

Review of:

State Water Resources Control Board
California Environmental Protection Agency

“Technical Report on the Scientific Basis for Alternative San Joaquin River Flow
and Southern Delta Salinity Objectives”

By:

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As a reviewer, I was asked to consider a series of questions regarding the adequacy of the Technical Report. I list them here, and make comments that directly address them. I then provide a differently structured set of comments on the report, following my natural tendency to review reports in terms of an overall assessment and then a series of points that arose as I was reading the report.

1. Adequacy of the hydrologic analysis of the San Joaquin River (SJR) basin compared to unimpaired flows.

Changes in flow regime of the SJR and its three major tributaries:

The report reveals that the highest flow month of the year used to be May and in some years April or June but the highest flow month is now much more variable (Table 2.6). There are now much lower mean flows from January to July but actually higher than normal flows in August to December (Table 2.5). There are fewer peak flows now than in the past (Fig. 2.1, 2.2, 2.7). The SJR's flow used to be almost entirely comprised of the three main tributaries but their contribution is smaller in recent years (Fig. 2.8).

P. 39 "Like Vernalis, spring flows in each of the major SJR tributaries have been significantly reduced while flows during late summer and fall (generally August to November) have increased, resulting in less variability in flow during the year. Additionally, the year to year variability in winter and spring flows has been greatly reduced. Boxplots for each of the tributaries (Figure 2.10 through Figure 2.14) depict the median, 25th percentile, 75th percentile, and the wettest and driest months for 1984 to 2009. These graphical comparisons of the unimpaired flow and observed flows demonstrate the magnitude of alteration in the timing, variability, and volume of flows. Flows are much lower, primarily during the wet season, and with much less variation from year to year and within the year."

Hydrodynamics downstream of Vernalis

Page 52:

"Flow conditions downstream of Vernalis are largely affected by export operations of the two major water diverters in the Delta, the USBR and the DWR. The USBR exports water from the Delta for the CVP at the Jones Pumping Plant and the DWR exports water from the Delta for the SWP at the Banks Pumping Plant. In addition to these pumping plants, there are many smaller local agricultural diversions in the southern Delta that can affect flow conditions (State Water Board 1999.)"

Page 55: Reverse flows

"SWP and CVP pumping operations also increase the occurrence of net Old and Middle River reverse flows (OMR) reverse flows. OMR reverse flows are now a regular occurrence in the Delta. Net OMR reverse flows occur because the major freshwater source, the Sacramento River, enters on the northern side of the Delta while the two major pumping facilities, the SWP and CVP, are located in the south. This results in a net water movement across the Delta in a north to south direction along a network of channels including Old and Middle Rivers. Net OMR

is calculated as half the flow of the SJR at Vernalis minus the combined SWP and CVP pumping rate (CCWD 2010). A negative value, or a reverse flow, indicates a net water movement across the Delta along Old and Middle river channels towards the CVP and SWP pumping facilities.”

“Net OMR reverse flows are estimated to have occurred naturally about 15 percent of the time before most modern water development, including construction of the major pumping facilities in the South Delta (Point A in Figure 2.16). The magnitude of net OMR reverse flows under unimpaired conditions was seldom more negative than 2,000 cfs. In contrast, between 1986 and 2005 net OMR reverse flows occurred more than 90 percent of the time (Point B in Figure 2.16). The magnitude of net OMR reverse flows may now be as much as -12,000 cfs.”

As Fig 2.16 reveals, the magnitude of reverse flows has increased markedly over time. I am not a hydrologist by training but I found these sections to be very helpful in establishing the overall “plumbing” of the system and revealing the major changes in water that have occurred over the years. I would characterize the section as more than adequate. A strong case is made for the significance, at least in physical terms, of the changes.

