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4 April 2017

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Reference: Your e-mail of 26 August 2016 with subject "Question on your tsunami risk presentation regarding Diablo Canyon Power Plant".

Dear Dr. Maurath:

I am writing to document our communications concerning my response to your above-referenced question in relation to my presentation on 21 June 2016 in Avila Beach, CA at the public meeting of the Diablo Canyon Independent Safety Committee (DCISC). For convenience, your question and associated background are included as attachment to this letter. Formally, DCISC has asked me to respond to your question; however, my response and related communications are my own, and are not intended as a response of the DCISC.

On 6 February 2017, I sent you an e-mail intended as a preliminary and informal reply to your question and a basis for starting a dialog in case there were additional, follow-on facets to your question that I could also answer. Some of the principal points conveyed in my e-mail included the following:

- My 21 June 2016 presentation to DCISC was provided, in part, to explain the motivation and basis for my earlier tsunami analyses, my 2003 draft report and my associated recommendations at the time. The 125 sq km source mentioned in your question was included in my presentation not because I viewed it or evaluated it in any way as the primary source of concern, but rather, because it was an example of a still-to-be-clarified observation raised in a relevant earlier report by USGS (and hence, was just one of several factors justifying, in my mind, further attention to the landslide tsunami threat offshore Central California).

- Largely through the efforts of DCISC, the Alliance for Nuclear Responsibility (A4NR) and media attention, my 2003 draft report became publicly available in late 2014. The report is now available on the Internet at https://www.nrc.gov/docs/ML1429/ML14293A559.pdf. It describes various scenarios for which I had performed numerical landslide tsunami simulations – a number of which are for slides along the continental slope and could be potentially of greater significance than the 125 sq km feature discussed by the USGS. The scenarios (and related sources) in my 2003 draft report were not (nor were they intended to be) comprehensive or weighted by frequency of occurrence, so as to enable an assessment of the sources and scenarios that contribute the most to the tsunami hazard at the Diablo Canyon Power Plant (DCPP) site. Indeed, a principal aspect of my 2003 draft report was to provide recommendations for further work to be done for better understanding the tsunami threat, including the primary (hazard-dominant) sources.

- I mostly concur with your observation that my presentation and 2003 report did not seek to provide information on the physical properties (grain size, angularity, etc.) of the material incorporated in debris flows, and we generally don't well know what will be the character of future flows (slumps, debris flows, turbidity flows, rock falls). To my knowledge, the various candidate data are not available, thus resulting in significant uncertainty concerning potential slide characteristics. The USGS report cited in my presentation itself provides various interpretations of the indicated slide feature and its potential slide significance (which is naturally coupled with the earthquake hazard). Undertaking further data collection and/or expert-based uncertainty analysis of relevant geotechnical / geological parameters and their potential impacts have been consistent recommendations of mine, as reinforced in my 2003 report and my 21 June 2016 presentation.

- Considering a paper by Harbitz et al. (2006), you noted that regardless of the type of flow, the initial acceleration and maximum velocity of material are critical elements relating to the velocity and size of the tsunami generated. You also noted from that paper that one can expect a rapid damping effect due to the nature of radial spreading of energy from a "localized source". Additionally, based on a 1.2 degree slope for a slide on the continental shelf, you indicated that the submarine landslide will be a sub-critical landslide (Froude number <1). I do not contend with these observations, but note that in my view they reinforce the need for the recommended further data collection, expert evaluation and uncertainty analysis. In the analyses for my 2003 report, rather than focusing on theoretical considerations, I primarily gave attention to empirical data/studies in my research concerning representative slide velocities and debris flow characteristics. Calculations of damping and flow criticality for failures along offshore Central California would require additional data and investigation, and would certainly be useful (I have recommended slope stability investigations and related analyses) – although, I believe, there are many additional elements of the overall tsunami hazard issue for DCPP to be yet effectively vetted by a suitable body of experts (e.g., according to, say, a SSHAC Level-3 type of approach), as I have previously recommended.

  I also agree with your observation that the orientation of many of the possible submarine landslides will lead to slide movement predominantly away from the coastline. For the particular submarine landslide scenario you noted (125 sq km feature moving along a 1.2-degree scope of the continental shelf) and many other –but clearly not all – possible landslide scenarios of interest offshore Central California, the momentum of the slide (which is aligned with the direction of mass flow of the landslide) and the resulting tsunami will be oriented away from DCPP. Principal direction of tsunami momentum has been observed as an important factor, but most especially for tsunami effects at distance. In the local vicinity of a landslide source, the slide interacts in a complex manner with the surrounding fluid; although the net momentum of a tsunami may be directed away from the coastline, the tsunami propagates outward in all directions from the source and has various important local effects (drawdown, run-up, debris-load transport, etc.) and energy dissipation that are generally capable of producing damage or adverse effects along the nearby coast.

