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Draft report issued by (?) West Valley Citizen Task Force

https://westvalleyctf.org/

Compiled by
SEIS Issue Identification and Assessment Working Group
of the Citizen Task Force

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ABSTRACT AND DISCLAIMER

In order to prepare for our review of an upcoming Draft Supplemental Environmental Impact Statement (Draft SEIS), the West Valley Citizen Task Force (CTF) seeks expert assistance on key issues. The Draft SEIS will support a decision by U.S. Department of Energy (DOE) and New York State Energy Research and Development Authority (NYSERDA) on whether to dig up and remove buried radioactive wastes at the West Valley nuclear waste site (~30 miles south of Buffalo, NY) or whether to apply some type of stabilization and leave those wastes in place.

As background, the site is actively eroding (geomorphically active) and is located in the watershed of Cattaraugus Creek, a tributary of Lake Erie. Wastes were buried in the onsite burial grounds between the early 1960s and early 1980s. The current EIS process began in 1988. The CTF was formed in 1997 by NYSERDA in conjunction with DOE.

The expertise now being sought by the CTF falls mainly in five areas:

- Remaining waste and associated radiation dose. Agency studies show significant discrepancy between volume, nature and radioactivity levels of waste in the two waste burial grounds known as the NDA and SDA. What is level of confidence and how to ensure appropriate risk is addressed in site models for the EIS?
- Defense waste. When it operated in the 1960s and ’70s, the site handled a combination of commercial and defense nuclear materials. Has the link to DOE defense projects been established in a way that would change the approach and potential funding streams for site remediation?
- Site erosion vulnerability and associated radiation dose. Erosion is being modeled 10,000 years into the future. Do the models sufficiently weigh the site erosion vulnerability? This is a key issue because the SDA burial ground is ~1/4 mile from the sheer banks of the largest (deepest) onsite tributary of Cattaraugus Creek and much closer to smaller tributaries, and because the region has a history of geologic instability (such as unforeseen slumping that significantly delayed the nearby U.S. 219 highway project).
- Overall site model sufficiency and associated radiation dose. Have the appropriate variables been selected and properly weighted in the modelers’ algorithms and assumptions? We are currently awaiting updates from agency contractor(s) on proposed methods but are not likely to get full details until the Draft SEIS public comment period.
- Community impacts including not only health but economic, social, and cultural. Have ongoing negative impacts been adequately scoped? Have future worst case scenarios sufficiently addressed the full impacts to neighboring and downstream stakeholders?

More detail is found in the 40+ tasks described in this report. These tasks outline important issues in a manner intended to provide guidance to potential experts. We hope to find a few experts who can talk through the various tasks with us, perhaps expanding or narrowing down what we're concerned about. Ideally we can get a written opinion from each expert.

Disclaimer: This report is a draft and will remain a working document for the foreseeable future! Pages may be added or removed as needed to explain the tasks and link them to comments previously made by the CTF and its members. The format of this draft is likely to evolve, and the list of tasks may be expanded or shortened. Note that the comments previously made by the CTF (and by its members in their individual capacities) are a matter of public record and express questions and opinions that were current at the time those comments were made. The newer text in this report outlines the 40+ tasks and expresses questions and opinions that reflect our current understanding – *which may change as a result of information received from experts*. Thus, there may be things in this draft that aren't stated quite right and will need to be corrected.

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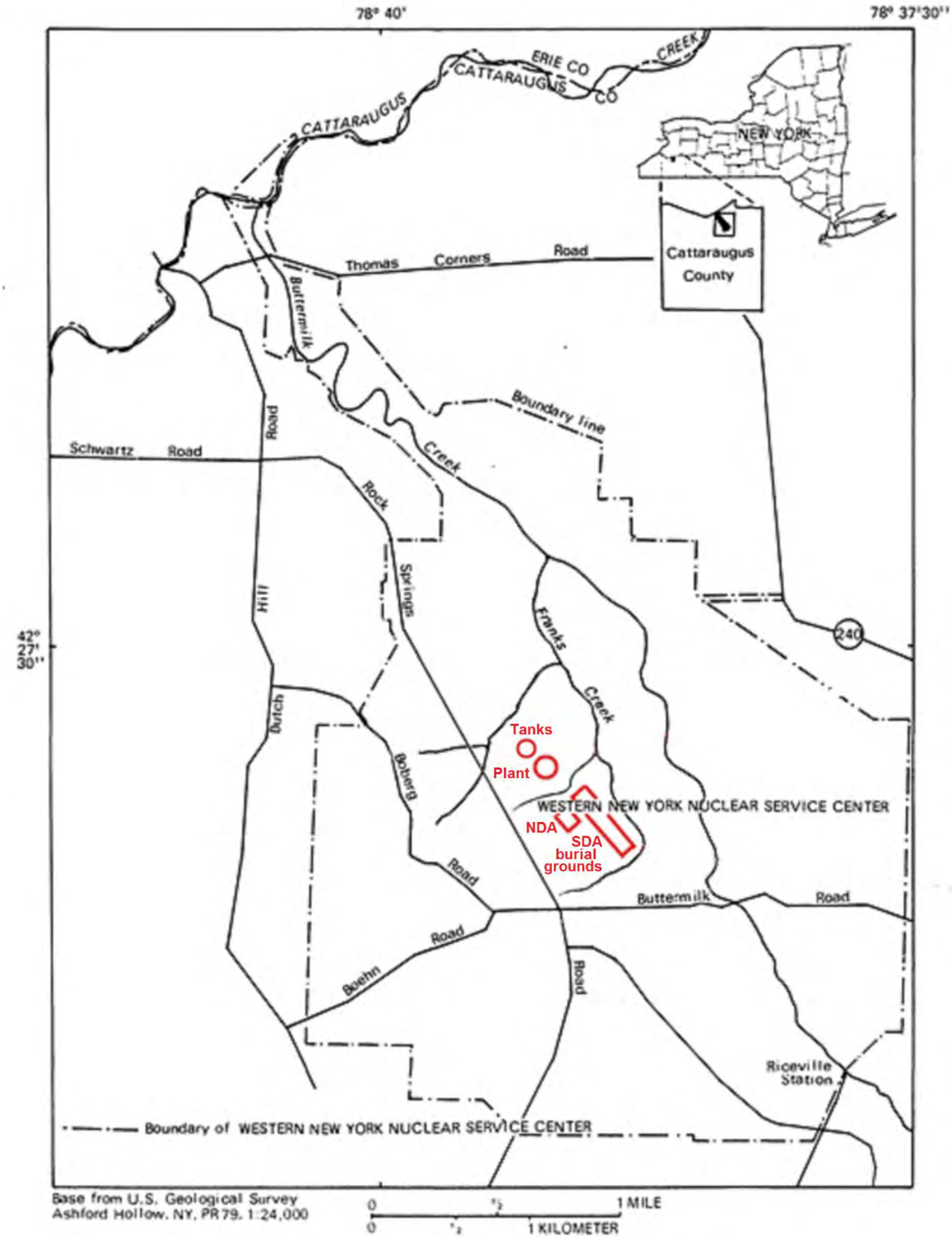


Site map adapted from USGS base map. See also map in Task 10.

INTRODUCTION

The West Valley nuclear waste site, 30 miles south of Buffalo, operated from 1966 to 1972 as a reprocessing plant for 'spent' nuclear fuel from commercial and defense reactors, and from 1963 to 1975 as a burial ground for radioactive waste from many types of offsite nuclear activities (commercial, academic research, military, etc.). No new wastes have come to the site since 1975. The site is owned by the State of New York, specifically by the New York State Energy Research and Development Authority (NYSERDA). A 1980 federal law known as the West Valley Demonstration Project (WVDP) Act assigned part of the site cleanup responsibility to the U.S. Department of Energy (DOE). To date, DOE and NYSERDA and their contractors have spent about $3 billion on site cleanup and closure activities, including vitrification of 600,000 gallons of high-level liquid waste from the onsite reprocessing plant. 'Phase 1' cleanup and closure activities were authorized by a 2010 Environmental Impact Statement (EIS), but the 2010 EIS deferred any decisions on the two onsite burial grounds, the wastes remaining in underground tanks, and certain other wastes.[1] These pending decisions, called 'Phase 2' and likely to be made in 2023, will be supported by a Supplemental EIS (SEIS) that is now being prepared. The main alternatives in the SEIS will be:

- Dig up & remove the buried wastes, remaining tank wastes, etc.
- Leave such wastes in place
- Some combination of these

The West Valley Citizen Task Force (CTF), appointed by DOE and NYSERDA, is concerned about the wide discrepancies in human health impacts that these two agencies predicted in two of their prior documents, one being the EIS they issued in 2010, the other being a Draft EIS (DEIS) that DOE and NYSERDA issued in 1996 but abandoned in favor a new EIS process that led to the 2010 EIS. These EIS documents have predicted the impacts that will occur *if the buried wastes, remaining underground tank wastes, etc., are left in place and not removed*.

The human health impacts predicted in the 1996 DEIS were much higher than those predicted in the 2010 EIS. Similarly, the onsite erosion predicted in the 1996 DEIS was much more severe than the erosion predicted in the 2010 EIS. The correlation between health impacts and erosion is not surprising, given the widely accepted threat that erosion poses to the integrity of the site. There is no dispute that geomorphic processes will not only downcut but also erode laterally (sidecut) into the waste burial trenches and holes. The question at hand is how quickly this will occur. The answer(s) reached by the agencies will guide their decisions on whether the wastes can be safely left in place, or, alternatively, must be dug up and removed.

Specifically, the human health impacts predicted in the 1996 DEIS were about four orders of magnitude higher than those predicted in the 2010 EIS. (In the 1996 DEIS, DOE and NYSERDA showed future doses ranging up to 300,000 mrem/yr for a future Buttermilk Creek resident in the year 2320. See 1996 DEIS, Appendix D at D-41. In the 2010 EIS, DOE and

[1] The 2010 EIS is also called the 2010 Final EIS or 2010 FEIS. It is not truly final, but, instead, is now in the process of being supplemented by a Supplemental EIS (SEIS) wherein the 'Phase 2' issues and tasks set forth here are important.

NYSERDA predicted no more than about 25 mrem/yr future dose to the maximally exposed individual.[2]) Similarly, these two agency documents make vastly different erosion predictions. See Fig. 1 for the onsite erosion predicted in the 1996 DEIS; it shows most of the buried wastes breached by erosion and carried downstream within 1000 years. In the 2010 EIS, onsite erosion during the next 1000 years is represented as minor, causing little or no breaching of the burial grounds. See 2010 EIS. See also Fig. 1 below for the low erosion predicted in the 2005 DEIS which is similar to the 2010 EIS. See also Fig. 3 below for the low erosion predicted in the current “EWG” erosion modeling predictions.

Since a high level of predicted impacts would require exhumation and removal of buried wastes, while a low level of predicted impacts would favor a decision to leave wastes in place, *the accuracy of these predictions is crucial*.

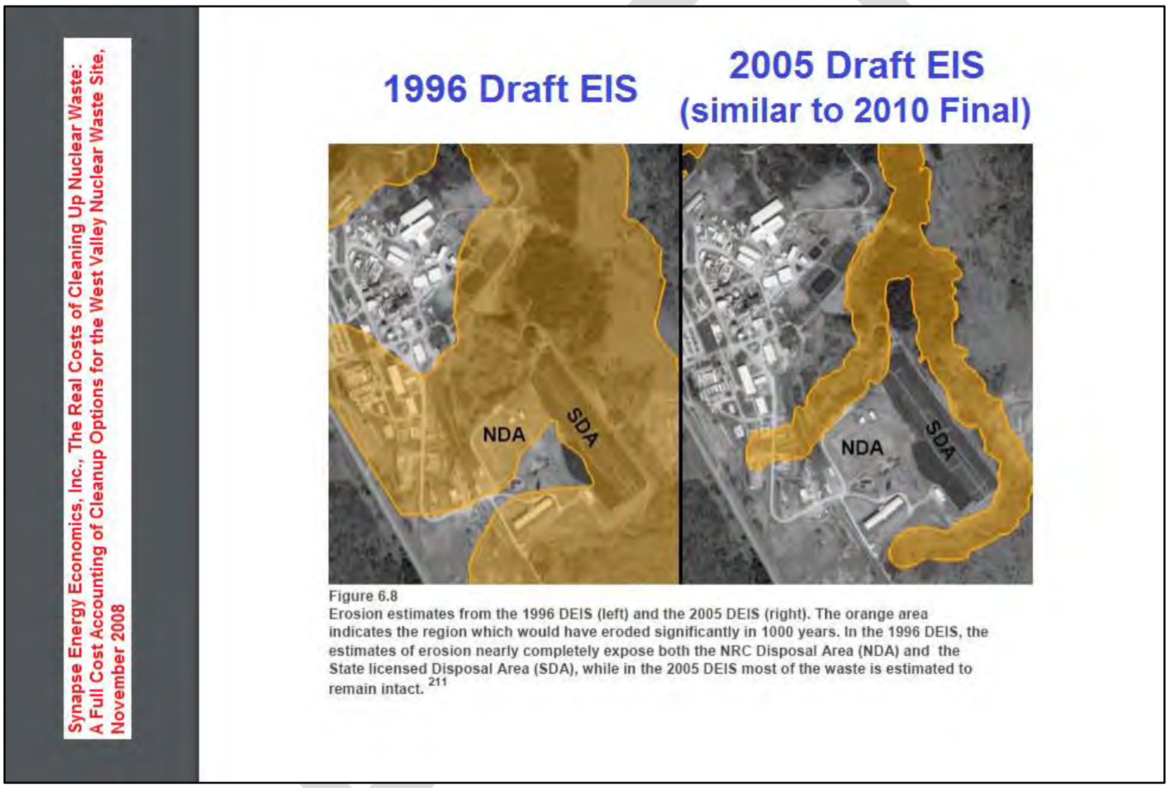


Fig. 1: Differences in predicted erosion, affecting both burial grounds (SDA and NDA).

To a limited extent, the CTF recognizes that newer predictive methods may be better than older methods. However, the discrepancies between the 1996 predictions and the 2010 predictions are too great to be quietly accepted without closer review, especially in view of the nature and magnitude of the impacts that are in the process of being predicted for the Phase 2 SEIS. Human health is at risk if these impacts are greatly underestimated in the SEIS. So too are the ecological health of the downstream Great Lakes watersheds; so too are the social wellbeing and prosperity

² While most predicted exposures are well below 25 mrem/yr, Table 4-23 of the 2010 EIS acknowledges that “There could be a peak annual dose of about 70 millirem per year to an onsite resident/recreational hiker (Table 4–40).” See also Table 4-34.

of the local West Valley community along with Buffalo and other adjacent communities of Western New York, the Seneca Nation of Indians, and nearby Canada.