- 2. Determination that changes in the flow regime of the SJR are impairing fish and wildlife beneficial uses**
- 3. Appropriateness of the approach used to develop SJR flow objectives**
- 4. Determination that more flow of a more natural spatial and temporal pattern is needed for fish and wildlife beneficial uses**
- 5. Appropriateness of using a percentage of unimpaired flow as the proposed method for implementing the flow objective**

The bulk of my assessment dealt with these questions, and I will try to summarize my conclusions here, followed by much more detailed comments and suggestions below. As I discuss below, the report itself shows some equivocation on the issue of how important other factors (e.g., marine processes) are in determining the overall population status and trends of steelhead and Chinook salmon. On the one hand, there is no doubt that the ocean plays a very large role in survival and growth of salmon and varies greatly from year to year. However, the river’s flow regime has been so radically altered that I have no hesitation whatsoever in agreeing with the report’s conclusion that the changes are impairing the river from the fishes’ standpoint. The approach taken is essentially to estimate, model, and otherwise reconstruct the pre-development (“unimpaired”) flow regime. As noted, I am not a specialist in hydrology by any means but the approach makes sense to me and the logic can be followed. More fundamentally, the approach of comparing observed to unimpaired flows seems like the correct one if we are to

understand the ways in which fish have been affected by the changes. This is not to say that all pre-development conditions are ideal for fish, wildlife and other natural resources. We are all well aware that nature can be harsh and often sub-optimal. So, we need to consider the ways in which the changes have moved the river towards a condition that is more or less favorable for the fish species. I find the report very convincing in its conclusion that, while there are other stressors to fish, a more natural flow regime is necessary if the fish are to recover. Indeed, I would further conclude that the other stressors such as contaminants and non-native fishes will be less consequential for salmon and steelhead in a more natural flow and thermal regime, so the benefits of flow enhancement will likely be both direct and indirect.

The report concludes that the shift to a more normal flow regime will be beneficial for the two fish species, as the status quo has much less water during some times of the year and somewhat more water than would be normal at others. The connections between flow and fish ecology are numerous and intricate, especially for fishes with the complex life history patterns of salmonids (e.g., obligate or facultative anadromy). Life history models that chain together a series of mortality rates in isolated stages of ontogeny without considering density dependence often miss the mark, and I am surprised to learn how many conspicuous data gaps seem to exist.

Given these complexities and uncertainties, I think the approach (percentage of unimpaired flow) is a very reasonable and defensible one, and the models showing 20%, 40% and 60% are revealing. Inevitably one can argue (or quibble) over which precise value to use. Perhaps a quantitative model could be created to evaluate the variants precisely but my examination of the plots indicates that this is very good compromise. It takes into account the fact that water years vary, and the needs of the fish vary seasonally with different life history stages.

You requested that reviewers consider several other topics, listed below (as extracted from the peer review request letter). Several of them are simply not within my ken such as those dealing explicitly with salinity, effects on crops, etc. The last one, "other issues" can be taken pretty broadly. While reviewing the document I had a number of thoughts and they appear below, along with more detailed comments on the text, references, etc. I intend all these comments to be constructive and hope they are taken in that context.

6. Appropriateness of the proposed method for evaluating potential water supply impacts associated with flow objective alternatives
7. Sufficiency of the statistical approach used to characterize the degradation of salinity conditions
8. Sufficiency of the mass-balance analysis
9. Determination that the methodology and conclusions regarding acceptable levels of salinity are appropriate for protection of agricultural beneficial uses
10. Other issues.

Overall assessment:

This report is well-written and organized, and presents a great deal of information in a readable and comprehensible manner. The graphs are largely of good quality, though a few have been copied and lost some resolution in the process. There are few typographical errors and it is generally well-produced. My expertise is strictly in the areas of fish ecology and conservation, and I therefore found it somewhat unexpected to have the heavy coverage of fish-related issues in much of the report followed by the final two sections (4 and 5, on salinity and flow) with no mention of issues related to fish. I assume that this was a design feature rather than an oversight, but the juxtaposition of fish ecology and salt tolerance of crops was a bit striking. Needless to say, both depend on water and so that is the fundamental unifying resource. I wonder if it might be possible to make this separation of these a bit more clear somehow in the organization of the report, perhaps Part 1 and Part 2, or something like that.

In general the report relies too heavily on secondary sources (e.g., Moyle 2002; NMFS 2009a, 2009b; Williams 2006). There is nothing wrong with these references *per se* but their use compels the reader to get that reference and find the relevant place in it. In cases where the secondary source is lengthy or not readily available, this is no small task. In addition, the referencing of work outside the basin and outside California is limited. I understand that the report has a sharp focus on the San Joaquin River but there are a number of places where work done elsewhere would be relevant. I have made specific suggestions below.

