Review of
Draft Policy for Maintaining Instream Flow in Northern California Coastal Streams

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As an economist with an interest in policy making in conditions of uncertainty, the main concern in my review is whether the scientific basis for the policy appropriately addressed the underlying uncertainty in setting its standards. I address in my comments below my assessment of whether the scientific portion of the proposed rule is based upon sound scientific knowledge, methods, and practices, focusing primarily on the eight points indicated in the Description of Scientific Issues to be addressed by Peer Reviewers.

1. Setting seasonal limits.

For the most part, I believe the analysis supports the Policy's recommendation for a diversions season from 10/1-3/31. As is true for virtually all the recommendations, I was frustrated by the lack of formal and/or statistical treatment of the uncertainty. There was also only limited discussion of the possibility of extending the seasonal limits beyond 3/31, and I was unable to tell whether there was truly scientific consensus on the end of the season.

To the extent that this recommendation does build on numerical or statistical results, I believe that the analysis is incomplete. It is stated in Chapter 5 that "Comparison of the results of the different Flow Alternative Scenarios in Appendix I indicates that the diversion season length has less influence on passage opportunities than the MBF and MCD." I am *not* able to draw this conclusion from the data presented. In the five scenarios presented, only alternative 4 includes DS3, in which diversions begin in October and this scenario is also the only one that includes MBF4 and MCD2. Based on the data presented in Appendix I, it not possible to confirm that season length does not affect the number of days available for passage or spawning. The authors do use some results from other scenarios to argue that in general the DS increase does not significantly affect spawning and passage opportunities. But I see no reason why this is not addressed more directly by comparing an alternative with the recommended suite of policies to one with a more restricted diversions season.

2. Establishing minimum bypass flow requirements

The analysis underlying the establishment of minimum bypass requirements is quite specific with regard to the uncertainties in the data and estimated relationships. Figure D-5 establishes the conceptual framework guiding their analysis. In short, a policy based on a function estimated using a mean regression of protective flow on stream size would only be expected to provide sufficient flows to about 50% of the rivers. On the other hand, a function at the upper end of the 95% confidence interval would be expected to provide sufficient flow in 95% of the cases. This conceptual framework is relatively simplistic because it does not formally address the inherent uncertainties in the data and assumes that a univariate function of the log-linear form proposed is sufficient. Nonetheless, given the need for a relatively simple rule, the conceptual framework seems to be a reasonable structure in which to carry out the analysis.

However, the analysts failed to implement the conceptual framework proposed. What they did was to estimate a log-linear function for MBF3, (E8.a) with a constant of 0.7591 and a slope of -0.4698. For the function to be used in the policy, they increased the constant by three standard deviations (σ) (3×0.0597) to 0.9382 or 8.7 in inverse log form. No change is made, however in the estimated slope. This is not consistent with the conceptual model discussed above. While it appears in this case that the 3σ shift in the constant term is sufficient to substantially increase the number of observations below the MBF3 line (Figure E-8), it is also clear that it is not the case that 95% of the observations are below the line. The analysts do not present the standard deviation in the slope parameter; this should be included. A 3 σ upward shift in the intercept alone will not necessarily lead to a line that is above 95% of the data. Indeed, it is possible for there to be substantial uncertainty in an estimated model even if the standard deviation of the intercept is essentially nil.

Shifting the constant rather than the slope has important implications for policy. For example, a shift of 0.05 in the slope without any shift in the constant would change the point at which the policy switches from the MBF3 function to the constant 60% of Q_m from 290 mi^2 to about 540 mi^2.

So what can be done instead of an ad hoc 3σ shift in the constant? A similar shift in both the constant and the slope would probably result in substantially more than 95% of the observations below the line. An alternative would be to carry out a constrained regression in which a log linear function is chosen to minimize the sum of squared errors subject to the constraint that 95% of the errors (estimated minimum flow less observed minimum flow) are negative.

