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William Carmen, Project Manager
Bollinas Lagoon Ecosystem Feasibility Study
MCOSD, 3501 Civic Center Drive Suite 415
San Rafael, CA 94903

Dear Mr. Carmen:

On behalf of the Sierra Club's 7,000 Marin County members, our 205,000 California members, and our 750,000 national members, we offer the following comments on the four documents: Projecting the Future of Bollinas Lagoon ("PWA"), Recent and Late Sedimentations Rates ("RB"), Peer Review Comments ("TRG"), and Littoral Sediment Budget.

General Tone

The Sierra Club is concerned that the February 2006 Draft Report on the Future of Bollinas Lagoon does not contain a clear, brief take-home message. That take-home message should be that the Report provides new and conclusive evidence that the sedimentation of Bollinas Lagoon is not a "problem" but rather a natural process that is virtually certain not to lead to the closure of the mouth of the Lagoon.

As a result of the Report's lack of clarity and brevity, the public is left to create its own mistaken take-home message. Public comments at the March 3 meeting of the Bollinas Lagoon Technical Advisory Committee can be paraphrased as:

"The Draft Report and the Corps Report came to the same basic conclusion that the Lagoon is threatened by sedimentation. However, Corps Report claimed the problem was caused by sediment from the watershed and thus could be solved by dredging, while the Draft Report shows the problem is caused by sediment from the ocean and thus should be solved by armoring the bluffs and/or enlarging the groin."

The above mistaken message is based on the incorrect assumption that a "problem" exists and thus requires a "solution" (replacing an old and discredited engineering project with a new and better engineering project). Problem-less solutions are futile at best and dangerous at worst.

Bluff armoring has been tried many times on our coastlines with consistently disastrous results. As the Coastal Commission's "Sample Policies for Planners" notes: *"Numerous studies...demonstrate that shoreline protective structures can have deleterious effects on beaches at their base and on more distant beaches due to interruption of sand supplies. ... Shoreline protective devices can and do substantially alter natural landforms by... accelerating erosion of the beach seaward of the device and of the bluffs on either side of the device."* Likewise, our understanding of the work of Professor Gary Griggs (UC Santa Cruz) leads us to believe that a large groin would not stop Lagoon sedimentation, because it would not affect the supply of ocean sand already stored in the vast reservoir of Bolinas Lagoon's tidal delta system. A large groin would, however, strip sand off adjacent beaches because the beach sands and the Lagoon sediments are part of the same system: any engineering "solution" that reduces one will reduce the other.

The PWA Report acknowledges that many local residents have made a logical but ultimately erroneous projection from their experience of localized sedimentation to arrive at an invalid "threat to the Lagoon." This erroneous projection has been supported by publicity intended to raise money and awareness to counter this "threat." The cumulative result is a public that is so intent on solving the sedimentation "problem", that any scrap of uncertainty in the Report (no matter how infinitesimal), any moment of hesitation in the Report (no matter how brief), and any mention of any possibility in the Report of Lagoon closure (no matter how remote)...is grabbed onto by the public as absolute proof that there is a imminent problem that needs an immediate solution.

Meanwhile, the Report's scientists are understandably reluctant to state in the Draft Report that their conclusions are absolutely totally 100% guaranteed certain (although the Report comes exceptionally close). Nonetheless the collision between the public's decades of logical but erroneous projection and the scientists' counter-intuitive new evidence underscores the importance of a clear take-home message that the sedimentation of Bolinas Lagoon is not a "problem" but rather a natural process that is virtually certain not to lead to the closure of the mouth of the Lagoon.

Sea Level Rise

The Sierra Club takes issue with the single sea-level rise scenario upon which all analysis is based. Sea level rise and earthquakes are the two most important factors driving the future of the Lagoon. The IPCC model used as a basis in the PWA Report bases its prediction of sea level rise on thermal expansion only and fails to account for the melting of the polar icecaps that

will punctuate this "dynamic equilibrium (Science 3/24/06). The PWA model assumes ~0.4 feet of sea level rise by 2050 based on thermal expansion only, but we estimate that including the impact from polar melting would at least double that figure. Closure scenarios models were done for different Tidal Prisms and different Inlet Widths, but all based on the same thermal-expansion-only model for sea level rise. The graph of Potential Sea Level Rise (Figure 5-2) demonstrates PWA's choice of a mean sea level between two almost equal thermal projections. In the now discredited USACE report, there was great criticism that the choice of alternatives was between two almost identical dredging projects. We believe that the same criticisms apply here with a choice between two almost identical sea level rise projections. We urge that duplicate closure scenarios and habitat projections be run using a sea level rise projection that includes the melting of the polar ice caps.

