

Final Notes October 27, 1998

IMPLEMENTATION TEAM MEETING NOTES

October 1, 1998, 9:00 a.m.-4 p.m.

NATIONAL MARINE FISHERIES SERVICE OFFICES
PORTLAND, OREGON

I. Greetings and Introductions.

The October 1, 1998 meeting of the Implementation Team, held at the National Marine Fisheries Service's offices in Portland, Oregon, was chaired by Brian Brown of NMFS and facilitated by Donna Silverberg. The agenda for the October 1 meeting and a list of attendees are attached as Enclosures A and B.

The following is a distillation (not a verbatim transcript) of items discussed at the meeting, together with actions taken on those items. Please note that some enclosures referenced in the body of the text may be too lengthy to attach; all enclosures referenced are available upon request from NMFS's Kathy Ceballos at 503/230-5420 or via E-mail at kathy.ceballos@noaa.gov.

I. Introductions and Review of Agenda.

Brown welcomed everyone to the meeting, led a round of introductions and a review of the agenda.

II. Updates.

a. In-Season Management. TMT chair Cindy Henriksen said the group has scheduled its first post-season review session for Monday, October 5; it will include the TMT's annual hydrologic review, a review of 1998 total dissolved gas data, an informational presentation on how fish passage indices are generated, and a presentation on an apparent relationship, observed in 1998, between increasing water temperatures and rising fish passage indices. We have also scheduled a review of the content and disposition of all of the system operational requests submitted to the TMT in 1998, as well as issues raised to the IT, she said. We will also schedule some follow-up meetings, to discuss procedural issues and ways the TMT process can be improved for next year, Henriksen said.

b. Plan for Analyzing and Testing Hypotheses (PATH). Brown introduced Dave Marmorek, explaining that Marmorek will be reviewing the recently-submitted Scientific Review Panel weight of evidence report. I was somewhat surprised at the level of controversy this report appears to be attracting, said Brown, because it seemed to me that it was simply the continuation of a process that began in the spring, when the results from the first prospective analysis on spring/summer chinook were made available. At that time, all of the various hypotheses were

weighted equally, and we began to discuss whether there might be a way to look at alternative weightings for the various hypotheses based on weight of evidence, Brown said. We initiated a process that resulted in a series of workshops held over the summer; the report that was then compiled from those workshops consisted basically of each individual participant's views on what the available evidence said in support of each hypothesis, he explained. That report was then submitted to the SRP for review; the science panel provided comments on the report as well as some suggested weightings. The science panel also provided some good comments on the process as a whole, said Brown, which in my view now need to go back to PATH for further discussion.

Marmorek said he would talk today about both the PATH weight of evidence process and the schedule for PATH. He distributed Enclosure C, copies of the Scientific Review Panel report "Conclusions and Recommendations from the PATH Weight of Evidence Workshop."

Marmorek went through a series of overheads, covering the background and purpose of the PATH weight of evidence process, the structure and membership of PATH, the weight of evidence tasks completed since March 1998 and a description of the PATH weight of evidence report. These overheads are attached as Enclosure D; please see this document for details of Marmorek's presentation.

Marmorek explained that the PATH weight of evidence report includes the following:

- ? Sensitivity analyses to determine key hypotheses

- ? An assessment of the evidence for and against key hypotheses – clarity, mechanism, consistency with empirical evidence, validity of method of projection

- ? 25 submissions from PATH participants, integrated into the main report and attached separately.

The report itself is a rather hefty document, Marmorek said; if anyone asks you what the weight of the weight of evidence report is, you can tell them 3.7 pounds.

In general, Marmorek said, the comments from both the independent scientists and the Scientific Review Panel were that the report is subjective, clear and well-organized, and is a fair summary of what was in the 25 submissions. He added that the actual weight of evidence report will be available for broader review in a week or so, via the BPA Internet site.

Marmorek continued on through his presentation, touching next on the objectives of the weight of evidence workshop; he made clear the fact that the SRP was not asked to evaluate alternative actions -- rather, they were asked to assess the relative likelihood of alternative hypotheses. Moving on, Marmorek touched on the relative weights on key uncertainties/hypotheses, the elicitation process and its results, the rationales for the assigned weightings, and results from the elicitation process. Among the latter:

- ? With regard to the passage/transportation models key uncertainty, Marmorek said, the reviewers tended to favor the simpler models with stronger empirical support for

underlying relationships; in general, they felt the CRiSP model was more complex, and had more mechanisms than the data could support. For this reason, they felt that some of the CRiSP results related to current operations were more optimistic than indicated by recent escapement information.

? With regard to the extra mortality hypotheses, the reviewers felt the evidence for regular, periodic cycles in the ocean and its effects on fish was weak, Marmorek said; they all agreed that there was considerable variability, but that it didn't show as strong a cycle as the one embodied in the regime shift hypothesis. Two of the SRP members felt that the hydro hypothesis was most consistent with the retrospective data in the analyses; two others felt that there was no evidence to distinguish between the hydro hypothesis (extra mortality is here to stay unless the four Lower Snake dams are removed) and the BKD hypothesis (extra mortality is here to stay even if the dams are removed).