Regardless of how a MBF function is established, it was not clear to me that it is appropriate to use the MBF3 function for the full range of the policy area. This issue is discussed in section E.3.2.1. However, I was unable tell with certainty that the there is not significant out-of sample prediction being carried out. Figure E-9 presents, "Percent of headwater basins upstream of steelhead critical habitat in the Policy area with drainage areas smaller than a specified value." For example, about 65% of the basins upstream of critical habitat have a drainage area smaller than 1 mi^2. This does not tell us what percent of the basins *with* critical habitat are smaller than 1 mi^2. From this figure it would seem that, at least at the boundary, there may be a significant number of streams with drainage basins that are less than one mi^2. Yet, from the data presented in the figures, the MBF3 function seems to have been estimated almost exclusively on basins greater than 1 mi^2. I did not find in the report a figure similar to E-9 for critical habitat in the Policy area. If the policy will affect a significant number of streams with drainage area less than 1 mi^2, then a clearer justification for using the function out-of-sample should be given.

3. Establishing maximum cumulative diversion requirements

The data available to evaluate the MCD options does not appear to be sufficient to allow for statistical analysis. On average, the proposed MCD restriction seems appropriate given the limited information available. However, from what I could tell the MCD limit does not adjust for year-to-year variation in flow. For example, I was not convinced that allowing diversions up to 5% of 1.5 Year Peak Flood will be protective if droughts persist for several years. Nor am I convinced that the 5% limit is not excessively tight in years that are particularly wet.

4. Conducting site-specific studies

When site-specific studies are used to request a variance from the regional criteria it should be possible to substantially reduce the uncertainty as to the effect of the proposed activities. As an economist, I have no expertise allowing me to judge whether the provisions for site-specific variances are adequate. The procedures did appear to be reasonable.

5. Assessing the cumulative effects of water diversions on instream flows needed for the protection of fishery resources

The process for calculating the cumulative water diversions is section A.5 in appendix 1. This essentially appears to be an accounting exercise and the data necessary to carry it out appear to be identified. Although the approach seems reasonable, I did not find anything that I would consider scientific analysis underlying this work. As I note below, I believe the analysis is generally lacking in analysis of how decision makers will respond to the how the incentives created by the Policy. It is my understanding that the marginal value of water withdrawals can be extremely high in this region because of the high-value crops being grown. As such, if the policy creates a situation in which vineyards lose secure access to water they will almost certainly respond by obtaining water in some other way. Analysis of such indirect consequences of the policy would help yield better estimates of cumulative water diversions.

6. Minimizing the effects of onstream dams on fishery resources

The scientific basis of the on-stream dam restrictions appears to be drawn primarily from a review of the literature and professional judgment. As experimental manipulation of the habitat of endangered species is not possible, this approach seems reasonable. I can not comment on the completeness of the literature surveyed. The monitoring program detailed in Appendix K, if followed, would provide very valuable new data on the impacts of dams in the area.

7. Providing passage for fish migration and requiring screening of water diversion intakes

The screening requirements recommended in this Policy are drawn directly from DFG-NMFS (2002) guidelines. It appears that no additional scientific analysis was carried out in preparation for the Policy.

8. Application of criteria developed to protect anadromous fishery habitat flow needs to fish habitat, in general, within the policy area

I did not find any study of the scientific basis for concluding that the provisions intended to protect salmonids will also protect smaller native fish. In terms of habitat, this seems to be a reasonable conclusion.

Big Picture Questions

(a) In reading the staff technical reports and proposed implementation language, are there any additional scientific issues that are part of the scientific basis of the proposed rule not described above? If so, please comment with respect to the statute language given above.

There were several pieces of analysis that were not done, but would have, I believe, resulted in a stronger scientific foundation for the Policy.

