s production of striped bass eggs, the annual index of young striped bass in Suisun Bay and in the Sacramento-San Joaquin Delta in Fish and Game data, and it shows the cumulative total withdrawals of freshwater from the system during that period.

Q Is there some conclusion that we are supposed to reach from the figure, and if so, what is that conclusion?

DR. HERZ: I think the purpose of presenting these data was to show the reciprocal relationship between these indices of striped bass abundance and the increases, cumulative increase in freshwater diversion out of the system, with the total amount being on the order of 40 times the volume of San Francisco Bay by the end of '83, I guess, having been diverted out of the system.

Q So, the figure purports to show a cause-and-effect relationship between the diversion and striped bass index?

DR. HERZ: It's a very interesting association between these two phenomena.

Q Well, isn't it true that you could have plotted almost anything on a cumulative basis between 1960 and 1983, and shown the same type of relationship? Can you think of anything that wouldn't have shown the same type of relationship assuming those data were cumulative as this information is?

Take the sale of rock-and-roll records, for example, can we make some equation there on the same analysis?

DR. HERZ: Well, I think that the cause-and-effect, number one, correlations do not necessarily, as you are trying to point out, show cause-and-effect relationships. They merely show an association between two phenomena, and what is interesting in this figure; number one, is the degree of reciprocity, the fact that the slope of the two lines seem to be mere images of each other, but let me point out once again, as I did in our direct testimony, that no one correlation stands on its own.

The fact that we have a variety of sets of correlations with three different species, pre-and-post project with commercial catch, recreational catch and other measures of fish abundance, suggests that these associations are not as fortuitous as the sale of rock-and-roll records, and decline of striped bass productivity in the system.

Q So, if I understood all of that, Figure 6-8 standing alone really doesn't show that cause-and-effect relationship; isn't that accurate? It can't be used for that purpose standing alone?

DR. HERZ: Standing alone, probably not.

MR. MAUGHAN: Let me interrupt. How much more do you have, Mr. Somach?

MR. SOMACH: I have some more.

MR. MAUGHAN: Three minutes or four minutes?

MR. SOMACH: More than three, perhaps half an hour.

MR. MAUGHAN: I would like to get a good place to stop.

MR. SOMACH: This is as good a place to stop as any. It doesn't get any better than this.

MR. MAUGHAN: Let's get back by one o'clock.

(Noon recess)

WEDNESDAY, DECEMBER 9, 1987, 1:00 P.M.

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MR. MAUGHAN: All right, we will go back on the record. Before we start, there is a little note from staff about the copies.

MS. LEIDIGH: Yes. For the record, I would like to note that staff has found the revised copies of Exhibit 20 which were sent to the board and we do have them.

MR. MAUGHAN: All right, that clears up that little bit of confusion. I hope everything else will be cleared up the same way.

All right, proceed, Mr. Somach.

MR. SOMACH: Q Isn't it true that striped bass spawn in the lower Sacramento River and in the Central and South Delta? Is that an accurate statement?

DR. HERZ: Yes.

Q Is there any evidence that the striped bass spawn in the Western Delta?

DR. HERZ: I can't answer that.

Q You don't know?

DR. HERZ: No.

Q Would anybody that assisted in the preparation of the report know that?

DR. HERZ: I'm sure Dr. Rozengurt knows the answer to that question.

Q What evidence do you have for the Delta that would indicate that where striped bass do spawn there are any salinity problems?

DR. HERZ: You will have to give me a moment or two for me to look up a couple of things in the report.

Q Well, let me kind of short-circuit this. If you look at page 121 you deal with the Western Delta and you indicate that there may be a salinity problem there if one were to assume that striped bass spawn there.

Do you see that? It's 121 of the old.

DR. HERZ: What is the heading?

Q The heading is Spring.

MR. CUMMINGS: Page 119 in the new report.

MR. SOMACH: Yes.

Q If you look at "Spring," then I think the statement generally is at the bottom of page 118 and the top of page 19.

DR. HERZ: Okay. What's the question?

Q The first question I asked you was whether or not you had any evidence that striped bass spawn in the Western Delta and you indicated that you simply did not know.

DR. HERZ: That's right.

Q Okay, then I asked you whether or not you had any evidence that would indicate where striped bass do spawn where there is a problem with respect to salinity.

DR. HERZ: Well, there seems to be ample evidence in the literature that salinity is an important factor for successful spawning of striped bass and that there are certain parameters, within a range of parameters, within which salinity must fall if you are going to have successful spawning.

Q But that really doesn't respond to the question because the question was looking for specific evidence for the Delta where striped bass do spawn salinity is a problem.

DR. HERZ: I can't answer that question either.

Q You can't answer the question because you don't know?

DR. HERZ: I don't know. In fact, to amplify on that, we really were not addressing salinity. We were only looking at the relationships between flow and production or abundance of fish.

Q But the implication is that salinity within the areas that striped bass spawn in the Delta create the problem of spawning. I'm not sure that I articulated that well.

But the implication is, in fact, there is a cause-and-effect factor in this Delta in the area where striped bass spawn; is that not true?

DR. HERZ: We didn't directly address those data sets, that information, in this study, so I can't really answer that question either way.

Q That's fair enough. So, what you are saying is the report does not purport to make such a statement?

DR. HERZ: That's correct.

Q Okay. And then, anything implied from that would be erroneous, implied from the report to establish that, would be erroneous?

DR. HERZ: Yes.

Q Your recommendations of 17 million acre-feet and 2.5 or 7.5 million acre-feet at the end of your report, I think we discussed -- I think they are on the same pages here as they were.

DR. HERZ: The recommendations are on page 146 of the revised report.

Q Okay, and we have the charts then which follow those pages, or are they on the pages in the revised book?

DR. HERZ: They are adjacent.

Q And one of them is labeled "Spring Runoff" and the other is "Annual Runoff"?

DR. HERZ: That's right.

Q I just want to make sure that I understand what appears to be the significance of that data. Are you familiar with DWR Exhibit 26?

DR. HERZ: Perhaps -- I don't remember them by number.
Q This time I happen to have a copy. You have never looked at that?
DR. HERZ: No.
Q Now, earlier on I had understood you to say that the data on unimpaired flows were derived from DWR sources; is that correct?
DR. HERZ: That's right, but all of this work was done prior to February, 1987, so it is earlier versions of DWR data, and I don't know the degree to which these data resemble the data that we used in our analysis.
Q Why don't you turn to page 37 of that report and take a look at that chart a bit to familiarize yourself with what it purports to show.
DR. HERZ: Okay.
Q Now, if you look down at the bottom of that -- first of all, why don't you describe what the chart is on page 37 of DWR Exhibit 26?
DR. HERZ: It states that it is Delta unimpaired total outflow, estimated outflow in thousands of acre-feet, 1921 to 1983. It presented monthly and total.
Q Okay, and there's a total for each column; is that right?
DR. HERZ: That's correct.

Q And there's also an average for each column; is that correct?

DR. HERZ: Q Okay. Do the figures on the table look similar to the types of figures that you were utilizing, particularly if one would look at the averages on the bottom, that you utilized when you made your conclusions, which I believe in response to questions were that the average for the April, May and June period, if you were in the middle range of your recommendations, you would need 64 to 70 percent of the unimpaired runoff.

Are those the type of figures that you utilized, and what I am talking about now -- are those the types of figures that you utilized, and when I am talking about types of figures, the figures that are averaged at the bottom of page 37, is that where you looked to determine --

DR. HERZ: I am, frankly, unclear because if I read this table correctly, it says the average annual total is only 28,000 acre-feet a year and that can't possibly be the total average for the 1921 to 1983 period, and therefore, I think there's got to be something wrong with these numbers. The table says estimated flow in thousands of acre-feet. I am reading the table the way it was given to me.

Q Let's assume it says a million then?

DR. HERZ: Then, I have got to rethink these numbers.

Q Actually, I think it is accurately presented there, but so as not to create any problem or dispute on that point, assuming that you are reading that 28 million --

DR. HERZ: It's nice to know that Tiburon Center isn't the only organization that makes mistakes in --

Q As I said, I don't think there's an error there.

MR. MAUGHAN: It's probably 28,000 thousand.

DR. HERZ: Oh, that may be. Okay. Your question is, is this similar to the information that we used to draw our conclusions?

MR. SOMACH: Q That is correct.

DR. HERZ: And since Dr. Rozengurt is the one that did those calculations, I cannot address that question. I didn't do it.

Q Let's assume for a moment that those figures are accurate and let's take again your range of May, June and July, the spring runoff, what is that range on your chart?

DR. HERZ: Total spring?

Q Right.

DR. HERZ: 6.9 to 7.5 million acre-feet.

Q Is that for the three months?

DR. HERZ: That's cumulative for April, May and June.

Q And in terms of how much of the --

MR. MAUGHAN: You divide it by three.

MR. SOMACH: Q That's the question, is that what you do?

DR. HERZ: If you want an average, you get 2.3 to 2.5 million acre-feet.

Q On the table, what are the totals in terms of April, May and June, averages?

DR. HERZ: 4,186,000, 4,239,000 and 2,711,000.

Q Okay. So, with respect to July that we are talking about, you gave some figures and I'm not sure where you got it, but if those figures are correct that you have just read for June, you would be talking about virtually the entire flow; is that correct?

DR. HERZ: You said July, did you mean June?

Q June.

DR. HERZ: Yes.

Q Dr. Leopold, if I understood your testimony, essentially what you did was review Exhibit 20 and then explain it?

DR. LEOPOLD: I don't know what Exhibit 20 is.

Q That's this report, the fat one.

DR. LEOPOLD: Yes, what I did was to study, particularly the table on annual flows.

MR. THOMAS: In the interest of having the record accurate, I think it was Tiburon Center Exhibit No. 1 that was provided to you for review, which was not this document, but the document that was submitted for the hydrology portion of the hearing.

MR. SOMACH: Q So, you have never taken a look at this exhibit for analysis purposes, this exhibit meaning Exhibit 20?

DR. LEOPOLD: No, I think this is a different one than the one I studied.

Q Well, that's somewhat of a curious thing, and I am wondering if you can explain why it was necessary to have someone take a look and explain a report that purportedly should explain itself.

DR. LEOPOLD: The Tiburon report is hard to read particularly for persons who have not been used to frequency analyses, and therefore, after having read it, I felt it could be better explained by a slightly different set of analyses, and that's the reason I made my own analysis.

Q Well, focusing on questions that were asked you earlier with respect to the four-basin index analysis, it appears to me that what you testified was that you rejected somewhat that analysis and moved towards some other data rather than the four-basin index; is that correct?

DR. LEOPOLD: I tried, as I said before, I looked at the four-basin index and I compared it in my own analysis against the similar data for 100 percent of the area and came to similar conclusions as to what the Tiburon report stated.

Q Do you have any independent evidence on Bay-Delta salinities?

DR. LEOPOLD: No, I do not.

Q Do you concur -- out of curiosity -- with the assertion that all estuaries can be treated exactly alike?

DR. LEOPOLD: Well, I think to make it a little clearer, what is not an estuary is when a river debauches directly into the ocean without going through any kind of bay, and what we mean when we talk about an estuary is defined as a system in which there is a bay of some kind. The Sea of Azov, San Francisco Bay, would be an example with river water coming into the head of that, so that you can talk about the bay eco-system or you can talk about the details of the Delta itself. But, in ordinary parlance, San Francisco Bay would be an estuarine system.

Q Well, let me follow up on that and say would it be proper in evaluating the estuarine system then to isolate on a point within the bay, Chipps Island, I think is what we are talking about generally here, as opposed to taking a look at impacts upstream of that point in the Delta and even upstream of that into the rest of that estuarine system? Can you make conclusions about the estuarine system without going further upstream than just "bay"?

DR. LEOPOLD: Well, that's the reason all of us are computing total inflow and total outflow because you have to consider this variable zone within which the mixing of the water occurs. None of us stick to only one point in the system to make an analysis.

Q But you stick pretty much to one issue, flow. Well, aren't there other variables within the system that can account for declines or other kinds of impacts upon the health of the species which ultimately find their way into the Delta system?

DR. LEOPOLD: Yes, there's a whole series of other parameters, but for the most part the most pronounced effect on an estuarine system is going to be the water balance, the balance between the incoming freshwater and the saltwater which it is displacing.

Q Now, in that analysis is there not an assumption in terms of the data you presented that there has been a reduction in the amount of water available to the Bay-Delta? I mean, isn't that a basic underlying assumption of what you have --

DR. LEOPOLD: Well, with the diversions upstream clearly the amount of freshwater outflowing from the Delta has been decreased. Is that what you mean?

Q That is right.

DR. LEOPOLD: That is right.

Q Okay. Are you familiar or is Dr. Herz familiar with State Water Contractors' Exhibit 260A?

MR. THOMAS: What is the title of that?

MR. SOMACH: I have copies. Let me show them to you.

DR. LEOPOLD: No, I have never seen this before.

MR. SOMACH: Q Dr. Herz, have you seen this before?

DR. HERZ: I have seen it, yes.

Q Did you hear the testimony with respect to that particular exhibit? It was actually presented in these hearings on bay inflow.

DR. HERZ: No, I didn't hear the testimony.

Q Do you understand what that exhibit purports to show?

DR. HERZ: I think I do.

Q Can you explain what you believe it purports to show?

DR. HERZ: Delta outflow is unchanged from the twenties to date.

Q Or unchanged perhaps in an uprward trend; is that correct?

DR. HERZ: I don't know if I would go that last step, but certainly, this purports to show that there is no marked change.

Q Now, assuming that were correct and granted that you haven't had a chance to take a look at the exhibit at any length and apparently didn't hear the testimony, but let's assume for the discussion here that it is correct; would that in any way alter the basic assumptions that went into the development of Exhibit 20?

Let the record reflect that Mr. Thomas is consulting, perhaps coaching with the witness.

DR. HERZ: What the record doesn't reflect is whether the witness accepts the counsel of his counsel.

Now that we have had that little exchange, can you restate the question?

Q The question generally was assuming that the information that is shown, the data that is shown on State Water Contractors' Exhibit 260A is accurate, assuming that that's accurate; doesn't that seriously undermine some of the basic outflow assumptions that were made within Exhibit No. 20?

DR. HERZ: I don't think it speaks to the assumptions underlying the report. The report was an analysis of data, of flow data and fish catch or fish abundance data, and it made no assumptions about how much water there was in the system or there will be. It was simply analyzing, comparing the relationships between flow and fish during the entire period of record.

Q Let me go on. That exhibit speaks for itself as does yours in that regard.

With respect to some graphs that you have gone through, if you can take a look at your graphs 6-2, 6-3, 6-4 and 6-5, that series of graphs --

MR. MAUGHAN: I thought you were speeding along by going to Dr. Leopold.

MR. SOMACH: Well, actually, we are moving here trying to keep everyone a little off balance by moving back and forth with some dexterity through the exhibits.

Q Have you got those at hand?

DR. HERZ: Yes.

Q Okay. Also, while you are pulling out exhibits, and this may make it much quicker, the recent exhibits you gave us --

DR. HERZ: The errata?

Q 7-5. Why don't you pull that one out also so you can see it?

DR. HERZ: Okay.

Q Aren't those graphs driven in great detail in the case of the six-dash numbered graphs by 1918 and with respect to 7-5 and other similar graphs within your report driven by 1918, 1917, 1916?

DR. HERZ: If you are asking whether if you remove those three points from the scatter plots, it would affect the correlations, the answer is obviously yes. You can say that about almost any three plotted points in any of the graphs that if you remove those, they are going to change your correlations, so I'm not quite clear on what you mean "driven by."

Q Well, if you were going to take a look at a line, I think I have done that somewhere in here, we have drawn a line through a similar chart; in fact, you presented those on the board.

DR. HERZ: Right.

Q Can we find one of those for an example? Let's see if I can find one quickly.

DR. HERZ: 7-1.

Q 7-1. There we go, shad. What does the line that's drawn through those data points purport to represent?

DR. HERZ: It purports to represent a best-fit line describing the correlation.

Q Right, and the correlation is an upward trend flow to pounds; right? Isn't that what that's showing?

DR. HERZ: Upward trend to pounds?

Q The more flow the more pounds?

DR. HERZ: Oh, yes.

Q That's right; isn't it?

DR. HERZ: Positive relationship between these two factors, yes.

Q And if you were to exclude 1916 and 1917 on graphs 7-1, you get pretty much of a flat line; don't you, assuming that's what you were going to do?

DR. HERZ: One could argue as easily that the lower part of the curve, since it is a curve that's got two slopes representing it, would remain relatively the same if you remove those two points, and it certainly is not a flat line indicative of no correlation.

Q Might there be other explanations for high poundage in 1916, 1917 and 1918?

DR. HERZ: Perhaps.

Q Have you explored other factors other than just simply this flow to poundage relationship?

DR. HERZ: Not specifically those years, no.

Q Have you taken a look at fishing effort, for example?

DR. HERZ: As far as we know from the information that we have available, the effort does not seem to be markedly changed during those couple of years relative to the years on the other side.

Q From an historical perspective, was there anything significant in the country during that time?

DR. HERZ: There were a few men off fighting a war.

Q And returning and so forth during that period of time, 1918 at least, which drives the other chart --

MR. MAUGHAN: Well, yes, I think you are trying to testify now, Mr. Somach.

MR. SOMACH: I am just suggesting. Did you take a look at any of those historical issues on whether or not they may have an impact or might not have an impact upon those data points? That's a simple question.

DR. HERZ: What we used were existing data on the commercial fishing effort that came primarily from reports of Fish and Game. There was only an indication. If you look, as I recall, at the salmon figure that we presented there, there is some speculation that the shape of that curve was affected by World War II.

Q So, there was at least in some of the analyses that correlation perhaps. I actually have looked at those charts for the later period and note in those years after the second World War you also have higher figures.

DR. HERZ: You have to bear in mind that the issue is not simply the level of effort, it is the catch per level of effort and if we had that information available, it would be a relatively simple matter of trying to correct for catch per unit of effort. With only the effort, with no information about how much the effort was decreased during World War I and increased immediately after, we cannot do very much to correct that point, and I think it is moot. We can't attribute that point to effort any more than it just also happens to be if you look at -- what year are we looking at, '16 and '17 -- '18 is especially high, but it is also an especially high flow year.

Q So, there are a number of factors. All I am asking is if there are other factors besides flow that may have contributed to the high poundage in those particular years. That really is all I was asking.

DR. HERZ: My answer was we didn't look at that information, we only looked at the flow and catch.

Q In your analysis, generally in Exhibit 20, you have used a number of different data sets, I believe 1916 through 1931, 1916 through 1936, and others; isn't that correct?

DR. HERZ: Yes.

Q That really isn't normally accepted practice in a statistical analysis; is it?

DR. HERZ: To use different data sets?

Q That is right. Isn't it acceptable practice for you to pick a data base and construct a model that best explains the data base as opposed to looking around and formulating your conclusions based upon data that you have picked up? I mean, is that inaccurate? You can say yes or no.

DR. HERZ: Not necessarily --

Q Rather than answering the question, since you haven't responded yet, let me ask you, isn't it accepted practice to pick your data base, then construct your model that best explains the data?

DR. HERZ: Well, you must remember that in several places in the report and several places in our testimony we indicate that we are doing either exploratory correlations which are undertaken to determine whether there are basic underlying relationships that seem worthy of further study and that's where we are at this point.

In terms of hypothesizing in great detail, having detailed hypotheses that guided every analysis, we were not doing that because that was an exploratory investigation.

Q So, you are saying it's not accepted practice to pick your data base and construct a model that best explains the data?

DR. HERZ: That's not at all what I said.

Q I know it's not what you said, but what you said didn't answer my question. I understand you did a lot of things in your report, and I am merely looking at accepted analytical practice. If you are not trying to explore a whole bunch --

MR. MAUGHAN: One more chance to answer, but I do think --

MR. SOMACH: Couldn't I get just a yes or no?

MR. MAUGHAN: Well, you can get yes, no, I don't know or I am not going to talk.

DR. HERZ: We believe that this technique of exploratory investigations is perfectly appropriate for the data sets that we use.

MR. SOMACH: Q Okay, let the record reflect that you didn't respond to the question in terms of yes or no.

MR. THOMAS: I believe he did respond. His response was that an exploratory method is acceptable.

MR. SOMACH: It's on the record and we will proceed from there.

MR. MAUGHAN: Yes, please.

MR. SOMACH: Q So, in the report itself you talk about peer review of the report. What is your definition of peer review.

DR. HERZ: Peer review is considered to be the distribution of a document to people with expertise in that field or who have published in that field to solicit their feedback on the document.

Q So, it is not more formal than that?

DR. HERZ: That's right.

Q Okay. So, it is just kind of sending it around to a bunch of people you know and asking for their comments, or that you know of?

DR. HERZ: It can be that collegial, it can also be sent to people who you know might be very critical of the work because you want an honest assessment of your work. You want to know whether it will stand up to severe cross-examination by the State Water Contractors.

Q Dr. Herz, you participated in the preparation of this exhibit?

DR. HERZ: I did.

Q What is your Ph.D. in?

DR. HERZ: Behavioral biology.

Q And how does that relate to this report?

DR. HERZ: Well, some of my published research has to do with the behavior of fish and the effects of toxic substances on fish. I actually do not claim to be a fishery research expert. My expertise was as it was claimed by Mr. Thomas at the outset, that my expertise over the past 15 years working on San Francisco Bay has been primarily to do with marine policy and the use of scientific and technical information in marine and estuarine public policy decision making, and I think this is a good example of where that scientific information is very relevant to these kinds of decision.

Q Mr. Thomas's flowery introduction aside, in terms of your academic expertise, what in that -- and I have looked through the regime in some great detail and I am trying to figure out exactly is it the experience over the last 15 years as opposed to any academic Ph.D. experience that you bring to the report?

DR. HERZ: I think the only things that I can claim in my academic background that are relevant are a variety of courses in speciology, biochemistry, neurophysiology of fish and fish behavior, and a number of publications, probably a dozen, somewhere between a dozen and 20 publications having to do with fish behavior, are the only academic claims I have in this area.

MR. MAUGHAN: I hope we are getting close to winding up on this phase.

MR. SOMACH: I just have a few questions related to some interesting issues that came up in direct.

Q You indicated that your recommendations, the recommendations we discussed earlier, which you cite at 64 to 70 percent and 63 to 70 percent, depending whether you are looking at three months, April, May and June, or annual, were consistent with the U. S. Fish and Wildlife Service's recommendation regarding flows; is that correct?

DR. HERZ: I said that the cubic feet per second flow rate during the spring were similar to my understanding of the Fish and Wildlife Service's recommendations. I didn't say anything about the proportion of flow.

Q Well, maybe I didn't understand that any better than I understood your answer. What was the purpose for citing the U. S. Fish and Wildlife Service recommendations in conjunction with your recommendations?

DR. HERZ: Only that it was my understanding that the numbers were quite similar.

Q Did you do any balancing, did you do any impact analysis with respect to the impacts of committing those amounts of flows to Bay-Delta uses in terms of the impacts upon consumptive users of water?

DR. HERZ: Our purpose was simply to look at the needs of the resource and that's the only thing that our recommendations are directed at.

Q Okay. With respect to recommendations made by you in terms of EPA, the National Academy of Sciences, I tried to take accurate notes during that period of time, and it appears to me that the assumption for making those recommendations is either that the State Water Resources Control Board is not qualified or it is not an independent agency.

Which of those two is accurate, or are they both accurate?

DR. HERZ: Have I stopped beating my wife? The issue that I was --

MR. WALSH: Did I get here just in time?

MR. MAUGHAN: The last question for Mr. Somach.

DR. HERZ: The issue being addressed was that it was the feeling that the voluminous information resulting from this set of hearings, particularly the hydrology, oceanography material would benefit by having outside review by a totally independent entity, which perhaps was more familiar with the world-wide experience of the effects of freshwater diversions on estuaries.

Q So then, you are talking about qualifications and independence, that's what I gleaned --

DR. HERZ: Talking about expertise, qualifications or expertise.

Q And independence?

DR. HERZ: Independence as well.

MR. SOMACH: Okay, I don't have any more questions.

MR. MAUGHAN: All right. That takes care of that.

Do we have anyone else that would like to cross-examine? Mr. Schulz. Anyone besides Mr. Schulz?

All right, sir. I know you are brief.

Is staff going to have questions?

MS. LEIDIGH: Yes.

CROSS-EXAMINATION

by MR. SCHULZ:

Q Dr. Herz, I was interested in your earlier discussion of the estuary and estuarine system, and as I understood what you were saying, the estuarine system was basically made up of, shall we say, three components; the Delta component, San Francisco Bay component and the off-shore component?

Would that be an accurate summary of what you said?

DR. HERZ: I think I said four. I said river, Delta, bay, coastal zone.

Q Okay. So, you would go above the Delta into the river system also in your description of the estuary?

DR. HERZ: Yes.

Q How far up the river would you go?

DR. HERZ: That's a difficult absolute question to answer. I would go up the river sufficiently far until I was satisfied that there was little or no influence left from the saltwater inflow and tidal input.

Q Okay. Now, I would like to try to have you answer questions breaking down the estuary into those pieces, and let me ask you to make the following breakdown, the river, the Delta to Chipps Island, San Francisco Bay and the off-shore coastal area, and I would also ask you in response to the questions to break down your answers from the bay down to, if you can, Suisun Bay, San Pablo Bay, Central Bay, which would be defined as from San Pablo Bay to the Bay Bridge, and South Bay. Is that an acceptable breakdown of the components for you?

DR. HERZ: Okay.

Q Now, you indicated that you dealt with three which you felt were better indicator species, salmon, striped bass and shad; is that accurate?

DR. HERZ: Yes.

Q With respect to salmon, did your analysis indicate which of those regions are causing any of the problems that you perceived in the salmon population and catch, and which of those regions did not seem to be related? In other words, are you aware of any problem in off-shore coastal areas that are affecting the salmon catch or anything else?

DR. HERZ: I think the best answer to that question is what we were addressing was the relationship of freshwater inflow or Delta outflow on the species, and we did not attribute the changes in the relationship to any one particular location over any other?

Q I am not trying to be devious here at all. I am trying to recognize that in these proceedings the board will be setting standards, be they salinity standards or outflow standards, and in order to determine whether there is a need for a particular outflow standard to protect the particular beneficial use, it seems to me they need to know where the problem is occurring. In other words, there has been testimony with respect to salmon that maybe the problem is in the area of Rio Vista and not below, and I am just asking you whether, in your analysis, you made any distinction between the estuary, the bay, the Delta in terms of your conclusions and recommendations?

DR. HERZ: We did not.

Q Are you aware yourself of any problems that are being created to salmon populations in the area of the estuary below Carquinez Strait?