? With regard to the life-cycle models key uncertainty, Marmorek said, the delta model assumes common year effects between upstream and downstream effects, whereas the alpha model keeps those regions separate, and does not assume common year effects. Three of the SRP members felt that the delta model had more empirical support, and fit the historical spawner/recruit and SAR data better than the alpha model, Marmorek said. One SRP member felt that the alpha model better captured differences in physiology between fish from the Snake River and the Lower Columbia.

Marmorek continued on through the other key uncertainties studied in the elicitation process (length of transition period, historic turbine/bypass mortality, predator removal effectiveness and equilibrated juvenile survival rate); in general, the SRP felt that the length of the transition period (the amount of time before survival improves) is likely to be longer than two years; that the historical evidence in support of both the turbine and bypass mortality hypotheses is weak; that the short-term benefits of predator removal effectiveness are unlikely to persist and, indeed, there may be the possibility of negative effects from predator removal, due to changes in predator populations and communities. Finally, on the question of the equilibrated juvenile survival rate after drawdown, Marmorek said, three of the SRP members felt that achieving the pre-dam survival rate (0.96) was unlikely, because of other changes to the river system.

Marmorek moved on to the implications of these various weightings on the results. In general, he said, for the 24-year survival standard, there isn't a huge difference in results (the average fraction of simulations exceeding the survival threshold) between the unweighted case and the results you get when you apply the SRP members' weights. In other words, the weightings don't seem to make a lot of difference in the 24-year standard, Marmorek said. Generally, the A3 alternative comes closer than the A2 alternative, but none of them exceed the 70% standard when the weights are applied. In response to a question, Marmorek added that Alternative A1 is current operations as described in the 1995 BiOp; A2 is maximum transportation without surface bypass collectors; A3 is drawdown of the four Snake River dams with no transportation.

Moving on to results from the 100-year survival standard, Marmorek said that, in the unweighted case, A3 is well above the 0.7 standard; A1 and A2 are just above. When the weights are assigned, they didn't tend to make much difference to the results for Alternative A3,

but they had a substantial negative effect on the results for A1 and A2, in some cases dropping them below the 0.70 standard.

The 48-year recovery standard is different from the 24-year and 100-year survival standards, in that it looks at the last eight years (years 41-48) and compares the geometric mean escapement to the recovery level for each stock, Marmorek explained. It looks at what fraction of the time we would be above that recovery level; according to the NMFS standard, the chances need to be 0.50 that a given action would exceed that recovery level. The 48-year recovery standard tends to separate the actions by a larger margin than the 100-year survival standard does, and there is a bigger effect of the weights when compared to the unweighted case.

In general, Marmorek said, the weights that were assigned to the regime shift made quite a difference to the results for the 48-year recovery standard. The bottom line is that, for Alternatives A1 and A2, the results didn't come close to the 0.50 standard in any of the panel weightings; in every case, weighted and unweighted, the results for Alternative A3 substantially exceeded the standard.

Moving on to other conclusions in the weight of evidence report, Marmorek touched on the SRP's conclusions regarding the adequacy of the models for decision-making purposes (see p. 13 of Enclosure D), their guidance as to whether and how to incorporate new hypotheses into the PATH analyses (pp. 14-15), their guidance as to how to collect information (p. 16), options for experimental management actions (p. 17) and conclusions regarding the evaluations of experimental management actions (p. 18).

The Corps' Doug Arndt raised a question with regard to the options for experimental management actions laid out on p. 17 of Enclosure D. Earlier, you had indicated that one of the SRP's findings was that the models were too complex to assess the relative merits of experimental management actions, he said; here you seem to be saying that the models can do the job. The SRP basically feels that we don't need the level of complexity currently built into those models in order to assess what the key performance measures are – namely, the overall changes in recruits per spawner that happen to various stocks, Marmorek replied. In other words, we can develop and use simpler versions of the models, allowing for more rapid turnaround.

Marmorek also touched on the current PATH schedule for October and November, 1998:

- ? Oct. 9: Passage model runs completed. Revisions to fall chinook. Additional actions (spring/summer, fall). Weighted S/S harvest results to DREW.

- ? Oct. 15: 1st quarter workplans finalized.

- ? Oct. 16: Life-cycle model runs completed. Fall chinook report submitted to ESSA. Steelhead report submitted to ESSA.

- ? Oct. 28: ESSA circulate 1st draft of final report.

? Nov. 4: Meeting to review 1st draft of final report.

? Nov. ?: Presentation to IT on FY'98 final report.

? Nov. 15: PATH final report for FY'98 completed.

? December IT meeting: presentation of final ESSA report.

PATH member Jim Anderson said he agrees with the SRP report's conclusion that simpler models are needed in order to allow people to see the consequences of some of the different assumptions. The models we are using in PATH take several hours to run, Anderson said; they are separate models, which are not even available to all members of PATH. In a sense, we have a very hard time understanding what our results are, he said; we can argue about many of the details, and those arguments have occurred within PATH. Using simpler models to, in a sense, re-do everything and throw out the things that aren't important might be very valuable, Anderson said.

I think the weighting group did a fairly good job, Anderson continued; however, I think they were misinformed on many elements. Various PATH members had questions or comments on the weightings, he said, and I hope that within the next few weeks, a mechanism can be developed to correlate those comments, together with the potential inconsistencies in some of their conclusions.