In terms of conclusions, the report makes a strong case that the shortages of salmon and steelhead are in large part related to the heavy modification of this river system. The mean flows and variances in flow that are normal in rivers of this region and for which the fish evolved have been radically altered (see more detailed comments below). It seems likely, however, that other processes have played a role over the years in the decline of these fishes, and will continue to hinder their recovery. Some of these processes may be synergistic with flows such as, perhaps, chemical contaminants or predation in streams, whereas other may operate independently such as fisheries management, ocean conditions, predation by marine mammals, etc. Regardless, several distinct life history stages of salmonids show some form of density dependence, making it difficult to tease apart the effects of one process or another. I understand that this report was not designed to address these other issues. It is worth noting, therefore, that my review also does not attempt to integrate these other consideration into an overall assessment of the efficacy of flow changes on the prospects for recovering salmon and steelhead in this system. Notwithstanding this limitation, there are many comments that can be made on this report and my format (below) is to identify sections or quote from passages that are especially relevant and comment on them. They are presented in the order in which they appear in the report. It is hoped that by highlighting aspects that were especially informative, their role is acknowledged. Perhaps more

importantly, if I have misinterpreted the key data in some way, by linking my comment with the source of information it will make my errors more evident, and thus easier to ignore.

Scientific Basis for Developing Alternate San Joaquin River Flow Objectives

Page 57

“ The State Water Board has determined that higher and more variable inflows are needed to support existing salmon and steelhead populations in the major SJR tributaries to the southern Delta at Vernalis. This will provide greater connectivity to the Delta and will more closely mimic the flow regime to which native migratory fish are adapted. Water needed to support sustainable salmonid populations at Vernalis should be provided on a generally proportional basis from the Stanislaus, Tuolumne, and Merced Rivers. Flow in the mainstem SJR, below Friant Dam, for anadromous fish will be increased under a different regulatory and cooperative water management program (SJRRP 2010). ”

Page 58

“ The SJR basin once supported large spring-run and fall-run Chinook salmon populations; however, the basin now only supports a steadily declining fall-run population. Scientific evidence indicates that in order to protect fish and wildlife beneficial uses in the SJR basin, including increasing the populations of fall-run Chinook salmon and Central Valley steelhead to sustainable levels, changes to the altered hydrology of the SJR basin are needed. Specifically, a more natural flow regime, including increases in flow contributions from salmon bearing tributaries (Stanislaus, Tuolumne, and Merced Rivers), is needed during the February through June time frame . ”

As noted above and discussed below, there are likely many factors affecting salmonids in this system but it seems likely that the flow regime changes have contributed greatly to the decline of these fishes, and rectifying this problem is probably necessary for recovery. Whether it is sufficient for recovery is a more complex question. The text in this section is clear and the presentation of data certainly adequate.

Page 57

- Observed flow is the measured streamflow recorded at USGS gages located at the most downstream location for each of the major SJR tributaries and at Vernalis.
- Unimpaired flow is a modeled flow generally based on historical gage data with factors applied to primarily remove the effects of dams and diversions within the watersheds. The modeled unimpaired flow does not attempt to remove changes that have occurred such as channelization and levees, loss of floodplain and wetlands, deforestation, and urbanization.

- Flow regime describes the characteristic pattern of a river's flow, quantity, timing, and variability (Poff et al. 1997). The 'natural flow regime' represents the range of intra- and interannual variation of the hydrological regime, and associated characteristics of magnitude, frequency, duration, timing and rate of change that occurred when human perturbations to the hydrological regime were negligible (Richter et al. 1996, Richter et al. 1997, Poff et al. 1997, Bunn and Arthington 2002, Lytle and Poff 2004, Poff et al. 2010).
- For the purposes of this report, a more natural flow regime is defined as a flow regime that more closely mimics the shape of the unimpaired hydrograph.

Salmon and Steelhead Biology

Chinook salmon biology

The section on Life History contains some errors and needs better references. The terms "ocean-type" and "stream-type" date to Gilbert (1913) and should ideally be linked to the reviews by Taylor (1990) and Healey (1991). The seasonal return patterns of adult salmon (e.g., fall and spring) do not necessarily correspond to the juvenile life history traits (ocean-type and stream-type). This is a common misconception; in many cases they are linked but it is best to use each set of terms for the life history phase to which it refers. In addition, the juvenile life history descriptors (stream-type and ocean-type) also include quite a lot of variation driver by with both genotype and phenotypic plasticity.