DR. HERZ: That's not what we were addressing, and anything I would say would be purely speculative at this point.

Q You, as having studied and worked in the bay for a number of years, don't have any independent opinion in that regard?

DR. HERZ: I am here to testify about the report that we did and that report didn't address the area of your question, and I don't feel that I can answer that question.

MR. MAUGHAN: Mr. Schulz, it looks like the record is clear on that point.

MR. SCHULZ: Yes. Previously when experts have appeared and they have information outside of their specific report, it has been allowed to ask them whether in their expert opinion they have any information in those areas.

MR. MAUGHAN: You did ask that and I thought you got your answer.

MR. SCHULZ: Q I would like to go to a couple of your tables. The first one is 6-7. I don't know what page it is at.

MR. MAUGHAN: Page 11; isn't it?

MR. SCHULZ: Yes, it is. It depends whether you are in the revised or not. I am in the original.

Q 6-7 is opposite page 116.

DR. HERZ: Figure or table?

Q Figure. Line No. 3 is the Delta outflow line; is it not?

DR. HERZ: The deviation of regulated outflow, yes.

Q Can you tell me why that line stops in 1982 while the rest of the data goes out to 1986 or '87?

DR. HERZ: Those are five-year --

Q Aren't they back averaged?

DR. HERZ: Well, no, as best I am able to tell, the inflow data we have that we were working with only went to '82.

Q But they are back averaged; aren't they, they are that year and the prior four?

DR. HERZ: Yes.

Q Are you familiar enough with the flows in the period afte 1982 to give us an estimate of where you feel that line would go if you extended it out 1986 or 1987, whether it would go up or flat or continue down?

DR. HERZ: Well, again, recall these are averages. You are not going to see an absolute response to the higher flows that occurred, and that, in fact, was not the reason why we did not use it because in some of our individual year plots --

MR. MAUGHAN: But it is a five-year average; up, down or flat?

DR. HERZ: It probably would go up a slight amount, but I don't know what amount.

MR. SCHULZ: Q So, if you extend that line out, you would have shown probably a slight increasing, using your words, in the outflow average by the continuing decline in the other figures; is that correct?

DR. HERZ: No, the other two lines would remain the same.

Q Would have remained the same, okay.

I would like to draw your attention to two of your figures, 5-4 and 5-18. 5-4 is one of the tables that follows page 83 and 5-18 is just before page 92. Now, as I take a look at those two tables, they both appear to be salmon catch in the bay and Delta, and the only difference is the one is 1916 to 1936, that's 5-4, and the other one is 1944 to 1958.

Is that correct, that they are both measuring the same catch, the same catch data, or essentially the same catch data?

DR. HERZ: Yes.

Q Okay. Now, on 5-4, you used a four-year running mean, lag one year, and on 5-18, you used a three-year running mean, lag two years.

Now, I want to ask you, was there something that happened after 1936 that makes that a biologically significant change? Why, if you were looking at the catch in just two successive periods did you change your correlation equation? Is there any biological significance?

DR. HERZ: I think the only answer that I can give you is that the critical period being represented by the year of catch in the preceding period is that they are all in the three-to-five-year window, where we consistently demonstrate the best relations, and whether it is three and two or four and one may not have a biological basis -- what I need to have in front of me and I don't have, and it would take me too long to find it, is the figures that look at three and two for the pre-project period and four and one for the post-project, to see how different those relationships are.

They may still be significant correlations. What we obviously did is present some of the strongest correlations in figure form.

Q My understanding of your three-to-five-year concept is that it is your conclusion that the Delta and the bay eco-system is an accumulative system and that, therefore, conditions that may have existed one or two years before a particular smolt comes downstream might affect its survival to adulthood; is that correct?

DR. HERZ: That is correct.

Q Okay. For salmon, can you tell me what biological conditions, more favorable biological conditions are created by that cumulative effect that you believe would be responsible for that?

DR. HERZ: I can hypothesize about some of them. I don't have the data that necessarily gives me total confidence to make the statement that this is a theoretical explanation for the relationships that we have, but all of the factors that we have listed in the report having to do with freshwater serving as a barrier to saltwater intrusion, the delivery of nutrients, influences on flushing capacity, all can have accumulative effects that might well influence the conditions under which the estuary is at -- the preceding several-years conditions relative to salinity, relative to nutrients, relative to pollutants, can determine the current state into which the organism comes to spawn.

Q Would it be a fair statement that the basis of your conclusion in that regard is founded primarily on the existence of the correlations that you found rather than on any biological investigations?

DR. HERZ: No, because, as I said at the outset, one of the basic premises that we began with in this work was that the system is this averaging system that does, in fact, respond on a cumulative basis and that there was, therefore, reason to believe that these lag flows would, in fact, be the most effective way to show some strong relationships.

Q I don't disagree with the lag flows. I think we have used them in previous testimony. I am dealing with the cumulative effects, the averaging.

DR. HERZ: Both the cumulative and the lagging are part of our basic premise that led us to do this work and, by the way, I wanted to add, in your asking about Figures 5-4 and 5-11, one of the reasons for the differences may be that the 5-4 is annual flows and 5-18 is spring flows being shown. That could very well account for the difference --

Q For the choice of different averaging and lag time?

DR. HERZ: Yes.

Q On page 39 of your report, you have the following statement: The striped bass, shad and Dungeness Crab have experienced almost the same level of decline.

Are we to draw the inference that it is your opinion or the opinion of this report that the decline in Dungeness Crab is related to the decreases in Delta outflow that you have set forth in your report?

DR. HERZ: That certainly has been put forth by some people as one of the explanations, but I don't know that we would definitively want to be tied to that as the only explanation. Fish and Game went through a very elaborate five-year study a number of years ago and they were not prepared to conclude that flow was the principal reason for the crab decline, but they didn't rule it out as having any influence, as I recall.

Q One page back you have the statement at the bottom of page 38, I assume it is still there, at least it's close to there. The paragraph starts out: Currently, the two projects store more than 20 million acre-feet, around 80 percent of the unimpaired mean inflow, et cetera.

Do you find that statement?

DR. HERZ: I do.

Q Can you tell me whether the words "the projects store" relates to what they, in fact, store on an annual basis or is it the storage capacity of the reservoirs?

DR. HERZ: I'm afraid that's another one of those questions I can't answer. I don't know.

Q Finally, my last couple of questions deal with Figure 8-1 which follows page 145. Can you tell me why you utilized for the post-project period 1975 to 1978 a four-year period which includes the two driest years of record, including the historic driest year?

DR. HERZ: I don't believe, according to Figure 8-1 that I am looking at, that we use that period at all. We used the period 1955 to 1978.

Q I'm sorry. I did misread that. I thought that was a 7, not a 5, on my copy. Okay. That takes care of the problem.

The checkered part or the hatched part, the spring outflow needed for successful catches, is that all fish? I mean, is that striped bass, salmon and shad?

DR. HERZ: Yes, that's what that was to designate.

Q And it was your opinion that they all responded in the same fashion?

DR. HERZ: It was our opinion based on the conclusions and findings of our report, yes.

Q And is it then your opinion that there have not been successful catches of salmon in the period 1955 to

1978?

DR. HERZ: That's a valid question. I think what we were implying rather than catches, we probably should have used production or abundance, because our only post-project salmon information or post-'57 commercial catch data was from the fish returning to Red Bluff Dam, and also, that hatched stand shows what level of flow was necessary to produce the lėvel of commercial fish that was produced in the pre-project period, the 1925 to 1940 period, and meant to imply that perhaps if we went to the upper level of range, that we might well be able to produce conditions that would be adequate for once again having commercial fishing in San Francisco Bay.

Q Commercial fishing in San Francisco Bay?

DR. HERZ: For salmon.

MR. SCHULZ: Okay, that concludes my cross.

MR. MAUGHAN: All right. Staff.

MS. LEIDIGH: Yes, a few.

E X A M I N A T I O N

by MR. CUMMINGS:

Q Dr. Leopold, you mentioned that based on Arizona studies where there were on the ground pinon juniper clearings to test for measurements in increased runoff, you said it would take a century to show a change had actually occurred. Is that because there's too much noise in the data, or is that because --

DR. LEOPOLD: The variance is too large.

Q Is Arizona precipitation as variable as that in California?

DR. LEOPOLD: Probably more so. You are speaking about seasonally or geographically?

Q Seasonally.

DR. LEOPOLD: I think so, yes.

Q Dr. Herz, I am confused. I was reviewing page 103 of your document which addressed salinity effects on striped bass, and I was under the impression from your response to an examination by Mr. Somach that you were stating that the report doesn't address whether or not stripers have a problem of salinity in spawning areas.

Can you tell me which is the case?

DR. HERZ: What I meant to say or thought I said was that we did not directly address the salinity issue in our report, but that it was my understanding from the literature that there are very definite salinity ranges that must exist for spawning and for hatching of eggs.

Q I think my other questions would probably be best directed at Dr. Rozengurt regarding the rivers flowing into the Sea of Azov. Would you be able to answer, and if you can, will you?

Are there rivers used by the fish that run into the

Sea of Azov affected at all by either agricultural or saline drainage?

MR. WALSH: Don't answer in Russian.

DR. HERZ: I would if I could, but I won't because I don't know the answer.

Q Okay.

E X A M I N A T I O N

by MR. SUTTON:

Q I would like to try to get a clarification of your discussion of cumulative effects in the estuary versus flows. A lot of the data that you are looking at is commercial and recreational catch data; is that correct, as opposed to abundance?

DR. HERZ: Yes. Some of it is catch, some of it is abundance.

Q Any particular years catch will reflect one or more previous years of good or bad conditions relative to recruitments to the adult stock; is that correct?

DR. HERZ: Yes.

Q So, in a series of years, can you have one very good year which will have effects over several successive years in terms of catch?

DR. HERZ: I suppose that's possible.

Q So that when looking at cumulative effects, is it your testimony that in the past when you have had several good years you have produced large catches or that several good previous years has a beneficial effect on one particular year class, and I am excluding previous recruitment here.

DR. HERZ: That's an excellent question, and I don't know that our data and the way we have broken it down and analyzed it makes it possible to discriminate between those two possibilities.

MR. WALSH: Mr. Sutton, I want to make sure I understood. Let me try to phrase it. Are you saying that a very good spawning year, return spawn year can mask the effects of maybe the subsequent two or three bad years?

MR. SUTTON: Yes. It hasn't been seen as much here, but particularly on the East Coast they do have striped bass in Chesapeake Bay which are called super classes which are so significantly larger that the catch for six, eight, as many as ten years afterwards, reflects that one super class and not necessarily reflects the impacts good or bad in subsequent years, or previous years.

MR. WALSH: Correct. Is that what you are saying?

MR. SUTTON: Yes.

MR. WALSH: Okay, thank you.

MR. SUTTON: Q You have no further response to that?

DR. HERZ: No.

Q On page 146 of your revised Exhibit 20, you are discussing the recommendations of flows and you state those flows "must be maintained for periods of at least two to three consecutive years."

My question is, two to three consecutive years out of how many?

DR. HERZ: I think the statement says what's necessary, which is two to three consecutive years. It doesn't make any difference out of how many. Consecutive means consecutive.

Q In other words, are you saying, in essence, if it is not out of a longer period that you have to essentially have those same flows every year?

DR. HERZ: For several consecutive years, which implies that you could have years where you didn't have that flow, but in order to have the kind of levels of fish that we are talking about, you can't just have one good year and expect that you are going to restore a declining fishery or produce a good year class that you catch three or four years later or two or three years later.

MR. THOMAS: May I have justs a moment on that point?

DR. HERZ: I am advised that Dr. Rozengurt says that he now prefers two out of three consecutive years, so that does leave a little more breathing room.

MR. SUTTON: Q So, you are recommending in particular for spring flows that two out of every three years, you would recommend to have the flows of 6.9 to 7.5 million acre-feet during April, May and June?

DR. HERZ: Yes.

Q In that regard, are you also proposing that these flows be approximately equal in all three months?

DR. HERZ: No. They can be distributed in any way such that the average -- I mean, that the total for the period is in that range.

Q Even if, for example, hypothetically you could have, shall we say, 3.5 million acre-feet in April and May, and zero in June? Do you have a minimum?

DR. HERZ: No, that -- 3.5, did you say? That wouldn't work. You need to get a total for the three months of 6.9 to 7.5.

Q 3.5 in April and 3.5 in May and June, would still equal 7.0.

DR. HERZ: Yes, that is what this is saying.

Q So you do not have a minimum flow recommendation for any of the months; is that correct?

DR. HERZ: Not that we have included in this report.

Q I also want to confirm, I believe you testified earlier that you do not have an opinion on recommendations for varying these standards in different year types such as presently exists in D-1485?

DR. HERZ: No. It isn't that refined.

MR. SUTTON: Thank you. That's all I have.

MR. MAUGHAN: Anything else?

MS. LEIDIGH: No.

MR. MAUGHAN: Board members? I have a single question for Dr. Leopold. There has been a great deal of reliance in terms of the expert testimony we have received over many weeks now and actually over many years in terms of trying to develop correlations between certain events and certain cycles of runoff, and what have you, and I have seen in the past some of them work but I also note that some very key decisions are made on that basis, and then, I like to see what happens after those decisions are made.

I have asked questions of several people why the striped bass index, which was related to a correlation and after it was decided and imposed, things haven't worked out the way that it appeared that they would.

Dr. Leopold, if you recall, too, in the Colorado River, there was a lot of testimony about persistence of wet periods and dry periods, and California -- and I was associated with this, felt that there wasn't any water left over for new projects in Arizona, namely, because it looked like there was a persistent dry period and you couldn't count on it.

There again, California didn't prevail and the project has been built. The reservoirs have been full, brimming and spilling the last several years, which does indicate that even though you develop these frequency analyses and so on, either you have got a chance of occasionally getting some wet years or there's not too high a degree of reliance on some of these forecasts, and in that case, as I say, is where a lot of testimony went and it hasn't turned out to be the way it looked like it might turn out to be.

Do you have any comments generally about correlations and frequency analysis that you would like to leave with us?

DR. LEOPOLD: No, but I think you brought up a point that has to be kept in mind. The climate, especially in North America, started to change between 1945 and 1951. The trend has been generally toward a wetter, cooler period starting in the fifties and that has been accompanied by a greater variability, year-to-year variability and season-to-season variability.

The increasing carbon dioxide which is now well established, especially from the long record at <u>Monaloa</u> --

these are the people who have been studying this problem to the conclusion that the increase in the warming resulting from the growth of carbon dioxide values in the atmosphere will reverse the present trend were it to continue about the year 2010 or something like that, and so that even if the general tendency for a wetter period starting in the fifties continues, which we don't know whether it will or not, it will certainly be overcome and reversed in the early part of the next century due to the greenhouse effect.

You are absolutely correct in saying that correlations are useful indicators, but what we usually miss is the causal connection between two things which are being correlated. It has been brought out during the day to day that there appears to be a relationship between two variables, but we don't know enough about the mechanism to say that this is a cause-and-effect relationship.

One of the things that is highly necessary, it seems to me, for the long-term management of any water resource, is to pick out the major questions that have to be answered, see to it that data are being collected that are specific to answering such questions, and that the mechanism of the relationship be developed through research.

These are long-term propositions but I think that all three types of things ought to be considered by the board in setting standards -- first, what kind of research is going to be needed, how are we going to get it done in an independent fashion, what kind of data are going to be needed.

I doubt very much whether in the long run we can continue in a practical manner to carry on continuously the kind of data-collection effort that we now have in the San Francisco Bay region. There are too many stations, for example, that are measuring, as we said before, salinity, and maybe we are measuring the wrong aspect of salinity so that somehow or another the longevity and the utility, the usefulness of the data collection which is being proposed or dictated, required by the board, that has to be given a great deal of thought because, as you say, you cannot rely entirely just on the matter of correlation because they will not necessarily hold up in the next cycle.

MR. MAUGHAN: So, when we get all these correlations, we ought to be very careful as we examine them to look at all the points you have just enumerated, not just take them on the surface.

DR. LEOPOLD: Yes, I think that is correct.

MR. MAUGHAN: All right, thank you.

Mr. Thomas.

MR. THOMAS: Just a couple of points on redirect, Mr. Chairman.

REDIRECT EXAMINATION

by MR. THOMAS:

Q Let me direct this question to Dr. Herz. You testified that Tiburon Center Exhibit No. 20 was circulated for peer review and that you received back a range of comments including a large number of favorable comments. I wanted to ask you, did you receive comments to the effect that given larger resources and more time, there was further analysis that could have been performed with this data and additional data could have been gathered?

DR. HERZ: Yes, a number of comments that we received spoke to the issue that what we were doing appeared to be exploratory in nature and that there were a number of additional comparisons and analyses that could be performed if there was sufficient time and resources available.

Q And were these the kind of comments that you meant when you stated there was a range of both positive and negative comments?

DR. HERZ: That's right.

Q Did any of these commentators indicate fundamental disagreement with the conclusions you drew given your limitations on time and resources, and the preliminary nature of this investigation?

DR. HERZ: None of the peer reviewers to whom we circulated the document fundamentally disagreed with our conclusions. The most critical comments that we received spoke to some of the statistical manipulations we used in performing our analyses, but none of them disagreed with the conclusions that we reached or felt that the work was fatally flawed in any fashion.

Q Just a brief question or two on the salinity data. You testified that it is your view that the data is insufficient. We, of course, have had testimony during the course of these proceedings on changes in levels of salinity before and after the operation of the state and federal water projects, and I believe you are familiar with the State Water Contractors' Exhibit No. 266 and Figure 3 from that document, for instance, which is a figure showing those pre-and-post-project values.

DR. HERZ: Yes.

Q What does this documentation on changes of salinity show for the spring months that are reflected there?

DR. HERZ: Well, it does suggest that there has been an increase in salinity post-project.

Q Are the spring months of any particular significance, in your judgment, based upon the research you did on the relation between flows and fishery resources?

DR. HERZ: Absolutely. As I have already testified, the spring inflows are the most important because they establish the conditions for spawning and migration of a variety of species.

Q Now, I note that that figure presents information on the average monthly salinity at the measuring stations for those months of the year. Do you have a view on how that graph might look had it used salinity levels only for the low and critical flow years, rather than an average of all the water years?

DR. HERZ: I think there's no question that there would be a much more increase in salinity with those low flow years.

Q And is there reason to be concerned that the frequency of low and critical flow years has been increasing since the construction of the water projects?

DR. HERZ: Yes, both for the reason you are suggesting, that the potential increase in salinity which would result with increases in dry and critical years, and in terms of impact on abundance of the fish species and in terms of other conditions in the estuary that are affected by those flows.

MR. THOMAS: Mr. Chairman, I have no further questions.

MR. MAUGHAN: I think there may be sort of a contradiction between the first question you asked Dr. Herz and what is has been stated by Dr. Leopold.

Dr. Leopold indicated the cause and effect and the need for perhaps not only more data but better identified data, and so on. Dr. Herz implied that we have enough data and if we had enough money to massage it, we might get better answers.

Frankly, I think there's a contradiction. I want to know if Dr. Herz wanted to add anything more to what he has just said.

DR. HERZ: Well, I think it is quite obvious that the data we have on salinity is not adequate for anybody. Nobody is satisfied with what we have. Right now it is extremely difficult to really describe the salinity conditions, but I think the purpose of this last exchange was to show even in the contractors' report that there have been increases.

MR. MAUGHAN: No, I said the first question Mr. Thomas asked you concerning whether or not if you had more time and more money whether you could have done more than what you have already done, and I am sort of questioning whether that is consistent with Dr. Leopold.

Maybe you have done all you can do because your data won't permit you to do more than you have already done.

Do you agree or disagree with that? It seems to me that there's a contradiction.

DR. HERZ: There is one scientist I have talked to who is actually a statistician who was very interested in particularly looking at the salmon data because he feels there's a lot more information in there that can be extracted if the right techniques and procedures are used.

MR. MAUGHAN: Okay, I just wanted to know.

All right, do you want to offer your exhibits then?

MR. THOMAS: We move for the admission of Tiburon Center Exhibit Nos. 20 through 31, including Exhibit 20A.

MR. MAUGHAN: Do you have any objections? Okay, hearing none, they will be accepted.

MR. TAMBLYN: You didn't introduce 26 or 28, which are statements of qualification for Michael Rozengurt and Alice Rich.

MR. THOMAS: Let me amend that to pick up those statements as well. I am handicapped for not having, for some reason, a copy of the index in front of me. I need to correct that, Mr. Chairman, just to be clear.

Apparently, what we should move to admit at this time are Exhibit Nos. 20 through 27. We need not introduce Exhibit 28, the resume of Alice Rich, because, in fact, she didn't appear to testify, but we do move for also admission of Exhibit No. 31 and Exhibit 20A.

MR. MAUGHAN: All right. Once again, any objection? Hearing none, they will be received in evidence.

(Romberg Tiburon Center Exhibits 20 through 27, 20A and 31, were received in evidence.)

MR. MAUGHAN: Thank you, gentlemen. We are going to take a break now. We still have two other direct testimony plus four rebuttals. We sure want to get them in today.

Fifteen minutes.

(Recess)

MR. MAUGHAN: All right, Mr. Sanger, you may go ahead.

MR. SANGER: Thank you, Mr. Chairman. I am John Sanger of Pettit & Martin for the Bay Institute.

Mr. Mortenson, you have previously been sworn in these hearings; have you not?

MR. MORTENSON: Yes, I have.

MR. SANGER: Mr. Chairman, could I ask that the record show that Mr. Mortenson's qualifications have been previously introduced.

MR. MAUGHAN: Yes.

MR. SANGER: Just preliminarily, so there's no confusion, if I could give advance notice of items to be marked for identification?

MR. MAUGHAN: Yes, you may.

MR. SANGER: We have an errata sheet for Exhibit 49, which I think it would be appropriate to mark 49A.

MR. TAMBLYN: We have two volumes marked 49A and B. Could you label this C?

MR. SANGER: That's fine. We have 38 copies for the board and 40 or 50 additional copies for members of the audience.

MR. MAUGHAN: That will be 49C.

(Bay Institute of San Francisco Exhibit No. 49C was marked for identification.)

MR. SANGER: Secondly, I just want to remark in advance there will be six slides shown which I suggest be marked in the order of appearance 59 through 64.

MR. MAUGHAN: Fine, or you can go A, B, C.

MR. SANGER: I think it would be less confusing. There are two maps that will be left here with the board that I request be marked for identification 65 and 66. They are blowups of Figures 24 and 25 in the report.

MR. MAUGHAN: All right.

MR. SANGER: The remainder of the items to be shown on the overhead projector will be figures or tables that are in Exhibit 49A or 49B, and also, just for information as a courtesy to the board and to the audience, we have brought copies in advance, Bay Institute Exhibit 28, which was introduced by reference, the Science Magazine article, Nichols, Cloern, Luoma and Peterson called "The Modification of an Estuary."

MR. MAUGHAN: All right.

(Slides marked for identification as Exhibit Nos. 59 through 64, maps marked for identification as Exhibit Nos. 65 and 66 of Bay Institute of San Francisco.)

WILLIAM MORTENSON,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. SANGER:

Q Mr. Mortenson, would you please describe the research undertaken by you, which is the subject of Exhibits 49A and B entitled "Investigation of Estuarine Circulation in Suisun Bay," including the appendix.

A Yes. The objective of this study was simply as stated, to investigate the dynamics of the null zone and how the null zone and the position of it responded to Delta outflow during the study period being from the end of September, September 28 to October 28, 1986.

Q Would you provide us, please, a working definition of the null zone as you have understood it?

residual motion. The term "residual motion" has been defined previously, specifically by Larry Smith from the U. S. Geological Survey. What we are talking about is after you filter out the tidal velocity, you are left with the net motion and that net motion is either directed up-estuary or down-estuary. At the point where the density driven landward net residual flow on the bottom intersects the river inflow or the flow coming down the river, at these two points it is defined as the zone of no net motion.

Now, I would like to clarify, because a lot of discussion has gone into the null zone and its implications, and previous investigators have looked at it in different ways. The null zone is really a boundary layer, a boundary layer created by the interface of freshwater and saltwater, and extends from the bottom up in a curved line to the surface, using being further seaward at the surface than at the bottom. At the same time, it is not a point in the estuary, it is not a line, what we are dealing with is essentially a curved surface. Some of the longitudinal sections showing the salinity gradient are essentially taken through the longitudinal part of the estuary and showe the null zone as a line. The reason it is shown as a line in those cases is because it's a cross-section through a surface.

So, what we are looking at then is not just how a point changes, whether it be at the surface or at the bottom, but how this surface, this boundary layer surface changes in response to changes in Delta outflow.

Q All right. Before going into further detail on methodology, and highly technical matters, could you summarize the major findings of your research?

A Yes, I would like to do that with just a few slides. Slide No. 1 here is just a satellite view of the bay area and the Delta showing the different embayments.

I would like to go on to slide 2.

MR. MAUGHAN: Each of these have a number?

A The first one is 59, if we can just key them once we will be referring to them as 1 throught 6, which correlates with 59 through 64.

All right, this is a view of San Francisco Bay taken from offshore. In the lower center of the slide you see the Golden Gate opening up into central bay, San Pablo Bay up to the left side looking at the slide, and going into South Bay down on the right side of the slide.

You can see from this slide, really, the dominance of the ocean in the central part of the bay. The ocean here flows into San Francisco Bay, into the central bay through the deep Golden Gate, and then up to the north into San Pablo Bay and into the south, and you can see clearly here that the ocean-bay exchange which has been testified to by the U. S. Geological Survey is just in its infancy of being documented, dominates the central bay and San Pablo Bay.

Now, as you go further up, if I may point on the slide here, here is San Pablo Bay, right here we have a narrow constrictions as we go into the strait. Up here on the other end of the straight is Benicia, Suisun Bay. This restriction changes the dynamics of the system. Whereas, central bay and San Pablo Bay are dominated by the ocean, as a result of the construction going to Suisun Bay, we now have a system where the influence of the ocean is tremendously reduced by this construction, and the influence of the freshwater discharge into the dynamics of the bay increase proportionally.

Next slide, please. Here we have a schematic of the null zone that we have just previously defined showing a net seaward residual flow on the surface and a net landward residual flow on the bottom. In the area of Suisun Bay, the little dots represent the turbidity maximum or the zone of turbidity maximum which exists in the region of the null zone.

Next slide, please. During most years, except for dry and critically dry years, in the early spring the null zone is positioned in San Pablo Bay. The exact Delta outflow required to position the null zone in San Pablo Bay has not yet been established and is one of the areas where a lot of research needs to be going.

