Public Comment
Bay-Delta Phase II Working Draft Report
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The Bay Institute
Natural Resources Defense Council
Trout Unlimited

By email and hardcopy

December 16, 2016

Felicia Marcus, Chair
c/o Jeanine Townsend, Clerk to the Board
State Water Resources Control Board
P.O. Box 100, Sacramento, CA 95812-2000

RE: DRAFT PHASE 2 SCIENTIFIC BASIS REPORT

Dear Chairperson Marcus,

This letter is submitted as the comments of the Bay Institute, the Natural Resources Defense Council and Trout Unlimited on the October 2016 Working Draft Scientific Basis Report for New and Revised Flow Requirements on the Sacramento River and Tributaries, Eastside Tributaries to the Delta, Delta Outflow, and Interior Delta Operations (Draft Report), prepared for the Phase 2 update of the 2006 Water Quality Control Plan (WQCP) for the San Francisco Bay/Sacramento- San Joaquin Delta Estuary.

General comments

1. The ecosystem of the Bay-Delta estuary is facing an existential crisis, involving the potential for a wave of native species extinctions and loss of the ability to support ecological services for humans, including water quality, commercial and recreational fisheries, and public use of estuarine and coastal habitats. The Draft Report appropriately recognizes that the current Bay-Delta WQCP is inadequate to protect the estuary ecosystem because it fails to provide adequate flow and operational requirements to protect native species, habitats and ecological services. Because flow is the master variable affecting aquatic organisms, habitats, and ecological processes in the estuary and its interactions with other environmental parameters drive estuarine conditions, the Phase 2 update of the Bay-Delta WQCP represents the best opportunity for designing and adopting a comprehensive ecosystem approach to protecting the public trust resources and designated fish and wildlife beneficial uses associated with freshwater and estuarine habitats. The Draft Report properly acknowledges the role of flow as an ecological driver and finds that improving the amount and timing of flows is an essential and foundational element of any plan to reverse the estuary ecosystem's current decline and to restore it to a desirable state. For the most part, the Draft Report accurately summarizes the vast scientific record documenting the powerful, widespread, persistent, and statistically significant biological and physical responses to flow conditions; the strong scientific support for using the percentage of unimpaired flow approach to mimic natural hydrological conditions in order to promote natural ecological functions; and the urgent

and clear basis for modifying the existing WQCP objectives to be more protective in preserving and improving public trust resources and designated fish and wildlife beneficial uses of the waters of the estuary and its watershed.

2. The best scientific evidence strongly supports the Draft Report's recommendations that the WQCP objectives be amended to secure large-scale augmentations of Sacramento River inflows and Delta outflows throughout the January – June period and to adopt year-round inflow and outflow objectives as well. The biological and physical response to higher inflows and outflows throughout the year and particularly in winter and spring is one of the strongest biological signals detected in the estuary, and requiring increased flows throughout the year is absolutely critical to protect and restore endangered species populations as well as preserve broader public trust values and fish and wildlife beneficial uses. It should be noted that while the 1995 WQCP amendments were intended to increase Delta outflow, recent evaluations (NRDC 2016, TBI 2016) have concluded that they were not successful in achieving this outcome, and the Draft Report confirms that the volume and variability of Delta outflow is substantially impaired under the current WQCP. The revised Report and draft SED should identify flow alternatives and implementation mechanisms that mandate large-scale inflow and outflow improvements and ensure that the mandated flows actually occur.

3. The draft Report appropriately identifies the need to amend WQCP objectives regarding interior Delta flows, such as Old & Middle River reverse flows, export limits, inflow:export ratio, and Delta Cross Channel gate operations, to provide greater protection for endangered species and other fish and wildlife beneficial uses. The Board should not assume that such measures will be implemented by other authorities. In addition, the revised Report and the draft SED need to consider adoption of interior Delta flows that are more protective than the minimum protections currently mandated under the state and federal Endangered Species Acts. As the Draft Report appropriately recognizes, the Board has an independent obligation to protect fish and wildlife that extends beyond minimum requirements under the state and federal Endangered Species Acts.

4. The Draft Report appropriately recommends the adoption of new WQCP objectives regarding coldwater management at upstream reservoirs. For instance, new scientific information developed over the past several years has demonstrated that current temperature management at Shasta Dam is inadequate to protect and restore winter- and fall-run Chinook salmon and other species. The Draft Report appropriately identifies the need to integrate protections for coldwater pool in upstream reservoirs with increased Delta outflow and other flow requirements necessary to protect beneficial uses.

5. While the Draft Report generally provides a solid scientific foundation for updating the WQCP, in places it uses imprecise language, mischaracterizes data or findings or does not visually represent information clearly. The Draft also frequently omits some of the most recent references in the scientific literature. The most serious result is to

underestimate the flow needs of some estuarine species. These shortcomings should be corrected in the revised Report and the draft Phase 2 Substitute Environmental Document (SED) in order to provide an even stronger basis for modifying the WQCP objectives. The most important examples are cited in the Specific Comments section below. Moreover, in revising the Report and preparing the draft SED the Board must take pains to better demonstrate the likely effects of flow alternatives on fish and wildlife beneficial uses, particularly with respect to achieving Plan objectives as well as SMART performance metrics. That environmental analysis should utilize, as appropriate, existing life cycle models for salmon and other species, as well as the Central Valley Flood Protection Board's salmon doubling floodplain model.

6. In order for adaptive management of unimpaired flows to succeed in protecting public trust resources and designated fish and wildlife beneficial uses, the SWRCB must require the timely adoption of clear biological goals, enforceable SMART performance metrics, and appropriate limits on flow shaping (i.e., deviation from a multi-day average of UIF rule) that prevent shifting of flows to months outside the target period or even into subsequent years. Absent these elements, the benefits of apparent improvements in the numeric and/or narrative regulatory requirements will be largely illusory. In particular, the open-ended approach to flow shaping described in the draft Report is a potential fatal flaw for successful implementation of a revised WQCP. In general, there is no scientific basis to conclude that shifting substantial amounts of flow outside the target periods when conditions need to be improved for aquatic organisms and habitats (and underlying ecological, hydrological, and geomorphological processes) is likely to do anything but degrade those conditions during the target periods. (There may be some limited potential exceptions to this conclusion, but that does not justify proposing or adopting such an open-ended approach; these exceptions are more properly addressed in the context of much more specific flow shaping rules or through other mechanisms). The specific implementation parameters in the revised report and draft SED should restrict flow shaping to occur within the target period for any particular regulatory requirement and identify rules describing how much the hydrograph can be shaped during the target period. (The adverse impacts of proposed unlimited flow shaping and shifting in the San Joaquin River basin will be described in greater detail in the Bay Institute – NRDC comments on the revised draft Phase 1 SED). The revised Report and draft SED should also provide clearer and more detailed guidance on the development of SMART performance metrics. Use of a "logic chain" approach to developing biological goals and SMART metrics is described in TBI et al, 2012 and illustrated in the recently released Stanislaus River Scientific Evaluation Process report (SEP 2016).

7. While the presentation could be improved in places, the Draft Report's comparison of current inflows and outflows with UIF conditions in the Sacramento River and its tributaries is extremely helpful. The Draft appropriately identifies the significant flow impairments in upstream tributaries that contribute to the significant reduction in inflow to the Delta and outflow to San Francisco Bay.