Procedurally, PATH tends to be a consensus group, observed Ed Bowles of IDFG. How do the SRP conclusions factor into the final PATH report – if there is not consensus, does the SRP report fall off the table, or is there some mechanism to include it on its own merits? I would agree that PATH does try to achieve consensus, Marmorek replied, but in some instances, where we achieve consensus is on areas where we disagree. That's important, because the evidence is not sufficient to be absolutely certain about a lot of these things – that's why we have alternative hypotheses. The SRP's report is one of the elements that will be incorporated into the final PATH report, Marmorek said; however, I need to stress that just because we assign weights and get a particular probability of recovery and survival does not mean that that is exactly what will happen – these are our best guesses, but the uncertainty is still there, and there are a range of possible outcomes.

Is the SRP aware that we have already tried to apply simple models to passage? asked BPA's Dan Daley. For instance, there was a spreadsheet model that was used in the development of the fish and wildlife program years ago, and it just didn't seem to cut the mustard – that's one of the reasons we've moved to progressively more complex passage models. Yes, they are aware of that, Marmorek replied -- I guess it depends on what kind of mustard you're trying to cut. If you're interested in looking at detailed operational management at dams – for example, what pattern of spill minimizes gas production – that's different than looking at changes in survival and recovery of stocks over a 100-year period.

It seems to me that the SRP is identifying a need for experimental management, Daley continued; further, it sounds as though their recommended incremental approach would be to

implement the lowest up-front cost actions. That's one of the two experimental management approaches they discuss, Marmorek said – the other is the “reverse staircase” approach. They're saying PATH should look at both of those approaches, and evaluate what risks, benefits and potential learning are associated with each approach.

In response to another question from Daley, Marmorek said PATH will not have time to evaluate Alternative A6 (maximum in-river) prior to releasing its FY'98 annual report in November. However, we do intend to get back to A6 as soon as we can, Marmorek said. In response to another question, Marmorek said five alternatives will be considered in the FY'98 annual report: A1, A2, A2' (maximized transportation with surface collection), A3 and B1 (Lower Snake drawdown plus John Day drawdown). He explained that, while the weight of evidence report only looks at three of those alternatives, the structure of the hypotheses used in the other two alternatives is such that PATH can still apply the weights the SRP suggested. In other words, we're not constrained in generating results for B1 or A2' because the SRP didn't have those runs done prior to their workshop, Marmorek said.

The SRP stressed the need for simpler models; they also discussed experimental management actions that would require that simpler model, recommendations that Jim Anderson says he supports, said Arndt. Is that pretty much a consensus viewpoint among the PATH membership, and if so, is it part of your current workplan to get that effort underway? I think that viewpoint is generally accepted among the PATH membership, and it is one of the things on the workplan, Marmorek replied. That's one of the items we hope to discuss with the IT in November, because we have a lot of things on our plate, and we need some discussion of PATH priorities.

Like Doug and Dan, I was struck by the SRP's recommendations concerning experimental management, particularly their comments about the fact that experimental management may be the only way to test some of the additional hypotheses, said Brown. How far might PATH get, in its final FY'98 report, in developing the experimental management approach? Not very, Marmorek replied. We still have a lot of work to do in evaluating B1 and A2', and in running all five of the alternatives through the fall chinook model. The final FY'98 report is unlikely to say much more than what you see in the SRP report, although we may be able to make a little progress in further exploring what some experimental management options might look like, in a very general way. The development and employment of the simpler models discussed in the SRP report is likely to happen in the December-February time-frame, Marmorek said.

BPA's Jim Geiselman, a PATH participant and member of the PATH Planning Group, said he and others believe that there is additional information that needs to be provided to the current synthesis of information. There were extremely tight time lines and objections to the process as the development of the Weight of Evidence Report proceeded, he said; we have several concerns about the adequacy of the information that was provided in the report. It was not a consensus report, it was a report by the facilitator based on the information that was provided to the facilitator. We objected to the report as an inadequate synthesis of the information, Geiselman said, and we plan on working within the PATH process to provide additional information regarding the alternative hypotheses. In particular, said Geiselman, we're concerned that the drawdown hypotheses as developed are overly optimistic and the transportation hypotheses are overly pessimistic.

It is also important to note that the updated information does not include any evaluation of the

effects of other management actions in the other H's, he continued. That is, there are no harvest, habitat, hatchery effects, or bird predation programs, included in the analyses. There are also other alternatives that will need to be evaluated for the hydro system. We are very concerned that those additional pieces of information need to be provided in the analyses, said Geiselman. We will have continuing time constraints on how much we can get into the analyses and some of this information will need to be developed after the FY98 report. But I think it is very important to view this need for additional information in context with the weighted modeling results, he said. They show for the survival standards that even with the current transportation program you are getting fairly high probabilities of meeting the 24-year and 100-year survival goals, but that the 48-year recovery goal is the problem. The question, then, is how many additional measures either within the hydro system or with other H's you need to implement to get over the 48-year recovery hurdle -- that's an important question we need to address in our continued analyses, said Geiselman.

One of the earlier reviews from the Scientific Review Panel noted that "given the urgency of salmonid management issues that include downriver stocks, coastal stocks, and other species such as steelhead, a total preoccupation with a single life-stage (outmigrating smolts) of the most threatened of the many depleted stocks seems short-sighted. Some thought should be given to how the models should be used to address a wider range of management alternatives than is currently being considered." I would say that it appears that the current limited scope of the analyses in our current report makes that comment very applicable today as well, Geiselman said.

