Spencer Kenner (SBN 148930)
James E. Mizell (SBN 232698)
Emily Thor (SBN 303169)
DEPARTMENT OF WATER RESOURCES
Office of the Chief Counsel
1416 9th Street, Room 1104
Sacramento, CA 95814
Telephone: 916-653-5966
Email: jmizell@water.ca.gov

Attorneys for
CALIFORNIA DEPARTMENT OF WATER RESOURCES

BEFORE THE

CALIFORNIA STATE WATER RESOURCES CONTROL BOARD

HEARING IN THE MATTER OF CALIFORNIA DEPARTMENT OF WATER RESOURCES AND UNITED STATES BUREAU OF RECLAMATION REQUEST FOR A CHANGE IN POINT OF DIVERSION FOR CALIFORNIA WATER FIX	TESTIMONY OF RICHARD WILDER

I, Richard Wilder, do hereby declare:

I. OVERVIEW

I have previously testified in this matter. A summary of my expertise is included in Exhibit DWR-1013, and a true and correct copy of my statement of qualifications has previously been submitted as Exhibit DWR-1002. There are no substantive changes.

This rebuttal testimony provides a response to issues raised by Protestants relating to the California Water Fix (CWF) and aquatic resources upstream of the Delta. I reviewed the written and oral testimonies of witnesses who discussed upstream aquatic resources related to the CWF, in particular the testimonies of Paul Bratovich, Tom Stokely, Chris Shutes, Tom Cannon, and Jonathan Rosenfield.

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To summarize, my opinions are:

1. CWF is reasonably protective of American River Chinook salmon and steelhead.
2. The CWF effects analysis fully considered the relationship between physical models and biological parameters.
3. The CWF approach of presenting results in multiple ways is appropriate.
4. CWF will provide reasonable protection of upstream life stages of salmonids.
5. Additional permit terms and conditions are unnecessary.

II. DISCUSSION OF TESTIMONY

A. CWF is reasonably protective of American River salmon and steelhead.

Mr. Paul Bratovich made two claims in his testimony that I will discuss. First, Mr. Bratovich concluded that there are unreasonable effects of CWF H3+ in the lower American River (Exhibit ARWA-700, p. 6, paragraph 28; March 19, 2018 Transcript, Vol. 18, p. 93:12-19). Second, he indicated that a 75°F upper incipient lethal temperature (UILT) for juvenile steelhead has been reported previously (March 19, 2018 Transcript, Vol. 18, p. 91:7-9.)

I believe that Mr. Bratovich's opinion that there are unreasonable effects of CWF H3+ on juvenile steelhead in the lower American River is incorrect. The only evidence Mr. Bratovich provided to support his opinion was exceedance plots of temperature model outputs. The Biological Assessment (BA) and Biological Opinion (BiOp) for the CWF, in contrast, are better supported. The BA and BiOp used additional temperature analyses, including summary statistics of temperature model outputs that characterize temperature trends by month and water year type (e.g., Exhibit DWR-1142, Appendix 5.C, Table 5.C.7-14, Figures 5.C.7-14-1 through 5.C.7-14-6) and temperature threshold exceedance analyses that quantify the frequency and magnitude of exceeding temperature thresholds (Exhibit DWR-1142, Section 5.4.2.2.3.1.3.2, pp. 5-481 to 5-483) rather than "eyeballing" exceedance plots. In addition, the BA and BiOp considered modeling limitations, real-time operations, and adaptive management in drawing conclusions (e.g., Exhibit DWR-1142, Section 7.4.1, pp. 7-11 to 7-13; Exhibit

SWRCB-106, p. 1005). Modeling was based on generalized rules based on historical operational trends and provide only a coarse representation of project operations. Therefore, as described in Exhibit DWR-1142, Section 7.4.1, p. 7-11 to 7-12, "...results do not exactly match what operators might do in a specific month or year within the simulation period since the latter will be informed by numerous real-time considerations that cannot be input to CalSim II." When considering the entire water temperature analysis, it is my opinion that CWF H3+ is reasonably protective of juvenile steelhead rearing in the American River.

