

Independent Scientific Review Panel

for the Northwest Power and Conservation Council; 851 SW 6th Avenue, Suite 1100; Portland, Oregon 97204

Independent Scientific Advisory Board

for the Council, Columbia River Basin Indian Tribes, and the National Marine Fisheries Service

June 28, 2005

To: Doug Marker, Director Fish and Wildlife Division, and Steve Waste, Manager for Program Analysis and Evaluation

From: Independent Scientific Advisory Board and Independent Scientific Review Panel

Subject: Preliminary Review of Draft Research Plan (ISRP&ISAB 2005-13)

At the Council's request, the ISAB and ISRP reviewed the draft Columbia River Basin Research Plan developed by Council staff. We understand this to be a preliminary review and suggest significant revisions to the draft plan. The next step in the development of a final plan is to have Council staff revise the plan based on our review and the reviews of the managers who will be partners in the implementation of the plan. Council staff then intends to request ISAB and ISRP review of a final draft, likely after the Regional Research Partnership has been convened, and research priorities have been negotiated and included in the plan. Because the ISAB and ISRP are recommending significant revisions to the draft plan, a follow-up review of a revised plan may be beneficial before the revised plan is used in a regional forum.

The ISRP and ISAB were asked to consider five questions as guides to their review:

1. Does the draft plan provide a vehicle for setting priorities under the Fish and Wildlife Program, and for partnering with external parties on research relevant to the program?
2. Does the draft plan describe the most important management issues facing the region and what research might facilitate their resolution? If not, what is missing?
3. Do the overview sections accurately capture the current state of the scientific issue at an adequate level of detail to provide the necessary context for the research recommendations?
4. Do the research recommendations logically flow from the overview, the management questions, and critical uncertainties?
5. Are the research recommendations at an adequate level of detail to guide the development of RFPs, proposal development, gap analysis, and scientific review?

Additionally, the boards were requested to consider five more specific questions regarding presentation/format:

Chapter II

1. Should the critical uncertainties be presented in a uniform format, following the discussion of critical uncertainties on page 17?
2. Should the research recommendations remain organized as they presently are by topic and sub-topic, e.g., for habitat, the research recommendations are categorized as physical, biological, or chemical?
3. Should the current typology of: management questions, critical uncertainties, and research recommendations be maintained for each topic? The current three-layer approach has generated a lot of re-statement of the same issue, resulting in redundancy.

Chapter III

1. Is the discussion of allocation of research expenditures under the Fish and Wildlife Program relevant or distracting?
2. Should the detailed walk-up to the gap analysis be included in the text or should the results of the analysis simply be presented in a table?

Finally, the Council memo noted several issues that had arisen from previous reviews of the draft Plan by various tribes and State and Federal agencies that play roles in fish and wildlife management and research in the Columbia River Basin, and by several universities, consulting firms, and individuals. These issues concerned collaboration and a recommendation in the Plan for a Regional Research Partnership, the diversity of suggested changes in organization of the Plan, and the audience and length of the Plan.

Below, the ISAB and ISRP provide: (1) responses to the questions from the Council, (2) comments on the issues noted in the Council memo, and (3-5) summary comments on each section of the Draft Plan. These are followed by an Appendix that contains additional more specific comments that may be of use to Council staff.

Executive Summary

The Draft Research Plan represents an important first step to develop a much needed regional planning document. The broad support for collaboration in developing regional plans, priorities, and coordinated research, as expressed in much of the comment that so far has been received on drafts of the Plan, is encouraging. Nevertheless, the Research Plan needs considerable revision. Most importantly, the draft is too long and includes too much unnecessary and counterproductive detail. There is a need to focus more closely on key, general, and overarching elements and reduce greatly the repetition and redundancy in the current Draft Plan, especially in Section II, the central topic content of the Plan. Such changes are needed to make the document clear, compelling, flexible, and useful as a planning and prioritizing tool. There also is a need to

directly, though briefly, state the purpose and intent of the plan, its scope within Columbia River Basin fish and wildlife management, and how it is intended to interface with other research programs for related natural resources in the basin and Pacific Northwest.

(1) Responses to the Questions from the Council

1. Does the draft plan provide a vehicle for setting priorities under the Fish and Wildlife Program, and for partnering with external parties on research relevant to the program?

The Plan should provide a broad framework for setting priorities, but, in order to serve this role and guide research efforts in the Columbia River Basin, it should be considerably condensed and reorganized. To be effective, the Research Plan should focus on key critical uncertainties that are worded in such a way that they can serve as a basis for developing and organizing specific research project proposals. The Plan also should provide a rationale for which of the possible issues are of the greatest significance, but description of the issues need not include extensive background review beyond what serves to establish significance of issues for which research is needed.

Success of the Research Plan will depend on the identification of the most important uncertainties that need to be resolved to recover and conserve native fishes and wildlife and the establishment of a Regional Research Partnership that effectively prioritizes the uncertainties.

The Research Plan must:

- agree upon a tractable number of well-chosen priorities;
- state the priorities in ways that promote effective research solutions;
- integrate with the Subbasin Plans;
- have a means of resolving disagreements on priorities; and
- be able to take advantage of serendipitous research opportunities that arise from advancements in technology and scientific knowledge or are simply facilitated by immediate environmental or social opportunities.

The list of research recommendations in the Draft Plan is problematic in that it is too long, unfocused, unbalanced, and includes elements of widely varying scope and generality. The lists of research recommendations run the gamut from very broad research areas that encompass many possible specific research questions to very specific questions (e.g., many recommendations relate specifically to operation of Libby and Hungry Horse dams). The high number of recommendations and their unevenness generate several problems: reaching consensus on priorities of such a wide array of research recommendations could be extremely difficult, the listing of many specific research recommendations may inhibit development of more integrative or innovative approaches, and the list of research recommendations is both highly redundant and missing obvious other equally germane specific research. The ISRP and ISAB recommend that the core scientific content of the Research Plan, the identification of research topics, be kept much more general in focus. The plan should only identify and describe the key elements (e.g., critical uncertainties). Keeping the topic descriptions at a higher level should help greatly in resolving the problems of (1) prioritization of research topics, (2) failure to foster innovative research, (3) critical omission of valuable research approaches, (4) confusing

redundancy in presentation of the Plan, and (5) probable inefficient redundancy of research effort. Keeping the topic descriptions at a higher level also is important if the document is to avoid quickly becoming dated.

