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February 28, 2011

City of Ventura
Attn: Karen Waln
Environmental and Water Resources Division
501 Poli Street
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Ventura, CA 93002-0099
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VIA EMAIL

Re: Second Set (February 28, 2011 set) of S. S. Anderson & R. F. Ambrose unified comments on Stillwater Sciences' February 2011 Administrative Draft of their Estuary Subwatershed Study Assessment of the Physical and Biological Condition of the Santa Clara River Estuary, Ventura County, California

Wishtoyo Foundation's Ventura Coastkeeper Program ("VCK") retained independent objective experts Dr. Richard Ambrose[1] and Dr. Sean Anderson[2] to conduct an independent expert review of the City of Ventura's Estuary Special Studies and the environmental effects of the City's Tertiary Treated Flow discharge to the Estuary to provide stakeholders with an independent expert evaluation of the affect of the City's water treatment operation on the Santa Clara River Estuary's water quality and aquatic life. Dr. Ambrose's and Dr. Anderson's first task was to submit comments to VCK on Ventura's Estuary Special Studies that were publically available for their review and analysis as of January 20, 2011.

[1] DR. RICHARD F. AMBROSE, Ph.D.; Director and Professor, UCLA Environmental Science and Engineering Program; Ph.D. in Marine Ecology, UCLA; B.S. Biological Sciences, University of California, Irvine.
[2] DR. SEAN ANDERSON, Ph.D.; Assistant Professor of Environmental Science and Resource Management California State University Channel Islands; Postdoctoral Fellow, Center for Conservation Biology, Stanford University; Ph.D. in Marine Population Biology, UC Los Angeles; B.S. in Ecology and Evolution & in Environmental Studies, UC Santa Barbara.

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Dr. Richard Ambrose's and Dr. Sean Anderson's comments below add to their comments previously submitted to Ventura by VCK on February 21, 2011 that were based on the portions of the Draft Estuary Special Studies publically available for their review and analysis as of January 20, 2011. However, Dr. Anderson and Dr. Ambrose need more time to provide their full and complete objective scientific analysis and review of the findings, methodology, analysis, and management recommendations contained in the Draft Synthesis Report, and need more time to analyze and review the Draft Synthesis Report's synthesis and discharge alternative sections. Dr. Ambrose and Dr. Anderson also need the Appendices to the Synthesis Report to complete their review.

Dr. Anderson and Dr. Ambrose expect to complete their Independent Expert Review of the City's Estuary Special Studies and the environmental effects of the City's Tertiary Treated Flow ("TTF","VWRF", or "VWRF Discharge") by May 31, 2011. This final independent review will contain their complete comments on the Ventura's Final Synthesis Report/Estuary Specials Studies.

Please address and incorporate Dr. Anderson's and Dr. Ambrose's comments contained in this letter into Ventura's Final Estuary Special Studies / Synthesis Report that will be submitted to the Los Angeles Regional Water Quality Control Board on March 6, 2011.

Comments on Stillwater Sciences February 2011 Administrative Draft of their Estuary Subwatershed Study Assessment of the Physical and Biological Condition of the Santa Clara River Estuary, Ventura County, California

S. S. Anderson & R. F. Ambrose unified comments February 28, 2011

Focal Species Selection

While this is indeed a useful approach to assess impacts from discharge on the broader estuarine community, we are curious as to why only Endangered species were selected. While steelhead and tidewater goby are natural selections based on their natural history, there are a host of other organisms that could have been selected to assess impacts that were better focal species than plovers and terns. The relatively low numbers (and regional downward trend) of both snowy plovers and least terns and the disproportionate impact of human disturbance on their abundance and distribution make them curious selections for a study on the effect so wastewater discharges. While indeed a part of this ecosystem, there were better

candidate species spanning infauna, epifauna, vegetation, and estuarine vegetation-dependent birds.

Section 3: Geomorphology

In the discussion about changes to the morphology, the authors argue there has been little gross morphologic changes between 1900 and 2005. It is somewhat unclear if their reference to current conditions is from the 2005 LiDAR surveys or from more recent (i.e. 2010) surveys of the site.