The proportions of males and females by age is a very important set of data and statements about them should be backed up with tables of data indicating the sample sizes in each year, etc. I comment on this later; the importance of this basic life history information cannot be over-stated.

The use of olfaction to locate natal streams deserves better citations than (NMFS 2009a, DFG 2010a). It would be better to cite Hasler and Scholz (1983) or perhaps Dittman and Quinn (1996).

P. 70

The statement "However, if natal streams have low flows and salmon cannot perceive the scent of their natal stream, straying rates to other streams typically increases." demands more details. There should be information on this important feature of the adult phase and appropriate references. I was surprised to find that there have been no tracking studies on the movements and travel rate of salmon in this system. Can this be true, and if so, why have none been done? This is off-the-shelf technology and clearly important to inform management in many ways.

I also have some sense (though I confess to not being sure precisely where I learned it) that there are much higher straying rates from the SJR than are considered normal, and that these result from transportation of hatchery juveniles downstream, and also from the difficulties that returning adults experience in detecting odors, given the altered flow regimes. Forgive me if I am mistaken in this regard but if there is any truth to the statement that straying is more prevalent than is normal, this certainly merits more attention in the report. There should be coded wire tagging data from the main hatcheries, I would think, and the analysis of them should be simple

at a first cut. The links to flow would seem to be obvious. In addition, if straying rates are above normal, then the use of fish in streams to indicate natural production and the presence of fish in hatcheries to indicate hatchery production is really questionable. Such assumptions rest strongly on the idea that all salmon return to their natal site. There are other situations (e.g., Pascual et al. 1995) where “pathologically high” straying rates have been observed, and this might be mentioned. There is also more recent work on the mid-Columbia River populations by Richard Carmichael of Oregon Dept. of Fish and Wildlife on abnormally high rates of straying, which seem to be related to transportation and also thermal regimes. For example, steelhead from the Snake River enter into the Deschutes River during their upriver migration and many are caught there by anglers or simply stay in the Deschutes and do not make it to their natal sites to spawn.

The statement that “streamflow alteration, dictated by the dams on the major SJR tributaries, affect [sic] the distribution and quantity of spawning habitat ” seems to call for more information. Presumably, the dams have reduced the sediment transport patterns but some detail and references to this would be helpful, or at least an explanation of the processes. The peak flows will play a role in these kinds of sediment transport processes. Is there a loss of intermediate gravel sizes, leaving cobbles and silt? Has the gravel become embedded and so less suitable?

Figure 3.1, which seems to be copied from the NMFS BiOp, needs a proper caption; as is, it is hard to interpret.

Figure 3.2 is quite interesting. Are there similar data for other years, and if so, perhaps a summary table or figure could be produced. Are the redd counts referring to new redds, or all that were counted on each survey? Were they flagged, and so how does the total redd count relate to the number of live fish? Were there tagging studies of stream life and generation of “area-under-the-curve” estimates? In general, I find myself wanting more detail about this kind of data.

Population Trends

Chinook salmon

P. 74

“ Escapement numbers for the three tributaries are generally similar in many years, suggesting that the total returning salmon may split into the three tributaries uniformly, or that the success of salmon from each tributary is similar. However, in general, the Tuolumne population has been the highest and the Merced population has been the lowest.”

A table with a matrix of correlations of annual estimates would be very useful. Figure 3.4 is striking but it would still be good to see the matrix, and a plot of each population against the others.

Page 75

“ The annual (fall) escapement of adult fall-run Chinook salmon is really three cohort sequences, based on the typical three year return frequency (e.g., cohort “A” returning to spawn in 1952, 1955, 1958, etc; cohort “B” returning to spawn in 1953, 1956, 1959, etc.). ”

Where is the evidence for this? I have gathered that the Chinook salmon are dominated by age-3 fish but this is such an important and basic point that it cries out for tables with the data. I'd expect to see age composition data, for each of the populations, as well as a quantitative separation of wild (naturally produced) and hatchery origin fish. Surely there are long-term age data from marked fish in hatcheries and wild fish on spawning grounds?

Mention is made of the fact that the escapement does not measure productivity because the fishery is not included. This seems quite surprising to me. Where are the catch data, and why is there no formal run reconstruction and set of brood tables? I do not mean to be harsh but the data on the salmon seem to be really limited. Surely there are coded wire tagging programs at the hatcheries and reconstructions of the runs? How else can the runs to the Sacramento be separated from the SJR? This is really basic information.