You can see San Pablo is shown in purple here and you can just notice the size of it for right now, what is important.

Next slide, please. This is a picture of Suisun Bay. It's hard to see. I will point out on the lower right-hand side, here is Montezuma Slough, Benicia is down here, and this is Suisun Bay coming up here, and this is all Grizzly Bay that you are seeing here.

Again, if you just notice the comparative size of the Grizzly Bay shoal area. My study found that outflows, measured Delta outflow by the DAYFLOW data measured at Chipps Island for approximately 15,000 cubic feet per second would position the null zone adjacent to this wide shoal area of Grizzly Bay.

Now, I would like to show this schematically on another exhibit that we have displayed here. This is taken from Figure 24 of the report and what it shows is this line shown right here represents the null zone. Now, we found out with flows of 15,000 cubic feet per second that we had a riverine flow in Suisun cutoff.

Q Let's clarify that. The line you are referring to is the narrow orange, reddish-orange line?

A The narrow reddish-orange line extending from approximately Chipps Island essentially down the estuary, not across over here by Montezuma Slough.

What we discovered at a Delta outflow of 15,000 cubic feet per second is that we had a riverine flow, meaning the residual flow in the surface and the bottom was seaward in Suisun Cutoff shown here by the two red arrows, red arrows indicating freshwater.

At the same time, in Ryer Roe channel, located here in the center, we had an estuarine circulation, meaning the net residual at the surface was seaward and the net residual flow on the bottom was landward. Again, in the main shipping channel we had estuarine circulation, seaward flow on the surface, landward flow on the bottom.

If you can just put up the next exhibit, Bill, that will be fine. During the course of our study, Delta outflow dropped from approximately 14 to 16 thousand cubic feet per second down to 9,000 cubic feet per second. When that reduction in Delta outflow occurred, it was recorded and observed by our meters located in the Suisun Cutoff and what we observed is that at 9,000 cubic feet per second an estuarine circulation developed in Suisun Cutoff, meaning the residual flow at the bottom of Suisun Cutoff reversed and went landward.

At the surface we still had the seaward flow. At

Ryer Roe channel here, we had an estuarine circulation as in the previous slide, and the same in the main shipping channel.

Consequently, at this reduced Delta outflow of 9,000 cubic feet per second, the null zone passed through Suisun Cutoff and so this can be shown schematically again by the location of the red line, the red-orange line. Here again, it moves some up in Chipps Island, but here it moved back through here, so now it is somewhere in this area here.

Now, if you notice, in the previous slide --

MR. WALSH: This is from when to when?

A This was from September 28, 1986, to October 28, 1986, and I will go into more detail on the hydrology of when it changed in just a few minutes. I want to give you a quick overview here.

MR. WALSH: Okay.

A If you notice here at 15,000 cubic feet per second, the null zone is adjacent to the large shoal area in Grizzly Bay but at the 9,000 cubic feet per second you will notice that the null zone has shrunk, has shifted upward and the size of the surface area is smaller, now being only adjacent to Honker Bay.

Again, the main thing is that as freshwater comes down and runs into the saltwater, it has a tendency, being lighter, to go over it as previous investigators have shown, but it also has a tendency to go to the northern side of the estuary. Consequently, at all depths and in both outflows of 9,000 cubic feet per second and 15,000 cubic feet per second, the salinity in Suisun Cutoff was fresher or the salinity was lower than in the main shipping channel, creating essentially a net horizontal flow.

What I did was compare the salinity distribution on the bottom meter of Suisun Cutoff at the start of our study when Delta outflows were approximately 14 to 16 thousand cubic feet per second with what they were at the same meter when the outflows had dropped to 9,000 cubic feet per second.

This first overlay here shows the data set, and I will explain this briefly. The most important thing to look at here is the salinity. The salinity is shown by this line right here. Down here is the salinity in parts per thousand. This is zero, five, ten, fifteen, up to twenty-five parts per thousand. If you notice, salinity distribution went from below detection right here, this means it went below 1.5 parts per thousand, which is the limit of the sensitivity of the meter, up to the shoulder here of about 3 parts per thousand, and then, climbed up to approximately 5 or 6 parts per thousand.

So, on every ebb tide the water was fresh at the bottom, below 1.5 parts per thousand. At high tide, high slack approximately, there was 6 parts per thousand. This, again, is at 15,000 cubic feet per second at the beginning of the study period.

Next slide.

MS. LEIDIGH: Could you just identify this figure and the exhibit that it is from?

A Figure 22, ex --

MR. SANGER: Q Exhibit 49A, Figure 22.

A This is Figure 23 of the exhibit. Again now, we are looking at the last few days of the study period and again, let's look at the salinity variation over the tidal cycle. Again, here is the salinity line, this dotted line. Here again, is the same scale on the zero, five, ten, fifteen parts per thousand.

What we are seeing at the same meter again, this is the bottom meter in Suisun Cutoff, we see that it never drops below 4 parts per thousand on the ebb tide.

All right, and on high slack tide here, it is reaching up to almost 14 parts per thousand. So, we then have a change in the salinity distribution in Suisun Cutoff from a salinity that ranged from below detection, 1.5 parts per thousand to 6 parts per thousand at Delta outflows of approximately 15,000 cubic feet per second;

and when the Delta outflow was reduced to approximately 9,000 cubic feet per second, we see this significantly increased salinity regime going from 4 or 5 parts per thousand all the way up to 14 parts per thousand.

Q Mr. Mortenson, what is the significance of the shift in the null zone from being adjacent to the shoals in Grizzly Bay to adjacent to the shoals of Honker Bay at the different levels of Delta outflow?

A The null zone, because it is a definable position in the estuary, can be quantitatively related to Delta outflow as shown by the data I just presented. In addition, we have heard testimony from California Fish and Game of the importance of the location of the null zone to the total biomass accumulation of the estuary.

Mr. Chadwick, of California Fish and Game, testified that the farther west in Suisun Bay the null zone was located, the higher the biomass of phytoplankton and zooplankton would be in the estuary. Data by previous investigators has shown the same thing, when the null zone is positioned in San Pablo Bay you have a higher density of both phytoplankton and zooplankton than as the null zone shifts and moves upstream.

The reason for this is that you can envision the shoal areas almost as a field, a crop field, in terms of acreage. When the null zone is positioned adjacent to the larger shoal areas in San Pablo Bay, the amount of total biomass that can accumulate, or the standing crop, is significantly increased.

Now, this can be compared then, you can compare the relative productivity or total biomass production in the estuary by looking at the surface area of the shoals in this overlay here which is Table 2 of our exhibit, you will see that the shoal area in the second row is 154 square kilometers in San Pablo Bay. In Suisun Bay it is 52 square kilometeres, and in Honker Bay the shoal area is reduced down to 12 square kilometers, so what we have then is essentially these are like fields, crop acreage.

If the null zone is positioned in San Pablo Bay, we have this total area of 154 square kilometers where the biomass accumulate when it's reduced, when it shifts to Suisun Bay, we have 52 square kilometers, and when it goes to Honker Bay, it's down to 12 square kilometers.

Now, the study that I was investigating, the Delta outflow shifted from 15,000 cubic feet per second which put it adjacent to Grizzly Bay, which is shown by the Suisun Bay, the 52 here, and at 9,000 it was adjacent to Honker Bay which only had a shoal area of 12 square kilometers, esssentially proportionally four to one between Grizzly Bay and Suisun Bay.

Q Subsequent to your study, did you analyze the availability of Delta outflows to determine the frequency with which this occurrence would have occurred historically; that is, the difference betwween Delta outflow at 14 to 16 thousand cubic feet per second versus Delta outflow at 9,000 cubic feet per second or less?

A I took a look at the unimpaired natural Delta outflows as presented by the State Water Resources Control Board in their errata book to their original exhibit, that is shown in Table 6. You have that overlay?

Here, for example, this is the Delta outflow under natural conditions as calculated by the State Water Resources Control Board and all I did was use this table. Any variations in these numbers would change the percentages.

I then compared these numbers month by month with DAYFLOW values, the actual values measured by DAYFLOW for the years of overlap. Now, the DAYFLOW data only goes back to 1959. So, what I did was look at this table and said, in April, when were the flows 16,000 cubic feet per second or greater, sufficient to place the zone of entrapment, place the null zone adajcent to Grizzly Bay, and then I compared that to DAYFLOW data to see when, as the result of all the upstream development, water resources development and changes in the system have occurred, has the Delta outflow been reduced to approximately 9,000 cubic feet per second.

When I made that comparison, the results are this: In April 37 percent of the years of the 20 years of overlap, the water resources development caused a shift of the null zone being adjacent to Grizzly Bay to being adjacent to Honker Bay. In May it was 58 percent of the years that this shift was shown to occur. In June it was 84 percent of the years, and in July 42 percent.

Now, I only compared these years because these are biologically the most important months of the year for the organisms. Now, again, these percentages are based upon two numbers, the calculated Delta outflow as presented in DAYFLOW and the numbers presented in this table. Any changes in either of those two numbers will change these percentages somewhat, but they are in the ballpark.

Q All right. Mr. Mortenson, would you provide some degree of summary of the specifics that support these conclusions based on the data you collected and analyzed?

A We have just briefly summarized the major conclusions of the study.

Now I would like to go into a ittle bit of data which we obtained which supported these conclusions.

To start briefly with the methodology, what we have here is a current meter array at the three locations, Suisun Cutoff, Ryer Roe and the main shipping channel.

This array was deployed and what we have here are ENDECO 174 current meters.

MS. LEIDIGH: Mr. Mortenson, would you identify the figure and the exhibit?

MR. SANGER: Sorry, Figure 12, Exhibit 49A.

A What we deployed here was the ENDECO 174 current meter. It has a tether and are hooked on with cables, and I have a big weight here and a subsurface float to hold this cable tight, and then the meters are hooked on here. The meters every five minutes record the temperature, conductivity, the direction and velocity of the current, and store this information on magnetic tape.

The bottom meter was placed two meters off the bottom and the middle meter was five meters off the bottom, and three meters from the bottom meter, and the upper meter was -- due to the tidal fluctuations, and what we have seen in the difference between the freshwater flowing over the brackish water, it was important to design a system that maintained the top meter one and a half meters below the surface.

It would be nice to have it a little bit closer, but if you get it a little closer to the surface, wave action interferes with it and you have a little more problem with the data.

So, this is the mechanism that was used and deployed in each of the three locations to obtain the data. This data was collected and stored on magnetic cassettes. This was then sent to Ocean Surveyors, Incorporated, from whom we leased the meters, and the data was processed. After that, the graphic presentation of the data is shown in the previous figures in which we were discussing the salinity distribution were prepared.

Figure 2 here of the report shows the location in Grizzly Bay that the meters were deployed. The black dots show the location in Suisun Cutoff, Ryer Roe channel and the main shipping channel where the meters were deployed.

This is the Delta outflow as taken from the preliminary readings of DAYFLOW data.

Q This is Table 3 in Exhibit 49A.

A The column outlined in yellow is the Delta outflow index for September. If you notice, the Delta outflow was running anywhere between 5,000, 6,000 at the beginning of September. At mid-September, by September 15, it was up to 9,000, right here. And then, by September 20, it was up to 16,000 cubic feet per second, and it reached 18,000 cubic feet per second on September 26. On September 28, as I mentioned, is when we deployed the meters.

So, in the initial part of the study period, we had a Delta outflow of approximately 16 to 18 thousand cubic feet per second.

Now, I am sorry, I don't have the October hydrological data, but if you look in the exhibit on Table 3 for October, you will see the continuation. Essentially all that happened is the flow dropped down to about 15,000 cubic feet per second and stayed that way until October 1, when the flows were 14,450 cubic feet per second and they stayed that way until October 14. On October 14, they started dropping and by October 19 were down to 9,000 cubic feet per second and remained at that level for the rest of the study.

One or two days, on October 26 they were down to 7,000 cubic feet per second. Okay. This is the --

Q This is Figure 13 of 49A.

A This is the progressive vector diagram for the surface, mid-depth and bottom meters in Ryer Roe Cutoff. Now, I will explain this figure. You notice there is a north arrow up in the corner. These are the axis running along the channel, and across the channel essentially where the meters were set up. Up at the surface here, this line here, I drew a line indicating this is a surface meter. Then, I superimposed the surface residual current or the progressive vector diagram, the middle progressive vector diagram and the bottom progressive vector diagram, all to the same figure for illustrative purposes.

A progressive vector diagram is made by taking the five-minute readings of direction and velocities on the current meter tapes, averaging them into 30-minute readings and coming up with a vector for every half-hour, so, for each half-hour of the deployment we end up with one vector showing direction and velocity for that time interval. These are then plotted head to tail for the whole time period, and what you end up with is a diagram that looks like these, the progressive vectors.

What is important to note about this is that to the west, being this way, searward, east being this way, landward, the surface meter, going back and forth with the tidal cycle, its overall progression over time is seaward, indicated with a residual seaward flow.

At the bottom, you can see we have just the opposite. We have a landward flow, residual flow that is landward on the bottom. This is characteristic of an estuarine circulation.

What we can conclude from this is that the null zone was east of this station, somewhere east. How far east, we don't know. Data was collected here. All we can tell from this set of data is that the null zone was east of Ryer Roe Station.

Next one, please. This is Appendix 1A, Figure 4A.

Q 49B.

Q What I would like to show you here are the residual currents. What I had calculated was the long-channel residual and the cross-channel residual current. This is the bottom meter at Ryer Roe and the positive direction, meaning this way, is landward, so you can see if you look at the bottom meter in Ryer Roe, and these are in centimeters per second, so this is five, ten, fifteen centimeters per second, and the net residual would average from ten to fifteen centimeters per second landward.

The landward direction is with the flood tide up-estuary of landward, and you can see it was continuously landward.

This is Figure 14 of Exhibit --

Q 49A.

A This is the progressive vector diagram for the mid-depth and bottom meter in Suisun Cutoff. Again, notice the north arrow up here. Landward is to the east or this way, seaward is to the west. The surface meter data, essentially the tape recorder ate the tape for the surface meter, so we didn't get any data back on the surface meter.

The mid-depth meter is shown here by this line here, the progressive vector, and it is seaward as would be the surface meter. The bottom meter, as you notice, also has a tendency to go seaward during this time, during the first part of the study period, actually down to

October 25. This number is October 25.

Now, if you look carefully at this during this period right here there was a tendency to head back landward. Again, when you get down here, there was a tendency to go landward. This is a response to the neap spring tidal cycle as you heard evidence in previous testimony.

During periods of neap tidal conditions when we have reduced tidal energy from the turbulent mixing, we get increased stratification of the water column and there's an increase in the density-driven circulation on the bottom. So, during neap periods the residual circulation or the residual flow on the bottom tends to be up estuary.

So, here is one of the neap tidal conditions that occurred and here it is occurring again. Now, from this state on, we would expect that as we move from neap to spring tidal conditions to proceed back this way, so that the overall progressive vector diagram would be in this direction. However, as you can see from this data that starting around October 25, as this should have been progressing back this way, there was a significant increase in the bottom residual landward current flowing this way.

And this is the change in the residual current that was picked up as the result of the reduction in Delta outflow. So, you can see during this whole period here with 15,000 cubic feet per second the residual flow was seaward characteristic of the riverine flow.

From here, from October 25 on, we had the bottom residual landward and the mid-depth and the surface would have been residual seaward characteristic of the estuarine circulation.

So, it is this reversal from a riverine flow in Suisun Cutoff to an estuarine circulation that's probably the most significant finding of this study and gave us a control on the system so we had some idea of where the null zone was positioned.

From what we can tell from this diagram is that for the first part of the study up to this point up here the null zone was west of this station. From this point on the null zone moved past the station and was now located to the east of the station.

This is Figure 16 in Exhibit 49A. Now, if you note right here is the residual current at the bottom meter from the time period October 19 to October 25, and here you are seeing a slight landward residual flow. This is during neap tidal conditions. October 23 was the actual neap tide, the weakest tidal energy and during that time we had a slight up-estuary residual flow.

As we passed the neap tidal period and were heading into the spring tidal condition, this line should have come back down or would have come back down and gone to a seaward direction if Delta outflow had stayed the same, but as shown in the previous thing on this day is when the significant increase occurred. We had a jump from 10 centimeters to 27 centimeters per second landward in the residual flow, and this is essentially the impact of the reduced Delta outflow and what it did to the residual flow.

MR. MAUGHAN: May I ask how much longer on the direct? You estimated 30 minutes and we are about 45 now.

A Real quick, we are almost there.

These two situations are shown schematically again in Figures 24 and 25, which we had shown up here. Again, if you notice here at a Delta outflow of 14 to 16 thousand cubic feet per second we have a riverine surface and bottom flow in Suisun Cutoff. We had an estuarine flow here, surface flow seaward, bottom landward and we had an estuarine flow here.

The null zone being shown schematically is located somewhere in this direction. Again, what we can tell, it was east of this point up here somewhere and it was west of this point up here somewhere, getting a general direction along these lines.

Figure 25 is the same schematic shown at a Delta outflow of 9,000 cubic feet per second, and here you see the switch from a riverine flow which we had in the previous Figure 24 to an estuarine flow, the surface flow being seaward, the bottom residual flow being landward, again indicating that the null zone had moved through here and was now east of this point.

Simultaneous with the study I was carrying out the Interagency Hydro Dynamic Study Group was out conducting their investigation and the data I am going to show you right now comes from their investigation. It was taken on October 17 during the spring tidal conditions. And this data was collected using the required instrumentation that was described by Jim Arthur and Lon Hachmeister, and these are salinity profiles obtained from that data set.

MR. SANGER: Q This is Figure 18, Exhibit 49A.

A What's important to notice here, this is on October 17, station L-657 was located within 50 to 100 meters of my station in Suisun Cutoff. All right. If you look here at the salinity you see that during spring tidal conditions the salinity near the surface was 4.1 parts per thousand and at the bottom was 4.4 parts per thousand, very little salinity stratification. The lines are almmost vertical.

Next slide, please. This is on October 17, the same date, Figure 21 -- on the same date but in the main shipping channel, same station, meaning in the same cross-section of the estuary, station C-657, and if you notice here -- excuse me, let me find this figure -- okay, I will have to talk about that one -- that overlay was not there.

Figure 19, the same station, L-657, the salinity varied from 6.4 parts per thousand at the surface, 6.4 parts per thousand at the bottom, again, vertically homogeneous from top to bottom, vertically mixed, but if you compare this stations with station L-657 from the previous figure, you will see that it is 2 parts per thousand greater throughout the water column.

So, throughout the water column at Suisun Cutoff it was 2 parts per thousand fresher or vice versa, the main shipping channel had a salinity of 2 parts per thousand higher.

Next slide. During neap tidal conditions on October 27, the interagency group collected the same data at the same station, and looking at this data, this is Figure 20, station L-657, you will see that the salinity varied from essentially 6.1 at the surface to -- looks like 10.8 at the bottom, so here during neap tidal conditions -- all right, on October 23 you can see we had a decrease in the tidal energy during neap tidal conditions and the decrese in tidal energy allowed the freshwater buoyancy to stratify the water column resulting in a 5 parts per thousand difference between the surface and the bottom salinity.

Now you notice this again goes from 6 to 10. If we look at the next slide collected in the center channel, Figure 21 of the report, station C-657, in the center channel the salinity goes from 7.8 near the surface to 12.8 parts per thousand at the bottom, again neap tidal conditions, weak tidal mixing, stratification of the water column and a significant increase in the salinity distribution.

But again, if you will notice, the salinity is 2 parts per thousand higher than it was in the Suisun Cutoff channel.

MR. SANGER: That terminates the summary of the study and the direct examination.

MR. MAUGHAN: All right, thank you.

Will you indicate by a show of hands who would like to cross-examine. Any staff -- oh, Mr. Schulz, you are too far back there in the dark, I can barely see you.

MR. SCHULZ: This will be short.

CROSS-EXAMINATION

by MR. SCHULZ:

Q I have two areas of inquiry, one predictable.

Your Table 5, pages 63 and 64.
A Yes.
Q You have used, have you not, the hypothetical unimpaired -- is that what you mean by natural?
A I'm sorry, Table 5, page 64?
Q Yes. When you were doing your comparison, your comparison was based on the assumption that the channels are channelized, that the reclamation has occurred, but no water is being used in the valley? That is the basis of your comparison; is that correct?
A What I used, as stated, was the DAYFLOW values which are what is stated here in Table 5. These are the DAYFLOW values as actually reported in DAYFLOW for Delta outflow and Table 6 that I compared it with, which was the Delta outflows under natural conditions, which my understanding is that these are the unimpaired conditions and they have all the assumptions built into them, as we previously discussed in great detail when this was presented originally.
Q Okay. So, the levees are in, the reclamation has occurred but there is no water use occurring, that is what you understand those assumptions to be?
A Right.
Q Do you have any opinion as to what the comparison between DAYFLOW and the natural flows would be if it were

a true natural condition?

A I am not a hydrologist and I did not look at that question at all. I just took these numbers as the numbers presented. As I stated, any change in these numbers or in the Delta outflow estimates would change those percentages.

Q Okay. Now, there was one thing in your testimony that sounded to me a little different than the testimony I had earlier heard. My understanding in terms of the accumulation of phytoplankton in the Suisun Bay-Grizzly Bay area was a function of the location of the entrapment zone which is a function of the null zone.

Does that accord with your understanding?

A The term null zone has a precise meaning. We use that as previously described when residency time in that area occurs and it varies from a salinity of 1.5 parts per thousand to 6 parts per thousand.

When we collect the biological data, all the biological data is collected in relationship -- or I should say displayed in relationship to the salinity gradient. Now, the concept of the entrapment zone was introduced in previously testimony. The entrapment zone is a more vague term. It is not defined as the null zone is. It is greater than the null zone. It extends somewhat in front of the null zone and a certain distance behind the null zone.

I, personally, don't like to use the entrapment zone because it is a much vaguer term. Null zone is something all scientists agree what it is pretty much and the range that we are talking about. The entrapment zone, which is larger than the null zone, includes the null zone in it and is a much more vague term and is improperly understood in many respects, and all the biology that is done is related to the salinity gradient and not to the turbidity maximum, even though the turbidity maximum occurs in this zone and it is referred to loosely as the zone of entrapment.

Q Okay, so you don't have an opinion as to how far downstream on your exhibit, Figures 24 and 25, the zone of entrapment would extend with the null zone at the locations that you have shown there?

A No, I would not venture a guess on that because there is no data to show exactly where that is.

MR. SCHULZ: Okay, that's all I have.

A I would like to add one more thing to that. Again, the biological data that is collected is related to the salinity gradient so it can be related to the null zone, and I have actually a few examples of the zooplankton data and how it relates to the salinity gradient which we can match to the null zone, but there is no data that relates it to the turbidity maximum that I am aware of.

MR. SCHULZ: Okay, fine. That's all I have.

MR. MAUGHAN: All right, thank you.

MS. LEIDIGH: Staff has no questions.

MR. MAUGHAN: Board members. All right, Mr. Sanger, you offer these exhibits, I assume?

MR. SANGER: Yes, we offer Exhibits 49A, B and C, and Exhibits 59 through 66.

MR. MAUGHAN: Any objection? Hearing none, they are accepted into evidence. thny very much, gentlemen.

(Bay Institute of San Francisco Exhibits 49A, B and C, and Exhibits 59 through 66 were received into evidence.)

MR. MAUGHAN: We will move along to Mr. Dawdy. I would encourage you, Mr. Dawdy, I appreciate your coming at the end, but anything you can do to summarize and highlight your main points will be appreciated.

DAVID DAWDY,

having been sworn, testified as follows:

MR. DAWDY: My name is David Dawdy and I have been sworn before, and I am representing myself, and for the information of the Water Resources Control Board, my address is 3055 23rd Avenue, San Francisco, 94132, and that the Bay Institute merely delivered my stuff for me. I am not representing the Bay Institute and I don't get mail at the Bay Institute.

I turned in three exhibits. The first was my qualifications, which was prepared by me and it states my qualifications. I have had some 30 years of experience as a hydrologist. I have a Bachelor's Degree in History, a Master's Degree in Statistics, 25 years of experience with the U. S. Geological Survey, and 20 of those years in research or administration.

One of my last jobs in the Geological Survey was Assistant District Chief of the California District for the Water Resources Division where I was the Assistant District Chief in charge of programs, the technical program.

My last job with the U. S. Geological Survey was as Research Adviser for the Surface Water Research Program, the national program in the Water Resources Division.

I have published papers as shown in my bio-data and I have held several positions in the scientific community. I am a member of several scientific organizations. I have been the Chairman of the U. S. National Committee for the International Association of Hydrological Sciences which is a subcommittee of the National Academy of Sciences, and I have served on several committees of the National Research Council.

At present, I am on the committee on the Glenn

Canyon Environmental Study for the National Research Council of the National Academy of Sciences and I have been in private practice for the last ten years.

I would like to discuss only part of my testimony that was submitted.

First, on page 8 of my testimony, my Exhibit 3 -- my Exhibit 2 is merely an executive summary and Exhibit 3 is the written testimony. On page 8 I have a table which shows the natural draining of the overflow areas, the overflow basins in the central valley, particularly in the Sacramento valley.

As we all know, the natural channels in the Sacramento valley in particular were contained within natural levees. When the waters overflows, those levees -- which it did periodically but not every year, then it would flow into the overlfow basins and flow down those overflow basins to the lower end and then back into the system through a series of sloughs.

And as we realize, levees, even when man tries to keep them from breaking, break. Natural levees break more than man-made levees so that when the water flowed into these natural overflow areas, flowed down to the lower end and ponded, it would eventually overtop the natural levees, immediately take out part of that levee and create a slough as it flowed into the lower end.

So, the lower end of these natural overflow basins were typified by sloughs and, in fact, this particular exhibit shows a series of elevations for the lower end of the Yolo basin in particular from the 1925 river profile -- actually, that should be reference 6 rather than reference 10 there, I think, at least I have it so marked on my page here, and what I have shown here is the name of the sloughs, the station miles above the mouth, the mouth being at the outlet into Suisun Bay, the bottom elevation of the slough, and then, for the Sacramento River where the slough enters the Sacramento River, the bottom of the Sacramento River, and the height of the natural levee.

The first difference is the difference between the bottom elevation of the slough and the height of the natural levee showing that the levees were some 20 feet down below the elevation of these natural levees.

Then I have the elevation of the adjacent land, the adjacent land on the other side of the natural levees, and then, the difference there, which shows the difference in elevation between the bottom of the slough and the elevation of the adjacent land.

This shows that the sloughs cut in from, say, 10 to 15 feet deep into that adjacent land and, therefore, had the capability of draining. So, this gives an idea of how these systems worked.