8. The Draft Report's consideration of the biological and physical effects of modifying the WQCP objectives is generally limited to the Delta and Suisun Bay. However, there is ample scientific evidence for, and an emerging societal awareness of, the significant effects of flow conditions on critical ecological attributes of the lower estuary and coastal waters (San Pablo, Central and South Bays and the Gulf of the Farallones), including the extent and location of the salinity field, abundance and distribution of invasive species, impacts to wetland and beach habitat formation, changes to water quality, and productivity of estuarine and nearshore marine foodwebs. These impacts are summarized in TBI 2016. The downstream effects of Delta inflow and outflow on public trust resources and designated fish and wildlife beneficial uses, and how they might affect modification of the WQCP objectives, should be more thoroughly considered in the revised Report and draft Phase 2 SED.

9. The Draft Report does not consider revised flow requirements for Sacramento River inflows and Delta outflows in the range above 75% of unimpaired flows. The State Water Resources Control Board's 2010 Delta Flow Criteria Report found that 75% UIF of these flow parameters was the <u>minimum</u> necessary to fully protect fish and wildlife beneficial uses; it does not follow that 75% is the optimum level and certainly not that it should be the upper limit considered for analytical purposes. Indeed, information from other aquatic ecosystems suggests that ecological functions are impaired if more than ~15% UIF is removed or altered from its natural flow pattern (Richter et al, 2011; Dahm, 2010). The best available science supports evaluating flow alternatives above 75% in the revised report and draft SED.

<u>Specific comments</u>

P. 1-2: In the first sentence of the last paragraph it would be more accurate to state the Bay's watershed historically drained about 40% of the state's land area, but now drains about a third. The Tulare Lake Basin contributed water more frequently before the construction of the current water supply system, but now does so only in very wet years.

P. 1-3 and p. 3-2: The draft cites Rozengurt 1987 to suggest that flow alterations > 30% in spring/45% annually can impair estuarine ecosystems beyond recovery, but states that there is no universal quantitative relationship between flow alteration and ecological response. In fact, more recent studies (Richter et al., 2011; Dahm 2010) indicate that there is such a threshold for harming aquatic ecosystem function, and that it is much lower. For instance, according to Richter et al., 2011, "Alterations greater than 20% will likely result in moderate to major changes in natural structure and ecosystem functions, with greater risk associated with greater levels of alteration in daily flows."

Chapter 2: The narrative and data presentations contained in the sections on individual streams in this chapter are inconsistent. Some describe historic surface and groundwater hydrology and geomorphology, others omit this information. Consistent information about all streams should be provided, ideally in a summary table that includes:

- Relative impairment provided by year type as well as by Water Year index (this is presented for each year in table 2.1-7 but could be summarized)
- Average runoff, median, 10%, 90%, annual and monthly flow, peak flows by return interval; could include or combine with data from Table 2.4-2 on page 2-64
- Short narrative on historic hydrology, geomorphology, regulation, and impairment

We also note the lack of discussion of the importance of the impact of unsustainable groundwater depletion on instream flows, or regarding hyporheic exchange. The Nature Conservancy has estimated that groundwater depletion reduced instream flows in the Central Valley by approximately 700,000 acre feet per year over the past 20 years, and, assuming 2009 land use conditions, will reduce instream flows by an average of 1.3 million acre feet per year (TNC 2014). In addition to the reduction in flow, there are broad ecosystem benefits from shallow groundwater flowing into stream channels from a greater percentage of unimpaired flow. A description of the benefits of this process to the ecosystem can be found in AR et al 2010. Moreover, increased instream flow will likely contribute to groundwater recharge, as has been demonstrated in other parts of the Central Valley.

Pp. 2-1 et seq: The introduction should be revised thoroughly to provide hydroclimatic context by including text regarding groundwater, including the important contribution from the tributaries draining the Cascades and the groundwater/surface water interaction on the Central Valley floor, and how it has changed over time; the changing climate bot prior to (e.g., extended droughts and colder periods in the last millennium) and following the start of the instrumental record (e.g., warming resulting in a changing percentage of annual runoff from snowmelt and greater variability of runoff). A summary explanation should also be provided of how UIF data is calculated and estimated, how it is used, how it should be interpreted (e.g., the inappropriate use of zeros and negative values from the raw dataset) and how it is different from both natural flows and historical or pre-development hydrology. How land use and water development, including groundwater pumping, "impairs" the flow, should also be explained (including impairment from imports and urban runoff, which augments the natural runoff and impacts groundwater accretions and depletions). It would also be helpful to describe and provide date, either in the introduction or later in the text, on the high runoff that the lesser regulated and unregulated streams (i.e. ones without large terminal dams) provide to the main stem Sacramento River, especially in the fall and early winter (particularly when mainstem reservoir storage is low and inflow is being stored), and to show the percent of impairment by regulated streams and how much of the flow of the Sacramento River is provided by the smaller tributaries in high runoff periods during the first several large storm events and even later in the winter during drought periods.

P. 2-1: It is unclear if the statistics in the second paragraph are meant to refer to California or the Bay-Delta watershed. The runoff numbers used from DWR 2016d are for the Sacramento Valley. TBI 1998 (page 2-4) states that 80% of the precipitation falls from November to March.

P. 2-1: "The Sacramento River has a mean annual flow of more than 22 MAF": It should be made clear when runoff numbers are provided, whether the number is unimpaired or actual and what base period applies (e.g. 1921-2014). The DWR unimpaired flow compilation for the 1921-2014 period indicates Sacramento Valley total outflow is 21.3 MAF.

P. 2-2: The seventh sentence (starting "The Golden Gate/Carquinez Strait...") is imprecise and unclear. High precipitation events occur when the storm winds and moisture transport are from the southwest, which is perpendicular to the general NW-SE orientation of Sierra-Cascade Ranges. Since this paragraph is trying to explain the high precipitation areas it should also explain the Clear Creek/ Mt Shasta/ Northern Valley precipitation maxima due to southerly storm flow and converging air at the north end of the Valley.

P. 2-9: We are encouraged by the development of the SacWAM model, and appreciate that the Draft Report will be updated to avoid use of the problematic CALSIM II model. Beyond its application to the Phase 2 analysis, the new model will be helpful in improving our ability to adaptively manage, evaluate and modify protections over the long term. To that end, independent of this or any other regulatory process, the Board should over time address the issues raised by the Independent Science Review panel, especially their recommendations for sensitivity analyses and validation; their concern that the use of a proprietary solver that costs thousands of dollars to license is a barrier to public use and that modifications should be made to allow the model to be run quickly at little or no cost; and that good ongoing communication between SWRCB and DWR is necessary to keep track of differences between future versions of SacWAM and Calsim. Finally, the Board should develop a daily model and subdaily temperature models in order to evaluate biologically significant shorter-timescale flow patterns.

P. 2-9: The draft Report should analyze flow impairment of shorter duration than one month (e.g., 3, 5, 10 or 14 days during high flow runoff events during the October – May period when shorter duration pulses are ecologically valuable) using regulated mainstem and tributary FNFs and actual flows (or some version of DWR unimpaired model) from the less regulated tributaries.