Note that NYSERDA, while not still endorsing the 1996 predictions, wrote a Foreword to the 2010 [Final] EIS that expresses a strong caveat on how that EIS handles erosion and certain other issues. See that EIS, page ix, for NYSERDA's view on the erosion analyses and results:

> The Final EIS soil erosion analysis, which is intended to show how soil erosion by water will impact the site and site facilities over the next 10,000 years, is not scientifically defensible and should not be used for long-term decisionmaking.
>
> The Final EIS presents the results from a computer program (also called a landscape evolution model) that is used to calculate changes to the existing land surface from soil erosion. The model uses mathematical equations and input parameter values (e.g., rainfall amount and intensity, soil type, vegetation, the slope of the land surface, etc.) to predict how the topography of the land will be shaped by natural erosion processes over very long time frames (i.e., thousands of years). These computer-predicted changes in the land surface were then combined with the conceptual designs for facilities that are proposed to be closed-in-place to determine how critical facilities and areas of contamination would be impacted by the computer-predicted erosion for each of the EIS alternatives.
>
> NYSERDA recognizes DOE's efforts in trying to develop a defensible erosion analysis, yet it is apparent that the science of landscape evolution modeling is still in its infancy. Although these models are used to recreate many complex individual processes, they necessarily represent nature in a very abstract, simplistic way. While current state-of-the-art landscape evolution models are capable of recreating very basic, gross aspects of a stream network or watershed, they admittedly cannot: (1) predict the location of streams, gullies, landslides, etc.; (2) address the wandering or meandering nature observed in local streams; or (3) explicitly account for the knickpoint erosion that is actively causing downcutting (downward erosion) of stream channels and advancement of gullies. As such, we cannot rely on the results from these models to make decisions regarding the long-term future of the West Valley site.
>
> The limited graphical information provided to support the long-term modeling results is incomplete and makes it impossible for the general public to distinguish, for example, between areas predicted to erode 25 centimeters or 1700 centimeters. Further, NYSERDA staff believe these results are not only unrealistic, but overly optimistic given the 10,000-year time frame. With the exception of one modeling scenario, the simulation results show no gully erosion of the South Plateau over the next 10,000 years. Even more astonishing, these results show streams surrounding the South Plateau filling in with sediment over the same time period. These results are wholly inconsistent with what is being observed at these locations today. The streams themselves are actively downcutting dramatically in some locations, and the stream valley walls contain actively eroding gullies. The modeling results for the North Plateau predict tremendous downcutting (up to 30 meters or 100 feet) on Quarry Creek, which borders the WVDP to the north, yet relatively little gully erosion protruding into the plateau. Again, this predicted landscape

> is not representative of observed site or regional topography. Where local streams have incised the landscape, deep gullies extend many hundreds of feet into the landscape on either side of the stream. These discrepancies suggest the modeling results are neither meaningful nor reliable.

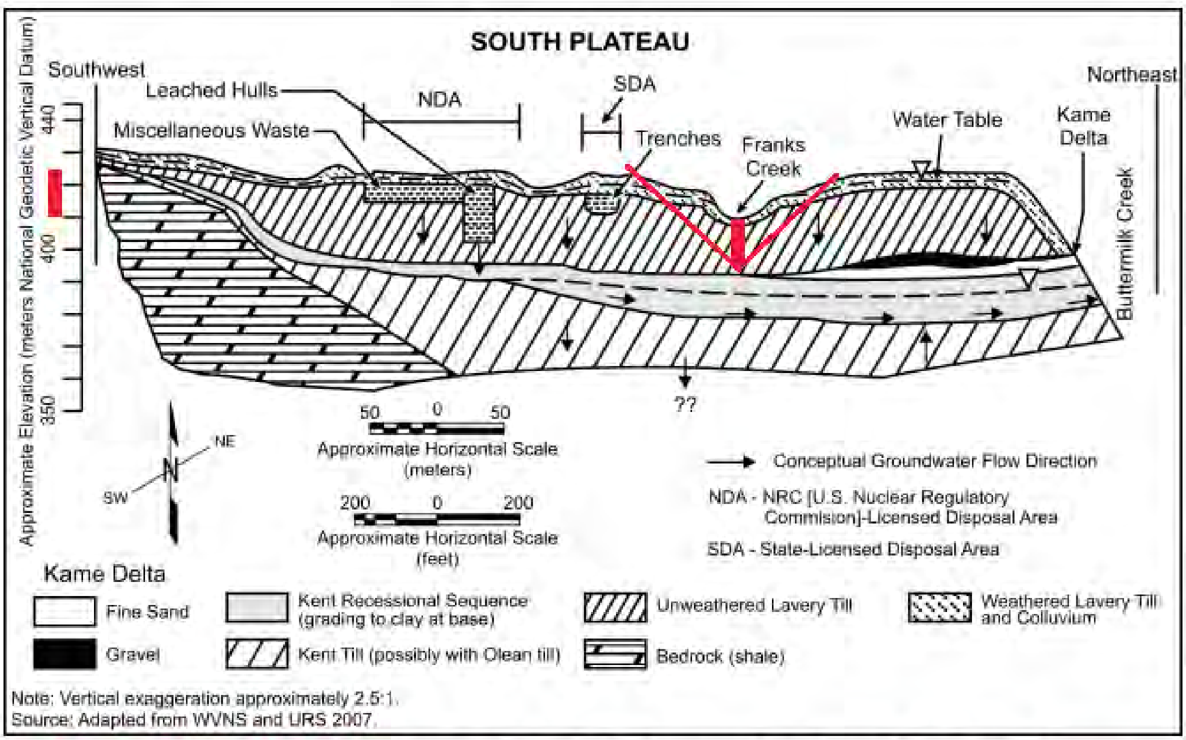
Moving on from the 2008 DEIS and 2010 EIS to the current "EWG" erosion modeling, its predictions also show minimal erosion of the waste burial grounds during the next 10,000 years. See Fig. 3 below – showing modeling results from the 842 model and the other 800-series models that were considered "best-performing" – where the white-colored areas, representing minimal erosion, include almost the entire SDA burial ground and NDA burial ground. Taking a more detailed look, however, it's evident that erosion is getting close to breaching the waste burial grounds and perhaps North Plateau facilities as well. As described by Tucker and Doty:

> Though Erdman Brook and the upper portion of Franks Creek are not projected to experience as much erosion as lower Franks Creek, the projected cumulative depth along these streams is nonetheless considerable: on the order of 10s of feet after 10,000 years. Because of their proximity to the major valleys, the plateau side-slopes are also projected to be vulnerable to erosion. The projections show the interior of the north plateau to be susceptible to erosion from the headward propagation of gullies, either from the northwest rim, the northeast rim, or both. Particular "hot spots" on the north plateau include areas near and upslope of the present-day NP-1 Gully on the northwest rim, and NP-2 and NP-3 Gullies on the northeast rim. High uncertainty is associated with these locations…because small changes in plateau topography can alter the relative drainage area that contributes runoff to each of these gullies, and can in turn promote or dampen the erosion rates among the different rim gullies.[3]

Fig. 2 provides a rough illustration of what Tucker and Doty mean by the plateau side-slopes being vulnerable to erosion in their EWG modeling. Their color-coded maps (see Fig. 3) show the SDA and NDA burial grounds on the South Plateau as almost entirely white, indicating minimal erosion, yet the projected downcutting of the adjacent Franks Creek channel is about 50 feet or more during the next 10,000 years. (While the stream channels themselves are shown in black in Fig. 3, so not able to be correlated to Tucker and Doty's color scale, the adjacent stream banks and slopes can be color-matched to their scale, showing slightly more than 50 feet of downcutting in the vicinity of 42.4495°, -78.6490° for model 842.) Assuming from this that the stream channel itself downcuts 50 feet, Fig. 2 provides a rough illustration of how this affects the adjacent banks and slopes, assuming a 21-degree stable slope angle. Material above the diagonal red lines would be eroded away. Such predicted erosion does not intrude into the SDA itself but is getting close. Granted, Fig. 2 is a rough sketch, not an exact cross-section, so it's a qualitative depiction rather than an exact quantitative measure of the evolving erosion risk to the buried wastes. Actual erosion may be less than indicated by Fig. 2 – *or it may be substantially worse*.

[3] G.E. Tucker et al., *Modeling Long-Term Erosion at the West Valley Demonstration Project and Western New York Nuclear Services Center*, Final Report, April 25, 2018, https://wvphaseonestudies.emcbc.doe.gov/documents/EWG_Study3_Modeling_Long_Term_Erosion_Final%20April%2025%202018.pdf, at 18.

Note that erosion capable of breaching waste facilities *need not be orders of magnitude worse* than the erosion indicated in Figs. 2 and 3. For example, erosion three or four times greater than Tucker and Doty's predictions can be expected to breach the SDA and probably other facilities as well. At the aforementioned location (42.4495°, -78.6490°), the existing meander in Franks Creek is likely to continue sidecutting toward the SDA, but such sidecutting is apparently not incorporated into the EWG erosion modeling (see Task 21 below). Other shortcomings in the EWG erosion modeling, such as those outlined in Tasks 3, 5, 12, 15, 16, 17, etc., also contribute to the underestimation of erosion – perhaps, in combination, by a factor of ten or more. Thus, with appropriate corrections, the EWG models can be expected to show significant breaching of burial grounds and other facilities left underground.



Adapted from 2010 EIS, **Figure E–6 Geologic Cross-section through the South Plateau**

Vertical red bar shows 50 foot (15 m) Franks Creek downcutting alongside SDA at about 42.4495°, -78.6490°, more or less as predicted by EWG erosion modeling by Tucker & Doty.

Diagonal red lines show 21° stable slope angle, adjusted here to match vertical exaggeration.

This figure is approximate and illustrative, not exact.

Fig. 2: Cross-section of South Plateau, adapted in red to show effects of Franks Creek downcutting

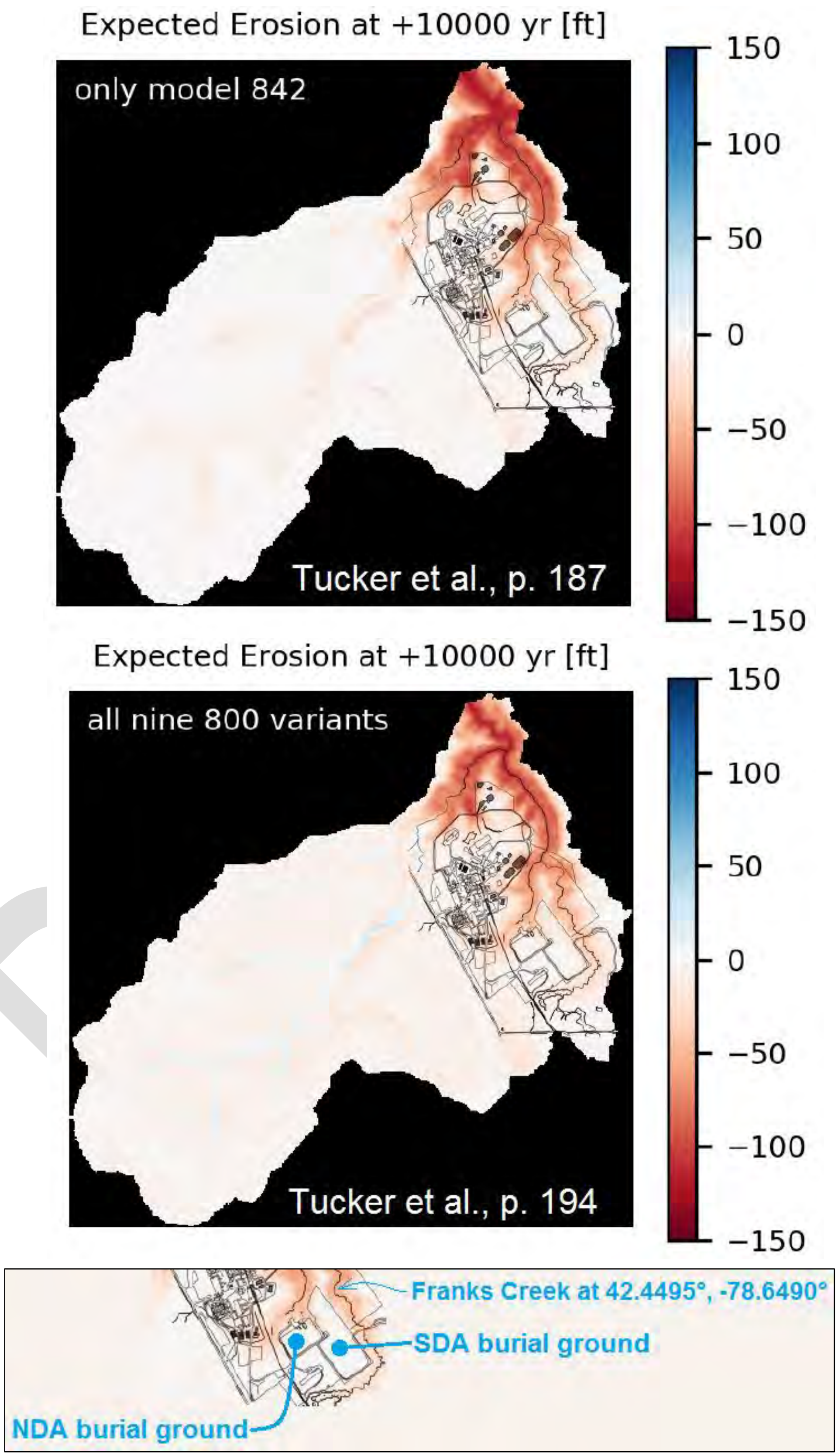


Fig. 3: EWG modeling results for model 842 of G.E. Tucker et al. and for all of their 800-series models

Many detailed concerns about the Tucker & Doty EWG modeling (the current landscape evolution model or LEM) are listed and discussed in the following pages. For example, the rainfall/runoff/erosion relationships used in the EWG modeling are clearly problematic, either under current climate conditions or under the worsening conditions caused by climate change.

Current conditions include the August 2009 storm, which anyone familiar with the site will recognize as an important benchmark that showed the erosional damage associated with a 5-inch rainfall. As noted in the 2010 EIS, page 3-35, "Erosion is occurring in the region and the greatest topographical changes occur after large storms...." Such a conclusion is very evident, yet we don't see this shown in the Tucker & Doty EWG modeling results – and the discrepancy will become worse when the effects of climate change, particularly the effects on frequency and intensity of storms, are properly incorporated into the modeling.

As another example, any erosion model that runs for 10,000 years needs to include 100 storms having at least a "100-year" intensity, of which 10 are more intense storms that would be called 1000-year storms, of which one is a more intense storm that delivers even more rain and would be called a 10,000-year storm. This set of 100 storms – judging from the August 2009 damage and without yet taking climate change into account – would inevitably cause greater erosion damage to the SDA and NDA burial grounds than is shown by the Tucker & Doty modeling results.

A probabilistic modeler may disagree with this logic by claiming that a 10,000-year modeling run needn't include exactly 1, 10, and 100 storms of the aforementioned intensities, as long as 1, 10, and 100 such storms occur *on average* in the 10,000-year modeling runs. While this is certainly true, it leads into other discrepancies that are discussed below, such as the lengths of the time steps and sub-time-steps used in the models. The main point of these two paragraphs is that 10,000-year modeling runs that include exactly 1, 10, and 100 storms of the aforementioned intensities would provide a clear, well-supported basis for comparison. The Tucker & Doty models are unfortunately not well suited to any comparison, as discussed in more detail below.

To achieve a better understanding of these various issues, the CTF invites input from outside experts who can supplement the CTF's own assessments. While the linked issues of erosion and human health are at the forefront, other important issues are also included here.

CHRONOLOGY OF THE EIS PROCESS

1988: EIS process began

1996 Draft EIS (DEIS)[4] issued for comments, then abandoned, partly because of predicted high levels of human radiological exposure. Future erosion predicted by HEC modeling.

2005 Draft EIS (DEIS) issued for comments; superseded by 2008 Draft EIS. Future erosion predicted by the then-current landscape evolution model (LEM) known as SIBERIA.

2008 Draft EIS (DEIS)[5] issued for comments; finalized by 2010 EIS. Future erosion predicted by the then-current LEM known as CHILD.

2010 EIS (FEIS)[6] issued for 'Phase 1' activities, intended to be followed by a supplemental 'Phase 2' EIS. Future erosion predicted by the then-current LEM known as CHILD.

2017: Conceptual Site Model (CSM) issued for the West Valley site; prepared by Neptune and Company[7]

2018: EWG erosion modeling report issued by Tucker & Doty,[8] documenting their current LEM (EWG erosion modeling) and its erosion predictions.

2022? Draft Supplemental EIS (DSEIS) for 'Phase 2' activities to be issued for comments. Future erosion to be predicted by the current LEM (EWG erosion modeling by Tucker & Doty)? Also by separate modeling of gully growth?

2023? Supplemental EIS (FSEIS) to be issued for 'Phase 2' activities. Future erosion to be predicted by the current LEM (EWG erosion modeling by Tucker & Doty)? Also by separate modeling of gully growth?

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[4] *Draft Environmental Impact Statement for Completion of the West Valley Demonstration Project and Closure or Long-Term Management of Facilities at the Western New York Nuclear Service Center*, U.S. Department of Energy and NYSERDA, DOE/EIS-0226-D (1996).