Now, there was a figure that was shown by the water users. It was Figure 2 and I don't know what exhibit, I forgot to mark it on here, but this figure was shown in their presentation, and please note that it says "not to scale." Let's put it to scale.

MS. LEIDIGH: For the record, the title of this figure is "Typical Cross-Section of Central Valley, Not to Scale (showing pricinipal geomorphic features and natural vegetation)." And it is a drawing with the river channel in the center and other features off to the side.

MR. DAWDY: Yes, it shows the river channel, it shows the natural levee, it shows the flood basin.

MR. LITTLEWORTH: I don't want to be too technical here, but Mr. Dawdy is now beginning to go beyond the testimony of his exhibit. In essence, he is beginning rebuttal, and I guess I don't really care if he puts in rebuttal, I just want to make sure he gets one shot.

If he goes into rebuttal now and then goes into rebuttal later --

MR. DAWDY: This is not meant to be rebuttal. It is to interpret my table.

MR. LITTLEWORTH: Actually, he is using a table out of our exhibit, which I think is rebuttal and has nothing to do with his own exhibit. I don't really want to insist that he sit down and get back up again. I just want to be sure if he is going into rebuttal he gets one shot at it.

MR. WALSH: He could do the same thing if he didn't use your exhibit and drew on the chalkboard.

MR. LITTLEWORTH: I don't know if he is complaining about ours or wants to just kind of talk about his own. If he wants to talk about his own, that's direct. If he wants to complain about ours, that's rebuttal.

MR. MAUGHAN: Do you understand, Mr. Dawdy, you only have one shot. That's clear?

MR. DAWDY: I have one shot at my exhibit. All right, I will defer and merely say that if we put my data into context it shows quite well that the Sacramento River cuts down to 25 feet down below the natural levees and that the sloughs cut down into the adjoining land so that they can drain the overflow lands into the Sacramento River. That was the point I was trying to make and I was hoping that I could be helpful to the board and to the water users and show them a little bit of scale for their on exhibit.

MR. MAUGHAN: You can do that now or later, but not at both times.

MR. DAWDY: No, I will defer.

MR. WALSH: Does that mean you will do it later?

MR. DAWDY: No, I probably will not do it later in that case. I just think one should keep in mind that is what I was trying to show.

My figures show I was trying to interpret it graphically. That's all.

MR. WALSH: Why dont' you give me that again being that we aren't to get it later.

MR. DAWDY: As long as it doesn't prejudice me, I will do it. Okay.

What I am saying is that if we were to have a figure here that had a cross-section on a chalkboard as you suggested, we would have --

MS. LEIDIGH: Mr. Dawdy --

MR. DAWDY: I am not wanting to violate the rule of the Chair, Mr. Chairman; what must I do?

MR. MAUGHAN: You go ahead and explain. If you are not going to come back later, you can use it right now.

MR. DAWDY: I am coming back later on this particular --

MR. MAUGHAN: I mean on this particular point. I didn't want to convey to you that you couldn't rebut other things, but you can't do it twice --

MR. DAWDY: No, I wasn't planning --

MR. MAUGHAN: All right, then, you can use it as long as you don't use that same point again. Sorry I didn't explain it properly, but we just don't want you to do the same thing twice.

MR. DAWDY: I actually would prefer at this point because of the objection --

MR. MAUGHAN: That's all right.

MR. DAWDY: Up here, if we start off at the bottom elevation of the slough, let's say here is zero, and we are talking about Babel Slough because it's the bottom, the elevation of the slough is at plus one. It is somewhere in here. There happen to be some three, six, eight sloughs I picked out in this particular fifty-mile reach that drain into the Sacramento River. The bottom of the Sacramento is some six feet below that, so it is down here.

The natural levee is 22 feet up here. The adjacent land is at 10 feet, which is about halfway there and let's put the natural levee over here and the river over here so we can draw this a little bit different. The natural levees do overflow, they do allow the water to pond down here, but this water in the sloughs drains these overflow basins and drains into the Sacramento, and the Sacramento, if it is running ten feet deep, let's say at this point, has ten feet of levee there containing it, but still can receive water and put water into the slough at some flows and receive it back at lower flows, so the sloughs at the lower end can receive water from the Sacramento and then drain back into the Sacramento.

But also, when they are not overflowing, those natural levees are ten to twenty feet up above the water. That puts it in perspective sort of.

Now, my next slide I want to show and discuss is on my page 10, a figure of contemporary count of the area of tules, and the point that I want to make here is that this contemporary count describes the location of the tules and the amounts, and they come out to some 600 to 700 thousand acres rather than over 900,000 that were suggested in the appendix, the testimony that was given in the --

MR. MAUGHAN: Here is where I think you --

MR. DAWDY: This is the amount of tules.

MR. MAUGHAN: Don't compare it with something else unless you want to make rebuttal. Just put down what you think it is.

MR. DAWDY: Part of the problem here is that in rebuttal testimony earlier the water users brought forward some data and this was done before I saw their testimony. This was done, this was handed in prior to seeing their exhibits for this.

MR. MAUGHAN: That isn't the point here. If you want it to appear twice --

MR. DAWDY: No, I am actually basing it on the numbers that they gave. I am basing it on numbers that they gave in rebuttal previously.

MR. MAUGHAN: Just give us your numbers.

MR. DAWDY: These are --

MR. SANGER: Mr. Chairman, it seems to me the witness is being unduly hampered. Dr. Fox appeared twice, once in rebuttal and again on the same subject in direct. As a matter of fact, Mr. Dawdy proposes to reverse the order of appearance and appear twice, which is the same subject --

MR. MAUGHAN: He can appear twice. I don't want him to testify and make comparisons now and then two hours later from now get back up and say the same thing. That's all I am trying to say.

MR. SANGER: Perhaps it could be explained that you just don't want him to repeat himself, which would preclude him from returning.

MR. MAUGHAN: Correct, and I thought I tried to say that. If I said it imperfectly, I didn't mean to do it that way.

MR. SANGER: I don't think he has fully understood.

MR. MAUGHAN: I think you are right.

MR. DAWDY: The point I am getting at is I want to use an exhibit that was provided for this hearing, and I guess that would be rebuttal, so let me pass that.

MR. MAUGHAN: You can use it, but don't repeat it when you come up again. That's all. You can do it, but you can only do it once.

MR. DAWDY: We get one shot at it.

Well, to put my report in context, and the thrust of what I am going to do is read from my report of historical evidence which I got intrigued with tremendously. I took a map which was provided for delination of the prehistoric natural vegetation, I guess it was called, and I then want to use that for a location of some of the descriptions of the historical explorers that are contained in my report.

Before I get into that, just so it won't confuse you, let me say that in addition to my estimate of areas of tules, there was quite a bit of discussion of annual tule fires and their effect. So that, as I point out in my paper, there should be some consideration of the fact that the tules were not always there, that part of the time they were burned and there wasn't much potential for use of water in consumptive use after they burned.

MR. WALSH: Two things, Mr. Dawdy. First of all, you are saying how many tules were there, how many square miles or acres?

MR. DAWDY: Well, the estimate in 1868 was some 600 to 700 thousand acres.

MR. WALSH: And how did they burn?

MR. DAWDY: They burned either naturally as the forest fires started this last year, or the Indians burned them down.

MR. WALSH: Thank you.

MR. DAWDY: As Cronise said in 1868, large areas, and this is pages 10 and 11, large areas of tules dried out each year and burned. "The basins and Delta areas were characterized by giant tules, said to be 10 to 15 feet high, so that a man on horseback could not be seen in them. Usually the water drained off the basin lands by mid-summer, and the tules were set on fire causing great clouds of smoke to cover the lower valley."

Then a traveler in 1833, a Mr. Zenas Leonard, traveled with the Walker party down the San Joaquin to Suisun Bay from the Merced River. "At this season of the year, which was early November, when the grass in the plain is dry, if a fire should be started, it presents a spectacle truly grand."

In October of 1837, Captain Edward Belcher wrote: "The spring tides overflow all the lower lands, which are well stored with long flag grass, and rushes of great size, of which later the natives construct their balsas. During the dry season the natives burn this down, and probably by such means destroy many oak plantations which otherwise would flourish."

Brewer also recorded the burning tules. On November 1, 1861, he noted: "The swamps bordering all the rivers, bays or lakes, are covered with a tall brush, ten or twelve feet high, called 'tule,' which drives up where it joins the arable land. On the plain below camp, fire was in the tules and in the stubble grounds at several places every night, and in the night air the site was most grand -- great sheets of flame, extending over acres, now a broad lurid sheet, then a line of fire sweeping across stubble fields. Every evening we would go out and sit on a fence on the ridge and watch this beautiful site, some nights finer than others."

So, there was quite a bit of evidence that prior to the advent of settlement of the central valley, that the tules dried and burned regularly. That burning reduced the water demands to that for open ground or even less if the surface were made relatively more impermeable for part of the year by the fire. Then it would be less than open ground, perhaps.

The other thing that is of interest is the savannas, not tules, were recorded by many travelers in the central valley.

On September 23, 1776, a joint river and land expedition of Spanish explorers started up the river. They missed connections; however, and only the land exploration continued. They missed connections --

MR. WALSH: Which river?

MR. DAWDY: San Joaquin and Sacramento. They started up the river and one went overland through Livermore and into the river and ran into the San Joaquin and missed the river people, so the land expedition was the one that reported. "As soon as it crossed the mountains through a pass of low hills which facilitated the march, found itself in the plain which is crossed by the large river, the San Joaquin. Seeing that they were much farther up than had been agreed upon, decided to continue through the plain up the river. He did this, following the stream for three entire days, traveling rapidly. The plain through which that river runs, he said, is as level as the palm of the hand, without any trees except in the bed of the river.

It is an immense plain, for he did not see the end of it, and he reached a place where it made a horizon in every direction, so that he saw the sun rise and set in the same way as if he were on the high seas.

"After traveling much further on the bank of the river, he observed that, although it was very wide, it apparently, did not carry much water, and he wished to try his luck in crossing it , but some heathen, seeing what he was about to do, made signs that he must not cross unless he followed them.

He did this, and ascending a little farther, they showed him a ford, and by it he crossed the great river of our Father San Francisco, guided by the heathen." This was the San Joaquin. "On the other side of the river, he found that the same plain and level land continued. They traveled over it all day. To the north in the same plain they saw some groves which, judging by the windings which they made marked the course of the rivers, but they did not dare to explore them less they be lost in that wide plain."

Here on this map at A is the stretch of the San Joaquin over which they were traveling and the area here shows as riparian forest is a band of trees on the map which is five miles wide minimum.

Now, the Morago expedition of 1808 gave a similar report to the previous expedition. In September of 1808, after about 12 leagues, they left Livermore approximately -- of course, Livermore wasn't there but I mean left the vicinity of Livermore, after about 12 leagues, about 33 miles, and my insert there is on page 12: "We arrived at the Laguna del Blanco on the banks of the Rio del Pescadero," and this identified by the editor as the west channel of the San Joaquin in its Delta area. "Having crossed a branch of that river, we spent the night safely."

Then, on September 27, "Leaving camp where it was pitched yesterday, this morning with four men I continued on toward the east, and after about two leagues, which is about 5.5 miles, I found the river and I followed it south for about four leagues, about 11 miles. No ford could be found in this distance so I returned to camp. In the afternoon I sent the corporal in a northerly direction in search of the ford in the river. He found it, but on the opposite side he was confronted by a very large tular and could not continue."

He marched 33 miles, he marched 5-1/2 miles, he marched 11 miles, and finally, he found some huge tules. This is B-1 approximately here (indicating on the map), and this is where searching for the ford and where they were wandering around looking for a way to get across the river.

Then, on the 20th of October, 1808, a couple of weeks later: "Today we followed the Merced River downstream, exploring it to a junction with the San Joaquin. The low plains of the river are nitrous to within a distance of two leagues, more or less, before reaching the San Joaquin. From there downstream the plains along the river are good and the soil is rich. There are some beautiful willow groves, but also, there is the disadvantage that one stone can be found. This is all that has been found at the junction of these rivers," and that's B-2 up here.

Now, at B-1, Ms. Fox says that this is a tule swamp over the whole route, 33, 11 miles, whatever, and they found them finally up at the ford at their last site mentioned up here in B-1.

The B-2, Ms. Fox says this is an area of tules and riparian forest all the way, and yet, they say that they found a few beautiful willow groves.

And then, Captain Belcher came along, as was mentioned earlier, and he went up the Sacramento River. "Midshipmen Simpkinson stayed behind on ship while Captain Belcher explored the Sacramento in 1837, but he wrote and said: "Whenever anybody had an opportunity of penetrating the thick, dense barriers of trees and shrubs that line the banks, he arrived upon a vast plain almost without an inequality covered with the richest pasture and interspersed with park-like groups of trees, on which large herds of elk, consisting sometimes of several hundreds, were constantly grazing."

"Oaks of a luxuriant growth, beech, walnut and ash, were the principal trees which lined the banks," and note the oaks.

On page 13 of my report, Surgeon Richard Brinsley

Hinds wrote further of the river trip: "It was late in the autumn of 1837, when an expedition up the Rio Sacramento penetrated from San Francisco some distance into the interior. The county exhibited a vast plain, rich in a deep soil, and subject to periodical submersion. Occasional clumps of fine oaks and planes imparted an appearance of parkland. They were already shedding their leaves. A small grape was very abundant on the banks."

Captain Belcher, himself, wrote, and I will skip some of this -- well, maybe I won't -- I am enjoying it. "Having entered the Sacramento, we soon found that it increased in width as we advanced, and at our noon station of the second day was about one-third mile wide. The marsh inland now gave way to firm ground, preserving its level in a most remarkable manner, succeeded by banks well wooded with oak, planes, ash, willow, chestnut, walnut, poplar and brushwood. On the 30th of October, at about four p.m., I landed at 'the fork,' which ws named Point Victoria." That is the forks of the Feather and the Sacramento, at point C up here. That's where the Feather comes into the Sacramento.

"Throughout the whole extent, from Elk Station to the Sacramento mouth, the country is one immense flat. Our course lay between banks, varying from 20 to 30 feet above the river level."

MR. WALSH: Why wasn't the first major fork the American River?

MR. DAWDY: He identified it by a location as being the fork of the Feather. It didn't say the first fork. It was the Elk Fork.

MR. WALSH: Okay.

MR. DAWDY: He named it the Elk Fork.

"Our course lay between the bank, varying from 20 to 30 feet above the river level." That's rather important because if the trees are 20 or 30 feet up, they have got a way to go to get the water. "... apparently, from its strata of differently composed clay and loose earth, produced by some great alluvial deposit. These were, for the most part, belted with willow, ash, oak or plane, which latter of immense size overhung the stream."

"Within, and at the verge of the banks, oaks of immense size were plentiful. These appeared to form a band on each side, about 300 yards in depth, and within (on the immense park-like extent, which we generally explored when landing for positions) they were to be seen in clumps, which served to relieve the eye, wandering over what might otherwise be described as one level plain or sea of grass.

Q During the rainy season, which commences about the middle of November and terminates about the end of

Feburary, the river is said to overflow its banks, when its impetuosity is such that navigation is then impossible. The annual rains do not, however, of necessity inundate these lowlands, but in severe seasons, after heavy falls of snow, they produce one immense sea, leaving only the few scattered eminences which art or nature have produced, as so many islets or spots of refuge."

Now, Ms. Fox says that this is tules and rain forest all the way.

The Russian, Admiral Otto von Kotzebue, ventured up the Sacramento as far as the fork with the Feather River in 1824. He wrote of his trip: "The weather was favorable and we set out working our way between the islands into the northern portion of the bay. We reached toward noon, at a distance of 30 miles from our ship, the common mouth known as the Carquinez "which he considered the mouth of the rivers.

"When we had proceeded 18 miles from our night camp and 23 from the river's mouth, we reached the confluence of the two streams (Sacramento and San Joaquin). One flows from the east and the other from the north. Since the River Pescadores (San Joaquin) was already known, I chose the other, which flows from the north, and is called Sacramento. Towards noon, after we had ascended it some miles, a violent contrary wind forced us ashore." This is about mile 38 and this is on page 14 of my report.

"We were obliged to give up for this day, pitched our tents in a pleasant meadow on the west side of the river. I then climbed a hill to enjoy a more extensive prospect; and observed that the country to the west swelled into hills of a moderate height, besprinkled with trees growing singly. In the east and southeast, the horizon was bounded by icy mountains. The distance of these mountains from my present station could not be less than 40 miles. Between them and the river, the country is low, flat, thickly wooded and crossed by an infinite number of streams, which divide the whole of it into islands.

"All along the banks of the river grapes grow wild in as much profusion as the rankest weeds." That is point C up here at the forks of the Feather.

"Early the next morning we prepared for our return, and soon quitted these lovely and fertile plains, where many thousand families might live in plenty and comfort."

Now, this is a mixture of riparian forest and grass plains, whereas Ms Fox saw tules.

I'm not sure that Admiral von Kotzebue got all the way up to -- yes, he went up to the forks of the Feather also.

William Dane Phelps traveled up the Sacramento in 1841 to visit Captain Sutter, who had established his fort at Sacramento in 1840. On 27 July --

MR. MAUGHAN: Excuse me, do you have several more that you are going to read?

MR. DAWDY: Yes, several more and then I am through.

MR. MAUGHAN: If they are similar to what the others said, if you can once again --

MR. DAWDY: I will skip over to one last compelling one, but let me assure you that they are over and over from the historical sources, similar, but there is one compelling one at the end.

MR. MAUGHAN: All right. The whole thing will go into the record.

MR. DAWDY: Yes, I realize that.

Let me read this. It is really a neat one.

There was a guy by the name of Bryant who came overland to California. He traveled overland along the route I marked E, down one of the overlfow basins and he described it. He looked for a tree to get some shade and he found no tules.

On page 18 Bryant continues: "On September 13, we commenced today our journey from New Helvetia to San Francisco," and this is F-1 and then F-2 along in here.

"We traveled in a south course over a flat plain and encamped on a small lake near the Cosumnes River. The stream is small but the bottom lands are extensive and rich. The grass on the upland plain over which we have traveled is brown and crisp from the annual drought. In the low bottom it is still green.

"September 14, we crossed the Cosumnes River and traveled over a level plain covered with luxuriant grass and timbered with the evergreen oak until three o'clock, when we crossed the Mokelumne River and encamped on its southern bank in a beautiful grove of live oaks. The soil of the bottom appears to be very rich and produces the finest qualities of grasses. The grass on the upland is also abundant, but at this time it is brown and dead."

Ms. Fox shows riparian forest about five miles wide of the Cosumnes and ten miles wide at the Mokelumne.

The last trip that I will --

MR. SAMANIEGO: What is the approximate distance between the Cosumnes and the Mokelumne?

MR. DAWDY: How long the distance is here -- who asked the question?

MR. SAMANIEGO: I did, up here.

MR. DAWDY: Between the Cosumnes and Mokelumne, I don't think he mentions the distance. He walked it, however.

MR. SAMANIEGO: He walked it in one afternoon?

MR. DAWDY: He traveled it.

MR. SAMANIEGO: Would you read that passage again? Does it say in the afternoon?

MR. DAWDY: On September 13 they camped. On September 14 they crossed the Cosumnes and traveled over a level plain until three o'clock when they reached the Mokelumne River .

MR. SAMANIEGO: From perhaps in the morning until three o'clock in the afternoon.

MR. DAWDY: Yes. I don't know if he was actually walking or on horseback.

MR. SAMANIEGO: It's a reasonable horseback ride.

MR. DAWDY: I would assume so since the gentleman did it.

MR. SAMANIEGO: I wanted to know if it is credible.

MR. MAUGHAN: And all seriousness, I think it must be in the order of 20 or 30 miles.

MR. DAWDY: I marched 20 miles with a full fieldpack and rifle, and after you have marched across the country as he had done, I imagine he could do 20 miles with a full fieldpack and rifle, too. So I think it is reasonable. I think the gentleman was an experienced traveler. He had traveled all the way across the country.

"September 15, our route has continued over a flat plain, generally covered with luxuriant grass, wild oats and a variety of sparkling flowers. The ford of the San Joaquin is about 40 or 50 miles from its mouth. At this season the water is at its lowest stage."

All right, we know now.

"The stream at the ford is probably 100 yards in breadth and our animals crossed it without much difficulty (they were on horseback), the water reaching about midway of their bodies. Oak and small willows are the principal growth of wood skirting the river.

"Entering upon the broad plain we passed, in about three miles, a small alkali lake. The grass is brown and crisp, but the seed upon it is evidence that it had fully matured before the drought affected it. We passed during the afternoon several tule marshes, with which the plain of San Joaquin is dotted."

This happens to be F-3. He is crossing in the depth of the South Delta which is shown as continuous tule marsh on the map of Ms. Fox. "We passed during the afternoon several tule marshes, with which the plain of the San Joaquin is dotted. At a distance, the tule of these marshes presents the appearance of immense fields of ripened corn."

So, for distances, there were no tules.

"The marshes are now nearly dry, and to shorten our journey we crossed several of them without difficulty. A month earlier, this would not have been practicable. While pursuing our journey we frequently saw large droves of wild horses and elk grazing quietly upon the plain."

And then, Mr. Bryant visited with Dr. Marsh over south of Antioch, near Brentwood and he went up and looked out over the Delta and he described it in much the same way that you heard here. F-4 is the old stone house there at Brentwood which still exists, but you can't get to it now. It's falling down.

And in closing, I want to say that there are many instances of early explorers and visitors noting the occurrence of freshwater in Suisun Bay. This would be evidence of considerable flow from the Sacramento and San Joaquin Rivers into the San Francisco Bay at the time of observation. Observations such as these, particularly in late August, September or October, would eliminate the no-flow into the bay, and the times in particular that I found were March 20, 1772, as described by Father Juan Crespi and Captain Don Pedro Fages; September 17, 1775, Captain Juan Manuel Ayala, accompanied by Father Vicente Santa Maria; in April of 1776, Father Pedro Font and Juan Bautista de Anza; and in November, 1837, Captain Belcher; and in late October, 1846, Bryant.

So, through the period from 1772 to 1846, there were reportings of the sweetness and freshness of the water at the upstream end of the Suisun Bay where the Sacramento and San Joaquin Rivers emptied into the San Francisco Bay system.

And that is essentially the points I want to make in my testimony. Thank you.

MR. MAUGHAN: All right, Mr. Dawdy .

Cross-examine. Mr. Littleworth. Anyone else? All right, sir, you may come up.

MR. DAWDY: Incidentally, I meant to pass this out. I do have copies of that figure here.

MR. MAUGHAN: All right.

CROSS-EXAMINATION

by MR. LITTLEWORTH:

Q Well, Mr. Dawdy, we are both amateur historians. I have a Master's Degree in History, too.

A Well, I am more -- well, not necessarily amateur, because I am a member of the historical organization that is involved with public history as they call it.

Q Let me begin with just a minor point that I thought you said when you were talking about the Morago expedition and the sergeant who was sent out to try to find a way to ford the river, page 12, that he came to very large tular but he had to go some 30 miles before he found some tules.

Did I understand you to say that?

A He had marched, that was the distance that he marched in different directions, and I was converting leagues into miles.

Q Tular is an area where tules are found; is it not?

A Yes, that's what I say, he found on the opposite bank there were tules there.

Q He did find large areas of tules?

A Yes, sir.

Q And a little further down in your same quote, he is out the next day and finally can't get to the American River, he is blocked by tule marshes?

A That is correct.

Q Mr. Dawdy, I take it that you were talking about flow under natural conditions. Would you agree that we should be looking at the period of time probably from the late 1700s where the early Spanish explorers were recording their travels maybe into the early 1800s; certainly, we would not want to go into the period of actual white settlement; would we?

A We wouldn't want to go into the period from the Gold Rush on, I would guess.

Q That's what I would think. The Gold Rush dramatically changed the hydrologic conditions in the state; didn't it?

A Yes.

Q And I think you agreed that under natural conditions there were these large natural floodplains that ran generally parallel to the Sacramento and the San Joaquin Rivers?
A Yes, sir.
Q I think in the State Water Contractors' exhibit we indicated that those flood basins had about 2 million acres that were tributary to the Delta. Would you think that's about right?
A I don't think I checked that figure.
Q You really don't know that one way or the other?
A No.
Q You do agree that there were large areas of tules that existed in those flood basins?
A In some of those flood basins, particularly in the Yolo basin, there was an area of tules in 1868.
Q You also agree that there were large areas of riparian forests under natural conditions?
A There were areas of trees reported by the travelers, yes. Not being too picky, but the large areas is your definition. I was merely quoting the travelers.
Q I thought you were putting some emphasis on all the trees that people saw.
A In some cases, I was also stressing the narrowness of the width, you might note; therefore, the large areas

is your interpretation. All I was doing was reading what the travelers said.

Q Dr. Fox indicated that there were approximately 1.4 million acres of riparian forests, trees along the streams under natural conditions. Do you have any reason to disagree with that figure?

A Yes, based on the travelers' accounts, I think that there's some evidence that some of those numbers are somewhat larger than --

Q So, from what you were reading us, you were making that determination as to acreage?

A Not as to acreage, but there's also other estimates of riparian forest that are available.

Q Going back to tules for a second, I think in your work, in your own Exhibit 3, you have described sources which state them as being giants, up to 15 feet high, of enormous height. You recognize those quotes; do you?

A Some of them I quoted, 12 to 15 feet high.

Q And some straight 15 feet high in your own work?

A Could be.

Q And also, the word "giant" and "enormous"?

A Well, large tules, yes.

Q I am now quoting out of your Exhibit 3.

A I was quoting the travelers and that's what they said, yes.

Q That's what you understand from the quotations in which they described them and the way I have just done?
A Yes, there were some descriptions like that.
Q It is true; is it not that tules standing up to 15 feet high would have a greater consumptive use than tules maybe 5 or 6 feet high?
A I did not see any evidence of that in any of the papers that I reviewed.
Q Do you think that the size of the plant doesn't make any difference as to how much water it uses?
A It depends upon the volume density, yes.
Q Taking a six-foot tule versus a fifteen-foot tule under the same conditions, do you think they would use the same amount of water?
A I am not positive. I would have to see the evidence on that.
Q Now, as I understand it, you reviewed Dr. Fox's work and concluded that she overestimated the annual consumptive use of tule marshes and, hence, underestimated what would be the natural flow into the bay; is that correct?
A May I say that that was not in my paper that was presented in evidence.
Q Well, isn't that what you were trying to do with your paper, to indicate that because she had overestimated

the consumptive use, she had in turn underestimated the natural flow into the bay?

A That's my opinion, yes.

Q That was the purpose of your exhibit; wasn't it?