P. 2-10: The description of Fig. 2.1-7 (impairment of January-June flows) states that red indicates more impaired flow and blue equals less impaired flow. This is not always the case. Cottonwood Creek positive impairment (significantly exceeding 100%) is still impairment; red colors should be described not simply as impairment but negative impairment, and blue colors are less-negatively impaired (but may be positively impaired) (2-10). The Water Board should consider the need for maximum flows for tributaries with flow augmentations, especially during late summer and early fall months when flows are naturally low. Figure 2.1-7 also uses a water year index, which should be defined to make clear it is not only based on annual runoff. A summary table with relative impairment on different stream systems could be provided by year type as well as by Water Year index to improve the presentation.

P. 2-10 and p. 2-64: The draft would be improved by a summary table by stream group with average and median runoff, 10%, 90%, annual and monthly flow, and peak flows by return interval; this table could include a short narrative on historic hydrology and geomorphology, stream regulation, and impairment.

P. 2-11: Use of the phrase "rain-on snow" can be misleading, implying that the presence of an extensive snowpack is a requirement for high runoff. The high elevation snow line is the main contribution to the high runoff. While "rain-on-snow" implies it is the melting of the snow that is required for high runoff, in fact the snowmelt contributes at most no more than about 10- 15% of the runoff volume. The runoff to storage ratio should be provided for all the major regulated streams and its implication for downstream flood flows noted.

P. 2-12: The first sentence of the third paragraph should be revised to acknowledge that exports go not only to the southern Central Valley but also to the Bay Area and Southern California. The second sentence should reflect that at times, even without imports, especially in dry years following wetter years, Shasta Reservoir storage releases during the year are enough to result in higher than unimpaired flow conditions at Bend Bridge. In the fourth paragraph, the differences of the Sacramento River hydrograph – particularly the upper Sacramento—from other snowmelt dependent systems should be described, particularly in a warming climate. More of the annual runoff occurs in the November – March period compared to the lower Sacramento and eastside tributaries. The high runoff in those months is an important hydrogeomorphic driver.

P. 2-17: The geology of Battle Creek, being composed of fractured volcanic rock, contributes to the infiltration and accumulation of groundwater.

P. 2-19: Due to the importance of these creeks (particularly Mill and Deer) for spring run Chinook salmon, the simplified hydrological characterization in the models is somewhat problematic. Also, regarding the description of beaver dams in the last paragraph, the presence of beaver dams tends to have an overall positive impact on ecosystems (Lundquist & Dolman 2016), including generally positive net effects on salmon. If this is the case on these streams, a less-biased characterization should be presented.

P. 2-31: A short description should be provided of how all the major terminal reservoirs are operated for downstream consumptive uses and how this affects the hydrograph. For instance, the text describes the Yuba River hydrograph impairment without mentioning that the watershed provides significant amounts of water for transfers across the Delta.

P. 2-34: The meaning and significance of the phrase "fluvial fan system" in the first paragraph should be explained. In the second sentence of the following paragraph, the phrase "a lower section which connects to the San Joaquin River and the Cosumnes River and Dry Creek" (emphasis added) is unclear, since it more precisely flows into the San Joaquin River and receives the inflow of the Cosumnes River and Dry Creek.

P. 2-35: The zero UIF values are primarily an artifact of how they are calculated. Evaporation from reservoirs and inaccurate reservoir storage change and diversion result in zero or negative flows, when in actuality the unimpaired flows would not be zero but very low (the unimpaired summer flows can be very low or in sections in the upper watershed could be dry due to the high proportion of bedrock in the Mokelumne watershed).

P. 2-37: The runoff to storage ratio—provided here for the Calaveras River—should be provided for all the major regulated streams.

P. 2-52: A brief geographical description of the Delta should be provided, noting for instance that it is an inland delta and geologically very young.

P. 2-54: At this first mention of Suisun Marsh, more geographical context should be presented, and a label should be added to Figure 2.4-1 on page 2-53.

P. 2-55: The first full sentence of the first paragraph somewhat overstates the role of salinity intrusion in the creation of the CVP. Salinity intrusion was certainly one of the important contributing factors; another was to provide a more stable water supply to the San Joaquin Valley and alleviate groundwater overdraft, which was exacerbated by the prolonged dry period after 1919 through 1934.

The last sentence of the first paragraph is not consistent with the data provided elsewhere for the Sacramento River in the draft Report, nor is it consistent with the figures in Fleenor et al. 2010 (where the statement in the original applies to Delta outflow, not inflow). The statement in Fleenor et al. 2010 precedes its Figure 7, which shows approximately a 2.5 MAF reduction in NDOI in April, a 2.5 MAF reduction in May, and a 1.5 MAF reduction in June; however the draft Report references Figure 5, which shows approximately for the San Joaquin Valley a 0.5 MAF reduction in April, a 1.1 MAF reduction in May, and a 0.7 MAF reduction in June, for a total 2.3 MAF Apr-Jun reduction from the San Joaquin, or 35% of the total reduction in NDOI. With the exception of June, Fleenor et al. 2010 Figure 4 shows approximate reductions from the Sacramento Valley are greater than the San Joaquin Valley during that time: 1.5 MAF in April, 1.2 MAF in May, and 0.2 MAF in June, for a total 2.9 MAF Apr-Jun reduction from the Sacramento, or 45% of the total reduction in NDOI. Even the June reduction in San Joaquin flows is less than 50% of the total reduction in Delta outflow, so the statement that the April-June reductions are largely the result of San Joaquin River diversions is incorrect. Even when the eastside tributaries to the Delta are included, the term "largely" is still an overstatement.

P. 2-57: The first two sentences of the third paragraph should be revised to reflect the fact that installation of the barriers alters the magnitude and *may* alter the direction of net flows. Since net flows are usually significantly negative, it usually doesn't alter the direction. OMR was positive on only 80 out of 2100 days since October 2010, or less than 4% of the time. On no occasion since October 2010 did the installation or removal of the HORB change the direction of OMR flow. Since this relationship is well documented, the phrase "there is evidence" in the third sentence should be rephrased or removed to convey more certainty, or moved toward the end of the sentence so it applies to entrainment and not negative OMR. According to Ruhl et al. 2006: "The USGS flow stations at Old and Middle Rivers have been operated since 1987 through support from the Interagency Ecological Program (IEP). These data are strongly correlated to San Joaquin River flow at Vernalis, export rates, and South Delta barrier configuration." The fourth sentence should be revised to say that net positive flows *may* resume (since they almost always remain negative).

P. 2-58: The last two sentences in the first paragraph of Section 2.4.5 omit the fact that peak water year exports occurred in 2011.

P. 2-60: The last sentence of the first paragraph notes that Delta outflow and the position of X2 are closely and inversely related with a time lag of about 2 weeks. The draft Report should consider the implications of the difference between this statement and the fact that Dayflow's daily equation has X2 responding to flow within 1 day, and discuss the methods used for compliance and why real time water quality measurements are not used for the X2 estimate.

P. 2-60: A discussion of NDO vs. NDOI and the Delta Science Program review of NDOI should be included in the revised Report. Also, while "remote sensing techniques…may require significant resources to be applied to the entire Delta," that investment should be compared to the alternative of requiring every diversion and return flow to report in real time.

P. 2. 64: Table 2.4-2 shows the percent contribution of UIF at various locations to unimpaired Delta outflow. A similar table showing the impaired or observed contributions should also be included, as well as the simulated contributions under each of the Phase I alternatives, assuming Phase 1-required flows become outflow as recommended in the Draft Report.