Since the process of science is dependent on insights of bright scientists, and insights are not very predictable, one of the main roles of a research plan might be to stimulate those scientists with funding, general topics to explore, and places to work. Salmon restoration isn't like going to the moon, for which every step could be planned in detail. We don't know enough about how the ecosystem works. Pointing out what general parts of the system we most need to know more about is a good role for the plan, as is structuring the Plan and its priorities to foster and take advantage of innovative approaches.

The proposed Partners for Implementation suggests a mechanism by which more specific research topics might be developed from the core list of critical uncertainties that define the basic research plan, similarly to the role once played by the Technical Work Groups (TWGs) that BPA established early in the evolution of funding of the fish and wildlife program. The TWGs were groups of scientists and managers with relevant expertise, who were given the responsibility of summarizing knowledge on a subject and of identifying current critical uncertainties and needed areas of investigation. Priorities were assigned to these uncertainties or gaps identified by the TWGs. Some mechanism of that sort may be the best that could be done in going beyond the general definition of subject areas for research. If the strategy is to establish a Regional Research Partnership to identify the priorities, then much of the discussion about technical issues is unnecessary. The plan should simply provide a core list of critical uncertainties and develop the rationale for the Partnership. The Partnership would have the role of developing the technical information required to assign priorities.

The Plan should include a specific mechanism for integration with the Subbasin Plans. The Research Plan should encompass the general priorities identified in the Subbasin Plans, if the two are to be integrated. Thus, we suggest that the Plan should outline how integration with the subbasin plans will proceed, but not perform that integration, which could be assigned to the Research Partnership.

The Plan imparts surprisingly little development of the idea that focused biological/ecological research is urgently needed to inform the major conflicts in the basin that have large socio-political implications, such as the unending disagreements about the relationship of flow and survival of fishes or the influence of hatchery fish on wild stocks. Fundamental issues of fish migration and of interaction of hatchery and wild fish remain poorly known and understood, yet the consequences are substantial for both listed species and the economy of the region.

2. Does the draft plan describe the most important management issues facing the region and what research might facilitate their resolution? If not, what is missing?

The Plan describes many important issues, but the approach of generating lengthy lists of specific topics, many reflecting ongoing work, is not helpful in identifying central issues or in revealing a basis for their prioritization. The draft Plan does not adequately identify and stress

the major topics, nor does it incorporate the needed flexibility in accommodating new information and approaches that would be desirable over the lifetime of a Research Plan.

Special emphasis should be placed on research topics that cut across the four Hs; for example, the interaction of hatchery releases and habitat quality on wild salmon populations. Designing research studies without recognizing that these multiple H interactions may be occurring and accounting for these interactions in the experimental design could cause serious problems in interpretation of results. Similarly, emphasis also should be given to research that integrates across multiple, key research topics identified under a single H. Finally, once the Council has identified key topics, there should be room within those topics for individual projects to be proposed. This speaks to the need for an Innovative category in the Research Plan, to foster inclusion of topics and approaches that are not yet anticipated.

3) Do the overview sections accurately capture the current state of the scientific issue at an adequate level of detail to provide the necessary context for the research recommendations?

The overview sections and the associated research recommendations are too specific to serve their purpose beyond the immediate time horizon. A set of general critical uncertainties should be identified in the overview that can serve as the foundation for specific responses in the form of project proposals.

4. Do the research recommendations logically flow from the overview, the management questions, and critical uncertainties?

The detail and repetition within the document, the range of specificity in the presentation of the topics and research recommendations, and the variation in organization of topics make for a confusing document that lacks a central organizing framework. A clear framework for prioritization, balance, and flexibility in responding to emerging issues and knowledge also was not apparent. A better approach might be, as noted above, to simply develop the synthetic list of research problems or challenges (perhaps captured best as Critical Uncertainties) as the core content of the Research Plan and let the responses of project proponents and the development of RFPs be the means by which the specific research agenda is implemented from the more general statement of research needs (the Critical Uncertainties, in this example).

5. Are the research recommendations at an adequate level of detail to guide the development of RFPs, proposal development, gap analysis, and scientific review?

See comments on questions 1-4. A reading of the list of research titles suggests that many reflect an inventory of research that is underway or already planned. On the other hand, some recommendations are exceedingly general and will be of little value in guiding project selection. However, the opposite situation, with very detailed and specific research needs, is not desirable, as it gives insufficient room for creative or integrative responses and no opportunity for

flexibility to incorporate new information and techniques. These would guarantee early obsolescence of the Plan.

The ISAB and ISRP suggest that the plan should not be at a level of detail that allows a cut-and-paste RFP, proposal, etc., but rather that the processes of building detail to implement research areas and design specific projects should follow the research plan. The plan should give core topics and their justification, not the specific research detail. Moreover, the plan does not need to give specific details for scientific review of proposals. This would be unrealistic and counterproductive, as relevant scientific background and research-support technology are continually changing and scientific review must be based on current best scientific understanding, not only what is encapsulated in a previously written plan.

(2) Comments on Issues noted in the Council's request

Collaboration and the proposed Regional Research Partnership

See answer to Guiding Council Question 1, above.

Change the Organization of the Plan

One could organize a Research Plan for the Columbia River Basin in a variety of ways. However, the ISAB and ISRP strongly recommend that the Research Plan be kept to a general set of core topics that outline the broad questions, leaving open the approaches to addressing them, and avoid listing many specific topics. The ISAB and ISRP strongly recommend that the Research Plan not include a list of all specific projects that any partner might consider as a priority, and instead be focused on general overarching research needs and critical uncertainties. It appears that some of the awkwardness in the current draft plan derives from an attempt to force lists of projects into categories that represent the core topics of a research agenda. It will likely be more useful to begin instead with the critical uncertainties and allow them to mold the research agenda, as is their stated role, with the more specific details left to unfold over time as the Plan is implemented.