A The purpose of my exhibit was to see what the travelers said about historical conditions when we started looking at some of those points that were raised.

Q Now, you take the view; do you, that she overestimated the actual number of acres that were involved in tule marshes?

A I think that's correct, yes.

Q And you know that she finally used in her exhibit, 946,000 acres. You mentioned that figure; didn't you?

Well, never mind, you don't need to go back and take the time to look that up. That's in the evidence. In any event, you are estimating between six and seven hundred thousand acres of tule marshland; is that right?

A That is correct.

Q And that's based on the book by Titus F. Cronise, entitled "The Natural Wealth of California in 1986"?

A That is right.

Q He estimated the conditions, the amount of tules in 1886; did he not?

A That is correct.

Q So, that was not what they were under natural

conditions but what he estimated they were in 1868; isn't that correct?
A That is correct.
Q Cronise didn't do his work as part of any government survey; did he?
A No, he did not.
Q And it wasn't part of any scholarly journal or any academic work; was it?
A No, not exactly.
Q It was sort of a popular book; wasn't it?
A He was an individual who was a native of the state and who was reporting upon the conditions in the state at that time.
Q He was a farmer in the San Joaquin valley; wasn't he?
A That, I don't know.
Q He wrote what was really a popular book to sort of encourage immigrants to the state?
A Probably so, but he wrote a book about California as of that time. As to his purpose now, you may be a better psychologist than I and can read minds better, but I am not sure exactly why he wrote it.
Q You indicated in the chart that was up there that this was a county-by-county listing of the tule acreage from which Cronise came to his six to seven hundred

thousand acre-feet figure. I think you were in the audience when Dr. Fox testified and indicated that he had omitted the acreage in Sutter County.

Do you recall that?

A I recall that.

Q Do you agree that there were tule marshes in Sutter County?

A There was an area which was mentioned in my report of the Sutter basin that probably did not drain according to the map.

Q So, he probably was underestimating then by leaving out Sutter basin?

A But probably overestimating in some other cases, yes.

Q Are you aware of the report of the California Surveyor General in 1856 that also listed the tule acreage by counties which reported 160,000 acres of tules in Sutter County?

A No.

Q Are you aware of a report by Mr. Hilgard, who I think was the first head of the agricultural experiment station at the University of California written in 1883, where he indicated that there were 339,000 acres of tule marshes in Sutter County?

A No, I am not aware of that.

Q Are you aware of Mr. Hilgard's conclusion that there were 1.178 million acres of tule marshes that were tributary to the Delta?
A What year was that?
Q Well, in a natural state.
A That was not a natural state. You said the natural state was before --
Q I said this was when the work was written.
A Yes.
Q You are not aware anyway of Dr. Hilgard's report?
A No.
Q All right. Are you aware that your friend, Mr. Cronise, wrote a second book two years later?
A I was aware, the reference of which I couldn't find.
Q That was entitled, I think, "Agriculture and Other Resources in California, 1870." Are you aware that in that report he talks about there being several million acres of swamp and overflow lands generally designated as tule in California?
A May I speak to that point --
Q Yes, you may.
A Swamp and overflow came out of the Arkansas Act, and it was a legal term, and in Arkansas if it was a swamp and overflow land, it was declared that you could settle

it for a small price. There was a story of the definition of swamp and overflow land, that if you could pass a boat over it, you could claim it, and so there was a farmer -- a land speculator, I should say -- hitched up a team of horses to a boat and went around a large area, and then went into court and won, that this was swamp and overflow land.

Swamp and overflow land was all these basins which were periodically inundated, but not necessarily areas which were tule lands.

Q Are you aware that the Arkansas Act of 1850 authorized the conveyance of swamp and overflow land to the state on condition that they be reclaimed?

A Yes.

Q And the title was inchoate and dependent upon actual identification of those lands, and a survey and a plot, and then, a final patent?

A And a lot of fraud.

Q But all of the conditions that I just mentioned?

A Yes.

Q And the character of the land was actually to be determined as of the date of the act which was September 28, 1850; wasn't it?

A That, I don't know.

Q You are not aware that they made fall measurements so they were determining what qualified under the act under fall conditions?

A No.

Q Are you aware that the definition of swamp and overflow land was, and I am quoting: Land that was wet and unfit for cultivation?

A That was land that was periodically overflowed, yes.

Q That is not what I asked you. I meant -- I asked whether you understood that the definition was, and I am quoting: Land that was wet and unfit for cultivation, and I am closing the quote.

A No, I was not aware of that. I was not aware of the legal definition of swamp and overflow land.

Q Are you aware that after that act the state and federal governments began to survey the swamp lands and to map them?

A With horses; yes, I am aware that there was a lot of problems with the definition of those lands. There was a lot of fraud involved and there was a lot of revisions of the terms of that act.

My understanding of the legal implications of all this is not all that great because I am not a lawyer, but there were many stories about how the Arkansas Act led to a good deal of problems.

Q Are you aware that the Surveyor General in California in 1852, in his annual report, reported 2.6 million acres of land under the Arkansas Act, swamp land and overflow land?

A I have seen that, not that number but I have seen a large number county by county.

Q I want to read to you an in-house memorandum dated September 27, 1985, to the Deputy Regional Director of the Fish and Wildlife Service regarding loss of central valley wetlands. There are two quotes I would like to read to you.

"In the Sacramento valley the wetlands exceeded one million acres. Bordering the two major rivers are natural levees. Breaks in these levees allowed flows to enter basins like (Butte, Sutter, Colusa, et cetera,) side channels and stream meander supporting tules and other wetland vegetation."

Over on the next page, "A portion of these vast wetlands frequently called 'tule lands,' was owned by the United States. With the passage of the Arkansas Act in 1850, the tule land, now identified as swamp and overflow lands, were released to the State of California. An estimated 1.7 million acres of swamp and overflow lands located in the valley were put up for sale."

Do you have any reason to disagree with this fact in this memorandum?

A I don't necessarily disagree with the fact, but I would say one has to interpret what swamp and overland acres meant.

Q Although this letter equated them with tule lands; didn't he?

A He equated them with swamp and overflow lands.

Q Well, I think the quote reads: "With the passage of the Arkansas Act in 1850, the 'tule lands' now identified as swamp and overflow lands, so he equated the two terms; didn't he?

A Yes.

Q Are you aware that there was a map in 1857 by Mandeville as part of the U.S. Swamp Land Surveys pursuant to the Arkansas Act, and I wonder if you are, if you have looked at that map?

A I have not looked at that map. I have limited resources to do this.

Q Did you look at any of the California reports of the swamp land surveys that were made between 1850 and 1929 on lands that were swamps and had tules?

A No.

Q Did you look at the map prepared as part of the Board of Commissioners on irrigation established by the War Department in 1874, and a map that accompanied that which again dealt with the swamp lands?

A No.

Q Did you look at a California Geological Survey map by Whitney in 1874 on the tule marsh lands?

A No.

Q Did you look at the first maps prepared by William Ham Hall, the State Engineer in 1880, 1887 and 1888, where he mapped out the tule marsh land?

A No.

Q Did you look at the map which accompanied the 1883 Hilgard report which maps tule marshes?

A No.

Q Did you look at the Cushler map which was prepared more recently which was the one that Dr. Fox finally planimetered or took her final measurements from, which was a natural vegetation map?

A Yes, I think I have seen that.

Q Did you planimeter the tule marsh areas shown on that map?

A No, I did not.

Q You are aware that map has been included recently in the California Water Atlas, 1979, on page 17?

A Yes, some of those areas noted as tule swamps is where the travelers traveled across wilthout seeing tules. I noted that, also.

Q You also know that Dr. Fox adjusted the acreage downward from that map?
A She didn't adjust it downward enough, according to the travelers' reports that I quoted.
Q It would be a little hard to determine acreage by travelers' reports; wouldn't it?
A I am not quite so sure if it is noted as 300 yards wide that it is difficult to compute mileage by 300 yards. You don't have to planimeter that. And if you have it five miles wide or ten miles wide, then I would think that you would tend to overestimate acreage.
Q Mr. Dawdy, are you aware that in 1884, Mr. Manson, who was William Ham Hall's assistant, was engaged in mapping the lands of the state entirely apart from the Arkansas Act?
A No.
Q You then haven't seen his report in which he discusses simply swamp land and says the aggregate area of these bodies of lands is over one million acres?
A No.
Q Mr. Dawdy, I take it that it is your claim that these natural flood basins actually drain fairly rapidly, and therefore, the tules were dormant a good part of the year?
A No, I didn't say very rapidly.

Q Are there some then that maintain water all year around?

A There were some that drained. They drained slowly as the capacity of the channel could take the water, but they weren't necessarily full of tules either. People who traveled through some of these basins mentioned the fact that they had obviously been wet and there were a lot of hoof marks, but they also mentioned grass. They didn't mention tules.

Q Well, it is not your claim then that the marshes drained and so the tules basically dried out and didn't use water all year around?

A I think that the tules in many of these basins -- well, the basins drained and what tules were there, many of them at the edge of the sloughs went dormant, and obviously, some of them burned.

Q I think there is some indication in your Exhibit 3 that some of the marshes had water in them all the year around; isn't that true?

A I didn't say that, I don't think.

Q You report travelers that did; don't you? Take a look at page 6 of your report where you say the Sutter basin didn't drain until under natural conditions; is that correct?

A Yes, that is correct.

Q And you can take a look at page 9 from Cronise. He talks about 200,000 acres in San Joaquin County covered at all times by a few inches of water?
A That is probably part of the area that they walked across and found that there were some tule marshes that they could walk through, but most of it was grass and dry.
Q If the marshes were really draining and the tules were drying out and not using the water, then I take it, we should not have historical accounts of them in the late fall; should we, or at least if they are they should be counted as dead?
A No, that doesn't necessarily follow.
Q You mean tules could be alive in the fall and not have water?
A If the water is up to the roots they can be alive with water one, two and three feet down. They go dormant and they use less water. They may go dormant and would use perhaps less water, but they wouldn't die. I don't think anywhere I said the tules died necessarily.
Q You are not taking the position then that under natural conditions we did not find very large areas of tules in the fall, you are agreeing that we did find them?
A Yes, it's a matter of the number of acres that we are discussing. A few hundred thousand acres makes a difference.

Q You were reading from some historical accounts. I want to call your attention to Father Ayala. Did you look at that expedition?
A I think that's one I mentioned.
Q That was in the October trip; wasn't it?
A I think they went up just to -- how far up the river did they go?
Q Well, he has entitled his diary "Exploration of the Eastern Shores of the Upper San Francisco Bay, San Pablo and Suisun Bays, and of the lower Sacramento and San Joaquin Rivers."
A Yes, if I remember correctly, he did not go up the river particularly, but that's only my memory.
Q Let me read you a couple of quotes from him: "October 17. There are various islands covered with tule rushes and thickets. At 14 leagues the rivers begin to form with tules on the banks. It is sheer swamp which prevents any landing on firm ground. On the 18th day, everything is tule swamp on each side with an occasional bush. On the 19th day, the river keeps on in the same with its windings covered with tules."

A little farther down the same day, "There were ponds and tule swamps."

"The 21st day: We journeyed about three or four leagues and stopped at a high spot which had a number of oak trees but was entirely surrounded by tule swamps."

"Twenty-second day: All the tule swamps is impassable." Still on the 22nd day: "The bank of the river still has some oak trees, but from here down where the tule swamps begin again."

"Twenty-fourth day: The previous night we slept in the tule swamp and the water reached our blankets at the turn of the tide. The whole area is this way for several leagues."

You didn't find anything in your historical account which contradicts this; did you?

A In fact, I quoted some things. In particular, he traveled up the San Joaquin probably, I would guess, and the San Joaquin, if you remember, I reported not only on the tules, but on the mosquitos and that they reported almost up to Stockton that there were tules on both sides and it was very boring.

The same gentleman who reported going over that same route you were just quoting at great length, who quoted all those tules, subsequently went up the Sacramento and came down from Sacramento to San Francisco, and agreed pretty well with other travelers, that it was not tules, it was more delightful than the trip to Stockton on the San Joaquin.

MR. LITTLEWORTH: Mr. Chairman, let me ask a question here. I have historical accounts at no end and I don't know whether it is worth my going through it if you maybe would take my representation that I can read from Spanish diaries showing travelers were seeing tules, and I can do it for a good deal of the rest of the evening, if one wanted to.

MR. MAUGHAN: No, thank you. Can you give a list of the references?

MR. LITTLEWORTH: Yes, we are going to put that in a rebuttal exhibit and maybe this is the way to handle it. I think every historical source that's ever been listed is listed with the quotations and so forth, and there are pages and pages and pages, and I think that's the best way to handle it.

MR. MAUGHAN: That would be much better at this stage because some time ago I just felt you might each find a few quotes that might give a little different viewpoint because travelers went different routes.

MR. LITTLEWORTH: We are actually relying primarily on all of the surveys and maps and so forth. That's where we really rely, but there are no end of accounts and they are in our rebuttal exhibits. I will do it that way.

MR. DAWDY: I would say, Mr. Chairman, that there is not no end in mine because I have limited resources but that I quoted mine, mapped them, showed where they were in relation to what was shown on that map, and so, I would feel, I would hope that you would do likewise, where you don't show tules you would show where the person was. It would help to identify some areas that are not what you say or what I say.

MR. MAUGHAN: Let me just say it, and I understand what you are saying, and to the degree the State Contractors ought to do that, I do realize we have different resources behind some of this kind of research, so I think all the Board members and the staff do, so that is something that is known.

MR. LITTLEWORTH: Q Mr. Dawdy, I want to just touch one other point and maybe I can end this more quickly if we are going to rely on the rebuttal witnesses.

MR. DAWDY: Mr. Chairman, it would have saved more time if you had done such a thing knowing the basis for your original exhibits because then we could have seen our areas of agreement and disagreement.

Q Well, Mr. Dawdy, I have the Cushler map right, if you want to get into it, but I think we left it with the rebuttal exhibits. I have the big map, though, if you want to take a look at it afterwards. You might be sort of interested.

A Not right now anyway.

Q I wanted to ask you just a couple of questions about the burning. It is your thought that burning by the Indians was very extensive?

A Yes.

Q I want to quote you a paper which we obtained from the Forestry Laboratory at the University of California at Berkeley called, "The Influence of Fire on California's Pristine Vegetation," a consideration of control burning, by Burcham. One paragraph reads as follows: "The records reviewed above agree with many others studied. They indicate some burning was done by Indians in Grasslands, but there is very little evidence of Indian fires in forests until an appreciably later time. While it can be established with relative ease that Indians at times burn vegetation, there are many indications that the frequency and extent of their burning was limited."

Then, over on the next page, "No evidence has been found to indicate more than very infrequent use of fire for hunting in brushy and forested lands. It appears highly improbable that the California Indians essentially lacking in manpower and physical facilities would attempt to purposely burn any but small tracts of land in which they were particularly interested on a systematic basis."

MR. WALSH: Where did that paper come from?

MR. LITTLEWORTH: It is a paper written in 1960 out of the Forest Laboratory at the University of California at Berkeley.

Q Do you disagree with that quote?

A I agree that there are anthropologists who have opposite views of that.

Q You indicated that the burning occurred only as naturally caused or by Indians. Are not aware that much of the burning occurred after the reclamation began as a way to actually reclaim the tule land?

A Well, yes, but we sort of agreed we are looking at it from an historical context before the Gold Rush. That's the way I spoke to that.

Q What about hunting? You agree some of the burning occurred in order to flush game out of the marshes?

A Perhaps. I am not discussing from 1850 on.

Q So what you are talking about then is burning by the Indians themselves or just from natural causes?

A Yes.

Q Indians actually used the tules; didn't they, for all kinds of purposes?

A Yes.

Q It seems rather unlikely they would burn down their source of fuel -- they used them for houses, boats, they made clothing out of them.

A They also burned them down so they could see the fields so the enemies couldn't creep up on them because they lived in the tules. They burned them down so that they could easily move about.

Q They burned them down so they could be exposed to their enemies?

A No, so they would not have their enemies creep up upon them.

So what I am saying is perhaps we can do a little job in comparative anthropology, but we don't know why -- well, we know the Indians burned. As to what their particular purpose was and how they felt about the different objectives that you are talking about, of course, is an argument among anthropologists.

Q We have no idea how much land was really burned; do we?

A No, we don't know how much land was burned.

MR. LITTLEWORTH: I think those are all the questions we have.

MS. LEIDIGH: Staff has no questions.

MR. MAUGHAN: Do Board members have any questions -- Board members have no questions.

You did have several exhibits. Could you repeat what they are?

MR. DAWDY: Exhibits 1, 2 and 3 have been handed in earlier. Exhibit 4 is my picture of the map with the location of the travelers upon it.

MR. MAUGHAN: Objection to receiving these? Hearing none, we will receive them into evidence. Thank you, Mr. Dawdy.

(Exhibits 1, 2, 3 and 4 of David Dawdy were received into evidence.)

MR. DAWDY: In redirect, I would like to say, Mr. Dawdy, in your quotation from the historical records, did you hide any evidence that was contrary to what you presented?

Well, no, Mr. Dawdy, I did not. I quoted directly from each of the sources that I found and each of these has excerpts where I tried not to distort the meaning of what I was trying to convey. I tried to give both sides reporting upon the trees, the tules, and the extent of the travelers' journeys around the landscape.

Thank you, Mr. Dawdy.

MR. MAUGHAN: All right, thank you. Before we break, I know that we have six individuals, or six parties, I should say, that want to present rebuttal testimony. There is no way I can tell you right now that we can stay that long tonight because some of them have indicated rather extensive rebuttal, but I also understand at least one gentleman is here from back East and, Mr. Somach, we definitely are going to go through that one and get the cross-examine done. Otherwise, I have no choice but to schedule after that for the 29th of December along with some other cross-examination.

I know that's a bad day.

Tom, we have long noticed that we are starting our program of implementation testimony and so, we just can't encroach upon that.

Now, if somebody wants to say here with me and Alice can stand it, and the people who want to cross-examine would be willing to stay, I will stay as long as you want tonight and go through it. Otherwise, we will definitely go through Mr. Somach after the break. You tell me if you desperately want to stay here later tonight and we will see what we can do to get that over. Otherwise, it will be carried over to the 29th.

We will take a 10-minute break now.

(Recess)

WEDNESDAY, DECEMBER 9, 1987, 5:20 P.M.

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MR. MAUGHAN: All right, Mr. Somach.

MR. SOMACH: By way of opening statement, I would like to describe the purpose of the testimony so that we can move through it as quickly as possible.

During the hydrology topic or hearing --

MR. MAUGHAN: Excuse me, just for everybody's information, there are going to be two witnesses tonight from what I have had people come up.

Mr. Whitridge has somebody that's related, but the rest of them have agreed to hold their witnesses over until the 29th. I'm sorry.

All right, Mr. Somach.

MR. SOMACH: Well, the testimony that South Delta, I assume will deal with is the same as what we are dealing with and that's the presentation by the State Water Resources Control Board consultants with respect to testimony regarding hydrology.

In particular, the focus of our testimony will be a model with respect to bay salinity, the BAYSAL model, which was presented by board consultants which I believe was, in fact, I know was State Water Resources Control Board Exhibit No. 10, or least the results were reported in that exhibit.

This model purports to predict salinity and presumably the reason that it is being introduced is so that it will be used to predict salinity in the context of establishing standards, salinity-related standards.

The question then is, can the exhibit be used for that purpose? Can the model develop the use for that purpose?

The Central Valley Project Water Association consultant, Dr. Blumberg, says no and that's what his testimony is about, to explain why he says no.

ALAN BLUMBERG,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. SOMACH:

Q For the record, Dr. Blumberg, could you spell your name?

A My last name is B-l-u-m-b-e-r-g.

Q Was Central Valley Project Water Association Exhibit 42 prepared under your supervision and direction?

A Yes, it was.

Q Okay. And I might add, I have given board staff 48 copies. I have provided a copy and these are packages which include Exhibits 42, 43, and Exhibit 43 has many sub-pages to it, and Exhibit 44. I believe I have also distributed copies to those in attendance here.

Now, Exhibit 42, which you have indicated was prepared under your supervision and direction, is a summary of your qualifications; is it not?

A Yes, it is.

Q And it is an accurate summary?

A Yes, it is.

Q Can you briefly summarize your qualifications with respect to the testimony that you are going to give here today?

A Beginning with the research I did for my Ph.D. dissertation, I have been involved in esturine and coastal ocean hydro dynamic circulation modeling. For the 12 years since I received my degree, I have been at the forefront in the development of the state of the art in three dimensional circulation models, and also, have developed and applied one dimensional and two dimensional models.

My work is well documented in the scientific literature. I have approximately 35 journal articles and a similar number of technical reports.

Q Have you read the direct examination, the transcript of the direct examination and the cross-examination of Dr. Gartrell with respect to Exhibit 10 that took place in July?

A Yes, I have.

third dimension.

Q In your opinion, can the model described in Exhibit 10 be used to accurately predict salinity in San Francisco Bay?

A No.

Q And it cannot because it does not include this vertical structure of San Francisco dynamics; is that correct?

A Correct.

Q San Francisco Bay dynamics. By way of an example, I would like you to demonstrate the significance of the vertical structure of San Francisco Bay in predicting salinity.

Now, I would like to focus on Exhibit 43A through, I believe, U, and for the sake of time, I will indicate for Dr. Blumberg that those exhibits are essentially divided into three areas. The first two areas describe the idea of the vertical in the three dimensional model, and I have asked Dr. Blumberg to move through those rather rapidly, but if the board has any questions in terms of following through that, it would be helpful if you would simply ask instead of spending a great deal of time.

The third section or last portion of that particular exhibit would be a more detailed analysis of exactly what should be done in terms of proper remodeling

Q Were you in attendance during the subsequent cross-examination of Dr. Gartrell in Concord?
A Yes.
Q Have you reviewed Dr. Gartrell's calibration information which was provided to me after the Concord hearings?
A Yes, I have.
Q And is that information marked as Central Valley Project Water Association Exhibit 44?
A Yes, it is.
Q Do you consider the model appropriately calibrated?
A No.
Q Now, Exhibit 10 is a summary of the two-dimensional model, BAYSAL model; is it not?
A Yes.
Q In your opinion, can you understand the dynamics of the San Francisco Bay and predict salinity of the bay without inclusion of the vertical structure of San Francisco Bay dynamics?
A In my opinion, you can't really understand the dynamics unless you include the vertical dimension.
Q And that vertical dimension is not included in the two dimensional model, the BAYSAL model; is that correct?
A Yes, I think that's a first step toward the eventual development of having a model that does include a

the bay with respect to salinity.

With that as kind of an overture, Dr. Blumberg, why don't you proceed with your description.

A Before I go to my examples, I would like to provide a quick overview of what kind of models are available and what kind of physics are available as far as the state of the art is concerned.

Q If you could, Dr. Blumberg, as you put up these overheads, refer to them with respect to their exhibit number. I don't think you have to indicate CVPWA. The one you have up here is 43A, and that will make the record clear as to what you are talking about.

A Also, in the bottom right-hand corner of my overhead, you will see that exhibit called out. This is 43A. The top portion shows what I feel to be the real estuarine circulation that goes on in the San Francisco Bay system. Basically, we have, once the tides have been removed from the dynamics, we average over a tide. We basically have flow in the surface layer going out towards the ocean and there's a return flow at depth toward the head of the estuary, so basically, we have a flow that is highly structured in the vertical, flow going out at the surface and in at the bottom.

There are four types of models that have been configured in the literature to address estuarine circulation. The first one is the most complete in terms of its structure. That is the three dimensional model. What that model seems to do is divide the estuary into a lot of boxes, both up the estuary, across the estuary and with depth, and in each of these little boxes you predict the salinity, temperature and flow field.

Q I was going to ask, also, we want to go quickly, but we want to make sure that the record reflects what you say, so take it a little slower and don't worry so much about the time.

MR. MAUGHAN: Thank you.

A Okay. Shall I repeat any of that on the three dimensional one?

The other models, the three lower ones, are models that have been simplified in terms of their depth resolution. The first one I call two dimensional laterally averaged, that is a model that seeks to only look at what's going on in the vertical, so here you have a model that divides vertical into big boxes that extend from one side of the estuary to the other, and really doesn't want to address the fact that the current on the shallow banks is slower than the current in the deeper portion, but it does seek to address what goes on in the vertical. That is the laterally averaged model.

Then, we come to the model that has been presented in the testimony and that is a two dimensional vertically averaged model that is a model that retains the structure along the estuary and across the estuary, but is not really concerned with what goes on in the vertical. Here you divide up your estuary into boxes that have no vertical resolution. You get the mean current from top to bottom.

If you look at our top slide, if there are, let's say, currents going out at, for example, 15 centimeters per second at the top and a current coming in at the bottom of 10 centimeters per second, what this two dimensional vertically averaged model will give you is a net of 5 centimeters, so you get the average from the vertical.

The last type of model, and that was a model that was presented in some of the analysis of flushing time, which was the Denton and Hunt model, which is a one dimensional model. That model really doesn't seek to understand what is going on laterally or in the vertical, but seeks to just get the average current as a function of the cross-sectional area, and here, we have just one box to represent what's going on at different cross-sections of the estuary.

My first two examples that I would like to focus upon are those using a two dimensional laterally averaged model. The state of the art of hydrodynamics modeling that looked at the vertical dimension started with this one and then proceeded to the three dimensional case.

The examples I would like to talk about are those that were done by my colleagues and me over the years. My first example has to do with an estuary on the East Coast and that is the Potomac River. The Potomac River is an estuary that flows into the Chesapeake Bay which would be out in here, and it's an estuary similar to the Western Delta area in that the intrusion length of salinity is about the same as in that one, compared to the Potomac.

The salt comes up and extends up about halfway up the estuary where we have very salty water here and we have very freshwater in this area.

The model that I am using is a laterally averaged model to understand what's going on. We predict nothing that's going on across the estuary, only look at every section as to what's going on with depth.

Q And the exhibit you were referring to is 43B; is that correct?

A Yes, it is.

The next few slides starting with 43C show how well the model can represent, first, the tidal dynamics, and this is the plot of the tidal range in centimeters versus distance from the mouth of the estuary in kilometers. We start at the bay entrance here at zero, and go way up to the head of the estuary, Washington, D. C., so it is a long distance, and we can see that the modeling can reproduce the tides pretty well.

The next slide, 43D, shows how well this model, being a simplified model, only a two dimensional model, can reproduce the currents in the vertical and here we have currents at three stations along the axis of the estuary, we have the model, being the dashed line and the data being the solid line. We have station 26 kilometers from the estuary, close to the mouth of the estuary, we go further up to 60 kilometers and then we go way up where there's no more salt left in the system to 96 kilometers.