- The reports pay almost no attention to socioeconomic responses that are likely to occur in response to the policies. A regrettable hallmark of most ecological analysis is that there is an implicit assumption that the firms and individuals that make use of the system will not alter their behavior in response to the policy proposed. This is rarely if ever true. Lacking any effort to predict socioeconomic responses, the analysts are reduced to making simplistic assumptions as to the likely response of agents to policy changes (e.g., to assume that "water diverters would take all available water until the full CDV was diverted," p. F-26). It is likely that water users in the Policy area will alter their schedule of withdrawals, and might even make major changes in how they use and store water in response to the new policy. I am not in a position to guess how decision makers might respond to the proposed rules and whether unintended consequences might result. However, there are too many examples of well-intentioned policies that result in perverse consequences and efforts to protect endangered species are among the most common of these. For example, recent research has found that the Endangered Species Act has created incentives that encourage landowners to cut young trees that would have offered habitat because at maturity. In effect the ESA in this case acts to diminish habitat for the species. There is no analysis in this document as to whether this Policy might also create such perverse incentives with unintended consequences.

- Appendix I had potential to be quite interesting and valuable, yet it does not include the analysis that would make it most useful. The recommended policy, MBF3, DS3 and MCD2 is not analyzed. This is particularly troubling since Alternative scenario 4, which includes the recommended policies DS3 and MCD2 (and the lower MBF rule, MBF4) is consistently the least protective of the five policies evaluated. It would have been particularly useful to evaluate the recommended policy combination with minor changes (e.g., evaluating with DS1 instead of DS3) to estimate the impact of each such change. To be even more valuable, sensitivity analysis on parameters with substantial uncertainty could have been carried out.
- One of the key elements to maintaining habitat is allowing for sufficient variability during the peak flow period. The policy tool that is used to achieve this result is a limit on the cumulative diversions, primarily because the monitoring requirements required for the Trout Unlimited (MCD4) proposal "effectively requires hourly hydrograph data." I believe that it would have been useful to consider approximations of this policy, perhaps using models to estimate the hydrograph based on measured data of precipitation. The proposed policy appears likely to achieve the goal of maintaining sufficient variability in flow. However, it is not the ideal tool for the task. In wet years, the criterion must necessarily be more conservative than would be needed if demand could respond to actual conditions and, in dry years, could lead to withdrawals in that might have deleterious impacts on the habitat. A system of rights that vary continuously over time is possible and would be preferable. An example of a program that facilitates the use of rights that are very time-specific is the Hunter River Salinity program in New South Wales, Australia.

(b) Taken as a whole, is the scientific portion of the proposed rule based upon sound scientific knowledge, methods, and practices?

The proposed policy was clearly developed based on a strong knowledge of the science underlying the relationship between instream flows and the life cycle of salmonids. Where available, the analysis built on appropriate data and, where primary data were not available, results from scientific literature was used. It is my impression that there is a fairly strong scientific foundation to believe underlying policy will be protective of the species.

There are two areas in which the analysis was somewhat deficient and, if include, would have resulted in more complete analysis of the problem leading to greater confidence in the proposed Policy. First, there was an insufficient formal treatment of uncertainty. The Policy setting is highly stochastic and there is enormous uncertainty surrounding the relationships studied. As such, the formal treatment of this uncertainty should have been a centerpiece of the analysis. Yet, only with respect to the MBF aspect of the policy were statistical methods used to characterize how the policy was designed to address uncertainty. Although it is not surprising that there are not sufficient data to allow statistical characterization of the uncertainties for all aspects of the policy, it is surprising that there is not more of such analysis. Bayesian or robust-control methods could be used to formally analyze uncertainty even when there are relatively few data points.

Second, as noted above, there is no analysis of how socioeconomic forces are likely to respond to the incentives created by the proposed policies. The ultimate effectiveness of the policy will depend upon how decision makers respond to the policies. The policies proposed are relatively restrictive, giving decision makers relatively little latitude so that unintended consequences may not likely. However, one should not underestimate the ability of economic interests to respond to regulations in a manner that can circumvent or diminish the effectiveness of a policy. Socioeconomic analysis would have been particularly useful with regard to the enforcement provisions, which rely to a great extent on self-reporting, which lends itself to error and misrepresentation. Furthermore, I believe that it would have been possible to develop a more flexible policy that would still achieve the policy goals, and the lack of economic analysis may have contributed to the failure to identify those opportunities. The lack of analysis of the incentives created by the Policy is, in my opinion, an omission that significantly weakens its scientific foundation.