It seems reasonable to conduct another LiDAR survey of the estuary. This would yield excellent time series: the 1990 Study, the 2000 Bathymetry from Entrix, the 2005 LiDAR Data, and now a purported 2010/2011 LiDAR survey. It would serve to bolster and confirm the author's arguments that little has changed in the system and that major (but rare) storm events are the major drivers of the hydrogeomorhpology of this system.

Excellent discussion of the historic hydrogeomorphology of the system. This a much more thorough discussion of this issue than in most similar reports, and it is very appreciated and helps put the current situations in context. Having said that, the authors can improve upon this with a clearer explanation as to their sources of information for these goings on. Clearly the historic imagery from the Watershed Protection District and historic rainfall data have played a major part. But a clearer citation of their sources is called for. In addition, while the role of dam failures and other watershed goings on have correctly been mentioned here, there is no discussion of perhaps the most important (after leveeing) hydrological constraints: the Freeman Diversion. Please add this to your otherwise robust contextual background.

Table 3-1 at first seemed to suggest perhaps some significant differences in sediment characteristics between sampling dates, but our exploration shows no or effectively no statistically significant differences within segments or between years and supports the authors claims.

Section 4: Hydrology & Hydraulics

Generally there is an emphasis on the "wetting" of the estuary. To be sure this is an important component of riparian/estuarine functioning. However, one of the most important hydrodynamic components of our southern Californian estuaries was the

shift from relatively wet to relatively dry periods on a seasonal basis. This, for example, was one method by which invasive or noxious organisms were limited in our historic estuaries and is the environment within which our organisms evolved. The general tendency of southern Californian estuaries to now be "wet" all the time due to anthropogenic inputs into these systems has facilitated the invasions of many non-native or nuisance species and is partially to blame for the increasing preponderance of species of concern. While the net aggregate influence of the water volume introduced via the VWRF may be a net benefit, it is not simply an issue of dumping more and more water into this system now robbed of water via the Freeman Diversion. This needs to be better articulated.

There is no Lower Santa Clara River flow data for the key 2005 high flow period. Figure 4-1 (p.37) has a critical data gap from 2005-2007.

"Seasonal variability in ETS flows is possibly driven by infiltration and inflow of the collection system, since effluent flows are higher on average in the wetter winter months (January, February, and March) than the other months of the year" p. 39. This is a strange statement. Don't we know the sources of variability of the outflows?

The dynamics of estuary breaching (p. 46) is a critical dimension of the estuary ecology. It would be ideal if a historical ecology study (which is being completed) would provide a better characterization of the pre-development dynamics. The synthesis report does not that the SCRE likely had a lower frequency of berm breaching. However, it is somewhat surprising that the report seems to focus on the length of the mouth berm rate, and consequent seepage rate, rather than the increase in water inflow due to the VWRF.

We have previously commented on the assumptions behind the water balance study (e.g. p. 48). The assumptions with the greatest likelihood of leading to an inaccurate estimate include the use of the 2005 bathymetry, use of pan evaporation measures from a site 6 miles inland, and estimated groundwater exchange based only on the monitoring wells south of the Estuary (although flow from the VWRF Wildlife Ponds are included in this report). In the previous report, estimates of subsurface flow across the mouth berm seemed fairly simplistic and did not seem to incorporate all available information. In the Synthesis Report, the estimates have been refined, although there are still some questionable assumptions (e.g., including basing the hydraulic conductivity in part on sediment samples from the southern floodplain GW wells). It is good that an attempt was made to provide an independent estimate of the mouth berm seepage rate, although of course this, too, has significant assumptions (mainly that it is the only unconstrained estimate). It is also good that

an effort was made to estimate the unmeasured groundwater flows (p. 68), although we would appreciate a better explanation of the assumptions made.

In general, the offered water budget seems reasonable, and shows a good match between estimated and observed volume values. The water budget provides a good perspective on the contribution of the VWRF during closed-mouth conditions, especially during the dry season.