P. 2-69: The six-year drought occurred during WY 1987-92, not 1988-92. The "Dust Bowl" drought occurred in 1928-34 (1928 runoff was a little below the average and was included in the SWP/CVP critical drought period operations analysis). The period of 1923-34 should be considered as a prolonged drought period, since every year except one (1927) was below average runoff and 1924 was the second lowest in the UIF record. The two-sentence climate change analysis at the end of the section is inadequate and needs to be expanded. There is no need to know "exactly" how the effects will occur; uncertainty is bounded by known likely extremes. Prolonged dry periods (50 to 100 years in length occurred in California and the Bay-Delta watershed several times in the recent geologic past (within the last millennium) – see multiple publications by Stine for Sierra watersheds using stratigraphic evidence and radiocarbon dating; also Graham and Hughes for the Merced; and DWR 2015 and Meko et al., 2014 using dendrochronolgoical data for the Sacramento River, which includes trees from Oregon. Human-caused climate change overlays a predictable temperature increase over existing drought hydrology, thus making equivalent hydrologic deficits more severe. The hundred year medieval droughts must be planned for, regardless of human-induced climate change, and reducing human water demands makes human and natural systems more sustainable and resilient to such variations in hydrology and climate.

P. 2-70: Does "historical levels" refer to pre-project or early 20th century levels? Historical should not mean pre-development since salinity incursions were muted during droughts by the large expanse of wetlands and generally more prolonged snowmelt inflows during the later spring and summer.

Chapters 3 and 4: Per general comment 8, these chapters largely omit consideration of flow effects on foodwebs, salinity regimes, habitat formation, water quality, exotic species and recreational and commercial fisheries in the lower estuary and nearshore coastal waters. The Report should be revised to reference and incorporate analyses and findings from the relevant studies summarized in TBI 2016, as well as studies that appeared since TBI 2016 went to press, including Warrick and Farnsworth 2016, Raimonet and Cloern 2016, and Cloern 2016.

P. 3-3: Lindley et al 2007 should be cited in the discussion in the last full paragraph regarding the importance of genetic and life history diversity.

P. 3-4: The following sentence should be added to the third full paragraph: "Flow variability is strongly tied to juvenile salmon productivity in rivers of the Central Valley (Zeug et al. 2014)." The text should also cite Beechie et al., 2006 and Johnson and Sturrock 2016.

P. 3-4: TBI 2016 should be cited after ("Kondolf et al. 2006; Poff et al. 2007") in second sentence of the last paragraph.

P. 3-5: The Board's own analysis of the serious temperature effects of dams in the Revised Draft Phase 1 SED should be cited in the second full paragraph.

P. 3-5: The third full paragraph should note that Reclamation failed to control temperatures to protect fishery habitat below Shasta Dam during the recent drought despite the operation of the Shasta TCD. Decisions on when and how much water to release for consumptive purposes are fundamental to temperature control; engineering approaches to temperature management may ameliorate temperature conditions but will not provide any benefit unless water releases and carryover storage are managed appropriately (Nickel et al., 2004).

P. 3-6: The statement that "warmer temperatures (8° Celsius [C] to 25°C) during salmonid rearing periods may also promote <u>optimal</u> growth" (emphasis added) in the first paragraph is incorrect. Optimal temperatures for juvenile rearing are 10-16°C in limited food environments; 13-20°C with unlimited food (i.e., off-channel on a productive floodplain). Adverse effects such as increased disease risk and increased predation occur at >20^{o}C. Growth and survival is possible up to 25^{o}C *if* food is abundant (e.g., on a floodplain), but 20^{o}C is considered the upper limit of rearing and migration temperatures for in-channel habitats. *See* USEPA 2003.

P. 3-7: The second and third full paragraphs should be rewritten as follows to more accurately reflect the best available science regarding flow-abundance relationships in the Bay-Delta estuary, i.e., that most of the relationships have not weakened at all (the relationship and the step-change are two different issues) and that assertions regarding the effects of the overbite clam invasion on certain native fish populations are contradicted by more recent research (additions in bold, deletions in strikethrough):

> Statistically significant inverse relationships have been demonstrated between the landward extent of X2 and the abundance of a diverse array of estuarine species ranging from phytoplankton-derived particulate organic carbon at the base of the food web through primary consumers, benthic fish, pelagic fish and piscivores (Jassby et al. 1995**, Kimmerer et al 2002**, **Mac Nally et al 2010**). The diverse taxonomy, biology and distribution of these estuarine organisms showing these strong relationships indicate a broad positive response of the estuarine community to increasing outflow (Jassby et al. 1995). The X2-abundance relationships of many estuarine species have persisted since systematic sampling programs began in 1967. In some cases the statistical relationships have ~~weakened or~~ shown downward step changes; ~~in response following the 1987 spread of the invasive clam Corbula~~ **however, some of these relationships have not changed through the period (e.g., Bay shrimp, striped bass survival) and one (American Shad) showed a step increase in the flow X2-abundance relationship** (Kimmerer 2002; Kimmerer et al. 2009). ~~nevertheless~~ **Most of the flow-abundance relationships** persist and continue to explain a large fraction of the variation in the abundances of these species. Updated flow-abundance analyses performed by State Water Board staff are included in the species profiles later in this chapter.
>
> As discussed in more detail below, the specific mechanisms underlying the flow-abundance relationships are generally not resolved **(Kimmerer 2002b)**. Salinity changes and flow are inseparable so these relationships are referred to as either flow-abundance relationships or fish-X2 relationships. Further investigations are recommended and ongoing (Kimmerer 2002a; Kimmerer 2004; Reed et al. 2014). However, most of the relationships continue to remain strong since first described and better understanding of the likely mechanisms is rapidly developing. **For example, Nobriga and Rosenfield 2016 studied the strong and persistent relationship between flow and abundance of longfin smelt and found that the relationship arises during the transition from adults to post-larval juveniles and that adult-to-larval productivity as a function of flow has not changed throughout the data time series; after disaggregating sampling data by longfin smelt life stage, they found no evidence for a step-change in juvenile-**

> **to-adult survivorship that corresponded to the invasion of *Corbula* clams (contrary to the suggestion of previous researchers). Their research suggested that forces driving a slow decline in juvenile-to-adult survivorship lay beyond the Delta.**

P. 3-8: The phrase “since the overbite claim invaded” in the second sentence of the first full paragraph should be removed, per the findings of Nobriga and Rosenfield 2016 regarding longfin smelt (see previous comment).

P. 3-9: The third paragraph should more clearly acknowledge that, in addition to salinity and currents, juvenile salmon likely also use smell (chemical gradients) to orient towards the ocean. Salinity is unlikely to help orient migrations in the freshwater Delta and currents alone are unlikely to orient migrations in environments where tidal excursion is larger than the effect of river currents.

P. 3-10: The first full paragraph on p. 3-10 should cite Castillo et al. 2012 regarding the fact that pre-screen mortality rates mean that the number of fish salvaged is orders of magnitude less than the number of fish lost in the export facility channel infrastructure.

P. 3-12 et seq: The organization of Section 3.4 should be revised to analyze flow needs for steelhead separately from Chinook salmon.

P. 3-12 et seq: The sections of the text regarding the timing of salmon and steelhead life histories should reference and incorporate the findings and analyses of Williams 2010; citations of Moyle 2002 should be augmented by Williams’ more recent findings; and consideration of hatchery influence should be informed by this paper.