[5] https://www.wv.doe.gov/drafteis.htm

[6] https://www.wv.doe.gov/finaleis.htm

[7] *Conceptual Site Model for the West Valley Site*, prepared by Neptune and Company, NAC-0073_R3, 23 June 2017.

[8] Tucker et al., op. cit.

ABBREVIATIONS (referring to material quoted or paraphrased below that has been written either by the CTF or by CTF members in their individual capacities)

CFI: CTF Final Report, dated July 29, 1998, http://westvalleyctf.org/1998_Report/CTF_Final_Report.pdf.

CSC: CTF scoping comments dated May 21, 2018; sent to Martin Krentz, DOE Document Manager, http://www.westvalleyctf.org/2018_Materials/05/2018-05-21_CTF_SEIS_Scoping_Comments.pdf.

VCT: Vaughan memo titled "Issues the Core Team Needs to Address," dated January 15, 2008, http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf.

VEC: Vaughan EIS comments, most of which can be found in the response-to-comments portion of the 2010 EIS, available at https://www.wv.doe.gov/final/EIS-0226_F-Vol3-CRDPart1.pdf, on pdf pages 238-303. Some of the Vaughan EIS-comment appendices that were omitted from the 2010 EIS volumes can be found at http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf .]

VSC: Vaughan scoping comments dated May 23, 2018; sent to Martin Krentz, DOE Document Manager.

VSM: Vaughan comment letter dated May 28, 2019, on Conceptual Site Model; sent to Dr. Lee Gordon, NYSERDA.

VWL: Vaughan comment letter dated Sept. 23, 2019, on U.S. Dept. of Energy's permit application for water withdrawal, application ID 9-0422-00005/00112.

Numbers attached to these abbreviations are generally section or paragraph numbers, not page numbers.

GLOSSARY OF OTHER ABBREVIATIONS AND ACRONYMS

BR&P – Buffalo, Rochester & Pittsburgh Railway (now called Buffalo & Pittsburgh)

CFR – Code of Federal Regulations

CSM – Conceptual Site Model

CTF – West Valley Citizen Task Force

DEC – NYS Department of Environmental Conservation

DEIS – Draft Environmental Impact Statement

DOE – (U.S.) Department of Energy

DSEIS – Draft Supplemental Environmental Impact Statement

EID – Environmental Information Document

EIS – Environmental Impact Statement

EWG – Erosion Working Group (see https://wvphaseonestudies.emcbc.doe.gov/)

FEIS – Final Environmental Impact Statement

ft – feet

GCMs – Global Climate Models

HEC – Hydrologic Engineering Center (see https://www.hec.usace.army.mil/software/hec-ras/)

hr – hour

LEM – Landscape Evolution Model, a type of computerized erosion prediction model

MACA – method of downscaling climate models ('Multivariate Adaptive Constructed Analogs')

mrem/yr – millirems/year, a measure of radioactive exposure or dose

m/yr – meters/year

NDA – Nuclear Regulatory Commission-Licensed Disposal Area (NDA burial ground)

NEPA – National Environmental Policy Act

NRC – (U.S.) Nuclear Regulatory Commission

NWS – National Weather Service

NYCRR – NYS Codes, Rules and Regulations

NYSERDA – New York State Energy Research and Development Authority

NYSGA – NY State Geological Association

PMF – Probable Maximum Flood

PMP – Probable Maximum Precipitation

PPA – Probabilistic Performance Assessment

RQD – Rock Quality Designation (broken or fractured rock is given a low RQD value)

SDA – New York State-Licensed Disposal Area (SDA burial ground)

SEIS – Supplemental Environmental Impact Statement

SEQRA – State Environmental Quality Review Act

TRU – Transuranic Radioactive Waste

USGS – U.S. Geological Survey

WEPP – Watershed Erosion Prediction Project, a type of erosion prediction model

WNYNSC – Western New York Nuclear Service Center (the 3300-acre West Valley site)

WVDP – West Valley Demonstration Project (occupying about 200 acres of the site)

<u>TASK 1</u>: Assess whether or not the radiological dose & erosion discrepancies need resolution

Context: Given the magnitude of the discrepancies between the 1996 and 2010 predictions for both dose and erosion, it is difficult to see how either the 2010 dose/erosion predictions or the current EWG erosion predictions can be accepted uncritically – especially in view of the close proximity of erosion to the burial grounds in the EWG modeling.

Note that, in the following quotes from comments already made by the CTF or its members, comparison is made to the 2008 Draft EIS – which was the then-current EIS document. The information presented in the 2008 Draft EIS was finalized, with some modifications, in the 2010 EIS.

VEC.139. Human radiological exposures under loss-of-institutional-control conditions are *drastically different* in the 1996 Draft EIS and 2008 Draft EIS. In the absence of a Supplement Analysis prepared in accordance with 10 CFR 1021.314(c), and in the absence of any comparative presentation or discussion within the 2008 Draft EIS, most readers and reviewers of the 2008 Draft EIS have no basis for understanding the changes in procedures and assumptions that have drastically lowered the predicted doses. For example, for exposures from the high-level waste tank farm under the close-in-place alternative, the 1996 Draft EIS shows a future intruder dose of 89,000,000 mrem/yr, while the 2008 Draft EIS shows a future intruder/resident farmer dose of only 556 mrem/yr. See 1996 Draft EIS, Table D-11, page D-36; 2008 Draft EIS, Table 4-33, page 4-76. As other examples involving exposures to a Buttermilk Creek resident from burial-ground wastes released by erosion under the close-in-place alternative, the 1996 Draft EIS shows a future dose of 47,000 mrem/yr from the NDA and 280,000 mrem/yr from the SDA; the 2008 Draft EIS shows a future dose of only 342 mrem/yr from the NDA and only 87 mrem/yr from the SDA. See 1996 Draft EIS, Table D-14, page D-39; 2008 Draft EIS, Table 4-40, page 4-85.

VEC.140. These major differences in predicted dose illustrate why it would have been important for DOE to provide either a Supplement Analysis in accordance with 10 CFR 1021.314(c) or some level of comparative presentation and discussion in the 2008 Draft EIS. Without these, readers cannot readily determine whether the changes were justified, or whether the preparing agency just didn't like the old EIS and wanted to replace it with a new one. Readers can obtain some insight from NYSERDA's strong objections to certain aspects of the 2008 Draft EIS (see 2008 Draft EIS, pp. v-xxiii, including pp. viii-x on soil erosion issues, pp. xiv-xv on long-term performance assessment, and pp. xix-xx on waste tank farm issues).

VEC.141. With respect to both the predicted radiation doses and the underlying erosion prediction methods, the 2008 Draft EIS provides no meaningful presentation or discussion of the major differences between the 1996 Draft EIS and the 2008 Draft EIS. Although the issue is mentioned in one paragraph of the 2008 Draft EIS (bottom of page F-10), this paragraph misrepresents or misstates the main point at issue, thereby precluding any meaningful comparison of the 1996 and 2008 approaches. The main point at issue is the predicted rate of erosion and its effect on the West Valley site, as depicted in Figure L-2 (page L-12) of the 1996 Draft EIS and Figure F-20b (page F-55) of the 2008 Draft EIS. The erosion projection in Figure L-2 of the 1996 Draft EIS is explicitly based on analysis of the return intervals of storms of various magnitudes (see pages L-8 through L-12, including Tables L-1 and L-2), yet the 2008 Draft EIS makes the false or misleading claim that the surveyed-stream-profile method used in the 1996 Draft EIS "does not take into account the wider range of precipitation values that are likely to occur over the long term, and thus, it is not considered to be representative of long-term conditions." (Pages F-10 to F-12.) As noted, this statement in the 2008 Draft EIS is false or misleading, and it effectively blocks any meaningful comparison of the 1996 and 2008 approaches.

VEC.142. Part of the aforementioned problem is the segmented treatment of the 1996 Draft EIS in the 2008 Draft EIS. The latter document fails or refuses to take seriously the logic of the 1996 Draft EIS; instead, it recites the various parts of the 1996 Draft EIS in isolation from one another. The reference in the 1996 Draft EIS to a downcutting rate of 0.6 meters per 10 years (which is criticized in the 2008 Draft EIS for failing to take into account the wider range of precipitation values likely to occur over the long term) is followed directly in the 1996 Draft EIS by the allegedly missing range-of-precipitation analysis. The 1996 Draft EIS provides this information in context (pages L-3 through L-12) while the 2008 Draft EIS takes it out of context (see separate portions on pages F-10 to F-12 and pages F-26 to F-28), thereby preventing any meaningful comparison of the 1996 and 2008 approaches.

VEC.143. The 2008 Draft EIS recognizes the importance of erosion issues at the site and points out, in general terms, how these issues must be addressed – but the EIS then fails to follow its own advice on how to address erosion issues. For example, the following statements from the 2008 Draft EIS show DOE's recognition of the issue and the important questions that need to be addressed and resolved:

> The three small stream channels (Erdman Brook, Quarry Creek, and Franks Creek) that drain the Project Premises and the SDA are being eroded by the stream channel downcutting and valley rim-widening processes. The streams appear to be incising rapidly, as suggested by convex-upward longitudinal profiles, steep V-shaped valley-side profiles, and the paucity of floodplains over a major portion of their length. The streams within the plateau areas flow over glacial till material that is highly erodible. As channel downcutting progresses, two specific mechanisms contribute to stream rim-widening. Streambanks are undercut, causing localized slope failures (i.e., slumps and landslides). This process commonly occurs at the outside of the meander loops and produces a widening of the stream valley rim. Even in locations where there is no bank undercutting, downcutting of the stream will produce a steeper creek bank that is subject to slumping. This second mechanism also produces widening of the floodplain.
>
> Gully advance is the third type of erosion process that results from local runoff and reflects soil characteristics... (Page F-4.)
>
> Glacial recession from the Lake Erie basin appears to be the ultimate cause of stream incision within the Cattaraugus valley and its tributaries. For purposes of erosion evaluation, however, the key boundary condition is the elevation history in the reach of Cattaraugus Creek, for it provides the base level for the Buttermilk Creek catchment. To estimate this base-level history, it was necessary to answer the following questions:

> When did incision begin here? How fast did Cattaraugus Creek incise here? Has this rate varied through time, and if so, how? (Page F-32.)
>
> ...future climate may differ substantially from the present one. Climate has a direct or indirect control on all of the landscape-forming processes at the West Valley Site. Rainfall frequency and magnitude directly impact erosion and sediment transport by running water, and indirectly influence the nature of the vegetation.... Assessment of the potential impact of future climate change on erosion patterns would require the construction and analysis of scenarios with varying climate states. (Page F-59.)
>
> [An important factor in calibrating erosion models based on postglacial landscape development is that]...climate in this portion of North America is known to have varied to some extent over the post-glacial period. (Pages F-59 to F-60.)

Unfortunately, the erosion models used in the 2008 Draft EIS either fail to address the above questions or address them in a substandard manner. Whether the computer models themselves are adequate remains to be determined; however, as discussed below in more detail, many of the assumptions used for calibrating and running the models are flawed. This is a classic case of "garbage in, garbage out" in computer-generated results.

VEC.144. Some of the main difficulties in the 2008 Draft EIS's treatment of erosion are: A) The question of whether any model or method is sufficiently reliable to predict future erosion during the next several centuries or millennia, such that a well-informed and protective decision could be made about future site integrity; B) the lack of any substantial or defensible analysis of the formation and headward advance of gullies, despite clear recognition that evolving gullies may breach waste containment; and C) miscalibration of the erosion model used in the 2008 Draft EIS and underlying calibration problems such as questionable logic and naive assumptions.

VEC.145. The 1996 Draft EIS and 2008 Draft EIS employ substantially different modeling methods to predict future erosion. Despite this major difference, both documents *could* have used similar logic and similar data sets for model calibration – but this was not the case. Erosion modeling in the 1996 Draft EIS was primarily calibrated against recent longitudinal profile surveys of Franks Creek. This calibration had the advantage of direct measurement but the disadvantage of a short time period (10 years). Such a short time period might be a risky basis for extrapolating centuries or millennia into the future. Erosion modeling in the 2008 Draft EIS is based on a much longer time period (several thousand years of postglacial downcutting in the Buttermilk Creek watershed) but, unfortunately, it relies primarily on assumptions rather than data. As described below, many of these assumptions are naive or otherwise questionable. Where data is used in the calibration of the 2008 Draft EIS erosion model, its interpretation and application tend to be highly dependent on the questionable assumptions discussed below.

VEC.146. Both the 1996 and 2008 methods of model calibration are logical and potentially useful, at least in theory. Indeed, if carried out properly, the two calibration methods should agree with each other within some margin of error, and the error bounds of each method should be reasonably comprehensible, perhaps even predictable. Additional studies conducted between Phase I and Phase II of phased decisionmaking should include *a defensible demonstration that* *the two calibration methods yield compatible results*, and these results should then be presented in supplemental NEPA documentation (e.g., Draft EIS) for future Phase II decisionmaking.

VEC.147. If it cannot be shown that the two calibration methods yield compatible results, then DOE should conclude that no model or method is sufficiently reliable to predict future erosion during the next several centuries or millennia, and that no well-informed or protective decision could be made about future site integrity based on currently available modeling methods.

TASK 2: Assess whether the LEM's time steps, sub-time-steps, and relationships of these time increments to daily precipitation are realistic

Context: The EWG erosion models[9] (i.e., Tucker and Doty's most current landscape evolution models or LEMs) use a post-glacial or "paleo" period of 13,000 years to calibrate the model. In other words, model input parameter values are determined during the "paleo" calibration runs of the model. During these calibration runs, the model starts with an assumed landscape or topography that represents the West Valley site immediately after glacial withdrawal. As the model runs from 13,000 years ago to the present time, the input parameters that produce the best match to the *present* site topography are chosen. These parameter values are then used to model the site topography 10,000 years into the future, thus generating the model's predictions of future erosion.

In the EWG erosion models – as in any model that simulates natural systems or processes over time – the length of the model's time steps (and sub-time-steps, if used) is an important choice. The most current Tucker-Doty (EWG) modeling, according to its somewhat unclear documentation, employs time steps of 100 years[10] which are either used "as is" or subdivided into sub-time-steps as short as 5 years.[11] Such time increments, ranging from 5 years to 100 years in the various EWG model runs, seem unduly long based on Tucker and Doty's own advice for their prior models. The long duration of these increments was apparently due to computer run-time limitations[12] which impose a modeling trade-off between the time increments (steps, sub-steps) and the model's spatial resolution (cell size).

Given the 10,000-year duration of the EWG model runs, and given their erosion predictions that show minimal erosional damage to the SDA and NDA waste burial grounds at the end of 10,000 years (see Fig. 3 above), it is evident that *something is wrong* with the models' relationship between time and daily precipitation.[13] As discussed in the next paragraph, this problem with the EWG models can be recognized without looking at the models themselves, but, rather, by looking just at their 10,000-year erosion predictions in combination with the impacts of the August 2009 storm that affected the site.

The August 2009 storm, which caused substantial erosion damage to the West Valley site, delivered about 5 inches of rain in 24 hours and can be considered a 100-year storm (*see esp. Task 3*), meaning an average recurrence interval of 100 years or a 1:36500 probability of occurrence on any given day. Even without taking climate change into account, any erosion model that runs for 10,000 years needs to include 100 storms having at least this "100-year"

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[9] Tucker et al., op. cit.
[10] Id. at 11, 141, 157, and 217.
[11] Id. at 74-75 (see Tables 5.1 and 5.2, showing 1-20 sub-time-steps within a time step).
[12] Id. at 107-108, 131, and 157. Note that the computer run-time limitations meant that no single modeling run could exceed 24 hours on the supercomputer that was used.
[13] Id. at 60 for this relationship, where "a precipitation intensity p is drawn at random from a Weibull distribution... Once a precipitation intensity has been selected for a sub-time-step, water erosion is applied for a fraction F of the sub-step duration. Here F is an intermittentcy factor that represents the fraction of an average year that precipitation occurs, defined as the total number of days with measurable precipitation divided by the total number of days in the year."

intensity, of which 10 are more intense storms that deliver more than 5 inches in 24 hours and would be called 1000-year storms, of which one is a more intense storm that delivers even more rain and would be called a 10,000-year storm.[14] This set of 100 storms – judging from the August 2009 damage and without even taking climate change into account – would inevitably cause greater erosion damage to the SDA and NDA burial grounds than is shown on the Tucker et al. maps in Fig. 3; *hence the EWG modeling by Tucker et al. cannot be correct*. This is a crucial point, based purely on the overall length of the modeling runs, not on how the 10,000-year period is broken down into time increments of 5 to 100 years.[15]

The time increments (sub-time-steps) come into play as follows. A daily precipitation rate is selected for each increment, and this daily rainfall rate persists throughout the increment of 5 to 100 years.[16] Thus, it is evident that:

- a high precipitation intensity (high daily rainfall value) will rarely be selected for a sub-time-step within a given model run, and
- if/when a high precipitation intensity *is* selected for a sub-time-step, the modeled erosion should essentially destroy the West Valley site – as, for example, a daily 5 inches of rain continues for at least five years, or potentially as long as 100 years, thus delivering overwhelming amounts of daily rain even when the wet-day factor *F* is taken into account. The fact that Tucker et al. have not reported such erosional destruction of the site suggests a defect in their modeling (e.g., that their models are somehow avoiding selection of high daily rainfall values?).