HARZA's John Pizzimenti said that, under contract to the Power Planning Council, his firm had developed estimates of the costs of some of the management actions under consideration by PATH. For example, he said, to draw down John Day Dam to spillway crest is estimated to cost \$167 million for construction only. To remove the embankments only at the four Lower Snake dams will cost between \$600 million and \$1 billion, again for construction only, he said. The annual cost of removing those projects is estimated to range between \$350 million and \$800 million per year, which represents 25-40% of the current benefit of the federal hydropower system. When you talk about a "reverse staircase" experimental design, in which you would experimentally remove dams, I don't think you're going to get very much credibility in the real world, Pizzimenti said. I understand what you're trying to achieve here, he said, but when you talk about dam removal, you're going to have to make a very strong case for that being the best alternative, which will recover fish.

Pizzimenti went on to describe some experimental manipulations that he feels may be possible: adding and removing screens, to determine whether fish that avoid screens may return in greater numbers as adults than fish that use the screen systems, and changing hatchery practices. Finally, Pizzimenti said, is our goal to recover hatchery fish or wild fish? Because looking at most of the information that has been provided in support of this effort, he said, that's unclear to me. Wild fish, was the reply.

To address some of Jim's comments, said Marmorek, the process did have very tight deadlines; everybody had the same tight deadlines. Many of the people involved in the fall chinook analysis had no opportunity to work on the weight of evidence process during April, May and June. In terms of how the weights come out of the submissions, Marmorek said, it works out pretty evenly -- BPA and the various contractors accounted for about 0.9 pounds of the 3.7-

pound total. We did the best we could to pull all of that information together objectively, he said – the report presents points, counterpoints, rebuttals to those counterpoints. It is not for us to judge whether or not the report turned out to be objective – all I can say is that the independent scientists involved in PATH and the four SRP panel members all commented that they felt the report was very objective in the way in which it synthesized the evidence provided in the submissions, Marmorek said.

I would add that hatchery and bird predation have been considered in the weights assigned, in particular to the extra mortality hypotheses, Marmorek continued. As I said, this is not the last word on the weight of evidence process; we've done some other sensitivity analyses on habitat questions beyond those that were done previously, and those will also be considered in the final report. I would add that we looked directly at the impact of harvest, and looked indirectly at sensitivity to the effects of hatchery and habitat, said Howard Schaller – the other Hs may not have been evaluated as intensively as we evaluated the hydrosystem, but they certainly were not ignored.

As far as the inadequacy of the information goes, Geiselman said, there were a lot of comments not included in the final weight of evidence report and, again, we have a lot of concerns about the inadequacy of the information that was included in the report. With regard to the other Hs, while we did try to look at historical hatchery effects, we did not include any hatchery management actions in the analyses; there were also no bird predation management actions included in the analyses. That's true, Marmorek replied. The same thing is true of habitat and harvest measures; the current results that we are looking at did not include any management actions for these two Hs. That's inaccurate, replied Schaller -- there were direct harvest actions implemented in the models. That's the current harvest rate, Geiselman said -- there were no analyses of changes to that increasing rate. At this point, Silverberg intervened, to suggest that it would be more appropriate to continue this discussion within PATH.

I'd like to try to come to closure on the contents of PATH's FY'98 annual report, said Brown. Dave discussed some of the things that will be included, one of which was the preliminary analysis submitted by PATH in March and subsequently updated, which this weight of evidence report speaks to directly, Brown said; how PATH pulls all of that together will be one major component of the FY'98 report. The report will also include some additional alternatives that have not previously been reported on in the PATH process; it will describe the fall chinook modeling that has been done to date, and the steelhead analysis as it currently stands. Those additional alternatives will not include A6, however, Brown said. Is the IT comfortable with that list of contents for the annual report?

I'll raise a concern I've talked about before, said Daley – PATH is continuing to narrow down the alternatives it is considering, to the point where now the SRP has discussed only three alternatives in its weight of evidence report. The SRP's own recommendation is that we follow an experimental approach, in one of two ways – an incremental approach, or a “reverse staircase” approach. We need to be aware that, even if the region concludes that dam removal is biologically the best thing to do, from a purely pragmatic standpoint, that still may not happen, Daley said. Therefore, to eliminate other alternatives from consideration is very dangerous at this point in time. PATH needs to give full consideration to all of the alternatives before it issues a draft report, and the SRP should be applying its weighting process to all of the alternatives, Daley said.

Again, the SRP is not weighting action alternatives, Marmorek said – the SRP is weighting hypotheses. We're doing the best we can; as you'll recall, there are a large number of alternatives under consideration, and what we agreed at a previous IT meeting was that PATH will do alternatives that represented potential extremes or bookends first, he said. Once those analyses were complete, we agreed to work on the alternatives that fell between those extremes next, Marmorek said. It's up to the IT to decide what our priorities should be, he added.

Brown added that there appears to be some confusion about the intent of the PATH report – this is their FY'98 annual report, not a draft of their final report. Completing their annual report for 1998 is something I expect PATH to do, not come to the IT with a draft, he said. This report is not the last word from PATH on the evaluation of alternatives, Marmorek added.

