Comments by the Groundwater Advisory Panel:

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On

"An Analysis of the Design and Performance of the Clay Cap Used to Control Groundwater Recharge Into the Fractured Bedrock Beneath the Former Sodium Burn Pit (FSDS) Boeing- Rocketdyne Santa Susana Field Laboratory" By William C. Bianchi.

1) The table or chart of hydraulic conductivity as a function of soil type is applicable for soils in their natural state. Artificial compaction is not accounted for in that chart and is not directly applicable to materials that have been compacted into an engineered fill.
2) Deep penetration of plant roots will help to ensure maximum and deep extraction of soil moisture. This will act to minimize deep percolation beneath the cover. Given that the potential evapotranspiration far exceeds the average precipitation, an increase in infiltration capacity does not compromise the function of the cover.
3) The lysimeter pans, indeed, provide a measurement of the unit area volume of deep percolation beneath the cover.
4) To our knowledge, the performance of the cover was never expected to be zero deep percolation. That would be unrealistic. Deep percolation has been very small, indeed, given the unusually high precipitation that has occurred.
5) The implication that measurement of precipitation and runoff would permit the estimate of deep percolation via the water balance approach is seriously flawed. ET would still have to be estimated. Even if precipitation and runoff were to be measured with great accuracy, the errors in the estimate of ET would exceed the actual deep percolation. Therefore, deep percolation cannot be reliably estimated as the residual number that closes the water balance.

In the case of the Area IV burn pit, wasn't the contaminated soil completely removed? Does this not take away the concern for continued leaching of the contaminants from the soil to the bedrock below? In terms of other contaminated soils onsite, water flow through these soils does not directly translate to deep percolation and transport of contaminants off-site. Although recharge does provide the impetus for groundwater flow, groundwater flow rates and travel times are distinctly different from solute migration rates to receptors. We fully acknowledge there is an active groundwater flow system, however the groundwater flux is small, and although groundwater velocities may be rapid, the rate of dissolved contaminant migration in the groundwater system in fractured rock is very slow due to diffusion-driven contaminant mass transfer between the mobile water in the fractures and much less mobile water in the rock matrix.

On

"Land-Use Conversion and Its Potential Impact on Stream Aquifer Hydraulics and Perchlorate Distribution in Simi Valley" By M. Ali Tabidian.

We found this report to consist of many seemingly unrelated facts, coupled with unsupported opinions and speculation with a report title that is highly misleading. This report lacked a consistent framework or clear conceptual model making it difficult to comment.

On

"Geologic Features and Their Potential Effects on Contaminant Migration, Santa Susana Field Laboratory" By Howard G. Wilshire.

We are unsure that anyone on the groundwater team has ever considered or referred to the faults and shear zones as impervious, or that these features caused blocks of groundwater to be independent of one another. The panel has always acknowledged that flow occurs through and along these features. They have certainly not been viewed as being responsible for the on-site containment of contaminants. That at least some of these features exhibit bulk hydraulic conductivities much less than the host rock continues to be a working hypothesis and remains under active investigation on more than one front.

Wilshire emphasizes the important and critical need to understand the geologic features in exceptional detail (e.g. detailed descriptive mapping of the faults through excavation, detailed measurement of mineralogy (although we have some of this that has not been reported on to date) or grain size measurements, nature and detailed distribution of gouge and interfingering within shale beds, etc) not resultant hydraulic properties per se, that can be measured directly. These geologic features are important but not as direct evidence of hydraulic characteristics but rather as indicators of why the hydraulic characteristics are what they are and their distribution in space. He overemphasizes the geologic characteristic variability and undervalues the various scales to which the hydraulic characteristics have been measured and documented at the site. The lack of appreciation of the abundant hydrologic, quantitative information collected at multiple scales and preference for the detailed descriptive information as being more relevant and "critical" is difficult to appreciate. Clearly, however, there is a bias against using the direct hydrologic evidence, relegating it to only local importance, and over-emphasis on the role of indirect geologic aspects of hydraulic conductivity.