Closure Prediction

Given the public's long history of concern about the threat of closure of the mouth of the Lagoon and given the Reports' acknowledgment that extended periods of closure could bring about rapid changes in the ecology of the Lagoon, the Report's analysis of closure potential seems remarkably brief, several statements appear inconsistent, and the presentation is confusing.

The PWA Report assigns a numeric closure possibility (about once every decade) to a tidal prism of 2 MCY (pg 8 #32), but provides no data to back that statement. Table 5-2 (Results of Inlet Stability Analysis) shows three scenarios, none of which includes a 2 MCY scenario.

The PWA Report does not assign a numeric closure possibility for a tidal prism of 2.5 MCY, even though this is the projected 50-year condition. Instead, the report simply notes (pg 8 #26) that closure is possible "*under extreme conditions of strong El Nino Storms and weak neap tides.*" That same general statement could not only be made for the current tidal prism of 3.5 MCY, but also for virtually any tidal prism. This is not helpful. It is irrational to ascribe a numeric probability to a tidal prism of 2.0 MCY which will be reached well beyond the 50-year scope of this study, yet not assign a probability to the 50-year condition at 2.5 MCY. Is that possibly because the probability is so vanishingly remote that no reasonable/conceivable set of circumstances could come close? If so, then the "extreme combinations" statement leaves far too much to active imaginations in which every winter storm may be considered extreme. Is there any evidence to support the "extreme combinations" statement?

Pg 84 mentions, *"the first instance of predicted tidal prism closure over the simulation period occurred for a lagoon tidal prism of 2.0 MCY-slightly below the value predicted in 50 years-and an inlet width of 300 feet."* However, Table 5-2, which is supposed to represent "the results of this analysis" shows no data for 2 MCY and instead shows 2 closures for 2.5 MCY at the 300-foot width. Which is right? Assuming Scenario 2 is really meant to be for the 2.0 MCY tidal prism and the 300-foot inlet width, then does the "about once a decade" calculation come about by dividing 2 by 17? (because the data was *"simulated over a 17-year time series"*?) These points of key interest are inadequately discussed.

PWA page 84 states, *"smaller inlet widths produce larger values of tidal power."* Table 5-2 shows its 2 instances of closure at 2.5 MCY tidal prism and inlet width of 300 feet, yet also notes *"no closure was simulated (projected?) when the inlet width was reduced to its expected value of 200 feet."* If the expected inlet width is 200 feet and has no closures, then it is not clear how the 2 closures at 300 feet every 17 years can be said to equate to a closure possibility of ~once a decade for the 2.5 (2.0?) MCY scenario.

PWA page 84 notes, *"large winter creek flows into the Lagoon...would eventually lead to natural re-opening of the inlet."* We believe this circumstance would also significantly decrease the potential for closure, however the data analyzed to project the closure potential were wave and tide data only. Surely occasions where combinations over the 17-year study period of wave and tides that were likely to close the Lagoon but that also coincided with large winter creek flows should have a much lower closure probability weight assigned. Similarly PWA page 84 notes, *"erosion of the beach barrier by (winter?) ocean waves would eventually lead to natural re-opening of the inlet."* Likewise then, if the instances of wave power and tides suitable for closure occurred in the winter, then they should again be assigned a much lower closure probability, since any closure would likely be only until the next tide when the normal erosive power of the waves/tides would return from its unique closure conditions.

Figure 5-11's 10 days of data does not adequately represent 17-years worth of data. It does not show the two instances of predicted closure so that the public can see to what extent the potential for closure occurred in the winter when large creek flows would be expected nor how many tidal cycles the predicted closure value exceeded 12. There is also no discussion of what it means when the closure index spikes for one tide, as Figure 5-11 shows on 1/23/95, but then for the subsequent tide, the closure index drops dramatically. Was this the case for the two instances of closure? Does the closure index dropping from 12 back to 1 mean that the closure lasts for only one tide? These are the very issues that the public has expressed concern about, yet figure 5-11 does a very poor job at representing the data. Figure 5-11 also notes the *"current tidal prism (100 mcf) was used."* But elsewhere the Report indicates the current tidal prism is 3.5 mcy, which \neq 100 mcf.

Pg 19 notes, "*Duxbury reef shelters this inlet from the prevailing northwesterly winds.*" For Figure 5-11, the wave power index uses "*waves from the Monterey Buoy (transformed to Bolinas Bay)*". Seemingly the wave power (one of the two determinants from which the closure potential was projected) is less at the inlet than in the more open ocean conditions at the Monterey Buoy, thus closure potential may be overstated depending on how the wave power is "transformed." But there is no discussion.

Lastly, Figure 5-11 attempts to do too much and the choice of the time period 1/20/95-1/30/95 is unfortunate not only because it does not include one of the two closure events, but also because the wave power graph exceeds the index threshold for closures, misleading the public into believing that this solid line rather than the circles represents the closure index.