The length of the sub-time-steps, ranging from 5 years to 100 years, is also problematic because such long time increments are incompatible with familiar properties such as soil porosity and variables such as soil saturation. Such real-world properties and variables have no connection to a model in which rainfall is held constant for at least 5 years – or perhaps somewhat less than 5 years if the model uses a wet-day factor *F*, but still far longer than it takes for soil to become saturated!

The modeling discrepancies described above become worse when the effects of climate change, particularly the effects on frequency and intensity of storms, are properly incorporated into the models.

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[14] While some of the numbers in this sentence (e.g., "needs to include 100 storms") should be expressed in a more probabilistic manner for real rainfall, the numbers are correct as stated for a meaningful model. Models that treat these numbers probabilistically can be meaningful as well, as long as they demonstrably average 100 storms of at least 100-year intensity in 10,000 years, and within those 100 also average 10 storms of at least 1000-year intensity in 10,000 years, and within those 10 average 1 storm of at least 10,000-year intensity in 10,000 years. Such a probabilistic approach has the disadvantage of producing rather spiky or jumpy results, especially when, as in the EWG erosion models, relatively few daily rainfall values are being picked "out of a hat," or from a Weibull distribution, in the course of each run.

[15] Note also that this point is independent of (unaffected by) the wet-day factor *F*.

[16] Id. at 60. To be precise, as stated on p. 60, "Once a precipitation intensity has been selected for a sub-time-step, water erosion is applied for a fraction F of the sub-step duration," where *F* is the wet-day factor.

The following comments made previously by CTF or its members, directed at previous versions of the erosion modeling, may no longer be strictly applicable – but Tucker's warning about excessively long time increments remains relevant.

VSC.65. The global time step T_g used in the stochastic models *may be ten years*. This was the time step identified by Tucker last year [2017][17] for somewhat different rainfall algorithms, but an unequivocal statement of the global time step T_g is needed before the current EWG rainfall modeling can be checked against real rainfall distributions.

VSC.66. The 10-year time step used last year [2017] in EWG erosion modeling runs is unacceptably long; it introduced an unrealistic rainfall intensity-frequency distribution[18] into those model runs[19] and may have a similar effect on subsequent runs that will be used in the SEIS process to support the Phase 2 decision. The EWG erosion modelers have recognized that an unduly short global time step T_g may be problematic, at least for the models being used in 2010:

> The model is relatively insensitive to Tg as long as its value is sufficiently small. To determine a reasonable value for Tg, a series of 1,000-year sensitivity tests were conducted using the modern topography of Buttermilk Creek as an initial condition. Results showed that values of Tg of approximately 1 year or smaller produce very similar results (average root-mean-square differences in model-cell height of less than 30 centimeters (11.81 inches) after 1,000 years of erosion). A value of 0.1 years was used in calibration and forward runs.[20]

Given this recognition, a ten-year time step can't simply be introduced without discussion.

See also Task 9.

[17] G.E. Tucker, pers. comm., May-June 2017.

[18] Note that the term "intensity" in the widely used phrase "intensity-frequency distribution" corresponds to rainfall "depth" – particularly the "depth" of 24-hour rainfall with a certain recurrence interval or probability – in the terminology of Tucker et al.

[19] For overview, see R.C. Vaughan, 6-28-17 CTF presentation, slide 10; also R.C. Vaughan, 9-27-17 CTF update presentation, slides 3 and 6-7.

[20] DOE and NYSERDA*, Final Environmental Impact Statement for Decommissioning and/or Long-Term Stewardship at the West Valley Demonstration Project and Western New York Nuclear Service Center*, DOE/EIS-0226 (2010), Appendix F, page F-29.

TASK 3: Assess whether current rainfall is properly represented. Is the return period or recurrence interval (probability of occurrence) of the August 2009 storm misrepresented?

Context: There are two issues here. How rainfall is represented in the LEM is one issue. The other is the Conceptual Site Model's misleading characterization of the August 2009 storm's recurrence interval (probability of occurrence per unit time).

How rainfall is represented in the LEM cannot be determined clearly from the available documentation. See, for example, page 60 of Tucker & Doty's report[21] which provides no quantitative detail for either their Weibull distribution or their wet-day factor *F*. See also Tasks 8, 14, and 15 below. The question of how rainfall is represented in the LEM is partly a matter of the rainfall distribution used, and partly a matter of how the model's algorithms relate each "parcel" of rainfall to the erosion it causes.

The Conceptual Site Model (CSM), pp. 95-96, is ambiguous about the site-specific recurrence interval of the August 2009 storm but implies that it is 200 to 500 years. The problem is that the CSM wrongly conflates the August 2009 "storm event [that] had a significant impact at the West Valley Site" with estimates and observations at Gowanda and Perrysburg, NY, where storm totals are known to have been greater than the rainfall at the West Valley site. This is evident from Fig. 2 of the USGS report by Szabo et al.[22] and also supported by other evidence such as stepwise integration of creek flow at the USGS Gowanda gage. The CSM cites Szabo et al., according to whom:

> The maximum daily total precipitation that was reported by a NWS cooperative weather observer, again the observer from Perrysburg, was 7.27 in. The Northeast Regional Climate Center assigned a recurrence interval for this daily total of 200 to 500 years.[23]

While such a recurrence interval for a daily total of 7.27 inches appears very reasonable, the "200 to 500 years" cannot be carelessly applied to a smaller daily total such as the storm's rainfall of about 5 inches that fell in 24 hours at the West Valley site! Szabo et al. go on to talk about other recurrence intervals of 500 years and "far greater than 500 years," but these are for rainfall durations of 120 minutes and 90 minutes, respectively, so do not apply to the 24-hour (i.e., daily) total of about 5 inches at the West Valley site. In summary, the August 2009 storm's recurrence interval at the West Valley site under current climate conditions is 100 years or less, not 200 to 500 years. See also Task 15.

VSM.6. The CSM's treatment of intense precipitation, probable maximum precipitation (PMP), and probable maximum flood (PMF) on pp. 93-96 fails to acknowledge and consider the 1942 Smethport storm and other readily available evidence of intense precipitation events near the West Valley site. (Smethport, PA, for example, is only slightly farther from the West Valley site than the Buffalo weather station is.) See, for example, the West Valley Citizen Task Force

[21] Tucker et al., op. cit., at 60.

[22] C.O. Szabo, W.F. Coon, and T.A. Niziol, *Flash Floods of August 10, 2009, in the Villages of Gowanda and Silver Creek, New York*, USGS Scientific Investigations Report 2010-5259.

[23] Id. at 3.

(CTF) 2015 memo on climate change,[24] the CTF 2018 scoping-comment letter,[25] my own scoping-comment letter dated May 23, 2018, and climate-related sources cited in all of these. Intense precipitation, PMP, and PMF need to be covered more realistically in the CSM.

VSM .7a. The CSM provides a very misleading characterization of the recurrence interval[26] for the August 2009 storm; this needs to be corrected. Page 96 of the CSM refers to a "recurrence interval of 200 to 500 yr" which applies to Perrysburg, NY, not to the Cattaraugus Creek basin upstream of Gowanda where the West Valley site is located. For the basin upstream of Gowanda, including the West Valley site, the storm had a recurrence interval of about 45 to 100 years (based on past data). Going forward into the future, this interval can be expected to shorten in response to climate change. The CSM needs to recognize the site-specific recurrence interval of about 45 to 100 years for the August 2009 storm, and also needs to acknowledge that such an interval will tend to shrink as a consequence of climate change.

VSM.7b. The CSM's recurrence-interval characterization is misleading because no other recurrence interval is mentioned aside from the "200 to 500 yr" which applies to the location (Perrysburg) where an observer recorded a 24-hour rainfall of 7.27 inches and a 90-minute rainfall of 5.98 inches. Both the CSM and the cited USGS report by Szabo et. al.[27] clearly indicate that these records pertain to Perrysburg. What the CSM fails to mention is 1) that the basin upstream of Gowanda, including the West Valley site, received less rainfall than Perrysburg, and 2) that the USGS report by Szabo et. al. lists a 45-year recurrence interval for the streamflow associated with the August 2009 storm.

VSM.7c. National Weather Service NEXRAD radar shows that the basin upstream of Gowanda, including the West Valley site, received less rainfall than Perrysburg during the August 2009 storm. My own stepwise integration of Cattaraugus Creek streamflow at the Gowanda gage shows that the basin upstream of Gowanda, including the West Valley site, received less than 7 inches rainfall, most likely 5 inches or slightly greater. These two complementary approaches, combined with the historic understanding that a 5.2-inch rainfall in the West Valley area has a recurrence interval of 100 years,[28] provide a good basis for concluding that the August 2009 storm's rainfall at the West Valley site had a recurrence interval of about 100 years – which will trend toward a shorter interval due to climate change. For NEXRAD radar showing less rainfall in the basin upstream of Gowanda (including the West Valley site) than in Perrysburg, see Fig. 2

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[24] CTF memo entitled "Actions Needed Related to Potential [Climate] Change Impacts," July 27, 2015, available at http://westvalleyctf.org/2015_Materials/07/2015-07-27_Memo-Climate_Change_Considerations_Incorporation_in_Decisionmaking.pdf.

[25] See http://www.westvalleyctf.org/2018_Materials/05/2018-05-21_CTF_SEIS_Scoping_Comments.pdf, esp. section XI (§ 55 ff.).

[26] Lest there be any doubt, "recurrence interval" is a widely accepted term that can also be understood as a *probability* of recurrence. It does not imply recurrence at regular intervals.

[27] Szabo et al., op. cit., at 3.

[28] USDA Technical Release No. 55, June 1986, Type II Soil Conservation Service rainfall depths, as listed in 2008 West Valley DEIS, Appendix F, p. F-22. Similarly, NOAA's Atlas 14 and online Precipitation Frequency Data Server show a recurrence interval of 100 years for a 24-hour rainfall of 5.39 inches at the West Valley site.

of the USGS report by Szabo et. al.[29] or Fig. 5 of my 2009 EIS comments.[30] For my stepwise integration of Cattaraugus Creek streamflow which shows that the basin upstream of Gowanda, including the West Valley site, received about 5 inches rainfall during the August 2009 storm, see my 2009 EIS comments.[31]

VSM.7d. According to the USGS report by Szabo et. al., "The peak flow in Cattaraugus Creek at Gowanda was computed, using the slope-area method, to be 33,200 cubic feet per second with an annual exceedance probability of 2.2 percent (recurrence interval of 45 years)."[32] This is not acknowledged in the CSM but should be. While streamflow cannot be exactly correlated with rainfall due to variations in the ratio of runoff to rainfall, the two measures are closely correlated in any major storm event.[33] Furthermore, to the extent that there is a difference between the two measures, surface flow such as streamflow will tend to be a more relevant measure than rainfall when evaluating site erosion. Thus, the CSM needs to acknowledge that both 45 years and 100 years are applicable site-specific estimates of the August 2009 storm's recurrence interval, based on historic streamflow and rainfall data; that either or both of these is a better site-specific value than 200 or 500 years; and that these 45- and 100-year recurrence intervals are trending toward smaller values due to climate change.

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[29] Szabo et al., op. cit., Fig. 2.

[30] Vaughan EIS comments, Fig. 5 – but note that Fig. 5 was omitted from the response-to-comments portion of the 2010 West Valley FEIS. Copy available upon request.

[31] Vaughan EIS comments §§ 210-215 and Table 2 – but note that Table 2 was omitted from the response-to-comments portion of the 2010 West Valley FEIS. Copy available upon request.

[32] Szabo et al., op. cit., p. 1; see also pp. 11-16. For comparison to peak flows in other years, see my scoping-comment letter dated May 23, 2018, Table 1.

[33] See discussion of the runoff-to-rainfall ratio in Vaughan EIS comments § 212 and in my scoping-comment letter dated May 23, 2018, §§ 55-62. See also the statement that "Saturated soil conditions existed prior to the intense storm that caused the flooding" in Szabo et al., op. cit., p. 3.

<u>TASK 4</u>: Assess whether paleo rainfall is properly represented. Dry paleo period ignored?

Context: Tucker and Doty's EWG erosion models (i.e., the most current landscape evolution models or LEMs) use a post-glacial or "paleo" period of 13,000 years to calibrate the model. In other words, model input parameter values are determined during the "paleo" calibration runs of the model. During these calibration runs, the model starts with an assumed landscape or topography that represents the West Valley site immediately after glacial withdrawal. As the model runs from 13,000 years ago to the present time, the input parameters that produce the best match to the *present* site topography are chosen. These parameter values are then used to model the site topography 10,000 years into the future, thus generating the model's predictions of future erosion.

Given this modeling procedure, the erosion predictions depend on the model's internal details and on the chosen input parameters (and also on the probabilistic range or distribution assigned to each input parameter). Erroneous assumptions about paleoclimate are among the many possible factors that will generate erroneous erosion predictions. If, for example, the modeling failed to recognize a prolonged "paleo drought" (a near-absence of rainfall during the 4000-year period from about 10,000 and 6000 years before present) but instead assumed uniform climate throughout the post-glacial period, the model's chosen parameters would be wrongly calibrated in the direction of artificially mild erosion, thus causing the model to underestimate future erosion. Granted, paleoclimate that includes a 4000-year drought may add to the difficulty of modeling, but what the modelers find convenient is far less important than attention to the available evidence and trustworthiness of the model.

In more detail, Tucker and Doty make an erroneous assumption that the near-absence of downcutting for about 4000 years (from about 10,000 to 6000 years ago) was due to erosion-resistant bedrock rather than a prolonged paleo drought. This assumption lacks stratigraphic support and biases their EWG modeling results toward underprediction of erosion. It does so by assuming that rainfall and its erosive power continued at a roughly constant rate throughout the 13,000-year-long model-calibration runs.[34] This conflicts with Wilson and Young's understanding that 4000 of those years were unusually dry ("There was less runoff during this mildly arid period and Buttermilk incision ceased.").[35] If Wilson and Young are correct, then the paleo rainfall and its erosive power had only about 9000 years, not 13,000 years, to change the landscape from its assumed post-glacial configuration to the configuration we see today. Three related points should be noted:

- Wilson and Young don't entirely rule out erosion-resistant bedrock as the cause of the base level stalling for about 4000 years at about 1280 ft elevation. They acknowledge this possibility but say "There is a 14-foot waterfall topping at 1274-feet in the west slope of Buttermilk valley across from Tree Farm; this sandstone might have been a base-level control for the abandoned meander, but climate change discussed in section 4.9 accounts better for the several thousand-year formation of the abandoned meander.... While resistant strata control of terracing in response to base level was active and significant,

[34] Tucker et al., op. cit., at 39-40 and 96.
[35] Wilson & Young, op. cit., at 81-84; the quote is from p 83.

the maintenance of elevation for thousands of years was likely from climate conditions as discussed in the preceding paleoclimate section, but base-level control from sandstone at 1274-feet could assist terrace development."[36]

- Tucker and Doty apparently don't even mention Wilson and Young's evidence-based understanding of the prolonged paleo drought!
- Given the two distinct and entirely different explanations of the "stalled" base level (erosion-resistant bedrock versus prolonged paleo drought), why didn't the EWG erosion models apply a probabilistic distribution between these two endpoints, weighting the distribution by the strength of the evidence??