Section 5: Water Quality

Interesting that salinity (p. 79) near the mouth was only15-16 ppt when the SCRE mouth was open. This is quite low, suggesting limited ocean exchange. We noticed the maximum was 37 ppt, so sometimes there is full exchange with the ocean, but a mean of 15 ppt (during winter) means that much of the time the exchange is quite limited (or river flow high). Also a bit surprising that the middle estuary always has low salinity (max 11 ppt and mean of 3 ppt when open).

The DO values (p. 87) from daytime grab samples are not very useful, but the values from the continuously recording data sondes could be very helpful for looking at DO minima – but Table 5-6 does not take advantage of that opportunity. The influence of algal productivity is discussed at the bottom of p 87, and DO is even mentioned, but nothing about low DO conditions, which are so important for the ecology of the estuary.

As it turns out, a better analysis is presented on p. 90. It would be useful if something about this issue and later analysis was mentioned earlier.

The axis for Fig 5.6 (p. 91) is not labeled. It appears to be % saturation. We like the figure – we haven't seen this method of presenting continuous data before. However, the selection of just a few cases doesn't allow us to understand how representative each case is, or really how much of problem low DO is in the estuary. Perhaps as a result of this, the cases presented don't really match my experience with diurnal DO patterns in systems like this (high DO during the day, low in early morning, shown in only one of the graphs and not when we would have expected it). We would have liked to see a much more thorough analysis of this important parameter – although the footnote mentions a limitation in the data. Still, we think more could have been done.

The discussion of nutrients on p. 92 is simple but perhaps too concise. There is very little attempt to put the nutrient concentrations in context – are these high or low,

for example? There is one sentence about the possible contribution of VWRF to phosphorus concentrations, and that is all (although the outfall channel concentrations are mentioned elsewhere). That sentence, incidentally, seems to imply (perhaps inadvertently) that only phosphorus is contributing to algal growth, which is not correct.

Although bacteria concentrations are not a major concern, they are practically ignored in the text, with no mention even if they exceed water quality standards. Also, no mention is made about whether this is the expected temporal pattern, and the postulated explanations are vague and unsubstantiated.

It is good to see section 5.2 (p. 103) provide some of the context for the water quality results we asked for above. However, here and elsewhere the caveat is added that historical data may not be reflective of current conditions. Of course that is always true, but to be repeated so prominently, we think some examples should be given for what has changed that would make current conditions likely to be different.

The discussion about ammonia (p. 104) is generally good, but some more explanation could be included. How much NH_3 enters via the river versus the outfall? It is implied that open mouth conditions remove accumulated algae from the SCRE, but is there any evidence for this? If so, it should be cited here.

The bacteria exceedances (p. 105) are asserted to be due to "the influences of migratory and resident bird populations and local runoff," but this is by no means proven or even necessarily true. Bacteria source identification can be complicated and this simple explanation isn't supported by enough evidence or even logic (though there is the beginning of a logical argument in noting higher proportion of exceedances when the mouth was open).

The discussion of chemical constituents (p. 106) leaves the impression that there are no concentrations that could be considered harmful, but it focuses exclusively on drinking water supplies. This is too narrow a focus. The evaluation should be done with respect to ecological impacts.

The discussion of DO on p. 108 is appropriate for the standard, but the daytime samples really are not representative of the biologically relevant occurrences of low DO, which is more likely in pre-dawn samples.

The report appropriately highlights the concerns about toxicity due to high levels of a number of parameters (p. 111).

It is difficult to assess some of these arguments without the appendices (i.e. species lists). While we have no reason to doubt the rigor of the vegetation inventorying, we can nevertheless provide only circumspect comments on the aggregate plant data. Your most recent vegetation surveys are consistent with our current understanding of the SCRE flora, but neither of us has experience conducting historic vegetation surveys here (pre-2005) and so this wish for additional detail is mostly directed at your pre-2009 surveys.