It also appears that a variety of suggested organizational schemes were forced into the plan. For instance, early in the draft, the concept of organizing research by mortality factors is introduced. However, this approach is not carried through the plan. The inconsistency of stating an organizing principle that then is not used adds to the overall confusion in the document. The mortality factor approach is not likely to lead to a good organizational scheme. Population dynamics reflect the balance and sequencing of production and loss, not just loss. It is very unlikely that elevated mortality is in fact the primary cause of decline in all fish and wildlife that are considered under the Fish and Wildlife Program. Much of the organizational difficulty may be resolved by adopting the approach of presenting a research agenda at a much higher level, rather than one dominated by many specific designated projects.

Plan Audience and Length

While the plan is a Council document, its purpose might be viewed as to inform the region as to the research topics of most significance in relationship to the Fish and Wildlife Program. The audience thus is relatively broad.

As noted throughout this ISAB and ISRP response, the Plan will be clearer, more accessible, and more useful if it is significantly shortened. The focus of the document should be on the core Research Agenda topics, and the Plan and its central topics must include sufficient flexibility to encompass innovative approaches and ideas. It may be desirable to put the lists of suggested specific research projects that is now included within the Draft Plan in an Appendix of suggested specific topics, but leave this level of detail outside of the core Plan. The specific projects are implementation details rather than core research needs, the approaches to which are likely to be much more varied, broad, and dynamic than the current detailed lists would foster or allow.

It is not practical to expand the Research Plan to include full syntheses of current knowledge on each core Topic. That would require a much longer Plan, and much of the information would quickly become dated. Brief reference to the literature, combined with outline of the core Topic and why it is important, should suffice to highlight the central issues while leaving open the challenge for investigators to broach creative approaches to advancing understanding of one or more Topics in a research project.

See also answer to Council Question 5 above, which addresses the level of background detail that is useful versus counterproductive.

(3) Comments on Section I. Planning for the Future, Taking Stock of the Present

This section should be condensed. The length of this section could be perhaps 3-6 pages, rather than the current 15. The historical background in Part I is good for setting the stage, but some rearrangement and rewording are needed if it is to make the case that the Council can serve as the home for a collaborative management and research program that includes the multiple entities involved in fish, wildlife, and hydrosystem mitigation in the Columbia Basin. The management aspects need more explicit mention, at least to say that the Council's mechanisms (public hearings, etc.) offer the opportunity for needed interface and coordination. Perhaps a more general introductory paragraph to this section could bring out that issue.

“Mandates for a Columbia River Basin Research Plan” (pp.1-3) is overly long and repetitious of “intent” for the plan from the Program and Governors, and, for each, restates the plan's objectives in various ways. Boiled down, the Program and governors agree that the plan should: identify critical uncertainties, formulate research recommendations, and identify priorities for funding. Not much more needs to be said, except a brief description of geographic scope, audience, etc.

Similarly, the “Background” (pp.4-7) could be condensed by perhaps 25-30% and more sharply focused, and “2005 Columbia River Basin Research Plan” (pp.7-10) also could be brief, without

as much historical detail, simply list other existing plans and enumerate the entities that contributed to this plan's recommendations. "Opportunities for Collaboration: Charting A Course for the Future" (pp.7-15) also could be combined with the above and condensed. The Regional Research Partnership and the collaborative and funding leveraging potential it represents could be discussed briefly in the context of plan implementation.

(4) Section II. Research Recommendations for the Columbia River Basin

a) Response to Council Questions

1. Should the critical uncertainties be presented in a uniform format, following the discussion of critical uncertainties on page 17?

Uniformity of format is needed for clarity of the Plan. The main suggestion of the ISAB and ISRP for organization of the critical uncertainties is that they be general and better integrated with other background and rationale for research topics. Redundancy across the several background sections (e.g., Critical Uncertainties, Management Needs, Past/Present/Future) should be eliminated by combining these into a single, more synthetic section that more clearly motivates the research needs.

2. Should the research recommendations remain organized as they presently are by topic and sub-topic e.g., for habitat, the research recommendations are categorized as physical, biological, or chemical?

The topical/subtopical organization in the Draft Plan is cumbersome and confusing and contains both redundancy and obvious omission. Much of the cumbersome detail in the long lists of specific topics (under the heading "Council's Research Recommendations") should be greatly synthesized. The lists of research recommendations should be removed and replaced with the simpler list of core topics and their description and rationale for emphasis.

b) Comments on Section II

This section contains the core scientific content of the Research Plan and should be made much more synthetic and general in focus. The plan should only identify and describe the key elements of the research needs. The section should be organized around a condensed set of central themes or topics, which present the top research needs, with a brief description of each in a common format (e.g., state of knowledge, critical uncertainties, recommended research), keeping the focus on broad research topics and the critical uncertainties related to them; they are what drive the needed research. Keeping description of these topics at a higher level should aid in organization of the section, allow flexible implementation of the plan, and will prevent the Plan from quickly becoming dated. Developing the identified themes into a work plan should be the task of the research partnership, and should be responsive to proposals that are developed and to the advancements of science and technology, as well as driving proposal solicitation. The themes

(which could be developed as white papers) and work plans should be peer reviewed, perhaps by the ISAB and ISRP.

This section has much repetition and redundancy, making the material difficult to read and understand. The introductory material, including a lengthy discussion of the material that will be presented later in the section, is unnecessarily detailed. Similarly, “Partners for implementation” could be listed once in the front matter of the report, but not described for each topic area and not discussed in the detail presented. The level of “past, present, future” detail is unnecessary in background presentations. The descriptions could be much more succinct; the plan should be a framework document guiding more detailed discussions of research funding allocation, etc., but need not contain all possible detail related to these topics. The description could be limited to the topic area and a synthesis of critical uncertainties affecting that area, which are enough for a strategic planning document of this type.

