From: Miller, Garyg
To: Becher, Kent
Subject: RE: USGS review of SJ FS
Date: Friday, September 27, 2013 12:13:00 PM

Probably be on Flexiplace – so we can do it over phone (if not furloughed!)

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From: Becher, Kent [mailto:kdbecher@usgs.gov]
Sent: Friday, September 27, 2013 12:03 PM
To: Miller, Garyg; Kent Becher
Subject: Re: USGS review of SJ FS

Thanks Gary. Are you in the office at EPA on Wednesday or Thursday next week? I think it would be a good idea to sit down and chat a bit about the model and FS comments.

Take care.

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On Fri, Sep 27, 2013 at 11:56 AM, Miller, Garyg <Miller.Garyg@epa.gov> wrote:
Kent,

Thanks for the comments on the FS, and your informative discussion below.

On your question about the cap stabilization in the past, that was done during the construction of the cap because the equipment hauling the cap material was sinking in the muck. Cement was mixed into the top 3 ft (approx) so that the trucks and other equipment could drive through the area to install the cap.

I do have one followup observation; at the I-10 bridge you commented that there was some deposition of sediment observed, while the model predicted some slight scour in that area. While the reason for the observed deposition at the bridge is not clear (restricted area, therefore higher velocity implying scour); it does appear that the model under-predicted deposition in that area.

Regarding the preferred alternative, one of my comments will be the FS should not recommend

any of the alternatives, but instead should present the pros and cons of each based on their merits. While alternatives 2 & 3 have their merits, alternatives 4, 5, and 6 also have their merits, which include additional mobility and volume reductions compared to #'s 2 & 3.

Looking at the figures, Alt 6 (full removal) had increased dioxin concentrations compared to the other alternatives in sediment and surface water in the area due to stirring up the mud while dredging. While this is to be expected, I wonder what value we can put on the results in the figures given the uncertainties with this or any model study. Do you have any opinion on whether the relative results are reasonable, too high? too low? I would appreciate any discussion you may wish to make on that issue.

Thanks again,

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From: Becher, Kent [mailto:kdbecher@usgs.gov]
Sent: Friday, September 27, 2013 11:11 AM
To: Miller, Garyg; Tzhone, Stephen
Cc: Kent Becher
Subject: USGS review of SJ FS

Hi Gary,

I have attached my comments (via memo) on the FS below. I wouldn't call any of my comments major, but I do have few comments for clarity. As usual, Integral and Anchor QEA have provided a well written and organized report. The modeling that they have done on this site is phenomenal even though I still have concerns on the data used for the model. They used all their available resources to obtain data. The lack of data collection during a real storm event is a concern.

Below I have listed some of the issues I have in regards to the data used for the model and some of the model output. The model has been approved previously, but I think we need to keep a couple of these issues in mind as the Superfund process continues on this site. The intent of the list below is for internal use only since a lot this information has already been discussed in previous comments and conference calls.

Please keep in mind that this model has been used predominantly on east coast river systems and the San Jacinto River with Lake Houston upstream is a totally different system. There is a lot of uncertainty with the data as we have discussed before, but I have listed several of the main ones below.

1: The total sediment load was calculated based on inflows to Lake Houston from USGS gauging stations above the lake. The total load coming out of the lake would be much lower than going into the lake. They did use some TMDL total suspended solid data from TCEQ sampling below the dam. Keep in mind this was a surface dip sample and not an isokenetic collected cross sectional sample. In addition, total suspended solid data is not the same as total suspended sediment data.

2. The discharge used from Lake Houston is questionable due to the differences in datum of the ratings and limited knowledge of how far the gates were open.

3. No cross-sectional velocity data was collected within the EPA site boundary. A single point bottom mounted instrument was deployed that only had a minimal cone of data collection along one of the shallower sides of the channel. In addition, no equal width sampling of any sediment was collected in the site boundary.

4. The sites where they had water elevation data were a long ways from the site and located within the bay area, thus any major flow would attenuate once it reached the bay. It is obvious from the data plots that the elevation data doesn't show what would be happening at the site. However, the addition of the I-80 water level datum (which was new) does help with

the WSE predictions.

5. The age-coring data collected to determine NSR was questionable at best and this method normally isn't very good when used in rivers due to erosion and depositional episodes (or mixing of sediments).

6. If you look at the model outputs for net sedimentation at the I-10 bridge you can see there is minimal scour (if any during 2, 10, and 100 year events). Actually there is deposition in some cases. This indicates to me that the model doesn't work so well in regards to sediment transport. USGS has spent many years working with DOT on bridge scour projects, so why don't we see scour at this bridge? The channel morphology at this area (constricted) would increase velocity through the bridge area during high flows thus scouring the bottom. Back water eddys will form on the backside of the bridge that would cause additional erosion.

7. USGS had concerns on where they set their automated sampler during the data collection phase due to proximity to potential boat traffic. Prop wash most l kely increased sediment concentrations in that area during data collection.

It is my opinion the model (even though I am not a modeler) over predicts deposition and under predicts erosion rates due to lack of quality data available in the area, so the MNR is questionable. In the future, we want to keep in mind that reduction in concentrations in sediment may not be due to cleaner sediment deposition. If scour occurs then contaminated sediments maybe transported down river.

The real test would to see what happens when a major event occurs. I would recommend a stipulation somewhere that states sampling will be conducted for any 10 year or above event.

I definitely support the selection of the alternatives 1-3. The reinforcement of the cap would be the main remedy to keep contaminants from transporting out of the pit area. I just hope it is strong enough to handle a major event. I believe there was about 5 year event since the current cap was in place and it was damaged from it. I didn't make any comments on that part since I don't know all the facts, but you might want to add a comment since the FS states the current cap was designed for I believe a 100-year event.

Again, I believe Integral/Anchor QEA did their best to model this site with the limited data to help support the hydraulic and sediment transport parts of the model. They did on awesome job on this report and model.

Gary, here is one question for you. On page 70, section 6.3, second sentence; they state that a portion of the soils in the Western Cell were previously solidified during the TCRA then they reference some figures (some of potential alternatives). Is that true? If not, I can add a comment about it.

Please let me know if you have any questions or concerns about my comments.

Thanks.

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