" ... since 1952, the average escapement of fall-run Chinook salmon has shown a steady decline. "

This statement is contradicted by the figure (3.5) associated with it. There is no obvious trend downward but rather there are a series of pronounced peaks (a pair of peaks around 1954 and 1960, then discrete ones around 1970, 1985, and 2003). Each of the peaks lasted about 8 years, with distinct "troughs" in between. I think the conclusion that this was a "steady decline" is not supported. Can there be some more sophisticated analyses? What we have seems like a visual examination. What can we make of these peaks and troughs?

Page 76

" There was no separation between hatchery and natural salmon that returned to the hatchery; the same is true for hatchery and natural salmon that spawned in river. "

Really? The use of the term "hatchery" to refer to fish entering the hatchery, and "natural" to those spawning in the rivers (Greene 2009; Figure 3.6) is inconsistent with the common usage of these terms. Naturally produced fish may be drawn into the hatchery, and hatchery produced fish spawn in rivers (Quinn 1993). These two processes are so common that only an assessment of marked and unmarked fish (e.g., thermal banding of otoliths, adipose fin clips, etc.) would be meaningful. Has there really been no systematic assessment of the proportions of salmon produced naturally and from hatcheries? If not, it is no criticism of the report but this important matter should be made explicit.

Page 77

A series of monitoring efforts are listed but data from them are not readily apparent. Why were the data not incorporated into the report? Are the patterns reported elsewhere in a comprehensive manner, and if so, what are the conclusions?

- Adult Chinook Salmon Escapement- DFG
- CWT Releases/Recapture- Cramer and Associates
- CVP and SWP Salvage- USFWS and DFG
- Mossdale Trawls- DFG

- Chipps Island Trawls- USFWS
- Beach Seines- USFWS
- Rotary Screw Traps on each of the major SJR tributaries- DFG, AFRP, Cramer and Associates, and TID
- Fyke Nets- DFG
- Ocean and Recreational Harvest- Pacific Fisheries Management Council

Central Valley Steelhead (P. 77)

I believe that it was Busby et al. (1996) who proposed the stream-maturing vs. ocean-maturing distinction, so that report should be referenced in this context. As far as life history differences, I would certainly add the fact that steelhead/rainbow trout are spring spawners whereas Chinook salmon are fall spawners. The former spawn much smaller eggs with a shorter incubation period, typically on the ascending temperature regime, whereas salmon spawn larger eggs with a longer period during a descending temperature regime. This is very important in the present context because it determined what period of the year (and thus flows) they will be in the gravel as embryonic stages.

The statement that “there is no reproductive barrier between resident and anadromous forms” with a citation of Zimmerman et al. (2009) needs a lot of qualification. I re-read this paper and was unable to find such a statement from the authors. I quote from the paper below:

“With such a small sample size we are unable to draw conclusions about the contribution of progeny of rainbow trout females to the emigration of smolts. Similarly, in presumed steelhead smolts collected in an estuary of a small central California coastal stream (Pilarcitos Creek at Half Moon Bay), juveniles of both steelhead and rainbow trout maternal origin were present (C. E. Zimmerman, unpublished data). Further work is needed to assess the contribution of rainbow trout progeny as smolts and the fate of these fish compared with smolts of steelhead maternal origin.” p. 288

It should be noted that work such as that by Zimmerman et al. (2009) relies on the fact that the core of the otolith reflects the environment in which the mother was rearing during the maturation process. Thus the offspring of steelhead and rainbow trout mothers can be distinguished. This says nothing about the father, and assessment of the genetic basis for anadromy and residency in a complex matter. Certainly, there are studies that indicate some exchange between rainbow and steelhead, but I think this should be approached in a careful manner and one should not go beyond the evidence.

The report states that all San Joaquin River steelhead are ocean-maturing (“winter”) fish but it then states that they enter as early as July. Surely this would be a stream-maturing or “summer” fish? Perhaps there are remnants of this life history form still in the system? I am also intrigued by the statement that “If water quality parameters and other environmental conditions are not optimal, steelhead may delay migration to another more suitable year.” Does this refer to adults or smolts? I had not been aware that there was evidence of adult steelhead returning to freshwater but then going back to sea without spawning because conditions were not favorable. It would seem that this important point (with respect to flow, temperature, etc.) should have

some reference and details, regardless of whether it deals with smolts or adults. The work by the NMFS group on Scott Creek is relevant to the issue of age composition and complex smolt migration patterns (i.e., fish that do not exit the lagoon – work by Morgan Bond, Sean Hayes and others).