The model pretty well captures the phasing of the current dynamics, doesn't always capture the total response in terms of amplitude. This shows that the model can reproduce some of the tidal activity, but we are not interested in what the tides are doing. We are interested in what is the circulation when the tides are removed. What does the mean circulation look like, and that has a lot to do with salinity.

My next overhead, and this is 43E, shows you how well the model can compute salinity. Here we have a vertical distribution, we have a longitudinal distribution of salinity in parts per thousand. This is the depth along this axis. The top picture shows the model, the bottom portion shows what the data looks like, the actual data observations that were collected in a field program that I designed were collected at the black dots on the lower portion.

This coordinate that goes along horizontally on the slide represents the distance from the mouth of the estuary and again, it's in kilometers. You can see there's a tremendous shape to the salinity profile and as we move further and further up the estuary, the isohalene becomes vertically homogeneous, and we would anticipate that about in this area there would be no net flow moving upwards in the estuary.

To confirm that, I would like to show my next overhead and that is the slide of what is the mean current. The top portion, again, is the salinity that I just showed. The bottom portion is what the model sees, what the model computes in terms of mean currents. We can see here in the surface that there's a flow in the head of the estuary going out toward the ocean and in the bottom layer we have a return flow of about equal magnitude to what is going on in the surface, and that water extends very far up the estuary, in fact, up into this area here of one to two parts per thousand isohalene.

Q That is Exhibit 43F?

A Thank you. That is my first example, the Potomac River. We see there are important tidal dynamics going on, and we seek that can compute the mean circulation, that will do the tidal action as well as the mean.

My next example is one closer to home and has to do with a similar-type model application to Sacramento-San Joquin Delta area. The model is configured to start here at Eckley in the Carquinez Straits as one boundary and moves up into the Sacramento River. It is, again, a laterally averaged model. The first overhead from that example that I would like to show is 43H. The top portion of this figure is a longitudinal view of the salinity starting here at river mile 20 from the Golden Gate and that's the Carquinez Strait portion and there are salinities in parts per thousand, 25, and it gets fresher and fresher. This is the observed salinity. The observed salinity is inserted into this model and we ask what other currents that are compatible with this salinity regime. We have here as an example some vertical current structure at three spots along the axis.

At mile 42.4 we have the model predictive flows in the upper layer going out towards the ocean and flows in the bottom layer coming up towards the head of the estuary. The data is denoted by the black dots. Those are measured values.

At mile 50, we have a similar structure. At mile 53, there's some salt. All the flow is going out toward the ocean.

To show you what that looks like in terms of velocity itself, we have here a plot of the mean average velocity. We have flows that are above th is horizontal line being flows that are directed downstream. We have above mile 50 all the water going down the stream and when it hits mile 50, this 2 parts per thousand isohalene there becomes a two-layer-type circulation. The flow in the bottom is upstream. It has a vertical motion and then the flow goes downstream.

The vertical motion as a function of distance from the Golden Gate is shown on the bottom portion of this figure. Here we have vertical velocity and you can see that the maximum vertical velocities occur right before the salinity response goes to zero. This is for a flow of 4400 cfs.

The next slide that I would like to show is one where the flow is increased to 10,000 cubic feet per second, and there you see a marked change in the salinity structure. That's Exhibit 43I. Here we are as before with the one or two parts per thousand, the isohalene was up at mile 55. Now it has moved ten miles downstream to right in here. Here again, we have three examples of what the currents look like in the vertical, now at mile 33, 38.6 and 40. We have a two-layer flow in this vicinity; that is, mile 33.2 area, we have a two-layer flow further up and when we get to mile 40, now we have unidirectional flow, all pointed downstream.

In the case we had previously, at 4,400 cubic feet per second, there was a two-layer flow in this vicinity, a strong two-layer flow. Now we see there is just a doubling of the freshwater of the Delta inflow, that we can have a strong reversal in currents.

Again, that's shown in the later averages picture, the third one from the top. We have flow going upstream in the bottom, turns around and goes upstream, and goes towards the ocean in the surface layers. The vertical velocity looks a little bit different, but it does show various peaks of strength in the vertical velocities. This, again, is for 10,000 cubic feet per second.

The utility of this kind of model is shown in the next slide where I have for those same two cases, the top two show results for salinity and suspended solids at 4400 cfs, and the bottom for the 10,000 cfs case, and this is Exhibit 43J.

The model and data results are plotted for salinity at the top, and the third picture, you can see that the model can reproduce the observed salinity rather well, and also, as a bonus, I have shown what the model can predict in terms of suspended solids and suspended solids are a nice thing to know for, for example, phytoplankton, because they have a lot to do with light extinction and growth and dying of phytoplankton.

Also, suspended solids provide absorption sites for toxic substances, so it is a good thing to have a handle on. We can see that the model using the flows can predict suspended solids at the peak concentration which occur at mile 45 for the low cubic feet per second case, and now at mile 40 for the high flow case. That is relatively high compared to 44, but not high compared to the grand scheme of things.

Q Now, those were the two examples that you have provided in terms of explaining the vertical aspects of the modeling process, and isn't it true then that the type of data that you described in those two first examples are the type of data that's left out of the two dimensional Exhibit 10 model of the state board?

A That is correct.

Q Will you proceed then to explain how one can integrate the vertical, or the type of data that you just described with the type of information that does, in fact, exist in the BAYSAL model?

A The model presented previously recognizes the importance of the lateral gradients of the velocity patterns across the estuary and seeks in the next step to look at the vertical. The examples that I have shown so far do not look at the lateral structure, they only look at what goes on in the vertical and my next example is really the state of the art, what is available in terms of a model that predicts what is going on laterally, longitudinally and vertically.

This is a model that I recently ran for the EPA in their program to restore Chesapeake Bay and this is an application of the model to Chesapeake Bay.

This is a model that divides the bay into a number of boxes across. Here we have four or five boxes across the Chesapeake Bay and we have about 35 boxes along the axis of the bay. In the vertical there are 10 boxes. I don't show them here because it is a little hard to draw. The major tributaries, the James River, the York, the Potomac that I have discussed before, as well as the Rappahannock. There are seven major tributaries included in this three dimensional model of Chesapeake Bay.

Q That's Exhibit 43K?

A 43K.

Fortunately, for the modeling effort that was applied, there was a period of time when the National Ocean Service was out collecting data in Chesapeake Bay and there was a very good time history, a month-long history of tide-gage information, and we have tide gage information at the triangle locations, at the mouth of the Atlantic Ocean, the mouth of the bay and we have one halfway up and one here, another here, and one at the head of the estuary.

The Delta outflow occurs in this area but it also occurs at the head of all these other tributaries that goes into the bay. There are current meter moorings that the National Ocean Service had out, and there was long-term wind information available in the midbay region.

One of the features of the model that I have been using is one that addresses turbulence in a very fundamental way. It writes down equations for turbulence mixing and turbulence mixing turns out to be a very important process which goes on in the vertical. It keeps water from being mixed from top to the bottom when it needs to and when the wind blows, for example, it can mix the water column from top to bottom.

To show that the model has some predictive capability, we ran a 30-day simulation using all the data collected by the National Ocean Service. Before running the model, this type of model requires a variety of inputs and here I have tried to summarize what those are.

This is a three dimensional circulation model.

Now, from the atmosphere we have to specify the wind stress everywhere along the surface of this model, at the Atlantic Ocean boundary, at the mouth of this estuary, we have to specify salinities and temperature as a function of time, so as we are running a 30-day simulation, we have to know salinity and temperature in the vertical for all times during the length of the simulation.

Also, we have to know the sea level fluctuations.

So, those are two important parameters we have to know at the Atlantic Ocean boundary. At the land boundary we have to know the Delta inflow for Susquehanna River in the case of Chesapeake Bay and at the head of every model tributary and we have, again, seven tributaries and the Susquehanna.

The interior region also requires a number of data to run the simulation. That is, first, we need to know a three dimensional distribution of salinity and temperature everywhere in the bay because in the National Weather Service model you don't want to predict what has happened the last hundred years, you want to start giving the environments to predict forward in time. This is Exhibit 43L.

The types of data, the actual data that went into the model for the period September, 1983, for a 30-day simulation that we have run are shown on my next exhibit.

This is 43M. Here we have four types of data, four of the types that I mentioned in my previous slide. We have as a function of the time, September, 1980, along the bottom portion of the slide. We have, first, at the top the wind stress, and here the reason September, 1983, was selected besides being a period when there was a lot of data, there was a very significant wind event which occurred about the 22nd of September.

Here the wind was blowing very strong to the north and these areas are scaled upward pointing north, and there is a scale of magnitude and the strength is denoted here on the axis. We notice on the 22nd the wind shifted from very strong to the north to very strong to the south, and what that did in Chesapeake Bay was mix this very stratified system that had very freshwater at the top, very salty water at the bottom, mixed it completely in a matter of hours, mixed completely from top to bottom.

Another boundary condition, another type of data that the model requires as a function of time is the sea-level elevation, sea-level fluctuation, fluctuations from my previous slide at the ocean boundary, and here's what the data shows. This is a 30-day record, you can see the spring and neap cycle that you have been accustomed to seeing in the data that has been presented now over the last two weeks.

43-0 These are the mean currents observed, predicted for September, 1983. On the left-hand portion we have the surface currents. The scale is denoted here in the upper left-hand corner. The length of the arrow -- and you can see some arrows here -- the length of the arrow is proportional to how fast the current is moving. The direction of the arrow is really in the direction of the flow.

On the right-hand side of the picture, we have the model's predicted bottom currents.

First, let's look at the surface currents. We have flows starting here at Susquehanna, coming down Chesapeake Bay, forced up against the western side, as it gets into the lower bay, the currents get bigger and bigger and make their way out into the ocean.

About here, a third of the way up, there's a very large residual eddy and pollutants could get trapped in there for extended periods of time.

The action along the bottom of the bay is even more dramatic. Here we have, starting at the ocean boundary, flow coming in, moving up the axis of the bay, switching from one bank to the other and moving all the way up almost 300 kilometers here to where the salt runs out and that is in this area here. These are the model's predicted mean currents for the entire month of September.

initial state of the bay presented in 43M, and what I am showing here is a longitudinal section up the axis of Chesapeake Bay. The ocean boundary is here at zero distance, and we go up to the Susquehanna, all the way, very far up north, 300 kilometers to the Susquehanna entrance, where the salinity goes to about zero.

This is a depth axis here, about 25 meters deep on these scales.

The black squares, the darkened squares on this plot are those areas where we have data, so the actual data points are few in number. They pass through what is called objective analysis technique that puts data everywhere in our Chesapeake Bay. It interpolates, in a sense, so that we can start with an initial condition and here we start with relatively freshwater of 14 parts per thousand, and at depth the water is about 23 parts per thousand, and that is a lot of salinity in the vertical, 9 parts per thousand.

The model uses this as an initial condition for September 1, 1983. It will start at 000 hours and go '30 days with tides, with real winds, with observed boundary conditions.

We would like to ask, can the model reproduce what is going on in terms of dynamics of Chesapeake Bay? My first results are the mean current shown here on Exhibit

Also, superimposed on these tidal undulations which go up and down are episodes, for example, about the 15th of September when the entire sea level in the bay rose about a foot, and these were caused by storms that were going along at the ocean boundary, on the continental shelf.

The other boundary conditions that are put into the model are boundary salinities. We have current meter measurements for salinity as a function of time. We have the bottom plotted here as a dashed line and we have surface salinity plotted. We also have temperature plotted. We have surface temperature, being warmer, bottom temperature, and if notice, about the 15th of September that situation reverses and all of a sudden we have very cold water or colder water at the surface than at the bottom, and that really is an unstable situation. You think of hot air rising and now we have hot air actually lowering, that would be an atmospheric analogy.

About the 22nd I would like to point again that the entire bay become well mixed from top to bottom. You would think a temperature inversion like that would do it, but it really doesn't. It takes a strong wind force event that occurred on the 22nd to do it.

Let me set the stage for this 30-day simulation. We used the data presented here in Exhibit 43M and the

They were obtained by integrating the model, by running the model using hourly information, so we have run every hour through the entire month and then averaged all the results to produce this picture.

To give you some confidence that these predictions are reasonable, I would like to show you three exhibits which compare modeling and data at stations midway up the axis of the Chesapeake Bay.

The first exhibit is 43P. We have three slides and this type of presentation will set the stage for the next two slides. We have time series in day os September, 1983, along the bottom part of the slide, and we have three time series that we are looking at. We have the mid-bay elevations; that is, the surface elevations, the tidal dynamics, which is the data on the top part. We have the model results in the center slide and we have the difference between the two on the bottom portion.

Not only have I shown the actual tidal signal, the signal predicted by the model, I have also time averaged the tidal currents out so you can see what the non-tidal response is.

We can see here the big event that occurred about September 15, well produced up in this data. The previous time series I showed was at the ocean entrance, and this is way up the estuary. You can see it in the data, you can see it in the model.

The relative average area between the model prediction and the data is seven percent for this example.

The next thing I would like to talk about are currents. Those are typically harder to predict than surface elevations. In fact, they are much harder to predict. Here we have the same scale, September, 1983, along the bottom axis and this is Exhibit 43Q. We have the data in centimeters per second, we have the model, and again, we have the difference between the model and the data. The various events that occur in the data are well represented in the model as well.

As far as the absolute signal is concerned; that is, it has the tides in there, it has the wind-driven circulation, the model has a five percent relative average error. However, if you remove the tides and ask how well can the model predict non-tidal velocities, it has a much greater error and that error is about 20 percent, a 20 percent relative average error, so we can typically say the tides are done in a much more consistent fashion with the data and it is much more difficult to predict the mean circulation, the circulation that drives the salinity.

This leads to my last example of how well the model is doing and that is a comparison of the salinities. Again, we have on the bottom axis the days of September, 1983, and this is the salinity near the bottom in the middle of the bay, about 150 kilometers from the mouth, we have the data first on the top, we have the model in the center and we have the difference between the two on the bottom portion.

This relative average error is 15 percent between the model and the data.

Now, I have to confess or admit that this is one of the better stations out of the six that I have compared. Salinity is even harder to do than currents; however, with a 15 percent relative average error, we know that the model is reproducing what is going on pretty well. You see here that about the 22nd of September you have the dramatic event which changes salinity by about five or six parts per thousand is picked up nicely in the model. The data, however, you notice recovers somewhat from this event with large undulations in salinity. The model doesn't capture that response.

Q This is Exhibit 43R?
A 43R, yes.

The utility of this model, it has been used in a Water quality model of Chesapeake Bay. I just want to show that for some perspective of what you would actually use this type of model for.

Here I have predictions of the model, water quality model, using the currents produced by the three dimensional circulation model for the summer of 1984. I'm showing here five different parameters that water quality people are concerned with. We have at the top portion salinity along this axis, the horizontal axis of this picture is distance from the mouth of Chesapeake Bay in kilometers, we have the ocean here, we have the Susquehanna River at this end, the surface data are in blue, the surface model results are in blue, and the bottom results in the model are in green.

Here again, we have the ten levels in the vertical and all I am showing you are the surface layer and the bottom layer.

What is key and I would like to focus in on, is the dissolved oxygen in Chesapeake Bay. There is a tremendous problem with dissolved oxygen in the summer. The deeper portions of the bay have no dissolved oxygen and that really reduces striped bass, clams, the whole fishery falls apart during the summer. The model reproduces that very well. The data are denoted by these horizontal lines corrected by vertical lines connected by a vertical line and a dot. That represents the mean of whatever data is available for the summer of 1984, plus one standard deviation. We can see then in the surface layer there is a lot of dissolved oxygen, but when we get in the deeper layers of Chesapeake Bay, all of a sudden there is a whole stretch, about 100 kilometers where there is zero dissolved oxygen in the lower layers.

The model also is used to reproduce chlorophyll , surface and bottom layers, phosphates and ammonia.

Q Before you move away from that, that is Exhibit 43S, and for the record, the lines in green are the surface; is that correct, the surface lines.

A Yes -- no, blue is the surface. Green is the bottom. And also, denoted in the key that doesn't have color as a solid line is the surface and dashed line as the bottom, and that key is provided on the picture.

The dimensional model really has started to come into its own. In fact, in the last two years, and I would like to close on two slides which address three dimensional modeling in general, and I would like, first to talk about the advantages of the three dimensional model.

The first one, and the most important, is that it gives you a good representation of the physics. It really helps you to understand what's going on, helps you to predict what's going on. There's a lot less parameterization that needs to be done, and by that I mean some of the previous testimony showed models that weren't three dimensional. What they have to introduce because they are simplifying the physics of the problem, are things called dispersion and dispersion needs quantification, and that is a problem that is difficult to quantify.

The third advantage for using a three dimensional model is now there are many robust models available. There's not just mine and there has been a lot of criticism in the past that my model essentially was the only one that's available, but now there are a number of robust models available, so you don't have to just focus upon the one that exists. There are now some very sophisticated turbulence-mixing models so that we can get a better handle on what's going on in terms of turbulence and mixing.

And the final advantage noted here is that three Dimensional models actually make better use of the available data and information, and by that I mean, if you are using a vertically integrated model, you need to compare your results against vertically integrated data.

Well, it is very difficult to obtain vertically integrated data. Typically you get a surface measurement of salinity or a bottom measurement of salinity but you never get a vertically integrated salinity. So, here a three dimensional model could converge surface salinity when that data is available or bottom salinity when that data happens to exist.

Some of the disadvantages of the three dimensional models are as follows: The first one is it really takes more computational resources. That kind of leads into the computer dollars. It does cost more money in terms of computer dollars to run a three dimensional model than any of the other simpler models as discussed.

The second disadvantage, and I find this one more important than the first one, and that is that the three dimensional model is more difficult to interpret. The model is very complicated, the results are very complicated. You have to look at the model results as if they were data, try to understand what's going on in context of the wind forcing, the boundary forcing and the salinity forcing.

The third disadvantage is that to really understand whether or not your modeling is a good one, it takes a large amount of data. The three dimensional models produce lots of simulations, there are many grid boxes similar to what I have shown for Chesapeake Bay.

We have a need then for having data in every one of those grid boxes to compare the model. The advantages and disadvantages of the three dimensional model are in Exhibit 43T.

The last slide addresses the computational resources issue. This is a slide that I carry around with me at all times. It shows along the horizontal axis time since 1950 going up to about 1985, and along the vertical axis it shows the time it takes the computer to do a plotting point multiplication, and that's directly proportional to how much it costs to do computations, and we can see that since 1950 there has been a tremendous decrease in the amount of cost for multiplication, and that cost is still coming down today.

We can see almost from this graph that about every ten years you can get a hundredfold increase in computer power. So that every year that goes on, our computer gets faster and faster and cheaper and cheaper, so eventually, three dimensional models will be off the shelf and very easy to use.

Q Let me ask you two follow-up questions and I do appreciate your moving through that as quickly as you did. In your opinion, can a three dimensional model be developed for San Francisco Bay?

A In my opinion, it can be.

Q Would you, in your professional opinion, make predictions regarding salinity to establish salinity standards for San Francisco Bay without the use of a three dimensional model?

A I would not make any predictions about salinity in

San Francisco Bay without the use of a three dimensional model.

MR. SOMACH: That concludes the direct examination.

MR. MAUGHAN: Do you want to take a deep breath? That's a lot of material. I think you explained it under the circumstances very well.

Who do we have that would like to cross-examine? Mr. Thomas -- anyone else? Mr. Dawdy.

Do we have staff, too?

MS. LEIDIGH: Yes, staff does have some questions, although we understand if Dr. Gartrell asks questions, it might eliminate the need for staff to ask questions.

MR. MAUGHAN: Okay, let's see what the order is. I guess Mr. Thomas is first.

CROSS-EXAMINATION

by MR. THOMAS:

Q Dr. Blumberg, I am Greg Thomas appearing for the Romberg Tiburon Center. Please appreciate as I ask these questions that I have seen your graphs only as you began speaking and I don't pretend to have any in-depth understanding of what they portend, but I did want to ask you if I understand correctly.

The implication of your testimony is that salinity measurements taken only at the surface rather than throughout the water column give a distorted view of the

average salinity levels; is that the implication of the modeling work that you have done?
A In those areas where the salinity is not vertically mixed, it does give a view of the surface salinity. It does not give a view of the average salinity.
Q And you expect there to be a mixing of saline iwth freshwater wherever you have an interface between the two; isn't that correct, where you have freshwater flowing into saltwater?
A Yes.
Q I also understand that the information in some of your exhibits regarding the San Francisco estuary where you show various values for river miles from the Golden Gate, are those data, predictions on your part, or do they reflect actual data?
A The second plot from the top --
Q I am referring to Exhibit 43H and 43I primarily.
A I am putting up Exhibit 43H. There are three areas where there were measurements, and those were provided to U.S. by the Bureau of Reclamation, and they were data at mile 43.4, mile 50, and 53. The data are the dots, and the model results are the solid line.

There are more model results than data and the model result is used in that manner.
Q Let me ask this: Based upon your modeling

technique and what you understand about the hydrodynamics of this estuary, would you have a prediction regarding the manner in which salinity levels will change in the estuary assuming that the frequency of low flow water years of outflow from the Delta is increasing over time? Is that a clear question?

A Well, I am really not prepared to answer that kind of question. I wanted to direct my testimony more towards why you should look at including the vertical dimensions into models of San Francisco Bay, but I really am not prepared to talk about what is the response of the bay to the various changes in Delta outflow.

Q I am not asking you for information in detail, I am only asking you whether it is consistent with your model and the premises on which it is based that as the frequency of low flow water years increases, you would expect to find increasing salinity regimes in the estuary as well.

MR. SOMACH: I think he responded to that question.

MR. THOMAS: He didn't respond to the question as I just formulated it.

MR. SOMACH: Under the rules, he can say no or I don't want to answer the question.

MR. THOMAS: I believe I saw him nod in agreement with that.

MR. MAUGHAN: I will go one step further. If he wants to answer it again, but I don't want to repeat it after that.

A I guess I would like to just leave my first answer. I really don't want to address it.

MR. THOMAS: After the interposition of counsel, I take it?

I will have no further questions, Mr. Chairman.

MR. MAUGHAN: All right. Dr. Blumberg, when you say there's circulation of salinity caused by factors rather than freshwater inflow, as you indicated by temperature, by wind and by other factors as well as freshwater inflow, and the three dimensional model will do some interpreting of those other factors?

A Yes, it will.

MR. MAUGHAN: All right. Mr. Dawdy.

MR. ANDERSON: While Mr. Dawdy is approaching the table, I wasn't aware that requests were being entertained, but I would like to request our expert, Dr. Brown, be allowed to give testimony either tonight, but at some point prior to the holidays.

MR. MAUGHAN: If Alice can hold up, I will, too.

MR. ANDERSON: It should be about 15 minutes.

CROSS-EXAMINATION

by MR. DAWDY:

Q I want to ask very few questions. Your Exhibit 43T on the three dimensional model which you are showing has some advantages and disadvantages; is that correct?
A It does.
Q If were selling two dimensional models, we might take the advantages and make them disadvantages, and take the disadvantages and make them advantages; is that not possible?
A It's possible.
Q Wouldn't it be true?
A Well, you wouldn't --
Q Yes or no, please.
A Could you state your question again?
Q I say that if we were selling a two dimensional model instead of a three dimensional model, the advantages might become disadvantages and the disadvantages might become advantages?
A Yes.
Q The disadvantages are the costs, it is more difficult to interpret, and it requires great skill and requires much data.
A Correct.
Q If you have practically no data, it may not have very much advantage to go to a data-intensive model; is that correct?

A Correct.

Q I want to look at your Exhibit 43P and 43R, and I wish to state also that you went through very fast and I haven't digested this so I would like to ask a couple of questions.

You have mid-bay elevations and you have stated your average errors, and you have stressed at some length the major events that happened apparently the 15th or something like that, and this was reflected in the data.

As you went by I saw on 43P that that event, the model response went from some 20 centimeters to 60 centimeters, and the error went up to some 25 centimeters, so that would be in the range of -- 25 out of 40, so it is 60 percent error. And if it were just absolute in terms of the 60, it would be 40 percent error. That's a fairly large error in relation to the mean error you have stated; isn't it?

A For that specific event at that location, the model didn't really predict the total surge.

Q And if that surge, that event is a major problem in decision making, then your model would not have predicted it?

A At this resolution, these many grid boxes that we have used, it would not predict it.

Q Then, its advantage over here on difficult to interpret and rigorous skill assessment requiring much data and more computational resources, should be multiplied by four in order to double the grid size?

A Well, for the Chesapeake Bay, in particular, if you were basing decision-making criteria as we are learning to do here, I would use many more grid points than I have used in Chesapeake Bay, at least four times more.

Q So, it would cost 16 times as much probably because it usually goes up at the square; doesn't it -- time to compute?

A It goes up at a factor of eight, even for Chesapeake Bay.

Q Cost of eight, difficulty of interpreting by eight orders of magnitude?

A More difficult.

Q Much more difficult, and the rigorous scale of assessment, how much would it go up?

A I would say roughly the same.

Q Another eight?

A Yes.

Q And the data required to calibrate and particularly to catch that point which is an important decision parameter.

A Well, you wouldn't need eight times more data. You would need to compare much better in those locations.

Q You would have to know those locations in advance where your model is not going to work?

A Yes.

Q Okay. It seems that there is a similar case at the end of the month, on the 30th. Once again, there was a large change and your model blew it; is that not correct?

A Yes.

Q If we look at 43R, the interesting thing, your difference is down at the bottom and you spoke about the average difference. It looks to me like for the first 20 some days your model is biased in the salinity; is that not correct?

A Biased low.

Q Biased low, yes, and then you had something that hit the system and it rang; didn't it?

A Well, it didn't ring, it mixed the system.

Q The data that are shown at the top have a semblance of what I call a ringing, going up and down, up and down. The mixing is the cause for those rapid ups and downs?

A Well, that probably is the fact that the current meter is now positioned at a spot where the salinity that is mixed almost to the bottom is moving up and down rapidly, so that's what you are seeing. The current meter is at a fixed location, but that location has a very severe change in salinity in the vertical so that change is moving up and down.

Q But you have a three dimensional model in the vertical.

A Yes, but it has limited resolution.

Q And it didn't have a dime of response; did it? The model was predicting that to be a little lower than the observed spot.

A A little bit lower?

Q It's not having any variability at all in there. The dynamics of the system was lost; was it not?

A At that location the model did not reproduce the physics of what was going on at that data point.

Q I have only looked at two. I can't look at all of them. Let's go to a few general questions. You said you didn't think you would make predictions without the three dimensional model in San Francisco Bay for the purpose of setting standards or something like that.