During his oral testimony, Mr. Bratovich indicated that a 75°F upper incipient lethal temperature (UILT) for juvenile steelhead has been reported previously (March 19, 2018 Transcript, Vol. 18, p. 91:7-9; see also March 2, 2018 Transcript, Vol. 9, pp. 253:9-20 and 255:21-25). During collaborative meetings with fisheries agency staff during development of the BA and BiOp, we decided to focus on sublethal thresholds associated with effects to growth (63°F mean monthly and 69°F seven-day average daily maximum (7DADM)). This was done primarily because sublethal growth-related temperature thresholds are lower and, therefore, more conservative than lethal thresholds, such as the 75°F value cited by Mr. Bratovich. Because they are lower, a broader range of thermal effects to fish was assessed (temperatures above 63°F instead of above 75°F). I believe that this was a reasonable approach to assess thermal effects to juvenile steelhead.

An additional line of cross-examination from American River Water Agencies (ARWA) (March 2, 2018 Transcript, Vol. 9, pp. 235:9 to 241:25) pertained to my statement that no fall-run Chinook salmon and steelhead redd dewatering field data were available for the American River to conduct a field-based redd dewatering analysis during preparation of the BA and NMFS BiOp and, therefore, an alternative analysis was conducted based only on changes in modeled flow rates (Exhibit DWR-1013, p. 53: 19-24). However, ARWA did have the data necessary to conduct a redd dewatering analysis at the time for their modified FMS report (Exhibit ARWA-702). Despite a search of the specific USFWS website where this type of data is typically housed, I was unaware that these data existed. To maintain the standard of conducting the most biologically-based analysis, I requested these data from ARWA and was provided that data on June 29, 2018. We have updated our effects analysis by incorporating these data. In doing so, my conclusions that CWF is reasonably protective of salmonid eggs and alevins in the American River would not change. The analysis indicates that the results are similar to those conducted in the BA. (DWR-1337.)

B. <u>The CWF effects analysis fully considered the relationship between physical models and biological parameters.</u>

Dr. Rosenfield states in his testimony that use of the term "small" to characterize marginal effects in the CWF effects analysis is misleading because the magnitude of change in a physical parameter may not be the same as that of a biological parameter (April 23, 2018 Transcript, Vol. 32, pp. 128:13 to 129:2). I agree that the magnitude of change in a physical parameter, such as flow and water temperature, may not be the same as the magnitude of change in a biological parameter. In fact, in addition to *magnitude*, the *direction* of change in a physical parameter may not be the same as the *direction* of change in a biological parameter. As a result, I believe that Dr. Rosenfield's criticism of applying physical model outputs to biological effects in the effects analysis is too simplistic. For example, based on field data collected by USFWS in the Sacramento River (Exhibit DWR-1106, USFWS 2003a), we know that increases in flow rate can affect winter-run Chinook salmon suitable spawning habitat availability in a nonlinear way: at flows below 5,000 to 11,000 cfs, depending on river segment, an increase in flow tends to increase suitable habitat availability; however, at flows greater than 5,000 to 11,000 cfs, depending on river segment, an increase in flow reduces suitable habitat availability (Figure 1). Although this example is specific to winter-run Chinook salmon spawning habitat availability, this pattern of increased flow leading to reduced spawning habitat availability has also been observed for steelhead[1] and fall-run Chinook salmon[2] in the Sacramento River and for steelhead in the American River[3]. This pattern has also been

[1] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-88, p. 5.D-290; Exhibit DWR-1106, USFWS 2003a
[2] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-89, p. 5.D-291; Exhibit DWR-1106, USFWS 2003a
[3] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-90, p. 5.D-293; Exhibit DWR-1256, USFWS 2003b

observed in several flow-suitable juvenile rearing habitat availability relationships for winter-run[4], fall-run[5], and late fall-run Chinook salmon[6] in the Sacramento River.