The reference to terms in the geologic logs neglects the fact that these logs are prepared by numerous people with varying qualifications and experience without reference to standardized nomenclature. Thus, descriptive information is subjective and qualitative and not the most important basis for the hydrologic site conceptual model. Wilshire puts great emphasis on these descriptions as fact (hard evidence) rather than opinion and discrepancies in word choices taken as evidence for strong variability in field conditions rather than variability in terminology or perspectives of the on-site geologist performing the task. For example, on page 6, he presents geologic descriptions from different boreholes prepared by different geologists that the of non-cemented “hard” sandstones in one set of logs refutes the importance of the “hard sandstones” recorded in the C1-C7 source zone corehole report. In one case the hard qualifies the sand grains and the other refers to the degree and hardness of the cementation process –clearly the geologists are describing two different things entirely. The role of the hard sandstones in the corehole report is based on detailed mass distribution data showing accumulation of contaminants above these layers as well as head drops that occur during drilling with multilevel installations that provide direct hydraulic evidence of reduced vertical hydraulic conductivity that must have sufficient lateral continuity to maintain the hydraulic differential. There is abundant information concerning the nature of the geology based on the many geophysical logs. Wilshire has chosen to ignore this information.

Wilshire makes a point that evidence of oxide staining is evidence of meteoric water (i.e. waters of recent atmospheric origin) implying that recent means in our lifetime or the past few decades, where in fact “recent” should refer to geologic time scales of at least 10,000’s years where oxygen is migrating as a solute in the recharge water into the subsurface. No mention is made that the color of the rock matrix and occurrence of oxidation staining on fractures is strongly a function of depth below the water table. Our detailed core logs show lack of iron and manganese oxide occurrence greater than 200 feet below the present day water table. This transport has been occurring over geologic time and shows strongly retarded (extremely slow) transport rates.

The Boeing site conceptual model allows for fractures in shale beds and permeable zones within faults, but these fractures are sufficiently restrictive in the way they are interconnected such that the groundwater disharge is small and the water table is shallow even with large regional gradients. There are local higher permeable zones but the net effect is that these features are not continuous higher K pathways to discharge locations, otherwise the water table would drain. Local variability in permeability does not automatically translate to interconnected, higher K pathways— the site data provides strong evidence to the contrary. The fact that the water table represents a clear mound that rises high above the surrounding lowlands is conclusive evidence that the bulk K of the mountain is relatively low

In his closing, Wilshire makes a strong statement that important geologic features and characteristics are likely “never to be known in enough detail to predict future contaminant migration with any certainty” – this shows his reliance and preference for descriptive and certain quantitative **geologic** information and evidence that he refers to as “essential and critical” to infer (rather than measure directly) the **hydraulic (i.e. hydrogeologic)** conditions at the site. Given the huge uncertainty of contaminant migration prediction, he goes on to conclude that the best option is then to embark on a comprehensive remediation program to appropriately remove and sufficiently treat the known contamination and establish a long-term monitoring system to identify contaminants that has escaped detection thus far. These statements show lack of appreciation for the state of the hydrogeologic science, both in terms of site characterization and use of hydrologic tools (rather than geologic inference) and lack of proven remediation technologies for fractured sedimentary rock where nearly all of the contaminant mass resides in the low permeability rock matrix. It also demonstrates a lack of appreciation for the huge uncertainty in remediation design and performance and long timeframes required for achieving measurable success. These two statements present two contradictory views of what is achievable and not achievable at the site and in fractured sedimentary rock sites in general.

Summary Comment

None of these authors discuss or even acknowledge our overall conceptual model for the site hydrogeology and for contaminant migration and fate in the bedrock at the SSFL site. It is this model that forms the core of our understanding of the site. We would welcome constructive comments, dialog, and discussion of our conceptual model and alternative hypotheses in that regard.