As already noted, paleoclimate is just one of many factors that may result in erroneous erosion modeling results. Note also that the set of input parameters determined from the "paleo" calibration runs needs to be realistic with respect to the recent climate periods that we've directly observed (e.g., the period around 1950 when climate change was minimal).

VEC.166. DOE creates a calibration error of unknown magnitude by assuming essentially uniform paleoclimate conditions (see Appendix F of 2008 Draft EIS, bottom of page F-59), resulting in an assumption of essentially uniform erosional power of runoff and creek flow from century to century, during the past several thousand postglacial years. In the immediate postglacial period and/or the transitional period from the last glacial retreat into postglacial conditions, enormous flows of glacial meltwater may have been needed, as discussed above, to incise the Zoar Valley gorge through shale bedrock and/or to clear older glacial deposits from a preexisting gorge. The new point at issue is what happened next, from the end of major meltwater flow until now. During the past several thousand years, were rates of water flow in the Buttermilk and Cattaraugus watersheds essentially uniform from one century to the next – or, alternatively, were there substantial long-term climate variations that caused major variations in water flow and erosion? DOE's method of calibrating the West Valley erosion model assumes essentially uniform flows, but much evidence shows otherwise. As discussed below in more detail, the evidence implies that the valleys and ravines seen today in the Buttermilk Creek watershed were eroded in a substantially shorter time than DOE assumes (in other words, the model is miscalibrated). The evidence implies that water flows were too low, and the erosive power of the flowing water was too small, to accomplish any substantial erosion during much of the past several thousand years. Thus, as noted, the erosion needed to produce today's valleys and ravines must have been compressed into a much shorter time span than DOE assumes.

VEC.167. DOE creates a calibration error of unknown magnitude by assuming that paleoclimate conditions were the same as today's climate. (See Appendix F of 2008 Draft EIS, pages F-59 to F-60.) This assumption is contradicted by many available sources, including those identified in my January 15, 2008, memo entitled *Issues the Core Team Needs to Address* (attached to these comments as Appendix E). The two sources cited in my memo were A.J. Noren et al., "Millennial-scale Storminess Variability in the Northeastern United States during the Holocene Epoch," *Nature* **419**, 821-824 (2002) and T.L. Holcombe et al., "Revised Lake Erie Postglacial Lake Level History Based on New Detailed Bathymetry," *Journal of Great Lakes Research* **29**,

[36] Id. at 84.

681-704 (2003). Based on sediments deposited in lakes in Vermont and eastern New York, Noren et al. identified four periods of intense storminess that occurred about 11,900, 9,100, 5,800, and 2,600 years ago. Interspersed between the second and third of these storm periods was the middle Holocene climatic optimum (9,000 to 6,000 years ago), during which “warmer temperatures and greater aridity” characterized the climate of the Lake Erie region, according to Holcombe et al.

VEC.168. In addition to the sources cited in my January 2008 memo, there are *many other* relevant sources that need to be consulted by the authors of the 2008 Draft EIS with respect to regional and local paleoclimate and its effect on erosion model calibration. Three examples are T. Curtin et al., “Holocene and ‘Anthropocene’ Climate and Environmental Change in the Finger Lakes, NY,” 19th Annual Keck Symposium, 2006 (http://keck.wooster.edu/publications); C.F.M. Lewis et al., “Water Levels in the Great Lakes: A Cross-border Problem” (http://sst.rncan.gc.ca/erccrrcc/theme1/t9_e.php); and H.T. Mullins, “Holocene lake level and climate change inferred from marl stratigraphy of the Cayuga Lake basin,” *Journal of Sedimentary Research* **68**, 569-578 (1998). According to Mullins’ abstract:

> A series of 12 radiocarbon-dated sediment cores (up to 15 m long) were used to define the Holocene stratigraphy beneath the Cayuga Lake basin in central New York State in order to evaluate the stability of Holocene climate in the northeastern United States. These cores contain an abundance of thick lacustrine marls (> 30% $CaCO_3$) that were used to reconstruct century-to-millennium-scale changes in lake levels and, thus, paleoclimates. The oldest sediments recovered (> 11.2 ka) consist of pink, proglacial clays that were deposited in Glacial Lake Iroquois between approximately 12.5 and 11.3 ka. Lacustrine sediment (non-marl) of Killarney-Younger Dryas age (11.2-10.3 ka) was recovered both north and south of modern Cayuga Lake, indicating relatively high lake levels during this well-known cold-climate phase. Following a brief (< 500 years) warm period immediately following the Younger Dryas, a relatively cool and dry climate persisted in the Finger Lakes region between < 9.8 and 8.5 ka correlative with global meltwater pulse IB. The Holocene Hypsithermal period (approximately 9-4 ka) in the Cayuga Lake basin was characterized by widespread deposition of marl that locally contains as much as 90% $CaCO_3$. These marls document a broad, first-order warming-cooling trend throughout the Hypsithermal, with the climatic optimum at approximately 7 ka. This long-term trend is consistent with insolation data as well as ice-core records from Greenland, and likely was a response to Milankovitch orbital forcing. Lake levels throughout the Finger Lakes region were relatively high during the Holocene Hypsithermal, implying an overall warm and wet climate in contrast to the traditional view of mid-Holocene drought. However, Hypsithermal climate and lake levels in the Finger Lakes region were not stable; rather they were characterized by significant century-to-millennium-scale variability, implying short-term climate changes. Marl deposition in the Cayuga Lake basin ceased at approximately 3.4 ka when lake levels dropped as global cooling set in at the end of the Hypsithermal. However, there was a brief return to a warm and wet climate at approximately 1 ka, during the Medieval Warm Period prior to the onset of anthropogenic effects.

In calibrating their erosion models "through a forward modeling exercise, which starts with a postglacial (pre-incision) valley topography and attempts to reconstruct the modern topography" (as stated on page F-31), the authors of the 2008 Draft EIS cannot treat the climate of the past several thousand years as a blank slate. Much is already known, as indicated by the above work by Noren et al., Holcombe et al, Curtin et al., Lewis et al., Mullins, *and many others*. The authors of the 2008 Draft EIS are not unaware of the problem (they acknowledge on pages F-59 to F-60 that "climate in this portion of North America is known to have varied to some extent over the post-glacial period"), yet they give it no further consideration aside from a comment about "some uncertainty in model forecasts." This is not an acceptable response. The authors of the 2008 Draft EIS need to engage in the necessary scholarship to find, interpret, and properly incorporate the paleoclimate work which is currently missing from their calibration efforts. Their "forward modeling exercise" is not an idle schoolboy exercise that tolerates guesses and omissions; it is part of a complex decision which, if done badly, will put Western New York and the Great Lakes at risk of substantial radioactive contamination.

CSC.63 and VSC.92. Paleoclimate needs to be reconstructed based on the best available evidence and needs to be adequately and transparently incorporated into EWG and PPA erosion modeling.[37]

CSC.64 and VSC.93. The period of approximately 4000 years of minimal Buttermilk Creek downcutting (between about 10,000 and 6000 years before present), as identified by the EWG report by Wilson and Young,[38] needs to be linked to causal factors such as reduced rainfall or other evidence-based factors.

CSC.65 and VSC.94. It is not that clear that the sensitivity analyses for the EWG erosion modeling runs cover the range of rainfall rates (including a cessation or at least a greatly reduced rate of rainfall) for the period between about 10,000 and 6000 years before present when Buttermilk Creek downcutting was minimal.[39] While there may be other explanations for this period of minimal downcutting, one such explanation would be a prolonged "paleo drought" (a near-absence of rainfall) during the 4000-year period.[40] Sensitivity analyses showing the sensitivity of EWG model results to the rainfall assumed during calibration runs for this 4000-year period – including results for the limiting case in which no rainfall occurs in any time step during this period – must be provided. These sensitivity results must also be appropriately incorporated into PPA model runs.

[37] Vaughan EIS comments §§ 166-68.
[38] Wilson & Young, op. cit., esp. Figs. 4.10-3 and 4.10-4.
[39] Tucker, QPM presentation [2017], slide 6, does not show such a sensitivity analysis. As discussed elsewhere in these comments, it's clear that the EWG erosion modeling (as reported by G.E. Tucker et al., op. cit. [2018]) does not treat paleoclimate rainfall variation as an independent parameter and thus provides no clear and direct basis for understanding its effect on model results (or conversely, for understanding the sensitivity of model results to paleoclimate variation).
[40] Or, for example, a prolonged drizzle-dominated period with little or no storminess.

TASK 5: Assess the mismatch (factor of about 3) between the LEM's assumed runoff and the site-specific runoff documented by other credible sources

Context: On a local or regional basis, the annual average *precipitation* (both rain and snow) equals the sum of annual average *runoff* and annual average *evapotranspiration*. Thus, only a fraction of rainfall and snowmelt flows downhill as runoff and contributes to erosion. The other fraction goes into the atmosphere and contributes to humidity, either via direct evaporation from surface water or via uptake by plants that emit (transpire) water vapor from their leaves. Tucker and Doty, in their EWG erosion modeling (LEM) work, do not make consistent distinctions between precipitation and runoff – and where they do make this distinction, they adopt a runoff fraction that is far too low, particularly for the South Plateau (only about one-third of the runoff fraction expressed by credible sources, including the source they cite). In so doing, they adopt an erroneous runoff fraction that is far less erosive than a correct (i.e., evidence-based) fraction. For additional evidence, see the Conceptual Site Model, pp. 89-91, and sources cited there.

VSC.55. The treatment of *runoff* in the EWG erosion models involves a substantial error and additional unresolved questions. As background, it's important to note that rainfall doesn't all flow downhill as runoff. Some percentage of rainfall soaks into the soil (thus contributing to groundwater recharge as well as uptake by plants and other evapotranspiration), while most of the remainder flows downhill as runoff. In a very light rain, there's essentially no runoff; almost all of the rainfall soaks into the soil. In a heavy rain, the soil may approach saturation, thus reducing the percentage that soaks in and increasing the percentage that goes to runoff. And it's important to remember that groundwater recharge must be balanced, locally or regionally, by groundwater *discharge* that provides the base flow that keeps streams flowing even in dry weather. Thus, on a local or regional basis, the annual average precipitation (both rain and snow) is equal to the sum of annual average runoff and annual average evapotranspiration. USGS maps compiled by Randall for the 1951-1980 period, while somewhat out of date with respect to climate change, are nevertheless very useful for general guidance.[41] For the West Valley area, Randall shows the annual average runoff as 24 inches (0.61 m).[42] This is the approximate amount of water that would have been available at the surface to cause erosion during the 1951-1980 period.[43] Climate change will certainly change the distribution of precipitation over time (more intense storms) and can be expected to increase evapotranspiration. Its effect on annual average runoff is less clear, and Randall's 24 inches (0.61 m) remains useful guidance.

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[41] A.D. Randall, *Mean Annual Runoff, Precipitation, and Evapotranspiration in the Glaciated Northeastern United States, 1951-80*, USGS Open-File Report 96-395 (1996).
[42] Id., Plate 1.
[43] While some number of inches per year would soak into the soil and flow beneath the surface as groundwater, and might thus be subtracted from Randall's 24 in/yr runoff, thereby making the surface flow less than 24 in/yr, *this would be counterbalanced on a local or regional basis by base flow,* so that the annual average amount available for surface flow in the West Valley area would have been about 24 inches during the 1951-1980 period.

VSC.56. While the EWG erosion models divert some rainfall to subsurface flow and allow it to rejoin surface flow within the modeled watershed,[44] it's not clear *where* the models allow the subsurface flow to reemerge and become surface flow. This needs to be answered, and the answer can and should be checked against the *gaining* stream segments that Zadins refers to, either by conducting new field work or by gleaning such information from the sources cited by Zadins.[45] Furthermore, the EWG models need to be consistent with 1980-1983 field work that showed "Nearly 80% of gaged flow from the burial areas was runoff and the remaining 20% was base flow," while "the north plateau flow consisted of 30% runoff and 70% base flow," with the difference between the two plateaus attributed to soil composition.[46] Note that the geomembrane covers had not yet been placed on the burial grounds at the time this field work was done and thus cannot have contributed to the difference.

VSC.57. In the EWG erosion models, it's not clear whether and how soil saturation serves to limit or cap the modeled subsurface flow. This needs to be answered, and the answer needs to be shown to be realistic. For example, how do the models apportion surface and subsurface flow during a 1-year sub-time-step, where soil saturation may be reached early in the year (i.e., within hours for a heavy rainfall event), after which runoff becomes asymptotically constant for the duration of rainfall within the 1-year sub-time-step?

VSC.58. The outright error in treating runoff in the EWG erosion models is the assumption by Tucker et al. that the annual average runoff is 0.2 m/yr. This is entirely unrealistic, either as an annual average runoff value or in relation to annual average precipitation. Tucker et al. claim that:

> As a means of constraining watershed-scale effective infiltration capacity, we consider estimates of mean annual storm runoff in the region reported by DOE and NYSERDA (2010, Appendix F). These estimates ranged from 0.2 to 0.6 m/y, with most estimates closer to the lower end. Because 0.2 m/y is the more common value and more broadly representative of watershed runoff coefficients, we consider this the best current estimate.[47]

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[44] Tucker et al., op. cit., at 233, which says "The bases of the hills represent locations where water emerges from the shallow subsurface to become surface flow that feeds the channel network" – but what hills? It is unclear whether the models have a fully developed water table (as in a MODFLOW model) or whether they rely on a generalized algorithm for groundwater discharge. If the latter, it's unclear whether all subsurface flow moves toward and into the nearest channel along the steepest available flow pathway and discharges there (essentially mimicking runoff), or whether all subsurface flow continues as groundwater until it reaches the lowest point in the watershed (the Franks-Buttermilk confluence) and discharges there, or whether subsurface flow travels and reemerges by some intermediate pathway.

[45] Z.Z. Zadins, *A Hydrogeologic Evaluation of "Geologic and Hydrologic Implication of the Buried Bedrock Valley that Extends from the Western New York Nuclear Service Center into Erie County, NY,"* Dames & Moore technical report, prepared for DOE and West Valley Nuclear Services Co. (August 1997), at 6.

[46] F. O'Connor, Hydrology Environmental Information Document (EID), Part 2, *Surface Water Hydrology*, West Valley Nuclear Services, WVDP-EIS-009, Rev. 0, 1/29/93, at 10.

[47] Tucker et al., op. cit., at 149.

This is a gross misrepresentation of the cited source. In fact, Appendix F of the 2010 Final EIS lists four runoff values ranging from about 0.2 to about 0.6 m/yr, but these are differentiated by watershed, with the highest of the four values (0.579 m/yr) being listed specifically for the Franks Creek watershed.[48] It is disingenuous for Tucker et al. to ignore the 0.579 m/yr runoff value for the watershed they're modeling and to claim without any supporting citation that 0.2 m/yr is "more broadly representative of watershed runoff coefficients." Randall's value of 24 in/yr (0.61 m/yr) says otherwise. Indeed, the 0.579 m/yr value for the Franks Creek watershed and Randall's 0.61 m/yr value for the West Valley area are in good agreement, indicating that the EWG model runs should have used 0.6 m/yr runoff rather than the 0.2 m/yr value that was actually used. This threefold error in assumed runoff is a serious problem – especially given the key role that runoff plays in erosion – and is another reason why the current EWG modeling results cannot be considered realistic or trustworthy.