P. 3-15: The last sentence in the first paragraph referencing Bjornn and Reiser 1991 and Snider 2001 should be replaced with a new sentence referencing a) USEPA 2003 regarding target temperatures ($\leq 55.4^{o}$F as a 7DADM) and examples of negative impacts of temperatures in the sub-optimal range; b) SEP 2016 which identifies 56.8^{o} 7DADM as “detrimental” (=fish exposed to such temperatures suffer high direct and indirect mortality); and c) Martin et al., 2016, which recommends even lower temperatures in the redds to avoid egg mortality.

P. 3-16: The fourth sentence in the first paragraph citing Brett 1952 and Moyle 2002 should a) also cite USEPA 2003 regarding the use of a 7DADM and b) replace the reference to Moyle 2002 with references to Marine and Cech 2004, Myrick and Cech 2004, and Myrick and Cech 2005 (which provide specific Central Valley temperature results which do not support Moyle’s speculation that tolerances differ between salmon runs).

P. 3-16: The first sentence in the second paragraph is overly broad and should be revised to be consistent with the rest of the paragraph, which describes complex lateral movements affected by numerous factors including but not limited to tides. The implication that salmon migration downstream is a passive response to the tides is misleading.

P. 31: Adult fall-run Chinook salmon migration is less common in July than it is in December (especially in the San Joaquin tributaries). Table 3.4-4 should be modified either to eliminate July, or add December to the fall-run row, or do both.

P. 3-33: The first paragraph appears to assume that the fall run was always the dominant Central Valley run, but the evidence suggests otherwise (see the various studies by Yoshiyama and his colleagues and Williams 2006). Construction and operation of the existing system of dams likely favored the fall-run life-history strategy over the spring-run life-history strategy, but the effects of warming temperatures on water management may make the spring-run strategy a more effective life-history strategy for Central Valley salmon populations, at least in some years. This is another example of the importance of considering the effect of alternative WQCP objectives on the ability to support the different life history strategies employed by different run.

P. 3-35 et seq: The section on juvenile outmigration should reference Satterthwaite et al. 2014, Miller et al. 2010, and Carlson and Satterthwaite 2011 regarding the importance of protecting life history variability and the temporal tail ends of each run's migration period, and should reference Michel et al. 2015, Cunningham et al. 2015, and Buchanan et al. 2013, regarding flow effects on survival through the Delta. The text should also reference Kimmerer 2008 regarding the potential effect of entrainment/salvage on endangered salmonids.

P. 3-40: The first four rows in Table 3.4-7 identify flows needed to increase the abundance and survival of Chinook salmon populations. In contrast, the last two rows represent minimum constraints on exports and diversions to prevent jeopardy of extinction to the endangered anadromous fish populations. The latter is quite different from the flows that would encourage population growth, and the Draft SED must consider alternatives that include *net positive* OMR rates and export rates that are less than 20-100% of inflow rates) for all Central Valley salmon populations. These two sets of rows should be labeled separately in order to avoid the demonstrably erroneous conclusion that negative OMR flows and some level of exports are necessary to support population growth and recovery. (This comment also applies to the tables for other species like longfin and Delta smelt).

P. 3-46: The second sentence in the second paragraph of Section 3.5.4.1 should be deleted because it has been superseded by more recent information. Kimmerer 2002 found a "step-change" in 1987 because he tested (only) for a 1987 effect; he then concluded that longfin smelt may have responded to a postulated food-web change related to the invasion of *Corbula* clam. Thomson et al. 2010 found support for multiple change points. However, Nobriga and Rosenfield 2016 found no evidence of any change through time in the statistically powerful relationship between Delta outflow and success of the adult-to-juvenile transition. They found stronger support for a step change in juvenile-to-adult survival that occurred a few years after the clam invasion than they did for a step change corresponding to the clam invasion, but (a) the juvenile-adult survival rate was not related to flow (i.e., no step-change in the flow relationship) and (b) models that incorporated a step-change in survival were indistinguishable from models that included a linear change in survival. The upshot is that: flow and adult spawning stock determine annual production of juvenile longfin smelt in a way that has not changed during the data series and "the mechanisms affecting juvenile survival are more likely to operate in mesohaline or marine environments than in freshwater or low-salinity-zone waters" (Nobriga and Rosenfield 2016: 55).

P. 3-46: The last sentence in the third paragraph of Section 3.5.4.1 should be deleted because both the conclusion, and the analysis it is based on, are not valid. First, there is strong evidence that the clam effect has been overstated or misinterpreted (Nobriga and Rosenfield 2016, see above). Second, the post-clam persistence of the flow-abundance relationship has been clearly documented by numerous published studies, including:

a. Kimmerer 2002;
b. Rosenfield and Baxter 2007;
c. Sommer et al. 2007;
d. Kimmerer et al. 2009;
e. Mac Nally et al. 2010;
f. Thomson et al. 2010;
g. CDFG 2009;
h. Nobriga and Rosenfield 2016.

The analytical approach of identifying necessary flow levels based on the longfin smelt flow-abundance relationship no longer represents the best-available science as several studies demonstrate that longfin smelt abundance is constrained by spawning stock, in addition to flow (e.g., Rosenfield and Baxter 2007; Thomson et al. 2010; Nobriga and Rosenfield 2016). Because it ignores the constraint of spawning stock, the method presented in this paragraph will overestimate the flows needed to attain any given abundance.

The analyses of flows needed to support longfin smelt populations should focus on flows necessary to support increased frequency of population *growth*, as opposed to the flows necessary to achieve an abundance target in a single year; the Board is well-justified in focusing on the frequency of population growth, as this is directly tied to the "productivity" criteria for viable populations (see McElhany et al. 2000; TBI et al. 2010 exhibit 1). This focus underlies the methodology applied in Figure 3.5-3, which recreates and updates the analyses originally presented by TBI in 2010. Although it is no longer clear that there was a step-change in the population in 1987 (see discussion above of Kimmerer 2002 and Nobriga and Rosenfield 2016), it still makes sense to truncate the longfin smelt time series in the late1980s or early 1990s because Nobriga and Rosenfield (2016) found evidence of declining juvenile-adult survival (which may or may not be best represented as a step-change in that time frame; see also Thomson et al. 2010) and it is appropriate to focus on the effect of flow on population dynamics produced by the more recent juvenile-to-adult survival pattern.

The Draft Report finds that when years subsequent to TBI's original analyses are incorporated, the flow required to produce a 50% chance of growth are lower than what we estimated using the data available in 2010. We agree that it is difficult to identify the precise flow level that corresponds to a 50% growth rate because natural variability and measurement error generate variance around the regression line; however, we also note a need to be careful (from the fish population's point of view) about setting the flow recommendation too low, based on this approach. First, it is likely that to recover and maintain a viable population of longfin smelt (or any species) conditions need to support population growth >50% of the time. That is different from saying the population should actually grow >50% of the time. Carrying capacity limits may constrain the frequency of growth at some point, and this is as it should be; density-dependent mortality is an important feature of ecology and evolution of many organisms. Secondly, the longfin smelt population has declined by somewhere between 99-99.99% over the period of record; population growth will need to occur in >50% of years in order to restore the population to viable levels. Thirdly, as with any endangered species, the precautionary principle must apply – the risks of catastrophic outcomes resulting from one year of population decline are extremely high now (and will remain so until the population has grown by more than one order of magnitude). In the light of these considerations, flow criteria based on this kind of analysis should be higher than those suggested in Table 3.5-1.