If you're not satisfied with the contents of the report as I outlined them, and you would like to see an analysis of Alternative A6 included, I guess I would add, at the expense of what? Brown asked. At the expense of schedule, or some other informational component PATH is currently planning to include? In response to another question from Daley, Tom Cooney of NMFS said point of releasing the PATH FY'98 annual report in November is to provide timely input to the Corps' EIS process. If we wait until January to release that report, that will cause real problems for the Corps' schedule, he said.

A lengthy discussion of the scheduling interactions between PATH and the Corps EIS process ensued; ultimately, Brown said that, from the perspective of the agency that is driving the overall schedule, NMFS would like to see as much of the discussion as possible on what we know now take place before NMFS submits its information to the Corps. I firmly believe that if we push out the deadline for completing this report, it isn't the final report that will be lacking when NMFS has to give the final word to the Corps – it will be the discussion that you all think needs to occur before that happens, Brown said. My advice to the PATH participants is, work to get your views into the PATH annual report.

In response to a question from Silverberg, Nielsen said he is comfortable with PATH proceeding with the finalization of its FY'98 annual report; given the very tight timelines facing everyone, he said, I would be much less comfortable delaying that report until the analysis of alternative A6 is complete. After a few minutes of discussion, no IT disagreement was raised to Nielsen's recommendation. I guess the critical thing is how the Corps deals with this issue when they incorporate the PATH report into their draft EIS, Daley said; that will be subject to public comment.

c. System Configuration Team (SCT). Jim Ruff of the Council staff said the SCT has been working to develop its rankings for the FY'99 Columbia River Fish Mitigation projects; it has been especially difficult this year, he said, because Congress has decided to appropriate \$60 million this year, rather than the \$117 million we requested. The discussion we just finished about Alternative A6, which emphasizes structural in-river passage improvements, was interesting, because we're not going to be able to do much -- if any -- of that work this year, Ruff said – the budget is very tight.

We had a meeting yesterday, at which significant progress was made toward developing our list of FY'99 priorities, Ruff continued. The Congressional report language directs us to fund two particular projects, he said: the Lower Snake Feasibility Study and the John Day drawdown

study. We are also required to complete or continue with ongoing construction activities; this is a significant clause, because it requires us to complete, among other things, the work on the Bonneville Powerhouse II outfall relocation and bypass system improvements. That project alone is budgeted at nearly \$22 million in FY'99, which is nearly 40% of our entire budget, Ruff said. In other words, we will have very little flexibility to implement any new projects, or to continue with studies, such as prototype studies for surface bypass, turbine passage survival improvement investigations or gas abatement studies.

As I said, in spite of these fiscal realities, we've made good progress toward developing an SCT list of FY'99 project priorities, Ruff said; that process will continue at a meeting on October 5.

This is the most difficult task the SCT has faced in its three or four years of existence, added SCT co-chair Bill Hevlin of NMFS. It's a grueling process; we're trying to cram a \$100 million program into a \$60 million straightjacket. And I'm sorry to say that, as a result, many of the longer-term fish survival fixes – surface bypass systems, gas abatement structures, fish-friendly turbines -- that would be included under Alternative A6 are going to be neglected in the FY'99 CRFM program, Hevlin said. We will get the FY'99 list down to \$60 million, Hevlin said, but I guess what I'm asking the IT is, do you see any other way to come up with some more money for the CRFM program in FY'99, so that we can continue to make progress on some of these survival measures?

Has NMFS begun to discuss with the Corps what this \$60 million appropriation may mean in terms of the Corps' ability to implement the reasonable and prudent actions called for in the 1995 Biological Opinion? asked Ron Boyce. We have, Brown replied, but until the FY'99 project list is finalized, we're not really in a position to say much about it, beyond the fact that, given this level of funding, it's pretty clear that this is going to be a difficult question.

In response to a question from Fred Olney of the Fish and Wildlife Service, Ruff said he anticipates that the SCT will be able to finalize its list of FY'99 priorities at Monday's meeting.

III. Introduction of Flow Augmentation Report.

This presentation is going to consist of three main parts, said Paul Wagner of NMFS. First, Steve Smith of NMFS will review recent survival study results; Karl Dreher, director of the Idaho Department of Water Resources, will present his paper on the interstate allocation of water in the Columbia River Basin. We will also hear a review of the Columbia/Snake River flow augmentation program from Darryl Olsen of the Pacific Northwest Project, Wagner said.

My purpose today is to provide a brief introduction and overview of flow targets and NMFS' flow program, Wagner continued. The first thing I would like to say is that allocation decisions are always tough – there is never enough water to meet all the wants and needs in the region, except in flood years.

Those wants and needs include the Federal Columbia River Hydropower System, 12,000 annual average megawatts of power production, one of the largest energy grids in the world, Wagner said. They also include an irrigated agriculture industry that now covers 7 million acres in the Snake and Columbia River Basins. Flood control is another major influence on how the system is operated; besides power production, the system also exists to protect Portland and other communities from catastrophic flooding. The fourth major player in the water allocation

arena, as everyone here is aware, is fish, Wagner said, and unfortunately, the fish side of this structure is collapsing. We are in the process of trying to shore it up, and one of the ways we're doing that is through flow augmentation. What we're after, in the long term, is a stable structure of water uses that will stay in balance over time, he said.