VSC.61. As noted above, erosion is more directly related to runoff than to rainfall. In reality, there are several interrelated variables that need to be handled correctly in the models:

- *Runoff*, as discussed above.
- *Rainfall*, as discussed above and elsewhere in these comments.
- *Snowmelt, either by itself or combined with rainfall*. Neither of these annually occurring types of snowmelt events appears to be considered in the EWG erosion modeling,[49] yet both can contribute significantly to runoff, and their contributions can be expected to increase due to climate change (due partly to larger/more frequent temperature swings). As can be seen from the online USGS table,[50] many of the highest annual flows recorded at the Gowanda gage on Cattaraugus Creek have occurred in winter months. This provides at least rough evidence of historical timing of peak flows (winter vs. summer) in the Franks Creek subwatershed.
- *Base flow*, which is considered to some extent in the EWG erosion modeling, as discussed above – but it's not clear whether base flow is reasonably correctly modeled, including in the headwaters of Franks Creek.

VSC.62. There is apparent confusion in the EWG erosion modeling report between rainfall and runoff:

> Once a precipitation intensity has been selected for a sub-time-step, water erosion is applied for a fraction *F* of the sub-step duration. Here *F* is an intermittentcy factor that represents the fraction of an average year that precipitation occurs, defined as the total

[48] DOE and NYSERDA*, Final Environmental Impact Statement for Decommissioning and/or Long-Term Stewardship at the West Valley Demonstration Project and Western New York Nuclear Service Center*, DOE/EIS-0226 (2010), Appendix F, Table F-9.
[49] The word "snow" appears in connection with paleoclimate on pp. 39-40, but nowhere else, in G.E. Tucker et al., op. cit.
[50] https://nwis.waterdata.usgs.gov/nwis/peak?site_no=04213500&agency_cd=USGS&format=html

number of days with measurable precipitation divided by the total number of days in the year…[51]

The second sentence quoted here defines *F* in terms of a rainfall cutoff (annual fraction of days with measurable precipitation). The first sentence, saying that "water erosion" is applied in the model for that same fraction of time, implies that runoff and/or stream flow is cut off abruptly when the *F*-limit is reached. If the description is accurate, this aspect of the model is unrealistic.

[51] Tucker et al., op. cit, at 60.

<u>TASK 6</u>: Assess fluvial process parameters and assumptions used in the LEM

Context: According to Tucker and Doty:

> We use the term "fluvial processes" to refer to the processes by which sediment and rock are detached from the bed of a stream or gully, transported down the channel, and (possibly) deposited. Our models combine these processes, or simplified representations of them, to describe changes in elevation of the channel bed through time.[52]

This is valid as a general statement, yet there are at least three questions about its implementation in the EWG modeling.

First, it's unclear whether the LEM incorrectly assumes that a given amount of rain causes the same erosion regardless of its intensity (for example, 3 inches per hour versus 3 inches per week) or correctly recognizes that erosion is worse when the given amount of rain falls in a short period. The 2010 EIS (page 3-35) correctly recognizes that "Erosion is occurring in the region and the greatest topographical changes occur after large storms...." Similarly, the Conceptual Site Model, p. 95, citing a 2006 Safety Analysis Report for the site, recognizes "that erosion resulting from intense precipitation events is particularly significant for the Site." However, the EWG modeling is unclear about this basic principle – and its results can be considered very inaccurate if it assumes that erosion is linearly dependent on rainfall depth without taking rainfall intensity into account.

Second is the question about whether *plucking* or *abrasion* of sediment grains and fragments is the dominant form of erosion in the LEM. This is an important distinction.

Third, the EWG modeling overlooks (omits) an important mode of sediment detachment and transport during intense storm events, namely, detachment and downstream transport of *rip-up clasts* of clay-like material, apparently till, that were observed in Quarry Creek by R. Vaughan and NYSERDA staff after the August 2009 storm.[53] These irregular balls or chunks, evidently a result of plucking by stream-channel flow during the storm, represent what appeared to be a very significant fraction of the detached sediment. Unless this phenomenon is investigated and quantified, the EWG models' conceptualization of "processes by which sediment and rock are

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[52] Tucker et al., op. cit., §5.5, at 66 ff.

[53] While the term "rip-up clast" is often used in other depositional settings, it applies here very well. It typically refers to sedimentary-structure features in bedrock, created hundreds of millions of years ago by turbidites that plucked chips or chunks of unlithified but cohesive sediment from the seabed and carried those chips or chunks short distances laterally before they came to rest and eventually lithified as distinct inclusions within the sedimentary rock. See also the *Encyclopaedia Britannica* entry for "armoured mud ball," which says that such balls "originate as clay chunks that are broken from a stream bank by erosion and then rolled downstream, acquiring armour as the sand and gravel grains press into the soft exterior..." In the 3/17/21 issue of the *Greenfield Recorder*, Geologist Richard D. Little noted that he had discovered lithified mud balls in Turners Falls, MA, and had published this discovery in the *Journal of Geology* in 1982. As he describes, such armored mud balls are rare and were "formed by streams or beach waves rolling sticky mud into ball shapes becoming coated by pebbles" – after which, "[t]o be preserved in the geological rock record they must be quickly buried before drying and disintegrating." The clasts seen in the bed of Quarry Creek had no obvious armoring. Without followup observation, it's unknown whether they broke up into progressively smaller pieces or survived by acquiring protective armoring.

detached from the bed of a stream or gully [and] transported down the channel" will remain incomplete, thus causing site erosion to be underestimated by an unknown amount.

Such rip-up clasts were apparently not observed in the EWG erodibility characterization work by Sean Bennett.[54] However, during the August 2009 field trip, they were numerous and obvious. It is possible that the sets of *joints* or *joint-like fractures* that Belcher,[55] Fakundiny,[56] and others[57] have observed onsite in shallow clay or till may play a role by enabling such clasts to detach along the fracture surfaces.

VEC.170. It is widely recognized that the rainfall-erosion relationship is nonlinear, such that a single intense rainstorm produces more erosion than the equivalent amount of precipitation received as rainfall over a more extended period. (For example, see T.J. Toy et al., *Soil erosion*, 3rd edition, Wiley, 2002.) Given this relationship, and given the predicted increase in extreme weather events as a consequence of climate change, erosional effects will become progressively more severe at the West Valley site. Current erosional modeling in the 2008 Draft EIS does not take these effects into account and thus underestimates future erosion at the site. This problem must be remedied.

VSC.59. *Stream power*, the rate of energy dissipation per unit surface area, is a central part of many of the EWG erosion models[58] and is closely related to stream velocity through the relationship between kinetic energy and velocity, $E = ½mV^2$. One test that could be readily applied to the EWG erosion models is whether their modeled stream velocities at specified recurrence intervals match those shown in the West Valley Hydrology EID for specific reaches within the Franks Creek system.[59] This is not a sufficient test – it doesn't take climate change into account, can't overcome erroneous erodibility factors (erosion coefficients), etc. – but is a necessary test that can and should be done.

VSC.60. More generally, it's not clear whether the EWG erosion models reflect the physical reality that different rainfall intensities (from drought to drizzle to intense storms) have a major effect on stream velocity, stream power, and erosion. This lack of clarity is reflected on page 96 of the EWG erosion modeling report, where the phrase "If one assumes a linear relation between precipitation and erodibility…" and other discussion on the same page[60] imply that such a linear relationship exists – which is either wrong or imprecisely worded. Erosion and erodibility are not linearly related to a given amount of rainfall (e.g., 5 inches) in the absence of a known *rate* of

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[54] Sean J. Bennett, *Report of the West Valley Erosion Working Group, Study 2: Recent Erosion and Deposition Processes*, March 1, 2017

[55] Donald J. Belcher, letter dated June 19, 1970, reproduced in *Geology Reports of the Coalition on West Valley Nuclear Wastes*, op. cit., at 14-18.

[56] R.H. Fakundiny, "Practical Applications of Geological Methods at the West Valley Low-Level Radioactive Waste Burial Ground, Western New York," *Northeastern Environmental Science* **4**, 116-148 (1985), at 129; also his presentation to NYS LLRW Siting Commission, November 16, 1989, transcribed in *Geology Reports of the Coalition on West Valley Nuclear Wastes*, op. cit., at 13.

[57] See the Conceptual Site Model at 72-73 and sources cited therein.

[58] Tucker et al., op. cit., at 33-37, 66-74, and 222ff.

[59] O'Connor, op. cit., at 19 and Table 2-4.

[60] Tucker et al., op. cit., at 96.

rainfall. Five inches of drizzle over a prolonged period is far less erosive than a five-inch downpour.

VSC.63. The water-erosion laws used in the EWG modeling work assume that *plucking* of sediment grains and fragments, rather than *abrasion*, is the dominant form of erosion.[61] This fundamental assumption may be unrealistic. Compare Beyer's characterizations of the Buttermilk and Franks watersheds:

> Incision of Buttermilk Creek occurs mainly through the movement of the bedload [of gravel and cobbles] scraping the underlying soil or bedrock.[62]
>
> The knickpoints [on Franks Creek] behave like the advancing head of a gully. The erosion and retreat of the knickpoint allows gravel and cobbles to be freed and deposited at the base of the knickpoint. Turbulence at the base of the knickpoint agitates the collected gravel and cobbles, creating a scour pool. The gravel and cobbles subsequently deepen the channel by abrasion while moving downstream.[63]

Beyer continues by describing the ongoing incision and intermittent blockage of the V-shaped stream valley below the knickpoint, involving channel deepening and sidecutting which releases more gravel and cobbles. He notes that downcutting "continues by the movement of the bed load that erodes the native till along the channel bottom."[64]

VEC.204. The relatively intense rainfall event which delivered a total of approximately 5 inches of rain to the West Valley site between August 8 and August 10, 2009, has important implications for the site's susceptibility to erosion, long-term site integrity, storm return intervals, climate-change-induced changes in storm frequency and intensity, and the need for reliable data collection.

VEC.205. Several very obvious erosion effects occurred on and near the site in short periods of time (e.g., several hours) as direct results of the rain event and associated runoff, as I observed during a site inspection several days later (August 19, 2009). For example, knickpoints on both Erdman Brook and Franks Creek migrated several feet upstream, with associated enlargement of their plunge pools. The Quarry Creek ravine underwent substantial scouring and sidecutting in several locations near the old Rock Springs Road bridge abutments. This caused the root systems of large trees growing on the banks to be partly undercut, caused other large trees on the banks to fall into the ravine due to more extensive undercutting and slumping, caused or enhanced the slumping of other blocks of earth on the sloping ravine banks, caused large clayey clasts ranging up to 12 or more inches in diameter (apparently rip-up clasts plucked from the ravine banks by the flowing water) to be deposited within the ravine as the peak flow receded, and apparently caused large quantities of sediment to be carried downstream beyond the ravine during the storm

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[61] Tucker et al., op. cit., at 234-235.

[62] B.M. Beyer, Hydrology Environmental Information Document (EID), Part 1, *Geomorphology of Stream Valleys*, West Valley Nuclear Services, WVDP-EIS-009, Rev. 0, 1/29/93, at 9.

[63] Id. at 13.

[64] Id.

event, both in the form of particles carried as suspended sediment and in the form of rip-up clasts (ranging up to 12 inches and more) that were carried as bed load by the flowing water....

TASK 7: Assess the LEM's failure to distinguish between U- and V-shaped valleys

Context: The distinction between U-shaped and V-shaped valleys is widely recognized as an important part of erosional processes, linked to both cause and effect. The West Valley Conceptual Site Model, p. 64, acknowledges that "Rapid stream channel incision is indicated by transitions from U-shaped to V-shaped profiles and migrating knickpoints in the drainages." See also L.M. Gordon et al., "Hindcasting, forecasting, and controlling erosion at the Western New York Nuclear Service Center," NYSGA 2013 Field Trip Guidebook:

> Near the headwaters of many of Buttermilk's tributaries, there is a sharp transition from a deep V-shaped valley to a more broad U-shaped valley, which coincides with a change in the longitudinal profile of the stream to a gentler grade (Figure 8). In Erdman Brook and Frank's creek, this transition has generally mirrored the location of large knickpoints (Trip Stops 2 and 3, Figures 11 and 12). Upstream of the transition/knickpoints, the floodplains occupy a wide, flat valley bottom, and in many cases (typically in wetland areas), a defined stream channel is not evident. While the V-shaped reaches are incised in Lavery till, the upland U-shaped reaches have been filled with 1 m to 3 m of fine-grained sediment in the recent past, evidently by beaver dams (Figures 11 and 12). Beaver dams are common in the area and effectively result in deposition of large amounts of sediment in these upland stream reaches. Beaver dams/ponds also serve as a natural means of erosion protection, providing grade control and energy dissipation. In order to monitor and manage the streams in a stable condition, beavers (and their dams) have been removed from Frank's Creek and Erdman Brook since the development of the Center (1960s). In the absence of beaver dams to hold the deposited sediments in place, knickpoints moving upstream out of the V-shaped reaches have encountered the highly erodible deposits, and over the past ~50 years have incised more than 100 m of both Erdman Brook and Frank's Creek. As these knickpoints have moved closer to the radioactive waste disposal areas, the state and federal agencies managing the site have taken steps to control the erosion.

Steps taken by agencies to control erosion cannot be assumed to be available beyond 100 years due to regulatory limits on how long institutional controls can be assumed. Thus, there are two issues here: 1) Can beavers resume their role of minimizing erosion? 2) Is the LEM's failure to distinguish between U- and V-shaped valleys a serious problem or inaccuracy in erosion modeling? The former is addressed in Task 24; the latter is addressed here.

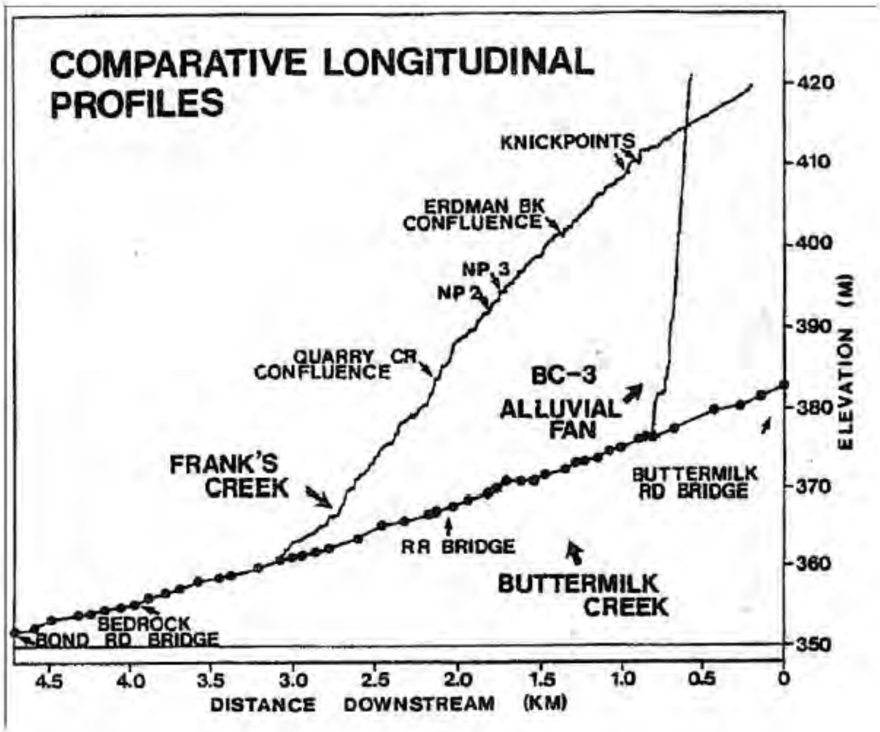

Figure 8 - Longitudinal profiles of Buttermilk Creek, Frank's Creek, and a small valley wall alluvial fan (Boothroyd et al., 1982)

VSC.52. Knickpoints and their upstream migration are a fundamental part of the erosion process on the Franks Creek watershed. As described by Beyer:

> Erosional processes acting on the Frank's Creek system may be differentiated between the V-shaped and U-shaped stream valley sections. The main erosion process on the [U-shaped] floodplain is the slow downcutting and meandering of the stream channel. Erosion scars on the adjacent hillslopes indicate how surface drainage patterns on the plateau have changed; these areas are more influenced by the sources and paths of water flowing over them than by any action of the main stream channel. The change from the [U-shaped] floodplain channel to the V-shaped channel occurs at a knickpoint. On Frank's Creek this change is total and abrupt. Erdman Brook has a transition zone between the two channel types that is several hundred feet long.
>
> The knickpoints behave like the advancing head of a gully. The erosion and retreat of the knickpoint allows gravel and cobbles to be freed and deposited at the base of the knickpoint. Turbulence at the base of the knickpoint agitates the collected gravel and cobbles, creating a scour pool. The gravel and cobbles subsequently deepen the channel by abrasion while moving downstream.
>
> As the channel deepens, the surrounding banks fail via undercutting and slope failure. With bank collapse, more gravel and cobbles are released from the till and the former

> flat-bottomed floodplain disappears as the stream valley develops a V-shape. Reforestation also places an increasing load on the channel slopes until they sporadically fail by slumping and/or sliding….[65]

Beyer reports that, for Franks Creek, "the transition knickpoint between the V- and U-shaped channels has been moving upstream at a rate of about 2.3 meters (7.5 ft) per year for the past thirty-five years." For Erdman Brook, he reports the rate as about 3.2 meters (10.5 ft) per year.[66]

VSC.53. The preceding paragraph indicates that erosion in the U-shaped channel above the knickpoint is a distinctly different geomorphic process from erosion in the V-shaped channel below the knickpoint. The EWG erosion models fail to take this difference into account. In this respect, the EWG erosion models are substantially unrealistic and cannot be considered trustworthy.