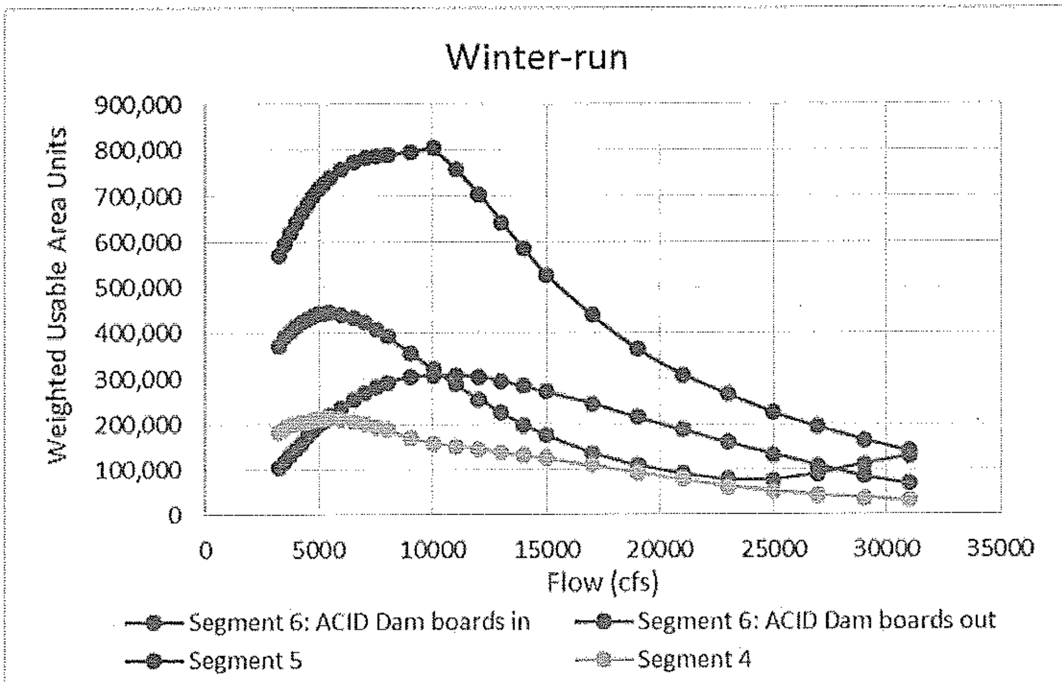

Figure 1. Spawning habitat availability expressed as weighted usable area units as a function of instream flow rate for Winter-Run Chinook salmon in the Sacramento River, Segments 4 to 6. Taken from Exhibit DWR-1142, Appendix 5.D, Figure 5.D-87, p. 5.D-290. Data source: USFWS (2003a). ACID = Anderson-Cottonwood Irrigation District; Segment 6= Keswick Dam to ACID Dam; Segment 5 = ACID Dam to Clear Creek; Segment 4 = Clear Creek to Cow Creek.

To ameliorate the problem noted by Dr. Rosenfield, we attempted to make our analyses as biologically relevant as possible whenever data were available. To do this, we linked the physical modeling outputs to biological parameters, whether it was by using field data-derived flow-spawning habitat availability or flow-rearing habitat availability relationships, winter-run Chinook salmon life cycle models, or other data-based analytical tools such as Reclamation's Egg Mortality Model and SALMOD. Only when no other biologically-based analytical tools

[4] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-94, p. 5.D-313 and Figure 5.D-95, p. 5.D-314; Exhibit DWR-1104, USFWS 2005

[5] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-96, p. 5.D-314 and Figure 5.D-97, p. 5.D-315; Exhibit DWR-1104, USFWS 2005

[6] Exhibit DWR-1142, Appendix 5.D, Figure 5.D-98, p. 5.D-315 and Figure 5.D-99, p. 5.D-316; Exhibit DWR-1104, USFWS 2005

were available did we rely on straight physical model outputs to make inferences about biological effects. In these cases, we relied on our best professional judgment by applying the best available scientific information to make these inferences. We are well aware that there are many sources of uncertainty in the analysis, and we acknowledge that Dr. Rosenfield's problem is one of them, which is why we minimized the use of only physical modeling outputs to draw conclusions whenever possible.