- Concerning the Harbitz et al. (2006) paper, it also notes that one of the most important determinants of the significance of a landslide induced tsunami is the size of the landslide. Consistent with this (empirically and theoretically justified) observation, in my 2003 report, I considered features offshore Central California that could support a variety of slide sizes. The range of sizes I considered is well within the range of submarine landslides that have been known to occur.

  In response to your principal question about the significance of the threat that may be posed from past mass movement of a 125 sq km source / feature on a 1.2 degree slope, I do believe the tsunamigenic implications of the feature are deserving of further investigation. My concern would certainly not be limited to, or focused on, just that scenario / feature. The threat posed by potentially larger events on the continental slope, as well as potential slides on the shelf that would be directed toward DCPP, should also be considered, as should the likelihood of event occurrences, in an overall tsunami hazard investigation. Some level of additional field exploration to obtain sufficient geotechnical data may be justified to demonstrate, disprove – or at least better judge – the existence of the hazard. Additionally, I believe that improved resolution of the issue can be achieved through the application of numerical modeling based on realistic estimates of parameters and their uncertainties; the numerical models should be tested on a consistent basis for hazard and risk applications, including error estimation and demonstrated as suitably representative of the center, body and range (CBR) of views of the informed technical community.

On 8 March 2016, we had a phone discussion concerning your initial question and the preceding points, as well as related questions. The main purpose of that discussion was to insure that my preliminary response sufficiently represented, and responded to, your scope of questions related to my presentation. My understanding from our discussion was that you found the explanations and discussion to be satisfactory and sufficient. I also made note of the following points from our conversation:

- There is an apparent need for tsunami hazard investigations and associated numerical modeling also for the non-nuclear coastal power plants in Central and Southern California.

- There is interest in how DCISC and PG&E may be pursuing the idea of further tsunami hazard study, as such work/study could be valuable also in relation to investigation for these other (non-nuclear) coastal power plants.

Considering these points from our discussion, I made the suggestion to the DCISC to update you and the California Energy Commission (coincident with submittal of this letter) regarding the status of its related activities, decisions, plans and associated recommendations to PG&E, State of California agencies, and/or potential others, concerning any further investigation of the tsunami hazard and risk at DCPP.

Should you have any additional questions concerning my presentation, or on my summary of response provided in this letter, please do not hesitate to contact me.

Sincerely yours,

R.T. Sewell Associates
Robert T. Sewell, PhD, PMP

cc: Dr. Peter Lam, DCISC Committee Chairperson

Attachment

To: Garry Maurath, PhD, PG, CHG
Ref: E-mail of 26 August 2016: “Question on your tsunami risk presentation regarding Diablo Canyon Power Plant”
Date: 4 April 2017

Attachment

The primary source that Dr. Sewell is considering for generation of a Tsunami is a large (125 sq km – I did not see an estimate of the actual volume of material, but I would think it on the order of <2-3 cu km) submarine landslide that occurs on a slope of 1.2 degrees. While slurry flows can occur at such a low angle of repose (for very fine grained material), no information on the physical properties (grain size, angularity, etc.) of the material incorporated in the debris flow was presented by Dr. Sewell. Thus, we don’t know if the mass movement was a slump, debris flow, or turbidity flow.

A concern of mine is that regardless of the type of flow, the initial acceleration and maximum velocity of material are critical elements relating to the velocity and size of the tsunami generated. There will also be a rapid damping effect due to the nature of radial spreading of energy from a “localized source” (Harbitz, 2006). Additionally, based on the 1.2 degree slope reported by Dr. Sewell, it is assumed that the submarine landslide will be a sub-critical landslide (Frounde number <1). Models of sub-critical landslides suggest that the propagation of the tsunami will be away from the direction of landslide flow, which in the case presented by Dr. Sewell, would be away from the Diablo Canyon Power Plant. Unfortunately there is no data presented (available?) providing the necessary details regarding slide propagation and velocity.

<u>Hence my question:</u>

Is there sufficient data to demonstrate that past mass movement on a 1.2 degree slope represents a credible threat, possibly requiring additional field exploration to obtain sufficient geotechnical data to demonstrate/disprove the existence of the geologic hazard, or can this issue be resolved through the application of numerical modeling based on conservative estimates of geotechnical parameters?

Harbitz, Carl B. and others, 2006, Mechanisms for tsunami generation by submarine landslides: a short review. *Norwegian Journal of Geology*, v. 86, pp. 255-264