P. 3-49: The text in Section 3.5.4.2 should reference Rosenfield 2010, including the observation that salvage is inverse to population size, making it likely that entrainment events may have severe population level consequences even if they are only periodic.

P. 3-55: The section on green and white surgeon should incorporate analyses from CDFW's conceptual models for these species (Israel and Klimley 2008; Israel et al. 2009). Like the other "DRERIP species life history conceptual models", these peer-review documents specifically evaluate flow and flow-related effects on the abundance and productivity (survival rates) of their topic species.

P. 3-65: Section 3.8.4.1 appropriately incorporates emerging evidence of a positive relationship between spring Delta outflow and Delta smelt survival rates as measured by comparing the annual change in abundance index values between different sampling programs. However, the draft Report is too quick to adopt the IEP's caution regarding this finding. For example, the second full paragraph on p. 3-65 states:

> The standardization suggests that both the number of available spawners (stock-recruitment effect) and the magnitude of spring outflow are important for determining larval abundance. More spawning adults result in more larvae, if outflow is favorable during the spawning season. The spring outflow and the stock-recruitment relationships together explained 59 to 65 percent of the variation in the 20mm index for the 11 years between 2003 and 2013 ($P<0.006$, IEP 2015). However, the IEP (2015) report recommended that conclusions based upon the relationship between spring outflow and Delta smelt population abundance be considered preliminary until additional data, analyses and review were conducted to confirm the robustness of the results.

A large and very significant statistical effect exists, and it is unreasonable for the Draft Report to discount this data. The Draft offers no rationale as to why the Board (or anyone else) should consider this result to be any more "preliminary" than any other result based on 11 years of data; the statistical analysis shows an extremely low probability that this pattern would be detected if the relationship between flow and estimated survival was actually random. Also, given the extremely low levels of Delta smelt that are detected in the estuary, it is very unlikely that we will be able to confirm or refute this finding with "additional data." The Board should solicit input from M. Nobriga at USFWS who we understand has conducted additional analyses of the existing data.

P. 3-67: Section 3.8.4.2 should reference and incorporate analyses and findings from Nobriga et al. 2008.

P. 3-69: Table 3.8-1 should include flow recommendations for spring Delta outflow since there is strong evidence (discussed above) that such flows are tightly linked to population growth (reduced intra-annual mortality) in Delta smelt.

P. 3-74: The statement in the second sentence of the first paragraph that starry flounder are a minor part of the commercial fishery is misleading. Ralston 2005 indicates that commercial harvest of starry flounder off the California coast was over 100 metric tons every year prior to the 1989 harvest. The commercial catch is always higher than the recreational catch, sometimes by an order of magnitude. The 1989 harvest represented a steep decline from previous harvest levels, reflecting poor recruitment of fish spawned at the onset of the 1987-1994 drought. Commercial harvest has remained low (but still many times the sport harvest) following that drought. This sharp decline in harvest suggests that the adult spawning population has collapsed as well.

P. 3-75 et seq: Section 3.9.4 should clarify that the flow-abundance correlation for starry flounder is between Age-1 individuals and flow the previous spring; the lag is used because Age-0 fish (fish recruited to the estuary in the same year that flows occur) are not well detected by the Bay Study's otter trawl gear (Kimmerer 2002).

The flow levels identified in this section as supporting starry flounder production should be recalculated based on the full time series available for this fish (1980-2015). The draft Report repeats Kimmerer 2002's finding that a step change in the flow-abundance relationship for starry flounder occurred in or around 1987; Kimmerer implies that this decline was due to grazing of primary productivity in the estuary by *Corbula* clams. However, it is equally likely that the only thing that has changed in the flow-abundance relationship is the precipitous decline in spawning stock of starry flounder that corresponds to the onset of the 1987-1994 drought (see harvest data presented by Ralston 2005). Given that starry flounder are relatively long-lived and iteroparous, it is likely that the "step-decline" reported for this population in the San Francisco Bay Estuary (Kimmerer 2002) is at least partially related to a very large decline in the stock population; as with the analogous pattern in longfin smelt, Kimmerer's suggestion that these step-declines were due to the *Corbula* clam invasion was not based on elimination of other potential explanations, such as stock-recruit dynamics. The hypothesis that *Corbula* grazing caused the step-decline of starry flounder seems highly unlikely because (a) starry flounder feed several trophic levels above *Corbula* clams and trophic cascades rarely extend so powerfully and quickly through so many trophic levels and (b) there is little evidence that the food web impacts attributed to *Corbula* clams extended to zooplankton (see the draft Report's section analyzing bay shrimp) or the zooplanktivorous fish that starry flounder prey on (see Kimmerer 2002, Figure 8 – other than longfin smelt, no other potential fish prey shows a negative step-decline in this period). This fact is one of the major findings of Kimmerer 2002 that is often misunderstood.

Thus, absent new evidence, the Report should refrain from attributing the decline of starry flounder to impacts from *Corbula*. Unless one assumes that the presence of *Corbula* limits and will continue to limit starry flounder population dynamics, it is appropriate to evaluate both the population decline and the flow-abundance relationship throughout the entire time series. Doing so will reveal that (a) there has been a significant population decline of at least one order of magnitude and that (b) there is a strong, statistically significant correlation between flow and abundance (see TBI 2016, Figure 11). In addition, the scientific basis report should include the 2015 starry flounder abundance index (=6, CDFW *unpublished*) in its analyses.

Once the entire data set for starry flounder is employed, the Board should revisit the findings in Table 3.13-1 and the flow recommendations in Table 3.9-1.

Finally, we question the rationale for the population target for starry flounder (Age 1 index = 293). Such population targets appear to be based on the assumption that the population cannot recover its former abundance levels (which were >5x of the target level) because of the presence of *Corbula* in the estuary. As described above, there is little reason to believe that the estuary's starry flounder population declined because of the clam invasion or food web limitation; the decline may very well have been due to the onset of drought, severe reduction in Delta outflow, and corresponding decline in starry flounder recruitment. If the fundamental growth-recruitment potential of the starry flounder population has not changed (as the unchanged slope of the starry flounder flow-abundance relationship indicates) then there is no justification for assuming that this once important commercial fishery cannot be restored to at least its average, pre-drought (1980-1986) levels.

P. 3-80 et seq: Section 3.10.4.1 presents three different methods of estimating flows needed to support Crangon shrimp populations. Of these three methods, the third is biologically relevant, the first has utility for comparison and planning purposes, and the second is arbitrary.

The first of these methods, identifying the flow level that has historically been related to a target population level, has value in comparing the results to the third method and in evaluating the selection of the target population level. This latter value is particularly important since Crangon is a major food source that is likely prey for all of the fish species of concern (except juvenile salmon), and setting the target too low creates cascading effects throughout the estuary foodweb.

The second method simply picks the median flow level between 1980 and 2013 – this has no relevance to protecting the bay shrimp population, because it reflects two periods in which we know that flows were inadequate to support public trust resources and designated fish and wildlife beneficial uses (i.e., flow conditions that necessitated the 1995 amendment of the WQCP and subsequent flow conditions).