C. The CWF approach of presenting results in multiple ways is appropriate.

In his testimony, Dr. Rosenfield reported that the characterization of temperature changes in percentage terms is scientifically meaningless (April 23, 2018 Transcript, Vol. 32, p. 129:3-5). While I agree that any way of reporting results has advantages and drawbacks, I disagree that reporting temperature change results in percentage terms is scientifically meaningless. In the BA (Exhibit DWR-1142) and FEIR/FEIS (Exhibit SWRCB-102), we reported water temperature modeling outputs in multiple ways, including "raw" difference and percent difference in temperatures between the No Action Alternative (NAA) and the Proposed Action or Preferred Alternative (PA). We did this in consideration of the wide range of readers of these documents and because we recognize that the manner in which temperature model outputs are presented can assist or hinder analysis of results. By providing model outputs in multiple ways, we responsibly provided readers with the option to use whichever reporting method they felt was most relevant to their interpretation of the outputs.

Dr. Rosenfield expressed discontent with our use of means in summarizing modeled water temperature outputs in the effects analysis yet offered no other method for presenting and evaluating model outputs (April 23, 201818 Transcript, Vol. 32, p. 129:6-19). The approach of evaluating mean differences between NAA and PA is just one of several methods used to report and interpret water temperature results and can provide additional information more easily and quantitatively than other methods. Reporting long-term trends across multiple years allows the analysis to remain quantitative while following the guidance from physical modelers for appropriate use of model results, which is that long-term averages are preferred

(Exhibit DWR-1142, Appendix 5.A, Section 5.A.4.5, pp 5.A-11 to 5.A-13; Appendix 5.C, Section 5.C.5, pp. 5.C-7 to 5.C.8). In addition, similar to my reasoning for the inclusion of percent differences when reporting water temperature model results, I report means by month and water year type in addition to exceedance results so that a wide range of readers can understand and interpret analytical results. Further, I use temperature model outputs to assess effects of CWF on salmonids in several models and analytical tools, including life cycle models, temperature threshold analyses, SALMOD, and the Egg Mortality Model.

D. CWF will provide reasonable protection of upstream life stages of salmonids.

Dr. Rosenfield claimed that there will be negative flow and temperature results from CWF that will cause negative effects to spawning and rearing salmonids upstream of the Delta (April 23, 2018 Transcript, Vol. 32, pp. 127:8 to 129:19). Dr. Rosenfield reported that monthly average temperatures increase during winter-run Chinook salmon spawning and egg incubation periods in below normal, dry, and critical years (April 23, 2018 Transcript, Vol. 32, p. 127:21-24), although he provided no evidence for this claim such as where in the river these increases would occur, by how much temperatures would increase under CWF, the baseline to which this analysis is compared, and where in the range of temperatures these increases would occur (e.g., are they near any thresholds or well below them). He indicated that these temperature differences would lead to a 59% increase in mean temperature-related egg mortality in below normal water years (April 23, 2018 Transcript Vol. 32, pp. 127:25 to 128:5). Although there is no support or citation provided in his testimony for this claim, results for SALMOD appear to be consistent with this reported value (Exhibit DWR-1142, BA, Chapter 5, Table 5.4-38, p. 5-249). Taken out of context, 59% may appear to be a significant change. However, the actual values underlying the percentage reveal a different picture. The model is seeded with 5,913,000 winter-run eggs each year. What Dr. Rosenfield did not include in his statement is that the 59% temperature-related mortality increase results from a 4,709 egg increase (i.e. from 8,021 to 12,730 eggs) from the NAA to PA, which translates to a 0.1%