What we need to remember, he continued, is that the fish in the Columbia evolved in a big flow environment. Flows would typically peak at 450-500 Kcfs in the spring; peak lower river flows of 800-900 Kcfs were not uncommon. In spreading this resource, formerly devoted to fish alone, among power production, irrigated agriculture and flood control, what we've done is smoothed out those ferocious peaks, and as a result, fish have suffered, Wagner said. Columbia Basin salmon evolved around the peak hydrograph – it was their signal about where to go, and when to go.

As we alter the flow in the river, we are sending those fish mixed signals, Wagner continued. As a result, we have slowed their migration. In response, NMFS developed its flow augmentation program, built on three lines of evidence. First, the flow/travel time relationship, he said – essentially, the higher the flow, the faster the fish travel. There are break points in this relationship, and those break points -- the points above which fish tend to do well -- are what was used to establish the NMFS flow objectives. The overall goal was to achieve a reasonable travel time for fish through the system, Wagner explained.

The second line of evidence is the evolutionary history of the fish – the fact that they evolved around a peak hydrograph, he continued. The third line of evidence is the smolt-to-adult returns observed in various historic water years.

NMFS established flow objectives for both the spring and summer seasons, Wagner said; in the spring season, reservoirs are now required to refill earlier in the season than they were historically; in essence, during this period, the augmentation program reduces the volume of water that would naturally pass down-river. In the summer, the storage reservoirs are used to augment flows in the river during the period when the competition for available water resources -- from recreation, irrigation, power generation -- is at its highest level, Wagner said.

IV. Review of Recent Survival Study Results.

Steve Smith explained that his purpose today is not to talk directly about the effects of water released for flow augmentation; rather, he said, I'm going to be talking about empirical data that has been gathered through the past six years, using PIT-tagged juvenile migrants to estimate survival through the system.

Smith worked from a series of overheads, touching on 1993-'98 PIT-tag releases, how survival probabilities are estimated, survival, by reach and year, for various chinook and steelhead stocks, the historical and PIT-tag flow survival relationship, estimated environmental exposures, recent and historic travel time data, by species and reach, estimated survival vs. temperature exposure, by species, and estimated survival vs. travel time, by species. These overheads are attached as Enclosure E; please see this document for details of Smith's presentation.

In summary, Smith offered the following conclusions for yearling chinook salmon and steelhead:

? There was a strong, consistent correlation between flow and travel time, within seasons and among years.

? There was no correlation between flow and survival within season, under conditions 1993-1998 and in the reaches studied.

? There was a weak correlation between flow and survival among years (mean survival correlated with mean flow, consistent with 1970s data).

Smith offered the following conclusions for subyearling chinook:

? There was a strong, consistent correlation between flow and survival from release to Lower Granite Dam (confounded with water temperature and turbidity?)

? The picture is less clear below Lower Granite Dam (the year with the largest ranges showed results similar to above Lower Granite Dam).

Isn't it possible that your conclusion for yearling fish that there is no within-season correlation between flow and travel time is due, at least in part, to how you measured your exposure index? Boyce asked. In other words, that the index doesn't accurately reflect the actual flow these fish were experiencing? It's possible, Smith agreed. The only way I think we're going to get at that is through a simulation study, which we've already begun to set up. We're stuck with the data we have, with current technology, Smith said -- we can only estimate survival for groups of PIT-tagged fish. These groups are going to disperse as they move downstream; the flow experience for individual fish within each group is going to vary, and the survival of individual fish is going to depend on that fish's flow experience. What we've started to do is simulate data as if we know all of the variables -- individual fish histories, when they were at each dam, what their flow experience was on those dates etc., Smith said. Through these simulations, we're able to generate data that has an individual-based flow-survival relationship within reaches; we then plug that simulation data into our estimation procedure, and the result is that, at least in the limited set of simulations we've done so far, we can find the individual-based flow-survival relationship.

Boyce suggested that it is important for Smith to add that the methodology used, as well as the conditions in 1993-1998 and the reaches studied, may be influencing the results of this study. If you don't state that plainly, I think it is intentionally misleading, Boyce said -- otherwise, people are going to walk away from this table thinking there is no relationship between flow and survival.

My second point is that, when you looked at the inter-annual relationships, you didn't really do a correlation of the 1990s data, Boyce said. That's true, but the correlation among those points is going to be very similar to the correlation that we saw through all of the individual points on the same plot, Smith replied. It's a shallow positive correlation; that line is essentially being fitted to the mean survival and the mean flow for each year -- that's essentially the same result you're going to get.

Is it possible to tie these survival estimates directly to adult return numbers? asked DeWitt Moss. Not directly, no, Smith replied -- we're only talking about two or three months in the life-cycle of these fish, and we're only looking at survival through specific river reaches; there are

many other factors that affect survival to adult return.

V. “The Columbia/Snake River Flow Targets/Augmentation Program – A White Paper Review with Recommendations for Decision-Makers.”

Darryl Olsen, a resource economist with the Pacific Northwest Project, a resource economics firm in Tri Cities, Washington, provided an overview of the flow review study his firm recently prepared for water users in Oregon, Washington and Idaho. Olsen introduced Jim Anderson and John Pizzimenti, co-authors of the study; he explained that their assignment, from the study’s sponsors, was to examine and review the information in the existing flow review program – the policies established for water management, the nature of the hydroregulation related to the flow targets, the relationship between that hydroregulation and existing water withdrawals, biological information for spring/summer and fall chinook, and economic factors.