The third method for identifying flow is biologically relevant – population growth is a biological parameter of obvious importance and the difference between population growth and decline is transparent. Viable populations should experience abiotic conditions in which population growth is possible in most years – in areas without such regularly supportive conditions, local populations would quickly disappear. Flow levels that produce population declines more often than population increases are clearly not sustainable and not a recipe for a viable population. The Bay-Delta estuary should provide flows that are consistent with *Crangon* population growth in more than half of years (this does not mean the population will grow in more than half of years, only that population growth up to carrying capacity is possible) – this population is a key food source for many other desirable populations and is the target of a fishery (Jassby et al. 1995; Kimmerer 2002).

Thus, flows that produce population growth in 50% of years (average 26,000 cfs from Mar-May) are the bare minimum conditions for a viable *Crangon* population. This should be the only value (perhaps with an error bound derived from the logistic regression) reflected in Tables 3.10-1 and 3.13-2.

P. 3-90: The flows in Table 3.13-2 should be revised to reflect the recommended changes to the analyses described above. In addition, the table should clearly reflect that all of the methods used to derive flow recommendations (e.g., flows that result in population growth in only 50% of years) are extremely conservative. These flows represent minimum flows needed to protect public trust resources and designated fish and wildlife beneficial uses. Setting lower flow requirements in an attempt to "balance" beneficial uses would result in loss of the fish and wildlife uses, since flow objectives that result in populations declining more frequently than they increase (e.g., flows that are lower than those that support population growth for a given species in 50% of years) leads inevitably to population collapse and species extinction.

P. 3-91: In addition to the months indicated in Table 3.13-3, the best available scientific evidence suggests that longfin smelt respond to flow conditions in June (Nobriga and Rosenfield 2016); there is no evidence to the contrary. The Report should include June flow recommendations for longin smelt. The evidence developed by the U.S. Fish and Wildlife Service and California Department of Fish and Wildlife also indicates that Delta smelt populations respond to freshwater flows in all months of the year (USFWS, *unpublished*). The Report should identify flow levels in each month of the year that are protective of Delta smelt.

P. 3-92: Per general comment #3 and elsewhere, the range of interior Delta flow alternatives in the revised Report and draft SED should include measures that are more protective than what is required by existing permits under the state and federal Endangered Species Acts. The Board has independent obligations to protect fish and wildlife that are more protective than the ESA-mandated minima, and these interior Delta flows are also important to protect migrating fall-run Chinook salmon (which are not listed). Restrictions on fall pumping to protect adult fall-run Chinook salmon returning to the San Joaquin Basin should be consideration in November as well as October. *See* Marston et al 2012.

P. 4-11 et seq: Grossman 2016 should be cited here, as well as in other sections in the Draft where predation is discussed, regarding the relative importance of predator removal, habitat restoration, and increased flow of fresh water as potential strategies to increase salmonid survival through the Delta. Grossman states: "It is likely that the most productive management strategy for decreasing predation on Chinook Salmon and other Delta fishes is to restore natural habitat and flows, especially in predation hot spots."

P. 4-12: The text in Section 4.4.1.5 should cite Mahardja et al. 2016 regarding the effect of flow on limiting/controlling *Menidia* populations.

P. 4-16 et seq: This section should reference Dettinger, et al. 2016.

Chapter 5: See general comment #9 for an explanation of why the Board should evaluate Delta inflow and outflow alternatives that are >75% UIF.

P. 5-3: The point should be made more clearly in the first paragraph that one of the benefits of the UIF approach is capturing natural variability both between seasons and between years. Richter et al., 2011 should be cited in the second paragraph.

P. 5-4: The point should be made in the first full paragraph that one of the benefits of the UIF approach is that water allocated for environmental purposes will be provided when it is needed and in full, rather than delayed or reduced because of operational management approaches that rely on conservative forecasts of available flow and more cumbersome year type designations.

P. 5-5: The first sentence of Section 5.2 states that the general approach is to recommend "new inflow requirements for anadromous fish-bearing tributaries in the Sacramento River basin and Delta eastside tributaries." Footnote 2 states that Cache Creek does not support anadromous fish and that it should be an exception to the general approach due to fish benefits in the Yolo Bypass and contributions to Delta outflow. However, Cache Creek historically and in the recent past has supported anadromous fish (Moyle & Ayers 2000). More broadly, the justification for the Cache Creek exception – the existence of Delta outflow and fish benefits downstream of the tributary – equally applies to all the tributaries in the watershed of the Bay-Delta estuary. Furthermore, every tributary that is not included in the percent UIF approach makes it harder for the other portions of the watershed to achieve the percent UIF requirements downstream in the estuary. The Board would be wise to include as much of the watershed as possible in this approach, even where no anadromous fish are believed to be currently present.

P. 5-5: The available tools to evaluate flow impairment are generally constrained to use of a monthly time step. In refining the SacWAM model and developing other analytical tools, the Board should consider evaluating impairment at a shorter time step – e.g., 3 days up to 15 days, given that the shorter duration pulse flows can be important for triggering key ecological responses (such as turbidity, migration pulses, and flood plain stimulation).

P. 5-7: The discussion in the first paragraph demonstrates why the late fall and winter flows on these smaller, relatively unregulated tributaries can be used as a rough surrogate for unimpaired runoff during high runoff periods and can be used to evaluate the relative impairment of Sacramento Valley outflow and Delta inflow during shorter duration high flow periods.

P. 5-9: It is unclear whether the discussion of flows at Freeport includes flows in the Yolo Bypass. This should also be clarified in chapter 2, which displays statistics separately for these two conveyances of Sacramento River flow, and which must be summed in order to determine the total flow from the Sacramento Valley entering the Delta.

P. 5-12: In response to the Board's request for input regarding the Draft Report's proposed approach, we recommend:

- Comparing the use of the 10 River Index to the 8 River Index, and using the former if it improves the regression significantly.
- Using unimpaired Vernalis flows instead of existing flows.
- Using the 7-day average most recently available, with each rim station lagged for travel time, instead of using the previous or current month index.

We are available to work with the Board to expand on and refine these recommendations.

Page 5-15: The Draft Report assumes that San Joaquin River basin flow contributions reflect existing conditions, and do not include potential increases in basin inflow as a result of the Phase 1 update of the WQCP. Given the effect of changed San Joaquin inflow on environmental conditions in the Delta and beyond, the revised Report and draft SED should incorporate increased Vernalis inflows including the UIF range identified in the Revised Draft Phase 1 SED as well as the 60% UIF recommended in SWRCB 2010.

P. 5-15: In developing the multiple regression discussed here and resulting in the parameters in Table 5.3-1, presumably the unimpaired Delta outflow estimates described in Chapter 2 and Appendix A were used. The statement that there is "very little change in unimpaired hydrology through the Delta" (A-38, Fig A-31) is inconsistent with DWR's unimpaired flow reports (table B-30 and page 3-15 of DWR 2016a as well as previous unimpaired flow compilations) which show about 1 MAF of unimpaired Delta net use due to unimpaired Delta losses exceeding precipitation. See comment on p. A-37 for further detail regarding our caution against using this regression without further investigation.