increase. He also did not indicate that mean winter-run Chinook salmon juvenile production in below normal water years predicted by SALMOD is 2,069,244 and 2,019,856 fish per year under the NAA and PA, respectively (Exhibit DWR-1142, BA, Chapter 5, Table 5.4-46, pp. 5.301 to 5.302). Therefore, production in below normal years under the NAA and PA is reduced by 0.4% (= 8,021 / 2,069,244) and 0.6% (= 12,730 / 2,019,856), respectively, due to temperature related mortality. This indicates that the temperature-related mortality, expressed as a percent of overall production, is 0.2% (= 0.6% under PA – 0.4% under NAA) greater under the PA relative to the NAA. In my professional opinion, this does not constitute an unreasonable effect to winter-run Chinook salmon. Using total eggs in place of production, total eggs lost in below normal years under the NAA and PA are 0.1% (= 8,021 / 5,913,000) and 0.2% (= 12,730 / 5,913,000), respectively, due to temperature related mortality. This indicates that the temperature-related mortality, expressed as a percent of total winter-run Chinook salmon eggs, is 0.1% greater under the PA relative to the NAA. Again, in my professional opinion, this does not constitute an unreasonable effect to winter-run Chinook salmon. Alternatively, results could be reported in terms of survival instead of mortality. When this is done, temperature-related egg survival of winter-run Chinook salmon, in terms of production, would be reduced from 99.6% under the NAA to 99.4% under the PA. Temperature-related egg survival of winter-run Chinook salmon, in terms of total eggs produced, would be reduced from 99.9% under the NAA to 99.8% under the PA. Expressed this way, temperature-related mortality of eggs is negligible to overall survival of the winter-run Chinook salmon population and would not constitute an unreasonable effect to winter-run Chinook salmon. In summary, although a 59% increase in mortality may appear on the surface to be a large effect, digging a little deeper in the context of the population as a whole indicates that this model result would not cause an unreasonable effect on winter-run Chinook salmon.

Another unreferenced claim by Dr. Rosenfield was that there would be an increase in winter-run Chinook salmon temperature-related pre-spawning mortality in half of years. (April

23, 2018 Transcript, Vol. 32, pp. 127:25-128:5.)[7] If this claim was based on SALMOD results, I am uncertain how he drew this conclusion. In Exhibit DWR-1142, Chapter 5, Table 5.4-38, p. 5-249, I report that temperature-related pre-spawning mortality is higher by 1% and 7% on average in wet and below normal years, respectively. The proportion of years in the 80-year period that are wet and below normal years is exactly 50% (40 years), which appears to be how he derived the "half of years" claim. However, a mean increase in egg mortality over these years does not indicate that every year had an increase due to variation in the values around the mean, especially when the mean increase is only 1%, which is the case for wet years. Therefore, the claim that there would be an increase in temperature-related pre-spawning mortality in half of years appears to be unfounded.

A third finding, with no reference, noted by Dr. Rosenfield is that there is a 56% increase in fall-run Chinook salmon temperature-related egg mortality during dry years (April 23, 2018 Transcript, Vol. 32, 128:6-12). This value also appears to be from SALMOD results (Exhibit DWR-1142, Appendix 5.E, Table 5.E-37, p. 5.E-150). As with temperature-related egg mortality for winter-run Chinook salmon, this value needs to be put in context of annual fall-run juvenile production and total number of eggs in dry years. When this is done, with respect to production, temperature-related egg mortality would increase from 4.1% of juvenile production (732,312 of 17,979,387 individuals) under the NAA to 6.4% (1,145,829 of 17,883,009 individuals) under the PA, or a difference of 2.3%. With respect to total eggs, temperature-related egg mortality would increase from 1.3% of total eggs (732,312 of 56,115,000 eggs) under the NAA to 6.4% (1,145,829 of 56,115,000 eggs) under the PA, or 0.7%. When put in the broader context of the population, it is my opinion that these effects are not substantial or unreasonable.

A fourth finding, again with no reference, noted by Dr. Rosenfield is that there is a 223% increase in late fall-run Chinook salmon temperature-related egg mortality during below normal years and an overall decline of the egg/larval life stages in every year (April 23, 2018,

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[7] Although the transcript reads, "...and an increase in temperature-related historian mortality in half of years," the audio from this day is clear that he said, "...and an increase in temperature-related pre-spawning mortality in half of years."