The list of management needs, critical uncertainties, and research recommendations should be shorter and focused on the major themes. The "Management Needs" sections in most cases simply repeat (in less detail) the items covered in the "Critical Uncertainties" section, and the repetition is confusing and distracting. Many of the research recommendations are repeated - the same research topic is stated in a different manner within the same topical section or is restated within a different topical section. As described above, these recommendation sections should be greatly reduced and reorganized under critical uncertainties (or Critical Management Uncertainties) that are more general than the current set. For example, a critical uncertainty could be "What are the responses of fishes to habitat restoration". A set of general research topics that fit under this heading could then be listed (e.g., what are the effects of nutrient augmentation on streams and fish populations), but these research items should be broad, not narrowly focused questions. The plan should not be a comprehensive list of all the possible research projects that are of interest to someone in the basin.

(5) Section III: Implementing Research Recommendations

a) Answers to Council questions

1. Is the discussion of allocation of research expenditures under the Fish and Wildlife Program relevant or distracting?

Allocation of research expenditures provides a context for the discussion of implementation. One central question is how much research realistically can be funded? With so little of the budget allocated to research, and given the likely cost of most research, few projects will be funded. This section of the Plan thus can help the Partnership adopt a realistic view of funding possibilities and apply this in deciding priorities. The comparison of current expenditures with research needs (topics identified as the core research agenda, or critical uncertainties) should be useful in decisions about future allocation of funds and effort to advance understanding and benefit fish and wildlife. The comparison of expenditures with needs also can contribute to development of schedules for termination of some ongoing projects. Such a large part of the budget goes toward funding ongoing projects that little funding is available for new projects,

limiting the ability of the FWP to respond to arising issues. The expected uses of the information in this section (e.g., those noted in this paragraph, or others) should perhaps be stated explicitly.

2. Should the detailed walk up to the gap analysis be included in the text or should the results of the analysis simply be presented in a table?

A brief description of how gaps were identified may suffice to demonstrate that the gaps are well thought out and could accompany a clear and self-explanatory table. The Gap Analysis should be redone after the Key Uncertainties are articulated at an appropriate and consistent level of detail.

b) Comments on Section III

This section should be limited to a discussion of current funding allocation, how it compares to critical uncertainties identified in the plan, and implications of the analysis for changes in funding or any other aspect of implementation (e.g. coordination). The tables and discussion of current funding allocations can be brief. “Project selection” could be limited to a brief description of how projects are selected now and a discussion of how this process might need to be adapted, if at all, to accommodate the research needs and partnerships identified in this plan. To avoid having the main points be lost, the section on “Integrating results” should present much less “background” detail and instead put more emphasis on the pertinent issues.

Appendix: Additional Specific Comments from Reviewers

The following comments represent a cross section of ideas from ISRP or ISAB members. They are provided to assist with revision of the draft Research Plan. The intention is not to imply that the revised draft need incorporate these specific comments. Rather, they are provided as background information that might serve to strengthen the foundation underpinning the revised Plan.

(a) Section I

Northwest Power and Conservation Council - Either in this subsection, or a new subsection, the parties responsible for fish and wildlife management in the river corridor and basin, and their interrelationships and responsibilities should be identified. The relationship between Bonneville Power and its funding of the Council program, ACOE AFEP, and NOAA ESA responsibilities, and other funds (Pacific Salmon Fund?) and other groups that manage the landscape (Forest Service, BLM, EPA (clean water act)) and spend money on research or implement actions need to be identified. The roles played by CBFWA, the compact, the federal caucus, and ODFW, WDFW, IDFG, and MFWP in the basin need to be identified. This should be described in two or three paragraphs. The important point is that complication created by multiple management authorities (above) and broad scope (below) generate the need for the research partnership and the Plan. The fact that the Council said they were going to write a plan in the 2000 FWP, and the request from the Governors, is not sufficient rationale for producing a plan.

Objectives, Audience, and Scope of the Columbia River Basin Research Plan - The Council said they would develop a research plan in the 2000 FWP, and the Governors requested a plan be developed in 2003. That said, there needs to be a clearer rationale for this effort. This document must state why a plan is needed, who is going to produce the annual workplan, and what the plan is intended to serve.

A Council research plan that develops links to other research efforts, so the Council can make improved decisions on the allocation of their FWP resources, is likely to be less threatening to other basin constituencies than one that looks like the Council is going to subsume their research program into the Council FWP and then direct its funding. There are several areas in Section I where there is pointed criticism at current research programs, but these assertions are not backed up by any analysis. Also, arguing that a Columbia Basin Research Plan under the auspices of the Council is needed because those currently executing research efforts have chosen subjects poorly may not be the first step to take in creating the collaboration the plan is intended to produce.

Regional Research Partnership - Under “A Forum for Collaborative Implementation and Funding”, the first paragraph states:

In the past, attempts have been made to convene executive level multi-agency groups for the purpose of coordinating resource management decision making across the Columbia River Basin. These unsuccessful efforts indicate that it may not be possible to convene a single “super group” that can address management decisions across all subject matter areas of resource management in the Columbia River Basin. This is in part due to

significant differences between programs in their missions, structures, proposal development, and proposal review processes. The region lacks a regional decision making forum that can arbitrate between competing initiatives to implement the All-H approach. Consequently, this plan simply recommends the convocation of a partnership to foster collaborative research.

There needs to be a stronger and clearer message sent. There appears to be equivocation on whether a “basin-wide”, rather than “FWP”, research plan is attainable, or advisable, and whether a Research Partnership is the way to manage such a basin-wide research plan. Nonetheless, the final sentence in the preceding paragraph is only weak rationale.

Regional Research Partnership – Collaborative Funding – This section states:

it is important to acknowledge the difficulty inherent in reprogramming existing Program implementation funds to support additional research initiatives, within the available direct program budget. This is not a question of how much investment in additional research BPA can afford, or the Program can sustain, on behalf of the region: but rather, how to develop a comprehensive regional research agenda that can be funded from multiple sources...

