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Discussions

Interactive comment on "Reconstructing the natural hydrology of the San Francisco Bay-Delta watershed" *by* P. Fox et al.

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The paper describes an effort to quantitatively estimate long-term, annual average Delta outflow under "natural" landscape conditions. This is an interesting exercise that may provide insight into the pre-development hydro-ecology of California's Central Valley. However, given the extensive physical and hydrologic alteration of the watershed, the universal recognition that the pre-development, "natural" system cannot be restored and is not the objective of current and future ecosystem restoration or regulation activities, and the coarseness of these model outputs (i.e., long-term average annual flow with no information on inter- or intra-annual variability), the results as presented have virtually no relevance to ongoing flow management and regulation, contrary to

the authors' contentions.

There are numerous other problematic issues with this manuscript, many already identified by other reviewers. My comments are limited to the fundamental mismatch between the research/model results, biological responses of Bay-Delta fish and invertebrate species to Delta outflow, and the regulation of annual and seasonal Delta outflow as a management and species protection tool. I am a biologist who has conducted research, published, and engaged in the regulatory policy arena as an expert in this system for more than 20 years. On the basis of my review of this manuscript, I conclude that there are substantial, foundational flaws in the authors' interpretation and suggested applications of their results. Therefore, this paper should not be considered for publication in Hydrology and Earth System Sciences without major revisions.

Specific comments:

Based on their research and modeling, the authors report that the long-term, annual average Delta outflow under pre-development "natural" conditions was similar to the long-term average of current Delta outflow (using data from 1922-2009). The authors then conclude that, therefore, "it is unlikely that reductions in annual average Delta outflow have caused the decline in native freshwater aquatic species" (pg 3849) and "it is unlikely that reduced annual average freshwater flows have contributed to ecosystem decline in the estuary" (pg 3869). Similarly the authors' contention that "development has simply redistributed flows from natural vegetation to other beneficial uses" (presumably these other "beneficial uses" are local and out of basin urban and agricultural consumption; pg 3869) demonstrates a basic misunderstanding watershed and aquatic ecosystem function that undermines the authors' discussion and interpretation of their results. These conclusions, which stray very far from the actual utility and potential applications of the research, are fatally flawed for several reasons.

First, the relationship between Delta outflow and the survival and/or abundance of numerous fish and invertebrate species is based on surveys and monitoring results

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for the past approximately 50 years in the existing (i.e., post-development) system. We have no analogous quantitative data on biological responses to flow, whether seasonal, annual or long-term annual averages, for the pre-development "natural" system. Therefore there is no logical or factual basis to conclude that the similarity in flow volumes between the authors' estimate for "natural" flows and current measured flow volumes precludes an effect of flow in the existing system on recently measured fish declines. Native species may have "evolved under natural landscape conditions" (pg 3868) but they are responding to the existing system and actual flow levels on an annual basis.

Second, the relationship between Delta outflow and species response, which is statistically significant, robust (multiple species and taxa), and persistent (the relationship has persisted for many decades and despite major ecological changes in the Bay-Delta), is based on inter-annual variations in Delta outflow, not long-term average annual Delta outflow. Therefore, it is both irrelevant and logically flawed to relate fish declines to long-term average flows, as the authors do. Further, long-term averages, particularly of a variable like Delta flow, which is not normally distributed, can be notoriously misleading, masking large, unequal variations by lumping dry and wet years together and skewing distributions of the component annual flow data. Instead, it is the inter-annual variations in flow, and in particular both the frequency and distribution of high and low flow conditions and magnitude of seasonal flows, rather than long-term average annual flows that affect species and ecosystems. During the recent multi-decade period for which we have measured fish declines, Delta outflows have been altered substantially, reduced in recent decades by 50% on an annual basis and by more than 60% during the ecologically important spring season. This in turn, has resulted in substantially increased frequency of years with man-made, drought-like flow conditions. It is these measured changes in annual and seasonal Delta outflows, not the long-term average, that correlate significantly with the concurrently measured fish (and invertebrate) abundance and survival and are thought to be driving population declines. In addition, as the result of decades of complementary, multi-disciplinary research, we have good and growing understanding of the multiple physical and ecological mecha-

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nisms underlying this relationship. While the authors acknowledge that their results do not include estimates of any of these variables (i.e., inter- and intra-annual variability, frequency of low flow conditions) and that this constitutes a limitation of their study (pg 3869), this deficiency is serious. At a minimum, the authors' conclusions are unsupported by their results and, until they provide annual and seasonal Delta outflow model output, premature.

Third, the authors have mischaracterized the regulatory approach currently being considered to use CDWR unimpaired flow estimates as a basis for setting new regulatory flow standards for Delta outflow. They state that our understanding of the relationship between flow and fish population declines is based on unimpaired flows ("The reduced outflow hypothesis advanced by some as a cause of declines in native fish abundance is typically based on "unimpaired" flows of 34.3 billionm3 yrôĂĂĂ1 published by CDWR (2007)." pg 3867). This statement is factually incorrect and conceptually implausible: the abundance of many native fish and invertebrate species is statistically related to actual Delta outflows, which are the only flow condition these organisms experience, not unimpaired outflows estimated on the basis of runoff in the watershed. Instead, the approach currently being considered by the SWRCB to use multi-day or monthly unimpaired flows as a dynamic benchmark for new standards for minimum Delta outflow is intended the provide (and protect) seasonal and inter-annual variation in flow to the ecosystem, the same characteristics of Delta outflow that fish population do respond to.

Overall, this analysis of "natural," pre-development flows is potentially interesting work that should be continued and, when it is better developed, published. I appreciate the opportunity to review this manuscript. I hope these comments are helpful and that the authors will thoughtfully consider and respond to them in their revisions.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3847, 2015.