The description of steelhead life history is basically correct but I am surprised that there was no figure quoted for the proportion of repeat-spawning steelhead in the system. Only a very dated figure from Shapovalov and Taft (1954) is cited and, if I recall correctly, their report was for small coastal streams. Are there no contemporary or historical data for the Central Valley runs?

P. 79

The terms potadromous and limnodromous are probably unnecessary jargon, and “fluvial” and “adfluvial” are more commonly used in any case.

Page 80

“The limited data that do exist indicate that the steelhead populations in the SJR basin continue to decline (Good et al. 2005) and that none of the populations are [sic] viable at this time (Lindley et al. 2007).”

This latter is a very strong statement and could use some elaboration. Presumably, the implication is that only exchange with resident trout maintains the steelhead phenotype. This should be stated more explicitly, and the biological basis for this exchange merits discussion. I am surprised that the interesting recent papers on California *O. mykiss* were not cited (e.g., those by Satterthwaite, Mangel and co-authors), nor relevant papers from elsewhere (e.g., Narum and Heath). This is not merely a matter of getting some additional references but it is fundamental to the status and recovery prospects for these fish. If the anadromous life history is latent in the resident trout then changes in environmental conditions may allow it to express itself, whereas if the forms are very discrete, as is the case with sockeye salmon and kokanee (the anadromous and non-anadromous forms of *O. nerka*: e.g., Taylor et al. 1996), then the loss of one form is likely more permanent. This extent of plasticity is directly relevant to the efforts to address the chronic environmental changes to which these fishes have been subjected, and the prospects for recovery.

It is also worth noting that the migratory behavior of steelhead differs markedly from that of sub-yearling Chinook salmon. Sub-yearlings spend a lot more time in estuaries and littoral areas whereas steelhead seem to migrate more rapidly (as individuals), exit estuaries quicker (as a population), and occupy offshore waters to a much greater extent. There was extensive sampling in the Columbia River system by Dawley, McCabe and co-workers showing this, and many references to the use of estuaries.

The summary of the importance of spring flows for Chinook salmon seems very reasonable but it would be good to actually see more of the data on which these statements are based. What relationship might there be to pre-spawning mortality or incomplete spawning of adults, or egg-fry survival?

Figure 3.8 would be better expressed after adjustment for the size of the parent escapement and some density-dependence. Plotting numbers of smolts vs. flow suggests a connection but I would think that multi-variate relationships should be explored.

Page 84-85.

“ In a 1989 paper, Kjelson and Brandes once again reported a strong long term correlation (R_2 of 0.82) between flows at Vernalis during the smolt outmigration period of April through June and resulting SJR basin fall-run Chinook salmon escapement (2.5 year lag) (Kjelson and Brandes 1989).

This relationship should be easy to update and I would like to see the recent data. Frankly, I find this correlation implausibly high. There are so many factors affecting marine survival that even a perfect estimate of the number of smolts migrating to sea will not have an R^2 of 0.82 with total adult return, much less with escapement (including both process and measurement error). I do not doubt that higher flows make for speedier passage and higher survival, but to link them so closely with adult escapement is stretching it. Indeed, it would seem that NMFS (2009) came to a similar conclusion. After acknowledging the shortcomings in this approach, it seems odd to see Figure 3.10, which is a time-series with flow during the smolt period and lagged escapement. If we much have escapement as the metric rather than smolt survival, can we not at least plot flow on the x-axis rather than date, and some form of density-adjusted recruit per spawner metric on the y-axis? I find it very difficult to see the relationship when plotted as time series.

Figure 3.12. This figure is a poor quality reproduction, and the y-axis is not defined. What is CDRR? (It is not in the list of acronyms). This report is pretty dense in terms of jargon and acronyms and abbreviation, so any effort to state things in plain English will be appreciated.