This is exactly about how much BPA can afford and the Program can sustain. Thus, there is a need to work cooperatively with entities that represent alternative funding sources, having responsibilities that overlap those of the Council. The argument from the Council to other regional entities needs to be that all the programs are limited by what they can afford and sustain, and that, if they are able to devise a scheme where they work together, all the programs can benefit from focused, coordinated expenditures. Rather than individually under-funding multiple projects in a category, perhaps they can jointly provide adequate funding for one or two well-designed studies.

(b) Section II

There was no mention of the ongoing (Wenatchee) and planned watershed RM&E projects. Identification of the issues being addressed by these studies could provide a basis for identifying those critical uncertainties that are not currently receiving attention. Some of the current supplementation studies (Yakima, Idaho Supplementation Studies) could be used in the same manner to identify gaps in the current approach to answering questions related to effects of hatcheries.

Implementation Scenario

Page 17, final paragraph. We recommend the schema of three mortality elements not be used. If it were to be used, then critical uncertainties would need to be clearly described and related to one of the three mortality elements. However, none of the critical uncertainties were linked to any of the three mortality elements. They need not necessarily be linked, but to say they are, and then not formally make the linkage renders the bullet point moot. The same problem occurs for

“there needs to be at least one research recommendation for each critical uncertainty” and “there should not be any research recommendations that do not relate to a critical uncertainty”. If the linkages are not made formal, the quoted text should not be used.

Research Topics

Twelve topic areas apparently are covered in this section: hatcheries, hydrosystem, habitat, estuary, natural variation and ocean productivity, harvest, recovery planning, monitoring and evaluation, impacts of climate change on fish and wildlife restoration, toxics, invasive species, impact of human development patterns on fish and wildlife restoration. However, note that reviewers had trouble figuring out which sets of headings in Chapter II were, in fact, the research topical areas. For instance (from one reviewer’s comment),

... Section II states that it introduces twelve long-standing and contemporary topics. But they are not listed in the introductory paragraph and, using the table of contents, I cannot figure out what those twelve are. There are seven items – hatchery effectiveness, hydrosystem, habitat, harvest, recovery planning, monitoring and evaluation, and emerging issues, as main items in the table of contents. Under these there are items that are either other programs – AFEP under hydrosystem, and five programs under M and E, or possible “topics” -- but these “subtopics” add to 14, not 12.

Habitat

The background section appears disorganized, perhaps reflecting an attempt to conceptually organize what began as lists of specific projects (many apparently ongoing or already planned) rather than as a core list of critical uncertainties, central and important research needs. For instance, there is a **biological processes** section, followed by **habitat refugia**, followed by **physical** and then **chemical processes**. It is not clear why the **habitat refugia** is stuck in the middle of these process sections. Also, many of the items discussed could possibly be placed under a different process. For example, almost the entire chemical process section was a consideration of nutrient inputs from salmon carcasses, which has as much to do with biological processes as chemical. Sections also are detailed, but omit many likely central potential research topics. For instance, the biological processes section does not appear to mention food webs, competition, predation, etc., which are biological processes. The section on habitat refugia only discusses bull trout refugia. What about refugia for anadromous salmon and steelhead? The critical uncertainties for tributary and mainstem habitat are not italicized lists as for the others. The Plan should strive for parallel treatment of the topics. Similarly, the estuary habitat section does not parallel the tributary and mainstem topic with the three processes and habitat refugia. The critical uncertainties are in a numbered list, but not italicized.

Hatcheries

The table of contents calls it “Hatchery Effectiveness”.

The overview for this topic does not get at the primary concerns and research needs very effectively. There could be a short paragraph on the “hatchery reform” efforts under way since

1998, that Congress directed the Council to undertake an Artificial Propagation Review, and how far along that process is. It also should be made clear that reform is a verb, meaning change (not solve), and that it is not an endpoint that once reached, results in hatchery fish being equivalent to wild fish. The essential issue for hatcheries is how detrimental are the releases from “segregated” mitigation and harvest augmentation programs to wild fish, owing to ecological interactions and interbreeding, and how detrimental are the supplementation programs to target and non-target natural populations, from ecological interactions and interbreeding.

Most of the material in the Present and Future sections of this topic should be deleted.

Management Needs: The paragraph following the bolded subtopic heading is fine. In the list of management needs – item 1 is fine; item 2. Change to “to what extent can mitigation hatchery replace the fisheries (harvest)....”; 3. Delete this one, which is just a rewording of the first two; Delete 4, which is just a rewording of the first three; 5. OK; 6. Are there combinations of habitat and hatchery practices that can be employed so self-sustaining spawning runs can be reestablished where natural fish populations have been extirpated or in newly restored or reconnected habitat. 7. An additional management need is to be able to predict the magnitude of detriment from segregated mitigation and harvest augmentation programs. 8. Development methods to predict and partition a balance of natural and artificial production at the subbasin, province, basin, and regional scale.

Critical Uncertainties:

1. Delete. Replace with: “The range, magnitude, and rate of deterioration of natural spawning fitness of integrated (supplemented) populations and the relationship of the deterioration with management rules, including the proportion of hatchery fish permitted on the spawning grounds, the broodstock mining rate, and the proportion of natural origin adults in the hatchery broodstock.” Omit much of the supporting text, which is not needed.

2. Delete. Just a reworking of 1 above.

3. Delete. Just a reworking of 1 and 2 above.

4. Keep as is. Delete supporting text, which is not needed or helpful.

5. Keep as is. Supporting text ok. The region may want to consider decreasing hatchery production when the ocean is in great shape to increase opportunities for rebuilding natural stocks if there is evidence that freshwater interactions are imposing density dependence on natural production.

6. Delete. Just a reworking of 4.

7. OK.

The following editorial suggestions provide an example of reducing the number, and refocusing the emphasis, for one set of the current draft Council Research Recommendations.

Wild-Hatchery Fish Interactions

1.1 **Develop an experimental design to estimate the impact of hatchery releases on the abundance and productivity of naturally spawning salmon and steelhead populations.**

- What are the competitive impacts of hatchery and wild fish in a system? Is competition between hatchery and wild fish a source of mortality for wild stocks in the Columbia Basin e.g., are early releases on fall Chinook competing with upriver transitory fall Chinook?
- What are the predation impacts of hatchery on wild fish in a system e.g., are hatchery releases predators on wild salmon and steelhead stocks?
- Does maintaining wild characteristics **in the hatchery population reduce the deleterious interactions?**
- **Delete this one, adding fish will not increase the productivity.**
- What are the effects of supplementation on resident fish?