VSC.54. The significance of knickpoint migration and the U- and V-shaped channels can also be explained as follows. The Franks and Erdman knickpoint locations can be extrapolated backward in time based on their current locations[67] and their above-quoted retreat rates. Such extrapolation, while approximate, will clearly show that the channels of both Franks Creek and Erdman Brook were entirely U-shaped until very recently (no more than a few centuries ago) in the vicinity of the burial grounds. Hence:

- The channels of both Franks Creek and Erdman Brook were U-shaped in the vicinity of the burial grounds during almost all of the 13,000-year postglacial period.
- The channels of both Franks Creek and Erdman Brook either are, or are rapidly becoming, V-shaped in the vicinity of the burial grounds.

Given Beyer's description of the qualitatively different erosion processes occurring in the U- and V-shaped channels within the modeled watershed, *the EWG modeling strategy of using post-glacial calibration runs to derive erodibility factors for modeling 1000 or 10,000 years into the future is entirely unsupported in the vicinity of the burial grounds*. If the models were realistic and trustworthy in all other respects, it might be reasonable to obtain erodibility factors such as K_1 or K_2 from post-glacial calibration runs (during the past 13,000 years), and to apply these erodibility factors to model runs that extend into the distant future, for any portions of the watershed that have *not* undergone a change from a U-shaped to a V-shaped channel.[68] But since this condition is not met in the vicinity of the burial grounds, the EWG modeling strategy can't realistically be applied.

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[65] B.M. Beyer, Hydrology Environmental Information Document (EID), Part 1, *Geomorphology of Stream Valleys*, West Valley Nuclear Services, WVDP-EIS-009, Rev. 0, 1/29/93, at 12-13.
[66] Id. at 11-12.
[67] DOE and NYSERDA*, Final Environmental Impact Statement for Decommissioning and/or Long-Term Stewardship at the West Valley Demonstration Project and Western New York Nuclear Service Center*, DOE/EIS-0226 (2010), Fig. 3-18.
[68] Different K_1 values would likely be needed for U-shaped channels in till and V-shaped channels in till. Whether different K_2 values would be needed for the two different channel shapes in bedrock is less clear.

<u>TASK 8</u>: Assess whether LEM is too abstract to be "ground-truthed" or checked against reality

Context and terminology: The current EWG erosion models either represent rainfall directly (via a stochastic procedure) or use a deterministic procedure that represents rainfall indirectly as part of an erodibility factor such as K_1 or K_2. The models that represent rainfall directly are said to perform poorly relative to those that bundle rainfall into an indirect measure such as an erodibility factor. These indirect measures are already one step removed from a directly verifiable input parameter such as rainfall and its distribution. Other model algorithms introduce further "indirectness" that confounds reasonable efforts to check the models and their assumptions against reality.

VSC.35. A fundamental problem with the EWG erosion models is that they are *being developed* at the same time as they're being used as a basis for the Phase 2 decision.[69] The use of such young models is questionable, especially given the far-reaching consequences of the Phase 2 decision. In general, it is risky and unprotective to use models that are too young to have "stood the test of time" to support socially important decisions. To their credit, the EWG models aren't entirely new (they're based on earlier models dating back a couple of decades) and have been checked against a validation watershed east of Buttermilk Creek. However, these "credits" need to be balanced against the models' shortcomings and limitations such as their use of approximations and their use of proxy or surrogate parameters in place of field-measurable variables.

VSC.36. Such shortcomings are illustrated by the differences in performance among different versions of the models, some of which have input parameters that represent rainfall directly (via a stochastic procedure) while other versions use a deterministic procedure that represents rainfall indirectly as part of an erodibility factor such as K_1 or K_2. Part of the problem is that the models that represent rainfall directly *often perform no better in the calibration runs – and sometimes perform worse* – than their deterministic counterparts. Furthermore, none of the models that represent rainfall directly are among the EWG erosion models that perform reasonably well.[70] This indicates that:

- The EWG erosion models are unrealistic, i.e., they're not correctly representing real-world erosion. They're not able to combine direct representation of rainfall with reasonably good performance.
- The rainfall distributions of the best-performing EWG erosion model runs (those that are intended to support PPA modeling and the Phase 2 decision) can't be identified or checked against real-world rainfall distributions because of the way rainfall is represented indirectly as part of an erodibility factor.

VSC.37. Authors of the EWG erosion modeling report put a different "spin" on the conclusion expressed above. They say:

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[69] Tucker et al., op. cit., esp. at 212 and 215.

[70] Id., Table 8.2 and Fig. 8.4, where models 100, 102, 104, 108, and 110 can be compared to models 200, 202, 204, 208, and 210, respectively, and all of these can be compared to the best-performing models such as 842, 802, 808, etc. See also high standard deviations in Tables C.12 through C.15.

> We find that stochastic models do not calibrate any better than their deterministic counterparts, even given their additional free parameters. We interpret this as an indication that explicitly treating the rainfall distribution does not provide additional explanatory power in this region. This result indicates that use of an "effective" erodibility factor appropriately subsumes the effects of sequences of high and low runoff.[71]

Such an explanation can't overcome the fact that *if the models were realistic, they could achieve reasonably good performance in combination with direct representation of rainfall*.

VSC.38. At the very least, models need to be accessible, reproducible, and physically plausible in order to be deemed trustworthy and realistic.

VSC.39. Models that are trustworthy and realistic need to be sufficiently accessible that they can be reviewed and discussed. In other words, they shouldn't be opaque "black boxes" whose workings can't be deciphered. The way the EWG models handle rainfall is a prime example. As noted above, there's no straightforward relationship in the "best-performing" EWG erosion models between rainfall and the models' input variables.[72]

VSC.40. Accessibility can be provided, for example, by good documentation of the modeling algorithms & code and/or followup discussion that can clarify points that are unclear in the initial documentation. The Final Report on EWG erosion modeling[73] provides insufficient detail on the modeling algorithms and code, and it is unclear whether the expiration of the Phase 1 Studies contract with the EWG erosion modelers will allow any opportunity for followup discussion.

VSC.41. Models that are trustworthy and realistic need to be reproducible, meaning *the results should be the same or similar* when different research teams run the same or similar models. In part, this means that quantitative sensitivity analyses (showing how sensitive the model results are to the various input parameters) need to be fully documented, so that small variations in input parameters don't produce unexpectedly large differences in results.

VSC.42. Models that are trustworthy and realistic need to be physically plausible. In part, this means that the underlying physical processes need to be properly represented in the model. Examples involving knickpoints, mode of erosion (abrasion vs. plucking), runoff, slumping, earthquakes, etc., are discussed below.

VSC.43. Models that are trustworthy and realistic need to be physically plausible. In part, this means that the models' input parameters need to be accessible, field-testable, and consistent with real-world data. Examples involving stream flow rates are discussed below. If deterministic input parameters are being used, their values need to be realistic and defensible. If probabilistic input parameter ranges are being used, the ranges need to be reasonably broad and defensible, and the probability distributions within those ranges need to be realistic and defensible. Whenever a surrogate or proxy input parameter is used instead of a field-testable input

[71] Id. at 129.
[72] Id., where Table 8.2 shows the best-performing models and Table 8.1 shows the (free) input parameters for each model.
[73] Id.

parameter, its relationship to the field-testable parameter needs to be well-defined and quantitative, and a well-documented and quantitative sensitivity analysis is needed for each such relationship.

VSC.44. If families or "suites" of relatively similar models produce relatively similar results when they are run with identical input parameters, this may confer some limited level of assurance but cannot establish the necessary level of trustworthiness without the above criteria (accessibility, reproducibility, and physical plausibility) being met.

VSC.45. In reviewing results from suites of relatively similar models, it's important to remember that *the accuracy of the model(s) is paramount*. Only time will tell whether a given model or suite of models is accurate – so at the present time we must usually substitute "trustworthy" for "accurate" – but it's accuracy or trustworthiness that we ultimately need to assess, rather than model attributes such as elegance or model-to-model consistency or simplifications imposed by run-time constraints. For example, consistency of results from a suite of models may appear encouraging, but trust would be unfounded if all models in the suite share a common error or weakness.

VSC.46. The EWG erosion models cannot be considered trustworthy and realistic relative to the above criteria (accessibility, reproducibility, and/or physical plausibility) based on currently available information on the EWG erosion modeling.[74] Various examples are provided in these comments. The SEIS process and related processes (such as the Phase 2 decision and NRC license-termination process) should therefore not rely on EWG erosion modeling unless and until such modeling is revised and can meet the above criteria.

[74] Id.

<u>TASK 9</u>: Assess whether the LEM's sensitivity analyses are too abstract to provide meaningful sensitivity results (i.e., *whether they show sensitivity to key inputs*):

E.g., probabilistic 24-hr rainfall depth/frequency relationship

E.g., probabilistic gully-initiation-and-growth behavior; RV Visual Basic model

E.g., probabilistic treatment of the LEM's runoff assumption

E.g., probabilistic base level at Buttermilk-Cattaraugus confluence

E.g., probabilistic history of paleo rainfall & the Buttermilk bedrock sill

VSC.65. The global time step T_g used in the stochastic models *may be ten years*. This was the time step identified by Tucker last year [2017][75] for somewhat different rainfall algorithms, but an unequivocal statement of the global time step T_g is needed before the current EWG rainfall modeling can be checked against real rainfall distributions.

VSC.66. The 10-year time step used last year [2017] in EWG erosion modeling runs is unacceptably long; it introduced an unrealistic rainfall intensity-frequency distribution[76] into those model runs[77] and may have a similar effect on subsequent runs that will be used in the SEIS process to support the Phase 2 decision. The EWG erosion modelers have recognized that an unduly short global time step T_g may be problematic, at least for the models being used in 2010:

> The model is relatively insensitive to Tg as long as its value is sufficiently small. To determine a reasonable value for Tg, a series of 1,000-year sensitivity tests were conducted using the modern topography of Buttermilk Creek as an initial condition. Results showed that values of Tg of approximately 1 year or smaller produce very similar results (average root-mean-square differences in model-cell height of less than 30 centimeters (11.81 inches) after 1,000 years of erosion). A value of 0.1 years was used in calibration and forward runs.[78]

Given this recognition, a ten-year time step can't simply be introduced without discussion.

VSC.67. [The EWG erosion modeling report] indicates that sub-time-steps ranging from 5% to 100% of T_g may be used.[79] Such sub-time-steps are still too long. The problem is a sampling problem that can be described as follows. Sub-time-steps as long as a year (=10% of 10 yr) or half a year (=5% of 10 yr) are unlikely to provide correct representation of intense storm events.

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[75] G.E. Tucker, pers. comm., May-June 2017.

[76] Note that the term "intensity" in the widely used phrase "intensity-frequency distribution" corresponds to rainfall "depth" – particularly the "depth" of 24-hour rainfall with a certain recurrence interval or probability – in the terminology of Tucker et al.

[77] For overview, see R.C. Vaughan, 6-28-17 CTF presentation, slide 10; also R.C. Vaughan, 9-27-17 CTF update presentation, slides 3 and 6-7.

[78] DOE and NYSERDA, *Final Environmental Impact Statement for Decommissioning and/or Long-Term Stewardship at the West Valley Demonstration Project and Western New York Nuclear Service Center*, DOE/EIS-0226 (2010), Appendix F, page F-29.

[79] Tucker et al., op. cit., at 60 and Table 5.2, where n_{ts} = 20 corresponds to 5%, etc.

For the sake of illustration, assume that T_g = 10 yr and that the sub-time-step is one year, and consider a 100-year storm, meaning a 24-hour rainfall event with a return period of 100 years. Such a storm will occur or recur, on average, only one day in every 36,500 days (=100 x 365 days). Thus, on a given day, the probability of such a storm is 1/36500. If an erosion model used 1-day time steps and treated rainfall as an independent parameter, each day's rainfall could be chosen at random from a bowl of 36,500 slips of paper, only one of which would have "100-year storm" written on it. (Each slip, after being chosen, would be returned to the bowl so that it could be chosen again at random on a later day.) A model that ran for 1000 years would have a total of 365,000 daily drawings from the bowl (=1000 x 365 days), and the slip for "100-year storm" would be chosen, on average, ten times during the 1000-year modeling period. A model that ran for 10,000 years would have a total of 3.65 million daily drawings from the bowl (=10,000 x 365 days), and the slip for "100-year storm" would be chosen, on average, a hundred times during the 10,000-year modeling period. Such models would realistically represent 100-year storms, assuming that the number of inches of rain in a 100-year storm was properly determined and appropriately adjusted for both climate change and paleoclimate. However, the EWG erosion models don't work this way. In these models, rainfall would effectively be chosen at random from a bowl of 36,500 slips of paper, only one of which would have "100-year storm" written on it, but slips would be chosen yearly (in accordance with the sub-time-step) rather than daily. Such a model that ran for 1000 years would have a total of 1000 yearly drawings from the bowl, and the slip for "100-year storm" would be unlikely to be chosen during the 1000-year modeling period. Such a model that ran for 10,000 years would have a total of 10,000 yearly drawings from the bowl, and there would be a less-than-even chance that the slip for "100-year storm" would be chosen at all during the 10,000-year modeling period. During hundreds or thousands of independently random modeling runs this sampling problem could be largely resolved, but the EWG modeling calibration runs are *not* independently random (the random seed is held constant in calibration[80]), and other problems remain unresolved as well. One of these other problems is that the best-performing EWG erosion models don't treat rainfall as an independent parameter, as discussed elsewhere in these comments. It's also not clear how stochastic EWG models that used a 1-year sub-time-step and treated rainfall as an independent parameter would deal with the situation in which the "100-year storm" slip is drawn from the bowl. *It's clear that the models would deliver a rainfall of about 5 inches* (this has generally been considered to be the 100-year storm) *to the modeled Franks Creek watershed every day for about 182 consecutive days*. This describes how the "100-year-storm" daily rainfall intensity would be drawn from a Weibull distribution, and how the models would hold this intensity constant for a fraction F (equal to about 0.5) of a 1-year sub-time-step.[81] Based on the August 2009 storm that delivered about 5 inches of rain, it is reasonable to presume that half a year of such storms, continuing day after day for 182 days, would severely erode both the real Franks Creek watershed and the modeled Franks Creek watershed. How the model would handle this – and whether any such result would simply be rejected by the modelers as an impossible outlier – are unclear. In sum, these are the main issues involved in the models' unrealistic time steps.

[80] Tucker et al., op. cit., at 95.
[81] Id. at 60.

And it should be noted that the above example of 36,500 slips of paper in a bowl will suffice for storms with average recurrence intervals of 100 years or less – but more intense storms with longer recurrence intervals also need to be considered for 1000- and 10,000-year model runs, involving more slips of paper in the bowl. The unresolved sampling and other issues outlined above would become increasingly problematic for such larger storms with longer recurrence intervals.