A Correct.

Q What level of accuracy do you think is required for a policy decision concerning criteria for San Francisco Bay for salinity?

A Well, let me first say that a model is not the only thing I would use to set standards. I would like to have data to make any standards, but I would like to back up some of the physical processes that are going on and I gain that understanding by looking at lots of data and trying to model those processes.

I would say if I were to set standards, I would use a model with lots more grid resolution which would pick up more of these events that are missed in the coarser resolution of Chesapeake Bay.

Q Well, that was not responsive to my question, I might point out, but we will pass on that.

What accuracy is required for a policy decision?

A I really can't address that.

Q Then, you can't say a particular model is not accurate enough for a policy decision; can you?

A A model that can reproduce --

Q Yes or no, please.

MR. MAUGHAN: Well, he can say yes or no, and then he can explain.

MR. DAWDY: And then he can wander from the point as much as he wishes, Mr. Chairman, but I would like a yes or no.

MR. MAUGHAN: He can say yes or no, but he can explain his yes or no. We have allowed everyone to do that.

MR. DAWDY: Yes, correct.

A Can you ask me the question again, please?

MR. DAWDY: Q What accuracy is required for a policy decision?

MR. MAUGHAN: If you can't answer --

A I really don't know the percentage of accuracy, but I should quantify that in terms of physical processes.

Q You asked me to repeat the question. I was leading up to the question. I asked you that and you didn't know.

Then, you cannot state that a particular model is not usable at some level of accuracy for setting policy standards?

A A particular model?

Q Yes.

A Well, I can't say what model you would like to use.

Q It's not I that is wanting to use the model.

MR. SOMACH: He responded to the question.

MR. MAUGHAN: He can't say.

MR. DAWDY: Q Have you heard of decision theory?

A Yes.

Q You know what perfect information is then?

A I have heard of it. I don't purport to understand all the nuances of it.

Q Perfect information means you know the system. Are decisions always made with perfect information?

A No.

Q Are they always made with complete data?

A No.

Q What is the trade-off between problems which might result if no decision is made because of inadequacy of a model in order to wait for what you would consider an adequate model, and what might result if a decision is made with what you consider an inadequate model?

A Well, I think there could be large differences in the decision. What I am trying to relate here is that these kinds of models are available so they should be taken advantage of in helping to set the standards. You might as well use something that's sophisticated and available if it would help out your decision.

Q If it is cheap enough, and what were the other criteria -- and if you had enough data?

A If you had enough data.

Q And if you had enough difficulty to interpret it and if you had the skill and all these other things, so that what I am asking, do you think there is a trade-off between the problems which might result if no decision is made in order to wait for a more adequate model and what might result if a decision were made with what you consider an inadequate model?

MR. SOMACH: That was a compound question. Why don't you try one at a time?

MR. DAWDY: There is one.

MR. SOMACH: It was compound.

MR. DAWDY: It is not compound. I asked one question.

Would you read it back, please.

(The reporter read the question: Do you think there is a trade-off between the problems which might result if no decision is made in order to wait for a more adequate model and what might result if a decision were made with what you consider an inadequate model?)

MR. DAWDY: Q I am asking for the trade-off.

MR. SOMACH: Did you understand the question?

A I'm not sure.

MR. SOMACH: Ask him to rephrase it or say you can't answer it.

A Are you asking me if trade-offs are to be made?

MR. DAWDY: Q I asked you what the trade-off is.

A It is my opinion that the state of the art is here and it is not a tremendous undertaking to do, so you really don't have to trade it off.

Q Do you think that we have the resources, the data, the skill and all this to go to the ultimate?

A I think you can get very close and go a long way toward that goal.

Q Even though in the process we miss these critical data points where we have particular interest, even though we go to a more complicated three dimensional model; is that correct?

A You may not miss them if you go to a model that has a lot of resolution.

Q We may miss them, though?

A You may miss them with the models that exist today.

MR. DAWDY: That is correct. Thank you.

MR. MAUGHAN: All right. Our consultant is hiding behind the post, so I didn't see him there. I thought maybe he was here in the spirit but not in person.

E X A M I N A T I O N

by MR. GARTRELL:

Q I just have a few questions. In Exhibit 43A, you described tidally averaged velocities. What do you believe are more important in San Francisco, tidal dynamics or tidally averaged dynamics?

A I think in the Western Delta, Suisun Bay area, San Pablo Bay area and many times in the South Bay area, I believe the non-tidal aspects would be very important, especially with regard to salinity.

Q Okay. As a follow-up on that, how much dispersion takes place in a tidally average flow compared to flow involving tides, in order of magnitude?

A Well, the models that have tides in them -- I'm sorry, I don't understand the question.

Q Okay. In a tidally averaged flow compared to a flow with tidal velocities in it, how much dispersion takes place in relative magnitude?

A If you have a tidally averaged flow, there is a lot more dispersion that you need to simulate.

Q No, in the actual flow, not in the model but in the actual flow, how much dispersion takes place in a real situation in the tidally averaged compared to one with tidal velocities?

A One with tidal velocities would require less dispersion than a situation that didn't have tides.

Q You are talking about a model, putting less dispersion in the model? Is that what you are saying?

A Yes.

Q On the exhibit concerning Suisun Bay you said that it was averaged across the bay horizontally; is that correct?

A Averaged laterally.

Q How did you account for flow in channels in Suisun Bay?

A The channels were ignored.

Q And the last question is: Does there currently exist a three dimensional model of the entire San Francisco Bay?

A No, there does not.

MR. MAUGHAN: Does staff want to add any questions?

MS. LEIDIGH: No.

MR. SOMACH: I have two short redirect questions.

MR. MAUGHAN: All right.

REDIRECT EXAMINATION

by MR. SOMACH:

Q Actually, I have one question and then a comment I want to make, but the first relates to Exhibit 43T, and that is the three dimensional model advantages and disadvantages, and quite honestly, it may have been the hostility of the question, but I'm not sure you focused on the question.

The question was: Would the advantages become disadvantages? Could you flip-flop them? How does the availability of robust models and turbulent mixing models, the fact that these models exist, how does that ever become a disadvantage?

A I was looking at it more in terms of the disadvantages becoming an advantage. In a simple model you use less computer resources, it is not as hard to interpret.

Q What you are saying in terms of taking a look at a simpler model, the disadvantages, which really are advantages of a simpler model, is that an accurate reflection of what you are stating?

A Yes.

Q Okay. So, you are not saying that the good representations of physics or all the advantages could ever be disadvantages?

A No.

MR. SOMACH: And the last thing I did want to say, and I want to say it for Dr. Blumberg, is that he was somewhat worried about criticizing Dr. Gartrell's work, and I want to make the statement because he made it to me several times, that he did not feel there was anything wrong with the work; in fact, he felt it was very good and moving in the right direction.

It was this two dimensional versus three dimensional utilization that he was focusing on.

MR. MAUGHAN: I think that came through.

MR. SOMACH: Then I would like to request admission of CVPWA Exhibits 42, 43 with all the subparts, and 44.

MR. MAUGHAN: All right, any objection? Hearing none, they will be received in evidence.

(Central Valley Project Water Association Exhibits 42, 43A through T and 44, were received into evidence.)

MR. MAUGHAN: All right, we have a few more to go. Mr. Whitridge.

MR. WHITRIDGE: This is on the same subject.

MR. MAUGHAN: I realize that.

MR. WHITRIDGE: Mr. Chairman, just a brief opening comment really. This is South Delta Water Agency's rebuttal testimony and this, again, concerns the board's model, but in this case, concerns a very specific input used to drive the model which was not properly used, we feel, and we wish to discuss that.

Basically, it's the board staff's flow-salinity relationship at Vernalis which has been discussed somewhat in their direct tesimony and the incorrect results derived from it.

GERALD T. ORLOB,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. WHITRIDGE:

Q Dr. Orlob, you have been sworn previously; is that correct?

A Yes, I have.

Q And your qualifications, I believe, are in the record as South Delta Water Agency Exhibit No. 2; is that right?

A They are.

Q And, in addition to what is shown on that sheet, I believe you are also Chairman of the Civil Engineering Department at UC Davis; is that right?

A Yes, I am.

Q Have you examined staff Exhibit 3 and the various errata thereto that have been periodically passed out?
A Yes, I have.
Q Are you familiar with their supposed unimpaired condition water quality at Vernalis and elsewhere in the South Delta as presented therein?
A I believe so.
Q Okay. Do these incorrectly show unimpaired TDS values to be significantly higher; that is, worse quality at Vernalis in most months, even than what was actually recorded for those months in the 1920s and 1930s, for example?
A I believe they do.
Q As an example, in the year 1932, do they show the unimpaired TDS would be higher or worse every month of the year than what was actually recorded that month as shown on South Delta Water Agency Exhibit 40 or CVPWA Exhibit 113?
A They do.
Q Okay. Does the historic measured data show that the mean monthly never got above 500 TDS at Vernalis during irrigation season in the period 1930 to 1946?
A To the best of my knowledge, it does.
Q Are you familiar with the water storage projects --
MR. MAUGHAN: Excuse me, Mr. Whitridge, I just want

to be sure -- I can't recall positively, were they actually modeling historical conditions or historical years with current conditions, and that would make a great deal of difference in the answer to some of these questions?

MR. WHITRIDGE: It's my understanding that they purport to show natural conditions in historic years, unimpaired conditions in historic years.

MR. MAUGHAN: Not with any kind of current development then?

MR. WHITRIDGE: That is correct.

MR. MAUGHAN: All right, go ahead then.

MR. WHITRIDGE: Q Are you familiar with the water storage projects existing on the San Joaquin system in the twenties and thirties which the staff has suggested is the only conceivable reason that these could have made this vast difference?

A I am familiar with them in general, yes.

Q Are these and their capacities listed on pages 9 and 10 of South Delta Water Agency Exhibit 4?

A I believe so.

Q Were the projects which were in existence in the twenties and thirties capable of providing, in your opinion, the vast year-around improvement alleged by the staff?

A I don't believe they were capable of the improvements indicated.
Q One other that was not listed on South Delta Water Agency Exhibit 4, I might mention is the sack dam on the San Joaquin River. Did that delay water at all? It had no storage capacity; is that correct?
A It had very little storage capacity. Basically, it was a diversion dam.
Q So that wouldn't have provided vast improvements?
Does South Delta Water Agency Exhibit 35, the 1910 USGS report present measured TDS data near Mossdale for the period 1906 to 1908?
A Yes, it does.
Q This is downstream from Vernalis; is it not?
A Yes, it is.
Q And thus, if anything, it would have had higher TDS than at Vernalis at the same time?
A That may be presumed to be so.
Q Is it true that these measured TDS from 1906 to 1908 never got above 400 parts per million except on one day when it reached 416?
A According to the recorded data, yes.
Q Now, 1908 was a dry year; was it not?
A Yes, that's my recollection.
Q Okay, so it would have a low flow, so that would

represent dry year conditions for that period?

A It would.

Q Are the 1906 to 1908 measurements basically as close as you can get to unimpaired natural conditions because at that time there were no upstream storage facilities for irrigation on the San Joaquin system or virtually no upstream facilities?

A To the best of my knowledge, these are the earliest data that are yet available for the San Joaquin system.

Q Is there any way that that data can be reconciled with the staff's supposed unimpaired condition at Mossdale which frequently go over 900 or 1,000 TDS?

A In my opinion, the indicated qualities at Mossdale for natural conditions as represented in state water board Exhibit No. 3 are incorrect and overestimate the quality conditions; that is the total dissolved solids that would occur in the system under such conditions.

Q What are the general errors in the staff's methodology which have resulted in these inexplicably high TDS numbers attributable to natural flow conditions in staff Exhibit 3?

A I think they are a consequence of misapplying the model, and in particular, the boundary conditions that were imposed on the model at Vernalis.

Q Okay. Was the flow-salinity relationship used throughout by the staff developed from data from the 1967 to '73 period?

A That's my understanding.

Q Can you explain why the flow salinity relationship developed for the 1967 to 1973 period would not be valid by applying it to the earlier pre-project period?

A Well, there have certainly been significant changes in the level of development in the watershed upstream, in the amount of return irrigation flow and salts that accrete to the stream, and consequently, I would find it very difficult to utilize a representation of a flow-quality relationship for a period in which those projects and developments had not yet occurred.

Q Okay. Have you subsequent to reviewing that recalculated what is, in your opinion, a proper flow salinity relationship to apply to the pre-project period?

A I have, using the same procedure that was used to develop the flow-quality relationship interpreted additional data which I think more correctly represent conditions that would be corresponding to the natural circumstance.

Q Maybe it would be quicker at this point for you to go through your exhibits and explain what you did starting with whichever one.

A First of all, I would like to call your attention to the fact that there are very little water quality data. The reference made to the 1906 to 1908 as a source of data are the only significant data sources I am aware of prior to about 1928 or '29. The only other data that are significant for this particular location are the chloride four-day grab sample data that were collected throughout the Delta during the period 1929 to 1973. These cover a period from 1930 to 1973, but they cover a period prior to the development of the major projects upstream such as that of the Central Valley Project and the Friant Reservoir, so they might be taken then to represent conditions without major project development upstream, and I have utilized those data as a basis for my testimony today.

I would like to show, first, to remind you of these data the array of information that is available from the chloride data covering this period, and I will utilize the information available for the period prior to about 1944 when the Friant Reservoir went on line.

MR. WHITRIDGE: Just for the record, this is South Delta Water Agency Exhibit 37 you are showing now?

A Yes, it is already in the record.

Now, I have taken the data from this record and plotted it on a new exhibit which has not yet been introduced. This is Exhibit South Delta Water Agency 122, and what it shows is the relationship between chlorides at Mossdale; that is, the location of the this grab sampling station, and the runoff at Vernalis downstream from the San Joaquin River represented here in thousands of acre-feet.

The data shown are of two types; one for the period prior to 1941, in this case, excluding the period of the war years, and one corresponding to the period 1955 to 1964 well after the advent of upstream development by the Central Valley Project. This is only, of course, by virtue of reference to these two periods, distinctly different by the changes in the hydrology in the system and in the storage capacity fitted in this case by eye are two curves which represent in general the trends of these data to show as the flow decreases, the quality generally increases; that is to say, the TDS concentration or salinity increases. However, it will be noted that this increase reaches a plateau which is perfectly logical in consideration of very low flows being derived from groundwater primarily, and from irrigation drainage returns, which are not expected to increase indefinitely as the flow diminishes to zero.

So, essentially, we are draining groundwater at a more or less constant quality into the system when the flows are very low, so both curves tend to flatten as one approaches a very much lower flow regime than might have existed in a larger portion of the time.

Also noted is that there are two curves, and the difference between these two represents a change taking place over time so it is not appropriate to use the same curve or the same sets of data for periods that are different from the point of view of storage projects and the impacts of those projects on downstream water resources.

So, what I have done is taken the data for the period prior to 1944, the chloride data, and using a relationship that we have already discussed --

Q If I could go back just one minute, that shows, does it not, that the difference between the 1931 to '41 period, and the 1955 to '64 period is specifically for very low flows in the later period the salinity is higher; is that correct?

A Substantially higher.

Q And at high flow similarly the salinity is higher?

A There is some difference, although it is a little less easy to define, and there are some variations there that need explaining if one was to look at what are conditions.

Taking these data and utilizing a relationship that we have discussed before, this is South Delta Water Agency

Exhibit 39 already in evidence, that shows the relationship between the TDS at Vernalis and chlorides at Mossdale for which a regression curve is presented in which TDS is equal to ten times the chlorides to the 0.77 power. Thousand curve, incidentally, was presented as a result of my analysis as a best fit of the data. The data were reviewed again by the Bureau of Reclamation and the general form of this equation was confirmed.

So, I think this is well documented in the record at the present time. So, these data represent now TDS at Vernalis for the period prior to 1944. If we take these data and plot them in the same form as had been presented previously in connection with flow-quality relationships, and from which we can derive such a regression equation --

Q This is South Delta Water Agency Exhibit 123?

A 123 -- we find an array of data that generally follow, if one can imagine a straight line inclining from the lower right to the upper left through the data, and if we follow the same procedure as was used in developing the regression equation on page 19 of State Water Resources Control Board Exhibit 3, we obtain a regression line which has a slope defined by these data. That regression equation has the equation TDS equals 19964.735x Q to the -.4385, and has a correlation coefficient of .82 or an R squared value in the order of about .65.

Q That was done using a regression analysis?

A That was done in this case using a regression analysis for those data that are shown here as small crosses on this plot.

Now, in line with my earlier argument that there's a practical limit, let's say, to the extension of this curve toward the origin, to the left that is, we would not want to use that equation for the data that do not conform to the flow-quality relationship indicated by the general trend.

And I have decided on the basis of the information available that it would be appropriate to use another relationship when the flow at Vernalis is less than about 35,000 acre-feet per month. So these data below 35,000 acre-feet per month belong really to another regime dominated primarily by groundwater accretions to the system and there is other evidence to show that there is an appropriate way to deal with such information included in exhibits, for example, by the Department of Water Resources and others.

Q Dr. Orlob, this addresses the problem; does it not, that we discussed with the staff on cross-examination that their formula did not have a cap on it and, therefore, as flows approach zero, the TDS values approach infinity?

A That is correct, and that's what one would get if you put a very small Q in the equation, you would find that the total dissolved slots would be enormously high. So, for very low flows, you get abnormally high TDS values and these, I believe are incorrect and represent in this case an incorrect application of that kind of regression equation.

I can illustrate this in SDWA Exhibit 124 by comparing the two regression equations, one derived, in fact, from information developed for a period subsequent to 1967, I believe that's the correct period, from 1967 to 1973, and which should be applied, as I understand it to post-project conditions; that is, post-Central Valley Project, or at least to that period in which the data were developed, and the other developed for the period prior to 1944 from the Mossdale data.

These two regression lines are shown for purposes of comparison. Quite obviously, there was a big difference between the two and if one particularly were to extend the regression equation on page 19 of State Water Resources Control Board No. 3 toward the left, at very low flows we would obtain very high values of the indicated TDS, and I think this is, in fact, what is indicated in the application of this equation without limiting it by another relationship which should apply at very low flows.

So, in my case, I have taken the regression equation, truncated it at a level of 35,000 acre-feet per month and applied a constant value from there on out. That is to say, a value which turns out to be about 415 TDS. This is a value that's consistent for very low flows with actual observations for such examples as that of 1908 from the data previously presented.

You will note that the equations are quite different, although they have similar slopes and the regression fits are comparable in both cases. That's not to say these are the best possible relationships that one could develop, but nevertheless, they are comparable in the sense that the use the same technology, and I used subsequently in my case to illustrate the differences that would result in the South Delta if one were to use what I think is the more correct relationship.

Q What is the difference generally in magnitude of the results of the --

A Well, this, of course, depends on what level of flow one is to consider, but consider, for example, the difference at a level of 10,000 acre-feet per month, which is not unusual, there were three or four months in 1977, for example, in which flows were less than his, the regression equation used in the state board exhibit would indicate something in excess of a thousand milligrams per liter. In fact, there have never been in the period of "records" at Vernalis concentrations of TDS in excess of this value or even as high as that value.

On the other hand, the equations that I would use would predict about 415 or thereabouts as the likely value for quality at Vernalis corresponding to natural conditions before the advent of major project development upstream. So there's a large difference of about two and a half times roughly in that particular example.

On the other hand, for very wet weather conditions, high runoff, probably there wouldn't be a significant and importance difference as we are really interested in low flows.

Q Does Exhibit 125 portray this difference for the year 1977?

A Now, taking these two equations and making a comparison, I return to the state board Exhibit No. 3, and utilizing the data for model simulations, at Vernalis as attributed to the Fischer model and also the boundary conditons that are represented in the regression equation that the state board has utilized, we find the upper of these two curves and a set of data points, the small crosses, that correspond to the Fischer model simulations, indicating the total dissolved solids that would be predicted with this set of boundary conditions for the station at Vernalis.

The slight difference between the boundary condition and the prediction of the Fischer model is attributed to the way in which the model treats the return salts, to the best of my knowledge. But, nevertheless, one would expect that this set of boundary conditions would be reflected directly in the prediction of the Fischer model because it is close to the boundary.

On the other hand, if we were to use the boundary conditions that I propose, which would be more realistic for pre-project conditions, one obtains the lower of these two curves, quite different in terms of maximum values that would obtain in the vicinity of Vernalis, obviously truncated in the periods of very low flow by a limit on the quality that would result there which is primarily irrigation drainage return or would be, and groundwater accretion to the system.

Q So, just to summarize, South Delta Agency Exhibit 125 shows the natural flow conditions which would prevail in water year 1977 conditions under both the board's erroneous flow-salinity relationship and the lower one is the one you have used?

A That is correct.

MR. MAUGHAN: What is SJR?

A That's San Joaquin River , and there is one and a two which represents the two cases.

MR. MAUGHAN: But it is not explained on the chart itself.

A I'm sorry, I apologize for that.

MR. WHITRIDGE: SJR1 utilizes the flow-salinity relationship that you have developed and SJR2 utilizes the one the staff utilized?

A I think it is the other way around, yes.

MR. MAUGHAN: All right, it's in the record.

A Now, of course, this is at the boundary and one really would expect to find predictions with the models, any model, to agree pretty well with boundary conditions because they are, by definition, what the model responds to as a input, and so, we have used these boundary conditions with our model, that is the Link Node model previous described as a tool to be utilized in simulation of the Delta system, it has been used by the Department of Water Resources and by the South Delta Water Agency, and we use the version of that model that has currently been redeveloped for purposes of the joint studies between the Department of Water Resources and the Bureau of Reclamation and the South Delta Water Agency as a vehicle for this analysis.

And we have simulated then for the same conditions in the Delta that we utilized in Exhibit 3 of the state water board to simulate qualities throughout the Delta, we have used this same modeling approach with the boundary conditions at the Sacramento River and other tributary streams identical with the one exception that in one case we changed the boundary condition for Vernalis to that which we have proposed as a more realistic representation of natural conditions.

In the first instance, we see a curve generated in this case by the Link Node model and its counterpart or its complement in water quality, the dark line that is shown here, and along with that the result of the Fischer model, and this incidentally is at a station near Clifton Court, and so, we are perhaps comparing them with a small displacement, but more or less the same locations in both models, but this is farther into the interior of the Delta and the point, of course, of showing this exhibit is to show that the boundary conditions do extend beyond the boundary and well into the Delta and affect results throughout the Delta area.

In this case, the two models agree fairly well, I would say, although they have certain differences that appear here. The Fischer model, for example, shows a rather gradual smearing of these data, but it follows pretty closely the dark curve. We have two peaks here that show in the Link Node model as prominent, not appearing in the Fischer model results.

This, of course, is for the boundary condition used by the state board in its analysis using the Fischer model.

The other result, resulting from the application of the boundary conditions that we think are more appropriate and represent natural circumstances, and that's shown by the lower of these two curves, and one can see, for example, in the irrigation season, these are 1977 hydrologic conditions in all other respects, although they apparently have no projects involved, we find a difference of about two to one in the relationship for that critical irrigation period between the two sets of results.

So, obviously, there's a great difference resulting in this case from an application of what we believe to be an incorrect boundary condition at Vernalis.

Q So, Exhibit 126, to summarize, is basically the same exercise as 125 except that it is at a different location and shows the difference by using the board's formula at a different location farther downstream in Clifton Court.

A This is an identical simulation at another location that results from the model output.

Q One or two final questions -- all of staff's errata for Exhibit 3, including the June errata and the November errata, contain these same basic errors that we have discussed?

A I don't think there were any changes in the boundary conditions that were applied.

MR. WHITRIDGE: Okay, that's all I have.

MR. MAUGHAN: All right. Who would like to cross-examine? Staff?

MS. LEIDIGH: Staff has some questions. I think we want to start with Mr. Farro.

E X A M I N A T I O N

by MR. FARRO:

Q Dr. Orlob, are you aware the state board staff was modeling pre-project conditions or any other condition -- can you explain what condition the state board staff was trying to model?

A As near as I can understand, that was to be a so-called natural condition, which meant without alteration, without the existing projects upstream and their accretions to the system.

Q Okay, is that comparable to the pre-project condition data that you used?

A Well, I would say as close as one might come. There would be some differences in the period from the thirties because there were some small projects on line at that time and they could, of course, modify the flow regime, but they are relatively small compared to the present system.

Q Are you aware of the present-day level of development that the staff tried to depict in the use of the equation?

A As I understand it, they were trying to depict conditions pre -- well, let's say natural conditions again, without project development and the level of development that considered, for example, no Central Valley Project with no cross-channels existing as they presently are, irrigation and activity within the Delta as it presently is, or virtually equivalent to that.

Q Well, would you like me to explain the staff position on that? I guess if you read the definition of natural flow or unimpaired flow, it was based on the present level of development and the condition was if there was an interruption in diversion for the agriculture at any time for the period of time, the flow would be equivalent to unimpaired or natural flow, so the level of development was involved with the present level of development with agriculture in place and for the period of time that no diversion was made to agriculture at that time. That's what this staff used, the post-project equation from the Department of Water Resources that we showed on page 19, Exhibit 3.

A My understanding was that was presented as a pre-project equation in the first place.

Q We used pre-project hydrology, we used 57 years of hydrology, but we used the present level of development and the unimpaired flows represent the unimpaired for 57 years of hydrology.

A I can understand the concept of unimpaired flow and I would use an unimpaired quality to go along with that.

MR. FARRO: I guess I have no further questions.

MR. MAUGHAN: Anyone else?

MS. LEIDIGH: No.

MR. MAUGHAN: I still think there's some confusion here between the witness and the staff. I don't know how to help them, so if that's all --

MR. WHITRIDGE: I have one question on redirect that might clear that up.

MR. MAUGHAN: All right.

REDIRECT EXAMINATION

by MR. WHITRIDGE:

Q Dr. Orlob, staff Exhibit 3 at page 18 states: The San Joaquin River equation was assumed to be adequate for the purposes of this study since it was developed for pre-1944 or pre-State Water Project and Central Valley Project conditions. Do you believe that statement to be true since the formula they used was developed by using the 1967 to 1973 data?

A I can't see how those two conditions are reconciled.

MR. WHITRIDGE: Okay.

MR. MAUGHAN: Well, I appreciate there's a difference of opinion, but I think there is still some confusion. I don't think there is a clear understanding on both sides.

All right, is there anything else then before we -- do you want to introduce those?

MR. WHITRIDGE: I would like to offer 122 through 126 in evidence as South Delta Water Agency exhibits.

MR. MAUGHAN: Are there objections? Hearing none, they will be accepted. That concludes that particular phase.

(South Delta Water Agency Exhibits 122 through 126 were received in evidence.)