It is great you were able to get access to the draft versions of the so-called t-sheet historical ecological conditions analyses. Please note that these data have now been publically released and are available (at http://www.caltsheets.org/socal/index.html) and so your reference should be updated. While the authors are continuing to work on peer-reviewed publications related to this dataset, it should no longer be considered "in preparation."

Table 6-1 and 6-2 (p. 123) should be unified, terms unified, etc. for easier comparison in one location. Placement of Fig 6-1 (p. 119) is strange in that it is not referenced or discussed for several pages.

Slightly unclear as to the historic condition of *Arundo*. Your bullet list suggests that "large, dense patches of non-native invasive giant reed" (p. 121) were present in all surveys, but you also report that the oldest study (Swanson *et al*. 1990) actually conducted wetland classification rather than species surveys. Was *Arundo* present and present in essentially the same locations as in the subsequent 3 surveys? This is later clarified on p. 126, but an earlier clarification will help the reader.

Your comments about aquatic vegetation (p. 122) are consistent with our experience. It is curious as most of the 29 other estuaries sampled during Bight '08 did have aquatic vegetation. The general hydrogeomorphology of the system suggests that we should have aquatic vegetation. Do you hazard a guess as to whether or not this was the pre-modification condition?

Section 6.2 is a good discussion of historic vegetation, reconstructed as best we can. We recognizing that a direct comparison between oblique historic photography and a modern vegetation mapping effort has its challenges. It would still be instructive, however, to include representative photos to illustrate your points. At a minimum you need to properly reference/document these sources. We trust some of these are the Fairchild Archival Collection. There are also various collections/photos at

the Ventra County Museum of Art and History (particularly those after the dam failure in the upperwatershed) which may or may not be of assistance here.

The suggestion (p. 125) that the overall extent of vegetation, has changed little, but that rather the distribution of that wetland vegetation needs further articulation. There must have been at least moderate salt marsh vegetation in/around the estuary historically (even if the SFEI-led effort couldn't well articulate this vegetation category). This needs to be better explained in a section discussing wetlands (not "freshwater" wetlands). We consider this change significant and one that should not merely be subsumed within the "changed distribution" rubric. Especially give the focal species you have selected for discussion.

Please cite a reference for the original historic use of *Arundo* in the SC River watershed (p. 126). We have often heard this explanation, but know of no rigorous documentation of farming here.

While *Arundo* removal may be beneficial in the SCRE, we have serious reservations as to the value of removal efforts here that are not integrated with (and subsequent to) upper watershed efforts.

Section 7: Aquatic Habitats and Focal Species

We feel that there is a more extensive literature of the effects of estuary condition and state upon steelhead that would add to this report. It would be beneficial to conduct a more wider-ranging survey of the literature (e.g. for p. 127)

Fig 7-1 (p. 133) could benefit form different color choices for each variable.

We suggest that Fig 7-3 (p. 137) be augmented to show potential "core" goby habitat of depths ~1m or less, as reflected in your seines. We too have tended to find them closer in towards banks (shallower areas) than out in the middle of main stem channels per se. This would in effect create bounding conditions for their potential habitat in the SCRE.

The tidewater goby is characterized as a weak disperser. While this is true in some senses, it is also true that the tidewater goby population is highly dynamic and extirpated goby populations are commonly re-populated.

Section 7.3 has some well articulated and key caveats. This seems to warrant somewhat more attention and discussion.

Section 8: Wildlife Habitats and Focal Species

Again, as previously stated, we do not believe that these focal bird species are the optimal indicator/focal species for the SCRE. Owing to human and human-related disturbance being a major if not the major determinant of nest site selection, hatching success, etc. why were they chose as in indicator or discharge into the estuary?

Why no historic data on SCRE plovers (ala Fig 8-5 for the terns).

Section 9: Functioning of SCRE

Table 9-1 confirms the lack of utility of the avian focal species for selection in this study.

Thank you for considering Dr. Ambrose's and Dr. Anderson's comments. Please feel free to contact us with any questions,

Sincerely,

Jason Weiner, M.E.M.
Associate Director & Staff Attorney
Wishtoyo Foundation's Ventura Coastkeeper Program