P. 5-19: The last sentence in the first paragraph is incorrect. Sturgeon life history strategy capitalizes on spawning, incubation, and/or early rearing conditions (e.g., high river flows) that naturally occur less frequently. This does not mean that making those conditions occur even less frequently has no effect on their population biology, chances of persisting in this estuary, or the ability to support a public trust resource (i.e., a fishery). The fact is that the local DPS of green sturgeon is threatened with extinction *despite* being adapted to variable conditions that only rarely support reproductive success. Water management regimes have made the recurrence interval of such conditions so infrequent that the population is trending downwards. The population decline may appear slow to us because the fish's reproductive life span is so long, and there are occasionally very wet years that support successful reproduction, but this does not mean that flow regime effects to green or white sturgeon should be dismissed – by the time we fully understand the impacts of flow modification on their population biology, it may be difficult or impossible to intervene to protect them.

P. 5-22: Per our comment regarding p. 3-80 et seq, Figure 3.5-6 should be revised to reflect the reality that a) the *minimum* flow needs of bay shrimp should be based on the occurrence of population growth in 50% of years (the "bay shrimp low" flow does not meet this threshold) and b) that the recurrence frequency for favorable conditions for longfin and bay shrimp (and other species) should be well over 50% of years (both longfin smelt juvenile and "bay shrimp high" flow are identified as those that supported population growth in 50% of years). In other words, the populations should experience conditions that support growth much more than 50% of the time (whether growth occurs or not reflects other variables, including carrying capacity). These species have high intrinsic rates of growth and demonstrate explosive population growth potential -- their population size would normally be limited by carrying capacity limits, *not* by their ability to grow to carrying capacity in most years. The fact that they are semelparous (spawn in only one year and then die) demonstrates that historically, reproduction was almost always successful, that egg-juvenile success was high relative to adult survival, and that populations were rarely limited by poor survival of early life-stages. It is important to note that rebuilding these populations is essential to the Bay-Delta estuary food web and the success of other fisheries (as well as fish-eating bird populations). Longfin and bay shrimp were once among the most abundant and widespread prey sources in the estuary – many other estuarine species relied on them for food. Both supported fisheries historically (bay shrimp still do support a very limited fishery).

P. 5-23: Per our comment regarding p. 3-75 et seq, Figure 5.3-7 should be revised to incorporate the results of recalculating starry flounder flow needs when the assumption that pre-1988 levels of the population cannot be reestablished is removed. This assumption is based on speculation that starry flounder populations are constrained by *Corbula* grazing, as opposed to spawning stock limitation. The latter, which is more likely to be the case, can be addressed by encouraging more frequent years of good recruitment. Starry flounder were until recently an important commercial and recreational fishery and the public trust value of the fishery should be restored. Providing flow conditions that support successful juvenile recruitment (i.e., sufficient Delta outflows) are essential to rebuilding this stock.

P. 5-26: There is no scientific basis for the assertion that estuarine species would be expected to derive "modest benefit from the 50% scenario." Average Delta outflows are ~47-50% of UIF, and the result is decline for all and near extinction for some of the species being considered. Maintaining status quo flow conditions (more or less) should be expected to facilitate continuing population declines and extinctions, rather than benefit these species.

P. 5-27: The assumptions underlying Table 5.3-3 and described in the text of Section 5.3.3 are incorrect. This section and the table need to be significantly revised to reflect the facts that the population change estimates produced here:

- Do not reflect stock-recruitment dynamics (i.e., how the population level resulting from a given flow is partly related to the spawning population level that experiences that flow) and the potential for increased *frequency* of growth to lead higher populations associated with any given flow.

- Appear to assume that particular flows produce a particular population level as opposed to flow levels being associated with a likelihood and rate of population growth (the latter is a more biologically realistic assumption).
- Appear to assume that status quo flow conditions will maintain populations at a certain average level, when the evidence indicates that status quo flow management (i.e. the "existing" line in all the previous exceedance graphs) has led to precipitous declines in numerous species
- Do not seem to reflect the explosive growth response to certain flow levels seen among most of the species analyzed (e.g., populations that increase many-fold or by orders of magnitude in a single year, in response to adequate flows).

P. 6-4: Peer review is needed of DWR 2016a, particularly with regard to its assessment of natural flows. Many commenters have identified problems with the report's assumptions regarding hydroclimate, natural vegetation and interaction of groundwater and stream flow; erroneous data; and unexplained and unjustified use of methods that are inconsistent with previous technical efforts. While DWR's unimpaired flow data can be used to inform this process, more documentation and refinement is needed before DWR's natural runoff assessment can be cited with confidence.

A-26: It should be clarified in the SVUFM description whether it relied upon CALSIM III or the outdated CALSIM II hydrologic characterization of rim inflows. CALSIM III represents a more up-to-date analysis and characterization of the unimpaired and impaired watershed and valley floor hydrology by DWR and would presumably be used in any new model development by them. Also, a review of the CALSIM III model should be performed by an outside group like CWEMF, similar to what was done for CALSIM II.

A-34: SVUFM doesn't route surface runoff below rim stations to small streams and the Delta (11 rivers are listed here). Runoff is routed directly to the mainstem Sacramento, Mokelumne, or San Joaquin. This results in an underestimate of UIF at the mouths of these tributaries (the draft identifies future work to achieve higher resolution of surface runoff). The implications of this may be similar to what is contained in the SWRCB's Phase I analysis: If a percent of unimpaired flow is set on a tributary, it might be based more on the rim station hydrology, but the additional runoff below the rim would be incorporated into mainstem flows. In contrast to the Phase I analytical approach which ignores the percent unimpaired at Vernalis, the Report and draft Phase 2 SED should conducting the analysis and developing the implementation necessary to ensure that required flows actually occur where the tributaries enter the Sacramento River and where the mainstem river enters the Delta. There are biological reasons why the unimpaired rain runoff pattern should be incorporated on the small tributaries and why potential impairment of these flows could have important adverse effects on pulse flows for salmon migration and other environmental needs.

A-37: Positive net Delta depletions do not affect flow in the Delta, but when negative, precipitation and leach water discharge contribute accretions to Delta. When precipitation exceeds consumptive use (CU, based on existing land use), the excess contributes to Delta channels. When CU exceeds precipitation, there are no Delta depletions in SVUFM. Thus, finding “very little change in unimpaired hydrology through the Delta” (A-38, Fig A-31) is inconsistent with DWR’s UIF reports (table B-30 and p. 3-15 of DWR 2016a as well as previous UIF compilations), which show about 1 MAF of unimpaired Delta net use due to CU exceeding precipitation. This discrepancy should be described and evaluated in much more detail, and the reasons for using the previous and new methods should be explained. This approach ignores the current “level of development” of the Delta which results in about 1 MAF of unimpaired Delta net use in previous studies, and is inconsistent with using the 2009 “level of development” of groundwater in the Sacramento Valley, which adds a drain on UIF; the net Sacramento Valley losses to groundwater disappear from the water balance and do not show up further downstream. More discussion is needed of these apparent inconsistencies in dealing with structural subsurface diversions in the Sacramento Valley vs. the Delta.

Thank you for considering our comments on the Draft Phase 2 Scientific Basis Report. We look forward to working with the Board to ensure that Phase 2 results in strong and timely new flow objectives that reverse the current degradation of and provide broad ecosystem protections for the Bay-Delta estuary. Please contact us if you have any questions regarding these comments.

Sincerely,

Gary Bobker
Program Director
The Bay Institute

Doug Obegi
Senior Attorney
Natural Resources Defense Council

Rene Henery
California Science Director
Trout Unlimited

LITERATURE NOT CITED IN THE DRAFT PHASE 2 REPORT

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