MR. MAUGHAN: We have one more, Mr. Anderson and Mr. Brown. All right, Mr. Anderson.

MR. ANDERSON: We have Dr. Brown here. He is here to offer rebuttal testimony regarding the evidence that was given by the Contra Costa Water Agency and Environmental Defense Fund, particularly Exhibit No. 4, regarding proposed standards for South San Francisco Bay.

I would note for the record that Dr. Brown has appeared before, has been sworn, and his qualifications are a part of the record.

In addition to Dr. Brown's testimony, we will also be offering DWR Exhibits 674 through 683, in rebuttal, which Dr. Brown will explain.

RANDALL BROWN,

having been sworn, testified as follows:

DIRECT EXAMINATION

by MR. ANDERSON:

Q Dr. Brown, do you have any biological concerns related to CCWA-EDF Exhibit 4 which proposed standards to provide for stratification and algae growth in South San Francisco Bay?

A Yes, I do. I think, perhaps the exhibit doesn't give enough information to allow the board to make a decision whether such a standard is reasonable or not, or provides reasonable limits of protection for South Bay biota. I think the authors of CCWA-EDF No. 4 have taken a proposed scientific explanation for an observed process and used that to develop a recommended standard, and I think the board needs more information than that exhibit provides to do such a thing.

I would draw mainly from the U. S. Geological Survey data set because basically they have the data in South Bay, although we have a little bit here and there, but they have data on the benthic diversity, benthic organisms, and the productivity in the system.

The major point I would like to leave you with, I guess you would call it additional information is that the theory about benthic grazing controlling the biomass in South Bay is still a theory. I am not saying I don't believe that theory, I think there is a lot of support for it, and the few field observations that we have taken so far certainly support that in certain parts of South Bay especially the main channel.

In the shallows of South Bay, however, as you can see in DWR Exhibit 75, which is a map of South Bay, South Bay basically is a pretty shallow portion of the estuary and the shallows do not stratify to the extent that the channel does.

If you recall the proposal by U. S. Geological Survey explained by Dr. Cloern and Dr. Hollibaugh, it is that when you get the spring pulses, the estuary stratifies, South Bay stratifies, and in this case I am talking about South Bay as the entire area south of the Bay Bridge. There are some definitions about South Bay but this is the one I am using here mainly.

The shallows, when it stratifies, the algae are physically isolated from the bottom and you get a bloom on the surface, and this bloom is not grazed upon by the benthos, so it develops a pretty high level of algae, and the problem with that theory -- and this is also mentioned by Dr. Cloern, his explanation is that this doesn't really work in the shallows as well because the shallows do not stratify as neatly. The wind and so forth breaks down the stratification, but actually, you get a bloom there in the shallows.

If you look at DWR 676, which is from the USGS report, you find two curves there; one with the solid circles and one with the open circles. As you can see, the solid circles, which is a channel stations, the bloom is higher, much higher than the shallows, although there was a bloom in the shallows at the same time.

The question is, how does it bloom in the shallows since the benthic organisms there are not isolated from the algae, how do they accumulate? We tried to answer that question, at least provide some answers to that question last spring by a combined study in South Bay with the USGS, DWR and the bureau out there. Unfortunately, for this experiment, the flow didn't cooperate and we didn't get enough flow to understand how the shallows and the channel exchanged.

We will try again next spring and hopefully we will have more flow to actually create a bloom and see how the algae move from the channel to the shallows, or if the bloom originates in the shallows themselves.

In addition to the phytoplankton in South Bay, there is an extensive layer of algae on the surface of the mud. South Bay is shallow so you get pretty good light penetration, so there is a layer of microalgae on the mud itself.

The clam, macoma Balthica, is a deposit feeder. It basically is in the mud at a certain depth, it siphons, comes up and can actually graze upon the algae grown on the mud.

We don't know exactly how the plankton in this microlayer on the surface of the mud reacts, but we haven't quantified that very well. We don't know what controls. In fact, I think there's a statement true in all this, that we don't really understand a lot of things in South Bay or the bay itself and how blooms are started and decline.

Now, I think a major point made by CCWA is that the bloom in South Bay is mainly -- well, if a bloom occurs, it contributes a major part of the total productivity of the system during that year, and it is certainly true in the channel, maybe 30 to 40 percent of the total algae production for phytoplankton occurs in the bloom period.

In the shallows, however, it's a much lower percentage, maybe 15 to 20 percent, not much more than what would occur just for this one month out of the year.

So, if you look at some data, and unfortunately, we don't have the data set, the data set is still in the USGS computer, but DWR 677 does show some algae levels taken in South Bay last spring and these are micrograms per liter of chlorophyll. The background probably is in the order of two to three micrograms and you will see in stations 41 through 48 we did get a bloom down in South Bay last spring. The bloom occurred mainly below San Mateo Bridge and the bloom occurred mainly in the shallows from San Mateo to Dumbarton Bridge.

Now, I don't know what happened before and after this bloom period, whether this was the peak, the start of whatever. The data set will be out, I'm assured, shortly by the Geological Survey, so we can analyze these data, but we do get a bloom and Dr. Cloern pointed out he can predict phytoplankton growth in South Bay by a comination of knowledge of outflow as well as temperature and sunlight.

The system responds to climate as well to these outflows. So, it is not unexpected that you get a bloom down there.

MR. MAUGHAN: And the outflow last year wasn't very high?

A Look at Exhibit 678, and the outflow averaged about 6,600 cubic feet per second. It was not very high for

April.

I think on the next part, how do you get from the phytoplankton to the benthos, and I think CCWA No. 4 was somewhat misleading and I think the misleading picture I got, the text, it was an overgrazed pasture, that basically the system is being grazed by these benthic organisms and they are overgrazing the system, and you need more algae for the benthos.

I think perhaps a better analogy to this would be this is a very rich pasture and that the signs of eutrophication, which would be dissolved oxygen problems, or too much algae, are being kept under control by the benthic grazers, that this is really to our advantage.

I think the following points may help explain that a little better.

South Bay receives a major portion of the discharge going into the estuary, and this discharge is high in nutrients. Increased treatment over the past several years have not done a whole lot to take out nutrients, so nitrogen and phosphorous are being discharged in large quantities to South Bay. In fact, the treatment of South Bay was to go away from ammonia discharge, which is toxic, to nitrate discharge, which is non-toxic, but it doesn't change the algae. They don't care whether it is nitrate or ammonia, so you can see in DWR 679, in this case, we have lower bay is the area around San Mateo Bridge and South Bay is the area below Dumbarton Bridge, and as you can see for all years both phosphates and nitrates are quite high in South Bay compared to the rest of the system.

So, there are ample nutrients, and the question we have often had by people looking at the system is why isn't South Bay greener than it is. That's been kind of a puzzle for us because it's got lots of nutrients and light is reasonably good down there, and you would expect based on nutrients and light that the concentration of chlorophyll would be higher than the basin plan guidance standard of 25 micrograms per liter routinely.

Now, is the guidance standard, basically if we get concentrations higher than that, it's time to start looking to see if it is causing problems.

MR. MAUGHAN: Randy, from '81 on to '85, there is really a big jump in both phosphates and nitrogen in South Bay.

A Yes. Some of that could have happened because of nitriphication -- I'm not sure what happened. If there was loss of ammonia somewhere in the system, I don't know, through increased discharge or -- I'm not sure what it is. There has been a measurable increase.

MR. MAUGHAN: Double almost.

A And those kinds of levels support a very large growth of algae. It would be at nuisance levels probably.

Now, in this analogy about overgrazed pasture, I don't think it is quite an accurate portrayal in that at least we don't have a lot of data, but 1980, which was a high flow year, in the shallows of South Bay there was always more algae produced than was being consumed by zooplankton or benthic grazers. So, in the shallows it was not overgrazed to the extent there was a negative production. There was always a positive production.

In the channel, however, because the channel is deeper and the algae can only grow in the light, there was not a net production all year round, only in the months of, I think it was May, and August and September, did you have net increase in growth in the channel.

Okay, the question is, is South Bay a pauper in benthos or is it enriched, and if you look at DWR 680, you will find that in general, South Bay is pretty high in benthic biomass. Currently in our system you will find it is in general the highest area. This is probably because benthos in our system is mainly controlled, it appears, by a combination of salinity and organic content of the sediments. Higher salinities, high organic sediments generally mean in our system more benthos. Salinity means stability normally, so South Bay is a stable system and the benthos can develop. Organic content means a rich system and benthos can grow. So, South Bay is rich in benthos.

Now, Exhibit 681, which is a complicated table, but look at the last line on that and compare South Bay productivity, which is net production, you see South Bay is higher than the rest of our system. So, in terms of San Francisco Bay, South Bay is probably the richest of embayments in terms of primary phytoplankton production.

Now, the question that was raised by Dr. Hollibaugh, I believe, is what level of production is necessary to support some higher trophic levels, in this case, fish through the benthic food chain or zooplankton food chain, and he used a figure from Dr. Nixon, which I reproduced DWR 682 to show that more production means more fish and that probably would not be harmful to South Bay, might even be beneficial to have more production. I think you have to look at 682 fairly carefully. You notice it's a log-log plot, so it kind of obscures a lot of the scatter in the data.

A lot of the systems that are richer than our bay are rather unique estuaries or lagoons in very shallow different places in the world, but even with our system, and this is primary production it says on whatever that axis on the bottom is, I guess the X axis, that we are about 100 to 200, which would be 10 squared higher in algae, so we are over towards the middle of the curve.

A lot of these systems have not only algae in them, but microalgae on the surface of the muds, as well as what they called the other day submerged aquatic vegetation, SAV, and here we are pretty productive with just the phytoplankton themselves.

I did look at a couple and I didn't have time to get the references, but I found a couple of -- one of them is Narragansett Bay on the East Coast. We are about the level they are, it looks like, in South Bay.

Dr. Nixon, in another paper which I have a reference to if you are interested, found that system to be what he called moderate to highly productive.

So, in terms of an estuary, South Bay is pretty rich comparatively.

Okay. I think the important point here is that South Bay is productive, and we really have a lot of trouble taking the data we have in South Bay and extrapolating to other systems in what basically is needed to optimize production.

With regard to macoma Balthica itself, I think a few things are in order here as to a little more explanation. Macoma Balthica was a very happy clam until the GS data came along. It was out there in the mud flat doing its thing by itself and now it's had a lot of attention. It is a common clam in the bay and it is indigenous. It was found here in the system. There is some idea that the one we have here now isn't the one they used to have, that an introduction has been made that may have changed the kind of clam from different areas of the world, we may have imported more clams.

Macoma is found around the Northern hemisphere in a lot of different oceans and bay. Our bay is at the southern end of the range, so this is about as far south as it goes before temperature is thought to be a major problem for it.

A clam reaches sizes of about 20 to 30 millimeters, and a maximum age of about two to three years in our system. Numerically it is not dominant in the South Bay, but because of its fairly large size, even at 20 millimeters it is a fairly important food source for some fish, and as I said, macoma Balthica is generally a deposit feeder, but it may feed on phytoplankton, but I'm not sure to what extent.

If you look at DWR 683, this is a comparison of growth rates of macoma Balthica in our estuary, in South Bay actually, with other places in the world where it is found. As you see, it is food limited. It is doing a pretty good job of getting around that because it has a very high growth rate.

To paraphrase Sam Luoma, who is a person who has done a lot of work on this particular clam, it lives fast and dies young. It does not reach the old age that similar clams do in other places in the world.

MR. MAUGHAN: It says 25 years.

A That's in colder estuaries. In our estuary it doesn't make it past about two years. It probably has something to do with the temperature as the temperature is probably too warm for it in the summertime, and growth pretty much stops in the summertime.

Okay, I guess a couple of final points is I think the report that was cited by the CCWA in their No. 4, their report that is unpublished as yet but at the press, by Thompson and Nichols, does't clearly establish, I don't think, the relationship between even the spring pulse and the phytoplankton and clam growth. In particular, this was a two-year study, 1983 and 1984, so it is a fairly short data base. They have two locations they studied, both -- four locations all together, two of which were in South Bay, and both of these locations in South Bay were high on the tide flats, there were on the tide flats, they were locations where they could walk out at low tide and pick up the clams and monitor clam growth.

The phytoplankton data from these clam sites came from the channel, so we aren't sure exactly what happened over the flats at the same time. So, I think the data base is pretty short.

The two sites had vastly different phytoplankton levels in the clam. One had about twice the other. However, the growth of clams in both sites was about the same, so it doesn't appear from this small data set that the more plankton growth, the more clam growth, because he didn't get a difference even though in one case it was two times the other.

The two-times level was three times the basin plan chlorophyll warning level or level of interest.

Another point about South Bay, I think, is that phytoplankton production doesn't seem to translate to zooplankton production. They mentioned that in their testimony, that this was probably a general case that more algae would mean more zooplankton. In the 1985 report edited by Cloern and Nichols, they had a paper there on zooplankton, and in three and a half years of study they didn't find a relationship between outflow and zooplankton abundance or chlorophyll and zooplankton abundance. Basically, it seemed to be marine zooplanktors that did not respond to conditions such as salinity, or responded to salinity by a change in species composition, but the general abundance is the same in three and a half years of study.

In summary, I think South Bay is a very productive system and I'm not sure that we are at the point now in South Bay of developing a standard to increase phytoplankton for a short time in the spring to get more or less productivity.

I suspect, in fact, that if you were to develop a standard for phytoplankton in the South Bay, you would not do it for April. It looks to me from examining the data from Nichols and Thompson, or Thompson and Nichols, that really, the growth of this macoma starts out in march and to be of use to the clam the pulses would have to be in March, and I think pulses in March are fairly well unregulated at this time, so probably in wet years you get pulses and in dry years you won't get them.

I think the period is wrong, too. The clam spawns in April. It has to have had a fair amount of growth before it starts -- you know, the food resource has to be available earlier, I guess is what I am saying.

Okay, I read CCWA report on benthos in Suisun Bay, the entrapment zone in Suisun Bay and the entrapment zone in San Pablo Bay, and I think I will leave my comments on that to Doug Ball with the Bureau of Reclamation, who did a thorough job of reviewing that. I did not review them to the extent necessary to really talk about them, but I do have some reservations about some of the things in there, too.

Q Thank you, Dr. Brown; is that your testimony?

A Yes, it is.

MR. MAUGHAN: All right. Mr. Nakagawa might be the only one -- who else would like to cross-examine, and a few questions from staff after Mr. Nakagawa asks his single question.

CROSS-EXAMINATION

by MR. NAKAGAWA:

Q Dr. Brown, I want to understand exactly the thrust of your testimony. Are you stating, in effect, as a general conclusion that stratification is not needed for phytoplankton production in South Bay in the channels?

A At this time, I think it would be premature to reach that conclusion.

Q Referring to CCWA-EDF Exhibit 10 entitled "Temporal Dynamics of Estuarine Phytoplankton, a Case Study of San Francisco Bay," written by James E. Cloern, and others, which has been admitted in evidence, let me read you a portion of this report at page 161: In summary, the phytoplankton community of South Bay turns over quickly, especially over the shoals and during summer-fall, and they may be consumed at an equal rate by benthos when the water column mixes.

Zooplankton grazing is an important process in the channel but not in the shoals where depths and degraded zooplankton biomass is small.

During the brief periods of stratification that occur in spring, phytoplankton biomass increases rapidly in the surface layer and a bloom occurs.

Do you disagree with that statement?

A No.

Q Let's turn to your Exhibit 677 that you just introduced, and can you tell me where stations 41 through 48 are located?

A Well, I think I better read this into the record: 41 is at San Mateo Bridge, about the center of the bridge; 42, 43, and 44 are on the shallows on the eastern side of the main channel; 48, 47, 46 and 45 are in the main channel.

Q 45 through 48 stations are in the channel below San Mateo Bridge?

A Yes.

MR. MAUGHAN: Here you are going further south?

A Yes.

MR. NAKAGAWA: Q Did you say that you did an independent analysis of the conditions in South Bay below San Mateo Bridge to ascertain the existence or lack thereof of stratification during the periods in which this bloom or blooms occurred?

A No, I didn't. We have a station at San Mateo Bridge which I could examine, but I didn't do that. I was looking mainly at the proposition that inflow from the Delta was a controlling factor and at 6,600 cubic feet per second average flow you wouldn't get stratification.

I did look at the data record from San Mateo Bridge this last year, but there was not a major stratification, but there could have been some. I don't know.

Q All right, let's turn to a notion that you expressed that there isn't enough data set in CCWA-EDF No. 4 to set a standard. What, exactly, did you mean by that?

A Well, the question is, what level of phytoplankton is necessary to support benthic populations. I guess the use that we are talking about here is -- I am not sure what the use is, but I assume in most cases it would tend to be towards fish. The question is what level of phytoplankton is necessary to support benthos or zooplankton which would result in increased fish production, and I don't think we have any of that data at this time.

Q So, it isn't your testimony that phytoplankton abundance isn't needed for South Bay for purposes of the fishery, or whatever other beneficial uses that might be made of phytoplankton that does occur?

A No, phytoplankton is probably at least one-half, maybe one-half of the total primary production in South Bay. The other half is submerged aquatic vegetation and microalgae on the bottom, so it is necessary to support populations. The question is what levels are necessary to support populations.

Q But you do agree in the same breath that, in fact, phytoplankton abundance procued by stratification is a desirable objective?

A No, I wouldn't right now agree with that. I'm not sure in what way it would be desirable.

Q Then, are you in disagreement with the conditions at least described by Dr. Cloern again in CCWA-EDF Exhibit No. 10 about phytoplankton blooms occurring due to stratification in South Bay?

A No, I guess I am not, I guess the question of desirability is the question, how much and at what time is that needed. Now, you are talking about a one-month bloom in April. Somehow the critters in South Bay have to survive for 11 more months, and if 30 percent of the total productivity is in that time, short time frame, I'm not sure what the animals are doing the rest of the year for a food source.

So, even though levels of algae in the water column may be low in South Bay the rest of the year, which is probably a good thing from a lot of standpoints, it doesn't say that the productivity is not getting to the clam because they are eating there all the time.

Q Didn't you also state that there is phytoplankton production in the shallows, the processes of which are not entirely understood, but that that is a food source for those benthic feeders there in the shallows?

A Yes, it is.

Q So, to the extent that while there may be an appearance of a disagreement over what it takes to provide phytoplankton abundance, your testimony didn't go beyond that and say that flows ought to be cut off from South Bay in order to eliminate stratification and, therefore, eliminate that portion of the phytoplankton population that is produced by stratification?

A No, but I do think, however, that the critical period probably is not April, it is a March period. I think flood control reservation and so forth would probably dictate there will be flows in March and that bloom, if the size of the macoma Balthica is an important animal that must be supported by the spring bloom, April may be the wrong month to do it.

Q Is it your testimony that macoma Balthica is the only consumer of phytoplankton in South Bay that is important to the maintenance of the fishery?

A No. I am not really sure how important macoma is to the fishery in South Bay. I know clams have been found in fish stomachs.

Q Can we agree that phytoplankton is an important source of food for mysids and other food sources for the fishery in South Bay?

A Probably not for mysids directly, but indirectly through the food web there would be. Mysids probably feed on zooplankton and zooplankton don't seem to be controlled by phytoplankton levels in South Bay, at least in the first three and a half years of the study.

Q Is that study performed by you?

A No, by the USGS.

Q All right. And is that reported specifically as not showing a direct connection for phytoplankton production and zooplankton abundance?

A It shows that in three and a half years varying levels of phytoplankton in varying water years, that there was no observed change in zooplankton levels, which is an indication that there is no relationship. It is a pretty short data base, again, but it is all we have in South Bay.

Q Looking at it from the other side, it doesn't seem to show that zooplankton does not consume phytoplankton.

A No, they do.

Q Now, given that fact, and if my memory serves me correctly, the Bay Area Dischargers Association exhibit in their biological study of South Bay found striped bass and others consume mysids in South Bay. So, to the extent that phytoplankton production in the channels encourages the growth of mysids making it available as a food source for striped bass, that's a good thing; isn't it?
A Well, I would disagree with you. I don't think there are that many mysids in South Bay and I don't think there are that many striped bass in South Bay at that stage . I think there are shrimp in South Bay. Crangon is important down there, but I don't think the neomysis shrimp is particularly important in South Bay.
Dr. Herrgesell's work shows that we don't find young bass down in South Bay except in very wet years like '83.
Q But the fish, including bass, are there?
A There are fish there. I don't think there are any striped bass there and not eating neomysis shrimp.
Q Now, your notion then that pulses might be good in March rather than April is limited, is it not, to your conclusion that the macoma Balthica apparently has a consumption rate and growth rate during that period in March that's related to phytoplankton abundance?
A Well, it's not really a conclusion, it's an

indication I get. If you look at the paper by Thompson and Nichols and look at the abundance of phytoplankton that occurred in '83 and 84, and the growth in macoma Balthica, there is a spring bloom in both years in March and about the same the growth spurt in macoma happened.

So, it could have been circumstantial or whatever, but the indications are that growth was triggered by an earlier bloom than an April bloom. That growth started to taper off toward mid-April, I believe, at those stations.

Q But, again, let's make sure when you talk about those pules in March, that statement earlier by you was related to your observation of the fact it is somehow related to growth of macoma.

A I am saying that there are pulses caused by floodwater flow, that macoma may respond to those pulses, I said I believe the stations are in intertidal areas, we are not exactly sure what the channel pulses have to do in the intertidal stations.

Q Turning to your Exhibit 680, was it your testimony that this exhibit shows that benthic biomass is so significant in South Bay that, in fact, there is more than adequate or a substantial population of benthos in South Bay?

A No. My contention is this figure shows, very poorly probably, that South Bay has a greater benthic biomass in the stations that they examined, a lot of them, than the rest of the bay does and that the reason why this is, is probably because of the stability, the high salinity of the water, and the fact it is the richest part of the system in terms of annual phytoplankton productivity. And this is an indication. There are more data, I believe, that will bear this out.

The stuff in the sixties done by Searle and by UC Berkeley show basically the same thing, that South Bay had a pretty high biomass of benthos.

Q Looking at it from the other side, it is not your testimony that it isn't desirable to increase the benthic biomass in South Bay?

A Well, I guess more could be desirable.

Q Now, in my attempt to understand a portion of your testimony that came in at the very tail end by you, Dr. Brown, concerning DWR 682, could you tell me again what it is you said about 682?

A I said, first of all, 682 is a compilation of a lot of data from around the world that has been plotted on a log-log scale.

Log-log scales tend to mask the scatter of the data and that without trying to go back to each system and getting the references and trying to see which of these citations include microalgae on the surface of the mud, the submerged aquatic vegetation, phytoplankton, these components, that it is hard to tell from this figure what the figure actually tends to show about the relation between productivity and fisheries yield in relation to our bay, in South Bay in particular now.

Our net production is about 100 to 120 for phytoplankton alone. If you add in other components to the productivity of South Bay, which would be the microalgae on the surface of the mud and the submerged aquatic plants, you bring it over to probably -- and this is all guess, but in systems I have looked at the microalgae would be one-half. I looked at one system on the East Coast, Narragansett Bay and phytoplankton production in our system was about the same as Nixon reported in that bay, and he called that a moderately rich estuary.

One of the higher levels over here was a bay in that report, Rotaria Bay which has an average depth of about three feet, two to six meters is the depth of that lagoon. It is very rich, higher than ours, but it is an entirely different system, so I think the problem with a plot like this is you really can't tell whether it has any relationship to what you are doing in your estuary until you look at it in more detail, and with the kind of plot here you can't do that very well.

Q Well, is it your testimony that you deny that there is a relationship between primary productivity and fishery yield or fishery abundance?

A No, I think, in general, there is that relationship, but for any specific case, it may not hold exactly, especially if you talk about algae being primary producers. It could be a system driven by aquatic plants in a lot of cases in a shallow estuary. Certainly, the more algae, often the more fish. In California the best data we have is Clear Lake and the Salton Sea, lots of algae, best fishery in California, but you do suffer some side disadvantages from that. Salton has no oxygen below 15 feet and Clear Lake could be characterized as having bloom problem.

Q Well, forgetting about the lakes in California and Salton Sea, with respect to San Francisco Bay, do you have any additional data that indicate that there is a food source of greater significance in this system?

A No, I do think for the clam macoma, the microalgae on the surface of the mud should be an important source, I mean even Dr. Nichols can't really tell whether these animals are eating the algae on the mud or algae in the water column. They both could be important sources.

Q But in looking at it just from a percentage standpoint of the contribution of a food source for this estuarine system, do you agree that, in fact, phytoplankton is probably in terms of significance 50 percent or better as a food source within this system?

A Yes.

MR. NAKAGAWA: No further questions.

MR. MAUGHAN: Good. I was going to say one last time.

Earle Cummings, did you have a couple of questions?

E X A M I N A T I O N

by MR. CUMMINGS:

Q I have two. The first one relates to DWR 677 and 678. You show on 677 that there is a moderately high level of chlorophyll A in the lowermost stations of South San Francisco Bay. Is the water that presumably supported that bloom delivered in April or is that delivered at the end of March, because I notice on 678 you circled April, but on April 7 of 1987, presumably the water that supported that bloom came from earlier in the season.

A Well, I am not a hydrodynamicist. I'm not sure if any water -- I guess I couldn't speculate on whether water of those magnitudes, 20,000, 11,000 cfs, have any effect on the lower South Bay, so I'm not sure.

Q Do you know how long it takes water from Delta outflow to affect conditions in South San Francisco Bay?

A No, I don't, especially at these low flows.

Q Okay. I had one other question. I thought I heard you say benthic grazers might be preventing dissolved oxygen problems in the South Bay.

A I did say that.

Q How do they do that?

A Well, if the benthos is not grazing on the algae, the algae could accumulate and the bay could become eutrophic and the algae in the water column could actually cause a problem at the bottom. This is, you know, like I say, in the South Bay in the seventies if you look at the waste discharge in South Bay, the level of nutrients was so impressive and the level of algae were so low that it was a contradiction, and we were amazed there weren't more problems with algae.

Q Is that still puzzling?

A Well, no, I think Cloern's explanation in the channels especially, that benthic grazing probably has an effect on algae and keeps the crop down.

MR. CUMMINGS: All right.

MR. MAUGHAN: Anything further from anybody?

All right, Mr. Anderson, do you want to offer those?

MR. ANDERSON: Thank you, Mr. Chairman. We would like to thank you for accommodating our request this evening. I would like to offer DWR Exhibits 674 through

683.

MR. MAUGHAN: Any objection? Hearing none, they will be received.

(DWR Exhibits 674 through 683 were received in evidence.)

MR. MAUGHAN: Nine o'clock tomorrow morning.

(Evening recess)