Transcript Vol. 32, 128:6-12). Assuming these were SALMOD results, with respect to production, temperature-related egg mortality would increase from 0.1% of juvenile production (1,186 of 2,069,244 individuals) under the NAA to 0.2% (3,836 of 2,019,856 individuals) under the PA, or 0.1%. With respect to total eggs, temperature-related egg mortality would increase from 0.01% of total eggs (1,186 of 13,325,000 eggs) under the NAA to 0.03% (3,836 of 13,325,000 eggs) under the PA, or 0.02% (data taken from Exhibit DWR-1142, Table 5.E-54, p. 5.E-225 and Table 5.E-63, pp. 5.E-270 to 5.E-271). Therefore, when put in the broader context of the population, it is my opinion that these effects to late fall-run Chinook salmon are not substantial or unreasonable. In addition, I am unable to locate any evidence to support Dr. Rosenfield's conclusion that there is a decline every year in late fall-run eggs and larvae. SALMOD results do show 1% to 10% higher mean total egg mortality under the PA compared to the NAA in every water year type (Exhibit DWR-1142, Table 5.E-54, p. 5.E-225), but as discussed two paragraphs above, this does not mean that every year within the mean for each water year type had higher egg mortality. Therefore, Dr. Rosenfield's conclusion is incorrect.

Another result cited by Dr. Rosenfield from the NMFS BiOp (Exhibit SWRCB-106, p. 841) is that redd dewatering will increase under the PA relative to the NAA for winter-run Chinook salmon in every water year type (Exhibit NRDC-58, p. 22:7-9). Further examination of these results (Exhibit SWRCB-106, p. 511, Table 2-137; Exhibit DWR-1142, Section 5.4.2.1.3.1.1.1.3, pp. 5-243 to 5-248) indicates that these increases would be less than 4%, except for a single month during the 5-month period in above normal, below normal, dry, and critical years, in which the increase would be between 5.3 and 6.8% depending on the water year type. Given the small magnitude and frequency of this effect on redd dewatering, it is my professional opinion that CWF H3+ would be reasonably protective of winter-run Chinook salmon.

E. <u>Additional permit terms and conditions are unnecessary.</u>

There were several proposals by interested parties for specific permit conditions related to upstream CWF operations:

1. Modification to the OCAP Reasonable and Prudent Action measure for Shasta Reservoir/Sacramento River temperature criteria– Exhibit NRDC-58, p. 21, line 10 to p. 22, line 2;
2. American River Modified Flow Management Standard (FMS) – Exhibit ARWA-500, paragraphs 6-7; Exhibit ARWA-700, paragraphs 29-30; Exhibit ARWA-702; CSPA-202, p. 16, liner 10-17;
3. Trinity River proposed mitigation measures – Exhibit PCFFA-87, p. 13, line 13 to p. 14, line 10; April 16, 2018 Transcript, Vol. 29, 124:24-126:17; Exhibit CSPA-202, p. 16, lines 1-9;
4. Shasta carryover requirements – Exhibit CSPA-202-errata, p.10, lines 7-11; Exhibit CSPA-306, slides 6-7; and
5. Carryover requirements in Oroville Reservoir – Exhibit CSPA-101, p. 16, line 20 to p. 17-line 20.
6. Central Valley reservoir storage and upstream temperature objectives in the Sacramento, American, and Feather rivers – Exhibit CSPA 204, pp. 9-10.

It is my opinion that each of these proposed permit terms is unnecessary because CWF is reasonably protective of upstream aquatic resources, as I describe throughout Exhibit DWR-1013. In addition, these proposed permit terms are for impacts unrelated to CWF and could be implemented with or without CWF in place. Also, the issues raised in the proposed permit terms are being addressed in other processes involving numerous regulatory agencies and on a different time table than the CWF change in point of diversion petition process.

Executed on this 9th day of July, 2018 in Sacramento, California.

Richard Wilder

REFERENCES

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