Based on our review of that information, we were asked to make some recommendations, Olsen continued. For that reason, we are calling this a white paper, he explained. Copies of this paper are available from Olsen at 509/783-1623.

Olsen began with the recommendations that lie at the heart of the white paper, aimed at state and federal decision-makers and resource managers:

Item Recommended Action Implications

NMFS Water Policy

“No net loss” water policy –

challenges state water rights Change or reject policy Reduces conflicts with states and water users; greater collaboration sought with economic stakeholders

Flow Targets/Augmentation Program

-- Weak/uncertain biological justification

-- Unrealistic hydrologic demands

-- High-cost program –

\$\$ – results? Restructure flow augmentation program -- Eliminate spring flow augmentation (some experimentation, low-volume uses in tributaries

-- Summer flow augmentation at approximately 1994 levels in Snake Basin – low water

-- Redefine summer flow augmentation for the Columbia River.

Restructured Water Use

Program and Optimize Use of Water

- Marginal biological benefit analyses
 - Cost-effectiveness analyses
 - Other optimization factors or considerations
- Apply optimization analyses/review (If spring flow augmentation is eliminated), new alternatives are available or possible
- More fish for the \$\$
 - Funding for new water resources projects in tributaries, water transfers
 - Implications for tribes?
 - Economic interests?
 - Other resource benefits?

Olsen spent a few minutes going through this list of conclusions; please see the white paper for additional details of Olsen's presentation.

Olsen then yielded the floor to Jim Anderson of the University of Washington, who spent a few minutes going through some of the biological rationale underlying these recommended actions. Anderson in turn introduced Pizzimenti, who focused his presentation on historical biological and ecological information. Among his remarks:

? “[Regarding the white paper], part of my role was to reflect on the biology and the ecology, those factors that we think might be at play in trying to understand the data Steve [Smith] keeps creeping away from explanations on. I can understand that; he has an agency with a PIT-tagging policy, and he has to be careful of how he interprets it, publicly.

? “Another part of my task has been to review the historical literature, and in that connection, I have read the Simms and Ossiander paper at least four times, from beginning to end... I'm fascinated by how that paper has been overlooked and misinterpreted.

? “A single sentence on Page 19 of this report set off a controversy that I think is still with us, despite scientific evidence of a cause-and-effect relationship: ‘If the rate of smolt migration affects survival, there should be a positive correlation between river flow and smolt survival.’ If you think about that, it's quite interesting – it sets this whole thing in motion, about whether this relationship is positive and provable – personally, I'm unsure that anyone has ever really proven that hypothesis.

? “A proportionate increase in spill, at low magnitudes, will yield a greater increase in survival than the same proportionate increase in flow – for example, the Simms &

Ossiander data show that during the first 10% of possible spill, a 28% level of chinook salmon survival is attained, whereas during the first 10% of flow, only a 1% level of survival is attained. In other words, a moderate increase of spill in the lower range of values will yield a substantial improvement in survival. I think these are very salient and cogent observations that we still seem to be neglecting, in terms of our management and scientific practices with regard to the operation of the dams and the system.

? “In the Salmon Decision Analysis Report that HARZA collaborated on a few years ago, we looked at the cost effectiveness of 15 different tools that could be employed in the federal hydropower system to better manage salmon improvement with regard to juvenile and adult survival. The worst-performing attribute, in our analysis of cost/benefit, was the use of flow to improve survival for spring chinook. It was even lower than dam removal, which is by far the most expensive option considered. Spill, on the other hand, was one of the most cost-effective tools, in terms of improving spring chinook juvenile survival.”

Please see the white paper for further details of Pizzimenti’s arguments.

I hope you’re not saying that, based on your review of the historic data, you believe we should walk away from flow augmentation in August, said Boyce – in fact, I think it shows that smolt-to-adult survival is so poor in August that additional relief is needed during the summer period. It also demonstrates that we may need additional flows during other parts of the year to address other needs.

Fall chinook are a species about which we need more information, and I tend to agree that flow may be a potentially valuable tool for fall chinook, Pizzimenti replied. If you’ve only got 10 Kcfs in natural flow in the Snake during the fall, 10 Kcfs in flow augmentation would make a dramatic difference for in-river migrants. On the other hand, adding 10 Kcfs to Snake River flows during the spring would hardly change the river environment at all. My recommendation is that we stop spending our capital in the spring, and move it later in the season, when it could be much more effective, Pizzimenti said.

The current flow augmentation program strongly encourages storage reservoirs throughout the system to be full by June 30, to prepare for summer drafts to benefit fall chinook, said Brown. I would suggest that eliminating spring flow augmentation is not going to make those reservoirs any fuller, he said; what it would do is allow deeper drafts in the winter period, the effect of which would be to have spring runoff refill that hole, rather than flow down the river. To reference a point you made earlier, Brown said, in order to provide the change you refer to here, you said you would have to use all of the stored water that is available for spring flow augmentation. That is not the program – the program is to go into the spring period with as much water in storage as possible, given flood control requirements, and end the spring period full. In recommending that we eliminate that spring program, and do away with the requirement that reservoirs be full going into the summer period, have you looked at the effects on the reservoirs? No, but I think that’s an important issue, Pizzimenti replied. Some of the optimization studies we recommend may answer some of those questions.