1.2 **At what level does non-local origin straying, and interbreeding become a problem for natural spawning local stocks?**

1.3 **Delete this one, it is irrelevant**

1.4 How can we better prevent and treat bacterial kidney disease (BKD) and predict the likelihood of vertical transmission of BKD from a specific female?

Genetics

1.5 What is the relative lifetime fitness of hatchery and natural salmon? **Reviewers would delete this.**

1.6 **If hatchery fish quality and health be are improved to achieve increased survival and reproductive success after release, is the reproductive success (fitness) of naturally spawning integrated individuals improved in contrast to integration with hatchery salmon of lesser quality?**

1.7 **Delete We don't need to ask what the risks are, we need to measure them!**

1.8 How can hatcheries be managed so that genetic and life history characteristics of hatchery fish mimic those of natural fish? **This question may, or may not be related to the bullet points below. That is, managing hatcheries so the hatchery individuals mimic natural fish may not be relevant to the appropriate broodstock collection, and spawning protocols. Reviewers recommend deleting 1.8**

- What broodstock collection protocols are most appropriate for supplementation programs?
 - What spawning protocols are most appropriate for supplementation programs e.g., how should jacks be used in hatchery spawning to mimic their contributions in nature?
 - How should we adjust the ratio of wild and hatchery fish and different age classes spawning in nature above a weir?
 - What release strategies will best distribute hatchery adults on the spawning grounds?
- 1.9 What is the relative reproductive success of hatchery and natural adults in nature?
- How do culture practices influence reproductive success?
 - What are the genetic effects of hatchery programs where a small number of parents produce a majority of the offspring?

- 1.10 Test the assumptions about survival differences between hatchery and wild fish; i.e., has interbreeding of hatchery and wild fish reduced the survival of wild spawning fish? **This is imbedded in earlier questions, so can be deleted.**
- 1.11 Conduct empirical research to identify links between fish genetics and life history patterns and unique adaptations and properties. (Montana) **It is not clear what this means, in terms of an actual experiment. It appears to be imbedded in, so redundant with, the larger questions on supplementation, even if the context from Montana is resident cutthroat, bull trout, or white sturgeon.**
- 1.12 Assess the feasibility of using additional genetic markers i.e., PINES, to determine the genetic integrity of individual fish. (Montana) PINES of course are now old school. The latest and greatest are SNPs – single nucleotide polymorphisms. **We discourage putting such specific projects into a strategic Research Plan. If there is a research need to develop new genetic markers, then that could be an item. In this case, a lab at the U of Montana is the only group developing PINES, and this would be a project designed specifically for them. Designing projects to keep affiliated Universities in funds is not desirable.**
- 1.13 Identify all sources of native fish species that are genetically pure and free of all reportable pathogens. (Montana) **It is not clear how this relates to the elements of mortality, uncertainties, and management needs for artificial production.**
- 1.14 Determine the rate of domestication and re-naturalization of hatchery salmon populations. What are the long-term effects of domestication, what culture practices cause it and how can we minimize it? **This should be deleted as a specific recommendation and subsumed under a broad supplementation theme.**
- 1.15 Determine the exact timing of imprinting in juvenile resident salmonids, including native westslope cutthroat trout and bull trout. (Montana) **See comment on 1.13 above.**
- Techniques**
- 1.16 What techniques best maintain wild characteristics in a hatchery population? **See comment on 1.13 above. What is meant by “wild characteristics”.**
- 1.17 Do differing hatchery-rearing methods lead to different physiology, behavior, and life history patterns of hatchery products when contrasted to natural populations. Can any of these methods be used to maintain life history types in hatchery, natural and composite hatchery/natural populations. **This should fit under a broad supplementation theme.**
- 1.18 Evaluate methods to reestablish spawning runs where wild fish populations have been extirpated. (Montana)
- 1.19 Determine the long-term persistence of natural elemental signatures in fish scales. **(Low priority) See comment on 1.13 above.**
- 1.20 Improve the persistence of thermal marks at the focus of otoliths in swim up fry to allow for subsequent detection. Although lethal otolith sampling is required to detect marks, this technique may still serve a useful purpose for certain research applications. **(Low priority) See comment on 1.13 above.**
- 1.21 Assess the effectiveness of batch marking of fish scales using applied concentrations of microelements as an alternative to thermal marking techniques in hatchery research. **(Low priority) See comment on 1.13 above**

Hydrosystem

page 31 “Each project (dam) has multiple authorized purposes and uses, including migratory fish passage”

Is an authorized purpose of a dam to provide migratory fish passage? That may be an operational condition, but certainly not a purpose.

The two paragraphs at the top of page 32 are pretty critical of the Corps’ research program. The critical statements may be true, but they are not attributed to any analysis, say a GAO or ISRP report. It would be helpful to cite the basis for the criticism. It would also be worth considering rewording the text into a more positive proactive form, especially if one of the ultimate goals of this document is to foster a Research Partnership, which includes the Corps’ program.

(c) Section III

Page 76. last two sentences third paragraph. “In the future, the sponsors of restoration projects will be discouraged from proposing additional research and/or monitoring elements to their proposals. Concurrently, project sponsors will be encouraged to submit proposals for dedicated research projects.” These sentences are confusing and may appear to be incongruent with “Adaptive Management”. First, it is not clear what is meant by restoration projects versus dedicated research projects, nor is it clear how these would be integrated. Would a supplementation activity like the Chief Joseph summer Chinook program be considered “restoration” and not need any monitoring? Could habitat restoration projects, without monitoring, compromise the experimental design for addressing efforts to evaluate the effectiveness of alternative restoration approaches, or tier 3 experiments?