Equilibrium Date

Report says (PWA pg 8) "*The ultimate tidal prism of this projected long-term equilibrium form where net sedimentation keeps pace with sea level rise could be close to 2 ± 0.3 MCY...*" However, the Report does not say when this equilibrium condition will be reached. There is a habitat map of the long-term equilibrium conditions, but the Tidal Prism Change Graph (Figure 3-14) is cut off at 1998. In our opinion, this Tidal Prism Graph should be extended out, not just to 2050, but also to and past the date that the ultimate equilibrium form is achieved. PWA certainly has this information: Response to TRG comment #3: "*In projecting an equilibrium form we have evaluated how the lagoon morphology would adjust over the next few centuries in response only to projected sea level rise. We find that this projected morphology and associated tidal prism – the asymptote of the evolutionary trajectory, does equilibrate as a full tidal system. In other words, the lagoon does not require another major earthquake within the next few centuries to persist as a tidal system.*" We also believe an extended (beyond 1998) tidal prism was presented as a power point screen in the November 7, 2005 public meeting.

If it takes 50 years to go from a tidal prism (based on sea level rise only from thermal expansion) of 3.5 in 1998 to 2.5 in 2050, and the rate of tidal prism change decreases as the tidal prism decreases, then this ultimate tidal prism must be beyond the date derived from a straight-line projection, which is 70 years from now. We would guess that a flattening-curve projection might reach this ultimate tidal prism closer to 100 years from now using only seal level rise based on thermal expansion. This date is important to reveal to the public.

These timeframes are far beyond the stated scope of the Report, but if the Report is going to mention this once-a-decade closure probability for the "ultimate equilibrium form" and map its habitats, then we believe it should also project a date when this ultimate form might be achieved. Given the 50-year scope for the Report and the public's high concern with the threat of closure, then we believe it is necessary to inform the public so they may know when the probability of once-in-a-decade closure is projected to begin. For example, if this once in a decade closure probability might begin, say, in 2051, just 1 year outside the scope of the study, then this would provide a completely different context to the Report than if the minimum 2.0 Tidal prism point would be reached in 2091, for example.

USACE Errors

It would be helpful if the problems found with the USACE Report were collected together in one statement so that the public could better understand what went wrong. For example, the USACE EIR/S did not adequately distinguish that an increase in sedimentations does not result on an equal decrease in tidal prism...ie sedimentation below low water and above high water do not change the tidal prism. Also the USACE bathymetry surveys were not all tied to the same datum (RB pg 19) and the USACE used the wrong tidal benchmark (RB pg 19).

Bluffs and Groins Past and Future

The PWA Report does not adequately distinguish between the littoral sources of the sediment directly after the earthquake (the destroyed bluffs) and the current littoral sources in which bluff erosion plays a minor role. This unclarity may have led the public to mistakenly believe that a groin would protect the current lagoon from littoral sedimentations. Furthermore, the Littoral Sediment Budget Report addresses the questions: *Have the Bolinas Groin and the armoring of Seadrift affected net sedimentation in the Lagoon? How have these structures changed the beaches adjacent to the inlet and the movement of sand?* However, these questions are for the past. There is no discussion of how these structures may or may not affect sedimentation over the 50-year scope of the report.

Possible Typos

Suggested typos ~~struckthrough~~ and corrections underlined: PWA page 6 #16: *Although the Pine Gulch Creek delta and Kent Island have sheltered areas along the ~~eastern~~ western side...* PWA pg 28: *"UC Berkeley cores were limited to unvegetated mudflats and subtidal shallows and do not account for the delivery of coarse watershed sediments..."*

Choice of Words

There is an uneasy tension between the PWA choice of words in 6.1 *"Managing in the Face of Uncertainty"* and the RB statement *"Is Bolinas Lagoon 'filling in' at a rate that will lead to its extinction and conversion to freshwater marsh within the next 50 years? The answer, almost certainly, is no."* Again, this concern goes back to our first concern that these documents lack a clear and brief take-home message. We fully agree with the comments of the TRG in this regard and believe that the TRG comments have not been adequately addressed or incorporated into these documents.

The 8-page PWA "Conclusions and Recommendations" is an inadequate executive summary and, for a take-home message, is 7.9 pages too long. The clear take-home message that should be in the very first sentence of any executive summary should be: The sedimentation of Bolinas Lagoon is not a "problem" but rather a natural process that is virtually certain not to lead to the closure of the mouth of the Lagoon. Each word of each component of these 4 documents should be analyzed to make sure that they conform to or at least are not subject to misinterpretation that could undercut this clear take-home message. To obtain this level of clarity in these four Reports would be the most useful tool to manage in the face of public uncertainty.

Thank You for the Opportunity for Review and Comment,

Gordon Bennett, Chair
Sierra Club Marin Group
SF Bay Chapter Executive Committee