The discussion turned to the ramifications of providing an additional 1 MAF in flow augmentation from the Upper Snake; Olsen said that, based on the data presented earlier in the

meeting by Steve Smith, a back-of-the-envelope calculation shows that, based on current SARs, that additional 1 MAF would produce about 100 additional returning adult fall chinook, at a cost of \$50 million to \$200 million per year. The question we're asking is, do we want to do that, or do we want to take some of the money we would get from restructuring the flow augmentation program and see what we can do in the tributaries? Olsen said. I'm not saying it's definitely the right way to go, he said, but I am saying that it's an alternative that we feel is not being adequately considered at the present time.

And that is the question before the IT, said Brown – where do we want to go with areconsideration of alternative flow augmentation programs, given this information, given some of the new information that has come to light since the 1995 Biological Opinion was written, and given the various regional perspectives on what that information is telling us. Perhaps we can discuss that at the next IT meeting, he suggested.

VI. Flow Augmentation -- Karl Dreher, Director, Idaho Department of Water Resources.

Dreher introduced his paper, "Competing for the Mighty Columbia River -- Past, Present and Future: The Role of Interstate Allocation" by explaining that he is not a fisheries biologist, but an engineer, with extensive experience in water resources and hydrology. He went through this paper, attached as Enclosure F, in some detail; please refer to this document for details of Dreher's presentation.

Dreher's talk included the following conclusions:

1. The development of irrigated agriculture, the construction of dams on the Snake River upstream of Lower Granite Dam and the regulation of the Snake River above Lewiston, Idaho have not:

? Significantly decreased flows in the Snake River below Lewiston during the 72-day time periods in the spring and summer for which NMFS has specified flow objectives;

? Resulted in significant increases in water particle travel times or decreases in equivalent average velocities between Lewiston and the confluence of the Snake and Columbia Rivers; and

? Significantly impeded outmigration of juvenile salmon or immigration of adult salmon to remaining habitat.

2. The construction of the four lower dams on the Snake River (Ice Harbor, Lower Monumental, Little Goose and Lower Granite) have:

? Resulted in significant increases in water particle travel times and equivalent average velocities in the Snake River between Lewiston, Idaho and the confluence of the Snake and Columbia Rivers;

? Resulted in significant hydraulic changes which impede both the outmigration of juvenile salmon and the immigration of adult salmon.

3. The impedance caused by the lower four dams on the Snake River is not rate-of-flow related and, therefore, the impedance cannot be overcome with rate-of-flow actions: ie., flow augmentation.

4. Flow augmentation can provide only small incremental improvements in average velocities. However, the improvements are not significant, particularly during the NMFS spring flow augmentation period when higher flows occur which reduce any effectiveness of flow augmentation.

5. The Snake River Basin does not yield water in sufficient quantities to meet the NMFS flow objectives in all years. To the extent there are other benefits to migrating salmon from augmenting flows not considered in this paper, flow augmentation should primarily be limited to dry years when flows would otherwise be lowest.

You contend that raising the average flow in the Snake from an unaugmented average of 39 Kcfs to 55 Kcfs, or whatever the summer flow target might be, would not significantly affect travel time or water velocity, said Brown. However, the information Steve Smith presented earlier in today's meeting suggests to some, at least, that there is a biological benefit to augmenting flows.

At first glance, in looking at the data for fall chinook, it appears that we don't know whether the survival relationship is the result of flow, or the result of temperature, Dreher replied. In other words, the mechanism by which we are delivering lower temperatures is cold- water releases from Dworshak. I guess I would respond to your comment with a question: is that survival relationship a result of temperature, or flow? Dreher said. If it is a result of temperature, then you're defeating your own purpose by augmenting flows with warmer water from the Upper Snake during the late summer.

It sounds as though we all need to be thinking about the current flow augmentation program between now and the next meeting, when we can engage in a substantive discussion about possible changes to that program, Silverberg said. That's correct, Brown replied – my intent was that the series of presentations we've heard today be the introduction to an IT discussion of whether we need to reconsider the current flow augmentation program, because we now have some new information, and because we've taken a fair amount of criticism from some groups about the value of the existing program. At the November IT meeting, I'd like to discuss whether or not it would be appropriate to consider changing the flow augmentation program prior to the 1999 migration season, as well as how flow augmentation may factor into the 1999 decision, Brown said.

VII. Sensitivity of A6 Hydro Reg. To In-Season Management.

This presentation was deferred until the November IT meeting.

VIII. Date and Possible Agenda Items for Next Executive Committee Meeting.

We have discussed the possibility of scheduling an Executive Committee meeting for late

November, said Brown – has anyone had a chance to discuss that with their executives, and is it the IT's feeling that this meeting should go forward? One IT participant said his agency's EC representative would like to see the November meeting go forward; one of the items he would like to see on the agenda is a discussion of the Corps' DGAS program – where it's headed, goals, scope etc. Brown said he will contact other relevant agencies in the next week or two to try to firm up the date and agenda for this meeting.

IX. Next IT Meeting Date and Agenda Items.

The next meeting of the Implementation Team was set for Thursday, November 5 from 9 a.m. to 4 p.m. at NMFS' Portland offices. Meeting notes prepared by Jeff Kuechle, BPA contractor.