Second, under the adaptive management rubric, management implements their “actions”, but the implementation is executed and monitored in such a fashion that information is gained. If management actions are undertaken, but are not part of a larger scale experimental design and monitored, then nothing will be learned. Finally, the sentences above seem at odds with establishing a collaborative research framework for the Columbia River Basin. Perhaps the proposal quoted above is a response to the poor quality of the information that has been generated by requiring that monitoring be associated with every project, since some project sponsors do not have the technical resources to do a competent job of evaluating the effectiveness of a project. The approach proposed here may be to direct the available research and monitoring resources to a smaller number of projects that are well designed and have the intellectual and financial resources to generate useful information, which would be consistent with some of the past suggestions of the ISAB and ISRP.

Page 81. Beyond technical merit.

“The project review process currently benefits from CBFWA’s application of management criteria and from the ISRP’s requirement that projects “benefit fish and wildlife”.

Note that “Benefits fish and wildlife” is part of the ISRP criteria from the 1996 amendment.

Page 82. second bullet point. “All knowledge gaps should be initially considered as research needs for which projects will be sought based on sequential priority, rather than implemented concurrently.”

It is not clear whether the gaps will be treated sequentially, or the projects within a gap treated sequentially. What about the priority of topics?

Page 84. Project selection under the Corps’ fish program. These are statements of the Corps view, but what is the bottom line for this Research Plan. As it now stands this small section does not add much.

Page 86/87. Evaluating Research Results. This section has lots of good words in, but no concrete action. Four elements need to be hammered out by the Council and any potential research partnership:

1. Who decides the priority of the research agenda?
2. Who designs the experiments?
3. Why and how is the data collected, stored, and analyzed?
4. Who is responsible for synthesis of the results and identifying management implications?

The answers may not be known at this time, but these items need to be highlighted in the Plan and an outline provided as to who will resolve the execution of these elements.

(d) Priority Research Topics

Below, in arbitrary order, are some general topics and questions that reviewers suggested should be incorporated into the research agenda of the Plan. Some of them were covered to a limited extent in the M&E section of the draft, but were not given the attention they deserved.

Innovative Research: One of the things we had hoped to see highlighted more prominently in the draft research plan was the need for innovative habitat research. The section on page 83 (Innovative Project Reviews) should be highlighted more prominently, and there should be some way for proposals to be submitted apart from solicited RFPs by the Regional Research Partnership. Sometimes really good ideas come from people who come from outside those typically conducting research in the Basin.

Large-scale, long-term habitat trends: Despite all the money spent on habitat restoration, we don’t really know whether we’re losing habitat to poor land use practices, development, water quality degradation, loss of connectivity, etc., faster than we’re gaining it back through various restoration programs. People argue that we don’t have the tools to answer this question with precision, but if we’re willing to accept coarse-scale evaluation some broad patterns ought to emerge. Where are the locations of the biggest losses? Where is habitat recovering most rapidly? How do these locations match up with the life history needs of the fish and wildlife we’re trying to recover? The ICBEMP project was a good start, but it was largely limited to federally managed lands. It is time for the Council to fund another attempt to get at the big picture throughout the basin. This information would be tremendously useful in developing a sound, basin-wide restoration strategy.

Restoration Effectiveness: Washington State is investing heavily in the Intensively Monitored Watershed approach, which probably holds the most promise for evaluating effectiveness of restoration. How can the Council help to complement this effort? There are several possibilities:

1. by identifying the need for and funding intensively monitored restoration work in large river systems, which are not currently emphasized by Washington State. The John Day River, for example, might be a good candidate for such a study.
2. by providing supplemental funding for aspects of the Washington IMW research studies that are receiving inadequate attention – a good job for the Regional Research Partnership proposed in the draft plan.
3. by providing a venue for all stakeholders to participate in the IMW effort in Oregon and Idaho as well. This means getting these two States on board with what Washington is doing, and being sure that the tribes and federal agencies, as well as the States, are engaged in the work. The ISRP/AB understands PNAMP is making some progress on this collaboration and encourages further description in the Plan.

Habitat and Food Webs: Trophic support questions are underrepresented in comparison to physical habitat restoration issues. Having the best place to live doesn't do you much good if there's nothing to eat. How much do we really know about food webs in the Columbia Basin, especially in the tributaries? How have they been altered by land and water use, by the introduction of toxics and non-native plants and animals, by harvesting, and by climate change? If you don't think food is important, ask yourself this question – When was the last time a juvenile salmon in the wild grew as fast as a juvenile salmon in a hatchery? Habitat in salmon hatcheries is not exactly optimal, yet the fish can belly up to the all-you-can-eat buffet several times a day. Imagine what wild fish could do if natural food resources were increased.

Non-native Species: Exotic species might well be considered as “habitat”. This includes the tremendously important problem of invasive riparian plants. We tend to think of non-natives only in terms of fishes, mostly from eastern North America, stocked for recreational opportunities. While these are important, there are many other non-native plants and animals that could have a large impact on salmonid habitat productivity. They include aquatic plants (e.g., Eurasian milfoil), aquatic invertebrates (e.g., New Zealand mudsnail, zebra mussel), riparian plants (e.g. Japanese knotweed, Himalayan blackberry, giant reed), and riparian-associated animals (mostly livestock). Additionally, effective control measures for many invasive species are poorly known and deserve research attention. The Council has an opportunity to be proactive in supporting research in this area.

Climate Effects on Habitat: Short- and long-term climate variability has received a lot of attention. We now have a much better understanding of climate patterns in the Columbia Basin, including precipitation and temperature, but we haven't done a very good job of addressing the effects of climate variability on tributary and mainstem habitats. The issue goes beyond flow; floods and droughts will strongly affect many other aspects of freshwater habitat. Climate change lends itself well to modeling and field verification, and the Council ought to support a strong research program. Many of the recommendations given under “physical” and “chemical” processes should be related to climate variability.

Mainstem Flow Manipulation: Given the potential importance of flow and load following, isn't it time the Council forced the question and asked what *can* be done to better our understanding of the effects of mainstem flow manipulation on survival? This suggestion includes more experimental studies of all aspects of flow manipulation, not just load following.