The text on the Importance of Flow Regime (3.7) is very sensible. It would be helpful to know what sources of the salmon mortality are most directly affected by flow reduction but, given the obvious data gaps, this seems unlikely. Thus overall correlations with survival and basic ecological principles have to carry the day. The text on fish communities, however, is rather confusing. I expected to see information of species composition, comparative tolerances to warm and cool water by various native and non-native fishes, ecological roles with respect to salmon, etc. However, there was a shift to population structure and importance of genetic and life history diversity for the success of salmon. This text (which would benefit from basic references such as Hilborn et al. 2003 for sockeye salmon, and the more recent papers by Moore and by Carlson on salmon in areas more extensively affected by humans) is fine but the reference to variable ocean conditions and marine survival seems to contradict the earlier statements that only smolt number going to sea really matter. Overall, I think this holistic view is more tenable than one only emphasizing the link between flow and smolt production. There is no question that marine survival varies from year to year but all you can ask from a river is that it produce juvenile salmon.

With respect to water temperature, the relationships between physical factors (local air temperature, water depth, solar radiation, groundwater, and heat loss, etc.) are quite well understood so it should be possible to hind-cast the thermal regime that would have occurred in

the SJR and its tributaries had the dams and diversions not taken place. An approach such as the one described by Holtby and Scrivener (1989) might be very useful and more precise than just saying that releasing more water would cool things down.

The section on water quality (3.7.6) should be better integrated into the arguments related to flow. As it is, we have a list of effects and possible connections to salmon but no way to link to the rest of the report. For example, salinity seems very likely to be a function of discharge but we are not given the relationship, much less the connection to salmon. Pesticides are probably prevalent but what will their interaction be with flow? Will more water reduce their effects, and will the patterns be linear or not?

Delta Flow Criteria

“Finally, the relationship between smolts at Chipps Island and returning adults to Chipps Island was not significant, suggesting that perhaps ocean conditions or other factors are responsible for mortality during the adult ocean phase. ”

This statement, referring to DFG data, also seems to contradict the earlier statements that marine conditions do not matter and that flow is all that matters. It would seem more correct to state that flow is the most important, among the things under our control.

On Table 3.15, it would be very helpful to present the status quo, so we can see the difference between the flows that DFG concluded are needed to double smolt production from present levels.

Page 105

“ State Water Board determined that approximately 60 percent of unimpaired flow during the February through June period would be protective of fish and wildlife beneficial uses in the SJR. It should be noted that the State Water Board acknowledged that these flow criteria are not exact, but instead represent the general timing and magnitude of flow conditions that were found to be protective of fish and wildlife beneficial uses when considering flow alone. ”

This would seem to be a critical, overall conclusion: Higher and more variable flows are needed, and can be ca. 60% of unimpaired flows. This is logical and well supported by basic ecological principles, as these flows would provide benefits specific to salmon at several life history stages, and broader ecosystem benefits as well. The various exceedance plots (Figures 3.15 to 3.20) indicate that there is substantial improvement from flow at the 60% level whereas 20% and 40% achieve much less in the important late winter and early spring periods. As the report correctly notes, this is inevitably a bit arbitrary (why 60% - might 59% not do just as well?). Just as with agriculture and wildlife, fish production depends on complex interactions among a number of factors, of which flow is very important but not the only one. Extrapolation from lab studies to the field, where so many things go on at once and where history cannot be played back in a different scenario. So, one can pick at this value, just as one might pick at any specific value, and ask whether the fish can get by with a little less overall, or at some time of the year. Likewise, how much water do crops really need? Can we give the farmers less without hurting production? Obviously, that would depend on soil, temperature, distribution of the water, insects (beneficial and otherwise), and many other factors too. I think that this value (60%) is well-supported, given these kinds of uncertainties. The fish would probably benefit from even more

water, but they will be more than glad to get this amount, as it will be a big improvement over the status quo.

Page 108

“Given the dynamic and variable environment to which SJR basin fish and wildlife adapted, and imperfect human understanding of these factors, developing precise flow objectives that will provide certainty with regard to protection of fish and wildlife beneficial uses is likely not possible. Nevertheless, the weight of the scientific evidence indicates that increased and more variable flows are needed to protect fish and wildlife beneficial uses. ”

I agree completely – this is very well-stated.

4. Salinity (pages 113-126)

The report has so much effort devoted to salmon and steelhead that the absence of reference to these fishes in the section on salinity is stark. Are there no issues related to estuarine dynamics or salinity related to salmon?

5. Flows

Same as above for salinity.

Suggested additional references, or references mentioned in the above comments

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