Coordinated Experiments: There is no mention of some of the coordinated experiments that have often been proposed in ISAB and ISRP reports, or by the Council itself (such as studies of the flow/survival relationship of juvenile salmonids). Uncertainties related to supplementation, tributary restoration actions, mainstem passage and survival, and other issues have been discussed in many ISAB and ISRP reports. These reports provide suggestions as to how these uncertainties might be addressed. In most cases, these reports suggest that answers can best be obtained by coordinated experiments (e.g., the recent ISAB suggestion for the load-following experiment). The Research Plan needs to account for these key questions and experiments that have already been identified. The coordination of experiments should be a product of the Research Partnership. Key uncertainties or research topics that cannot be addressed by these experiments also could be identified and an approach for dealing with them would become one of the goals of the plan.

(e) Monitoring and Evaluation and Their Relationship to a Research Plan

New unique intensive research will arise as a result of interaction of: (1) current biological theory and (2) existing inventory data and data arising in mensurative experiments (Tier 1 and Tier 2 monitoring of populations/habitat status and trend).

In a Research Plan, there are certain ongoing research projects whose objectives have been identified by interactions of past data from relatively long term biological monitoring plans (perhaps well designed; perhaps not) with theory (perhaps well understood; perhaps not). A good example of this interaction giving rise to a research need is the data from relatively long term PIT tag/CWT fall chinook monitoring and theory of the life history of fall Chinook. Monitoring data recently indicated that some proportion of fall Chinook juveniles (Snake River and elsewhere) do not migrate to the ocean as sub-yearlings, and that Snake River fall Chinook that do not migrate as sub-yearlings may have higher smolt to adult return rates. Theory indicates that natural selection might have favored an increase in the segment of the population that does not migrate as sub-yearlings after the Snake River Dams were completed in the 1970s. A research project is needed to establish whether or not the portion of the population of returning Snake River fall Chinook adults, whose offspring over-winter in the Snake or Columbia Rivers, is increasing. A variation of this research need is stated in the Research Plan. A significant element of this research requires determination of whether, in spite of a possible higher smolt to adult return rate than the zero-age out-migrants, the holdover life history strategy might result in a net reduction in number of recruits compared to that which would have resulted from 100% outmigration as zero age fish. This could come about if the holdover fish experience a higher mortality rate in the reservoirs than they would have experienced in the ocean. In other words, it requires an estimation of their survival rate within the reservoirs, as well as an estimate of their smolt to adult return rate.

The Research Plan could identify and prioritize broad areas of research needs that have been identified by past biological monitoring and theory. This is basically the content of the current plan, but it is a little too specific.

The content of the current Research Plan is necessarily limited by the quality of the past monitoring data and understanding of theory (theory of genetics, nutrition, life history, Ricker curves, toxicity, etc.). If current monitoring data are biased by those “good old index sites”, the specific hatchery population being PIT tagged, the locations of creel samples, the methods for estimation of spawners or escapement, etc., etc., then the current Research Plan and future Research Programs are on a shaky foundation.

The foundation for a future Research Plan is an effective and economical relatively long term monitoring program that is grounded in basic theory; with the help of scientists who analyze the monitoring data and understand the theory. Thus, building a Research Program requires first building an effective and economical relatively long term monitoring program. Second, hire bright, well-trained scientists who know some theory. The Columbia Basin has some effective and economical components of a long term monitoring program, e.g., counts of returning anadromous adults at dams, estimates of number of out-migrating juveniles, harvest estimates, hatchery production, etc. The Columbia Basin could develop further important effective and economical components for a long term monitoring program: long term PIT tagging of important populations of anadromous populations, coordinated estimation of spawners or escapement into tributaries by standardized sampling and estimation methods, standardized habitat and water quality sampling and estimation methods, etc. The Basin also has some bright scientists, well trained in theory. The missing ingredients for an improved Research Program are better coordinated long term biological monitoring, cooperation among institutions, and improved communication among scientists.

The “Monitoring and Evaluation” section of the Research Plan starts off with the following: “In tandem, research and monitoring are two program elements that provide the basis for evaluation. Although often associated, they are different types of activities.”

Monitoring data can describe what happened; research is often needed to help explain why and how it happened.... Monitoring involves measuring and sampling physical, chemical, and biological attributes of the resources. Research involves analysis or experiments to establish mechanisms that explain observed correlations.

-- Comprehensive Monitoring, Assessment and Research Program,
CALFED, 1999.”

The Research Plan immediately comes back with confusing statements to the effect that monitoring can establish mechanisms that explain observed correlations, i.e., that monitoring gives rise to “research.” Monitoring in mensurative experiments can give rise to “research” in the sense that the mechanism/causes of observed effects are better understood. Those situations when mundane/pedestrian/everyday/economical, but absolutely necessary, monitoring (e.g., adult counts at dams, harvest surveys, weir counts, measurement of spill at dams, gas in tailraces,

status and trend of habitat and populations, etc.) do and do not give rise to “research” should be better explained.

A better approach in the Research Plan may be to establish the foundation for an effective Research Program along the lines of text above. Establish the need for long term biological monitoring that does not immediately give rise to “research”, then list the current effective long term biological monitoring programs in the Columbia Basin (e.g., adult counts at dams, CWT, harvest surveys, spill, juvenile fish passage, flow, etc.). Second, list and prioritize the needed improvements in long term biological monitoring programs (e.g., coordinated and standard methods for estimation of spawners/escapement, coordinated and standard methods for monitoring of habitat, etc.). Third, list and prioritize the needed new long term biological monitoring programs (e.g., PIT tagging of probabilistic samples of certain hatchery and wild anadromous populations, monitoring of avian predator populations, monitoring of marine fish and mammal predator populations, etc.).

The annual job of the Regional Research Partnership would be to update and prioritize current identified research needs and coordinate with research efforts of the various State, Federal, and Tribal agencies to try to ensure that Requests for Proposals are issued for the highest priority research needs. The current Research Plan could try to identify and prioritize broad areas of needed research, but it is probably unproductive to try to be very specific.