VSC.68. In summary, any and all such modeling runs need to have *recognizable* rainfall intensity-frequency distributions. Independent experts and the public must be able to review the rainfall intensity-frequency distributions, and must be able to compare them to realistic current rainfall distributions and to defensible estimates of paleo (post-glacial) and future (climate-change-adjusted) rainfall distributions.[82]

VSC.69. The various erosion modeling runs employ *other* input parameters in addition to their direct or indirect rainfall-distribution parameters. These other input parameters must likewise be reviewable, such that independent experts and the public can compare them to realistic field-tested or field-testable parameters. If any of these other parameters are not directly recognizable and field-testable, independent experts and members of the public will need to "translate" such parameters into quantitative measures that *can* be checked against reality.[83] Doing so will take time and require access to the computer code. Followup discussion with the EWG erosion modelers may also be needed, and they should remain available for this purpose.

VEC.220. In general, the FEIS prevents readers from comparing its climate assumptions to current climate data, and likewise prohibits readers from comparing its erosion-model calibration to current erosion data. Its climate assumptions, particularly its rainfall intensity-frequency relationship, are merely concealed but presumably could (and should) be provided. I continue to request them. The FEIS seems to say that its erosion-model calibration is outside the realm of testable verification, which I disagree with as a matter of scientific principle. Both of these issues are reviewed below in more detail.

VEC.221. The FEIS provides no traceable response to my comment 172 about the current climate assumptions used for erosion modeling. Response 110-103 indicates that the rainfall intensity-frequency relationship used in the erosion model was changed between the DEIS and the FEIS. Responses 110-103 and 110-104 provide apparently contradictory descriptions of the intensity-frequency relationship that is now being used (either precipitation statistics from 5-minute precipitation data at the site, or a probabilistic approach that is apparently built into the model), but the FEIS provides neither the statistics nor a description of the probabilistic approach. For example, there is no description of the rainfall intensity-frequency relationship, or of how it might be calculated or constructed, in several places where it would be expected, such as response 110- 103, response 110-104, and/or Appendix F of the FEIS. I request this information (and/or a meeting or technical workshop session at which it is presented and compared to the site's current rainfall intensity-frequency relationship). Please note that I am interested in the intensity-frequency distribution but not necessarily a probable maximum

[82] Regarding paleo and future rainfall distributions, see Vaughan EIS comments §§ 166-71.
[83] Vaughan, 9-27-17 CTF update presentation, slide 8.

precipitation (PMP) value. I recognize that an appropriate intensity-frequency relationship can provide essentially the same information as a PMP-bounded approach. "Essentially the same" means that the asymptotic curve of the intensity-frequency relationship for an unbounded probabilistic model would be reasonably close to the x- or y-intercept that represents the PMP.

VEC.222. The FEIS provides no traceable response to my comments 169-171 about the future climate assumptions used for erosion modeling. Response 110-101 states that the mean annual precipitation is doubled for some of the model runs, but this is uninformative in the absence of information on the intensity-frequency distribution used for the model runs. See comment 221 above. Erosion is particularly sensitive to extreme weather events, as pointed out in my comments 169-171, so information about the mean must be supplemented with the distribution. I request this information (and/or a meeting or technical workshop session at which it is presented and compared to some of the predicted future rainfall intensity-frequency relationships).

VEC.223. The FEIS effectively dismisses my comments 166-168 about the paleoclimate assumptions used for model calibration by a) acknowledging the "uncertainty associated with assuming uniform paleoclimate conditions" and b) assuming them anyway. Given the model's complete dependence on these paleoclimate assumptions for purposes of calibration, there is a severe risk of "garbage in, garbage out" – hence the need for testable verification of erosion rates against current erosion observations. The precaution offered in the FEIS for guarding against model miscalibration (the "wet" scenario described in response 110-101, which changes the mean annual precipitation) does nothing to avoid the calibration error caused by ignoring well-documented swings in paleoclimate that occurred over periods of centuries and millennia.

VSC.94. It is not that clear that the sensitivity analyses for the EWG erosion modeling runs cover the range of rainfall rates (including a cessation or at least a greatly reduced rate of rainfall) for the period between about 10,000 and 6000 years before present when Buttermilk Creek downcutting was minimal.[84] While there may be other explanations for this period of minimal downcutting, one such explanation would be a prolonged "paleo drought" (a near-absence of rainfall) during the 4000-year period.[85] Sensitivity analyses showing the sensitivity of EWG model results to the rainfall assumed during calibration runs for this 4000-year period – including results for the limiting case in which no rainfall occurs in any time step during this period – must be provided. These sensitivity results must also be appropriately incorporated into PPA model runs.

VSC.95. The recently released EWG erosion modeling report says that the results of the EWG sensitivity tests "showed that climate-driven variation over time in the erodibility coefficient has only a small influence on the model's output…"[86] This is not a credible conclusion because

[84] Tucker, QPM presentation, op. cit., slide 6, does not show such a sensitivity analysis. As discussed elsewhere in these comments, it's clear that the EWG erosion modeling (as reported by G.E. Tucker et al., op. cit. [2018]) does not treat paleoclimate rainfall variation as an independent parameter and thus provides no clear and direct basis for understanding its effect on model results (or conversely, for understanding the sensitivity of model results to paleoclimate variation).
[85] Or, for example, a prolonged drizzle-dominated period with little or no storminess.
[86] Tucker et al., op. cit. [2018], at 96.

Tucker and the other EWG modelers didn't directly use rainfall as a time-varying input parameter in their models, nor did they rely on any paleoclimate research[87] such as the work reviewed and presented by Wilson and Young.[88] Instead of looking at paleoclimate research, Tucker and the other EWG modelers consulted a long-period climate model simulation known as TRACE21ka. And instead of using rainfall as a time-varying input parameter, they varied the erodibility coefficient for their model-calibration runs. Based on the results, they concluded[89] that paleoclimate variations didn't make much difference and that "the calibration procedure assumed a steady climate over the calibration period." This approach begs the question. It fails to treat paleoclimate as an independent parameter, assumes that it can be represented by a different parameter (the erosion factor or erodibility coefficient which, for example, fails to distinguish between U-shaped and V-shaped valleys), and concludes based on this circular logic that paleoclimate variations don't make much difference and that the calibration model runs could simply assume a steady climate over the calibration period.

VSC.96. The EWG erosion modeling report explains that the EWG erosion model-calibration runs employed the erodibility coefficient as a proxy or surrogate for paleoclimate variation through time, such that the erodibility coefficient was allowed "to either increase or decrease over time, reaching a stable value after a specified period of time has elapsed…" Specifically, "the erodibility coefficient was set to stabilize at 5,000 years into the model run, representing 8,000 years ago," where 8,000 years ago was the time at which the TRACE21ka climate model predicted that "the annual precipitation rate and its apportionment among various forms became approximately steady…"[90] This allowed erodibility to vary by an adjustment factor of 50% to 150%, *but only during the first 5,000 years of a 13,000-year model-calibration run.*

VSC.97. Unresolved issues in the EWG erosion modeling report[91] include a lack of clarity on the quantitative proxy relationship between rainfall and erodibility coefficient (including how the known non-linearity between precipitation and erosion is handled) and the resulting sensitivity relationship between these two variables. Other such issues include a) the disregard for paleoclimate research, some of which was specifically presented by Wilson and Young, and the substitution of a presumably easier-to-plug-in climate model; b) the uncritical acceptance of the climate model's paleoclimate stabilization 8,000 years ago, contrary to what paleoclimate research has shown; and c) the loss of realism and field-testability when rainfall and erodibility coefficient are rolled into one rather than being independently variable input parameters.

VSC.98. Differences between paleoclimate research and TRACE21ka climate model results – or between different paleoclimate research studies – shouldn't necessarily be decided in favor of one over the other. But by using only the TRACE21ka climate model results, the EWG erosion model runs have arbitrarily truncated the range of input variability and thus failed to demonstrate the sensitivity of model results to this type of input. This is a microcosm of the problem (arbitrary choice of deterministic values) that PPA modeling is meant to overcome – and if such

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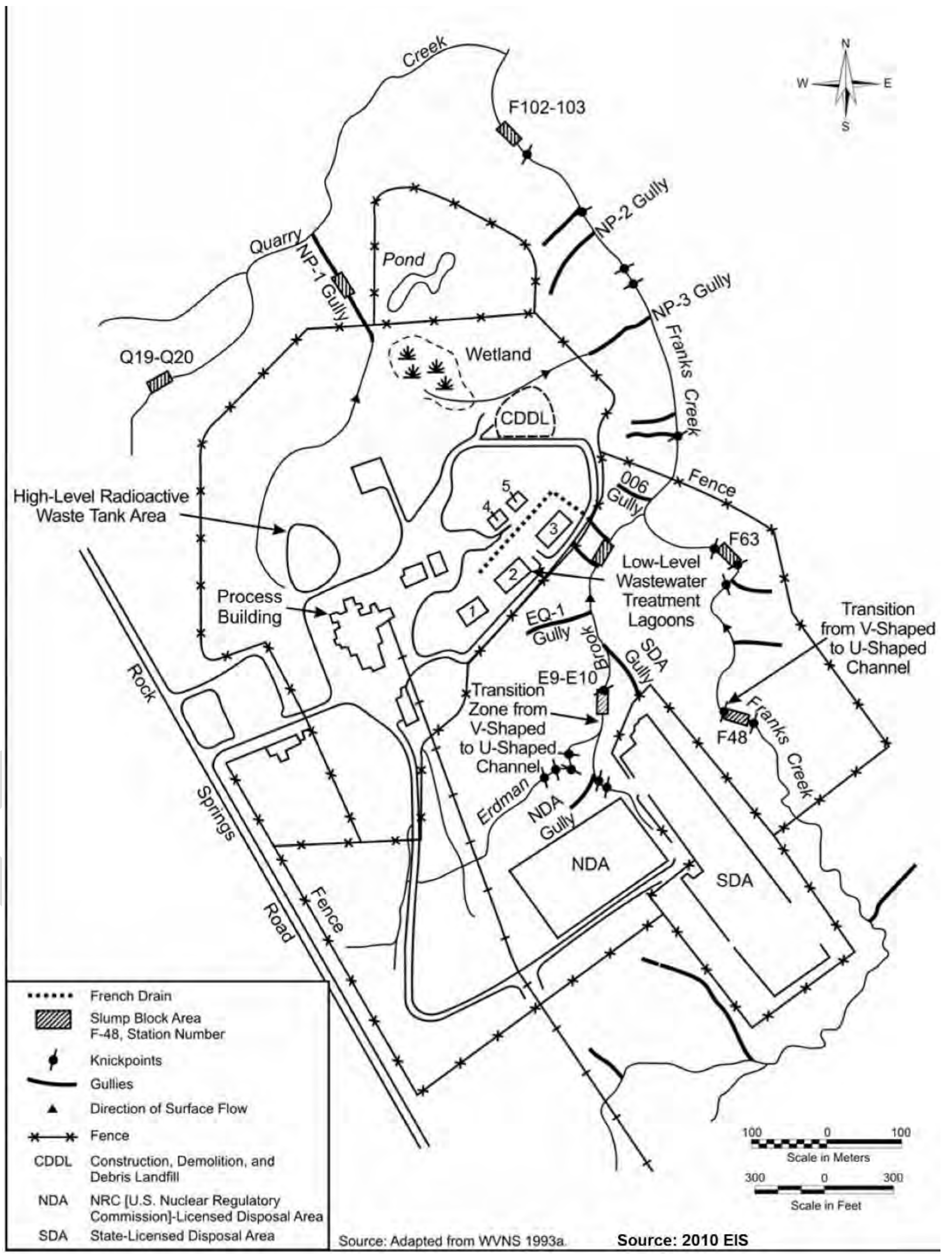
[87] Id..
[88] Wilson and Young, op. cit., Vol. 1, § 4.9 ("Paleoclimate Factors") at 79-83.
[89] Tucker et al., op. cit., at 96.
[90] Id.
[91] Id.

a problem can't be overcome in the current EWG erosion modeling which provides such major guidance to the PPA modeling that will support Phase 2 decisionmaking, then the PPA modeling shouldn't be considered trustworthy.

VSC.99. Genuine uncertainties in numerical values that represent paleoclimate need to be handled probabilistically in a robust and transparent manner. While this should go without saying in PPA modeling, it is also an important point in the EWG erosion modeling that will guide the PPA modeling. Specifically, EWG erosion model results based on erroneous or unsupported paleoclimate inputs should not be accepted as inputs into PPA modeling.

TASK 10: Assess how slumping/landsliding are represented and incorporated

Context: Slumping is pervasive throughout the Cattaraugus Creek basin, including the West Valley site where the most dramatic slump, with a vertical height of about 160 ft, is on the west-bank slope of Buttermilk Creek about 1500 ft ENE of the SDA burial ground. The figure below shows some of the smaller slump blocks that are closer to the SDA, NDA, and underground waste tanks.



VSC.82. Slumping and landsliding are well-known modes of mass wasting (slope failure) that are widely observed in the Cattaraugus Creek basin, including well-known examples on Buttermilk Creek and elsewhere on the West Valley site. While slumping and landsliding may occasionally be accelerated by earthquakes, they usually operate as continual (sporadic) aseismic

processes in which masses of soil or glacial fill move downslope, thus modifying the landscape[92] in concert with erosional processes such as plucking and abrasion. The masses of soil, typically but not always in the form of discrete blocks, move downslope due to gravity in combination with surface water that undercuts the toe of the slope and/or groundwater that seeps toward the face of the slope from behind. *The EWG erosion models treat slumping/landsliding as a form of creep,*[93] *but this is unrealistic for the typical form of slumping/landsliding found on the West Valley site and throughout the Cattaraugus watershed*. Creep involves plastic deformation with little or no loss of cohesion, while slumping/landsliding typically involves detachment (loss of cohesion) along a quasi-planar or rotational failure surface. As a result, slumping or landsliding usually opens up a water pathway behind any block that has slid downward. Creep doesn't do this. Thus, the approximation of using creep as a proxy for slumping/landsliding (without having established an onsite correlation) is unrealistic and adds to the uncertainty of the modeled results.

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[92] M.P. Wilson & R.A. Young, Phase 1 Erosion Studies, *Study 1 - Terrain Analysis*, Final Report, Vol. I (Feb. 2018), p. 79.
[93] Tucker et al., op. cit., pp. 34-35, 56-58.

<u>TASK 11</u>: Assess the assumption that South Plateau is a largely undisturbed post-glacial surface, taking into consideration its varied surficial composition (till, sand lens(es), peat, and 400-year-old fill in the vicinity of Erdman knickpoint)

Context: The assumption has been made that the South Plateau's surficial and near-surface geology is till and that its surface elevation has not changed much during the post-glacial period of about 13,000 years.[94] In fact, the South Plateau's surficial and near-surface geology is partly till, partly sand lens (found a few decades ago to be affecting SDA trench 14), partly 400-year-old fill (as detected after the August 2009 storm), and partly peat (recently found to be affecting SDA trenches).

The landscape evolution models (SIBERIA, CHILD, and EWG) rely on this faulty assumption and are thus miscalibrated. Specifically, these models are calibrated by 13,000-year paleo runs that end at the present time (and that "predict" the present land surface if well-calibrated), and the models are then run 10,000 years into the future to predict future erosion. One of the major goals is to see whether future erosion will breach the South Plateau at or near the SDA burial ground. The fault in this logic is that such breaching already occurred a few hundred years ago. This was detected along the north slope of the SDA after the August 2009 storm, when radiocarbon dating showed that Erdman Brook is downcutting into fill that was emplaced about 400 years ago rather than glacial till – but this is not recognized in the erosion modeling. The EWG models, by not recognizing either this Erdman downcutting or the post-glacial period (about 12,600 years) during which it was achieved, are miscalibrated.

This miscalibration is roughly analogous to the miscalibration described in Task 4, where the paleo period of active erosion is misjudged as 13,000 years rather than the 9000-year period indicated by the evidence for a 4000-year paleo drought. It may be appropriate to apply a single correction to these two miscalibrations, thereby correcting the paleo period of active erosion from 13,000 years to 8600 years. But have there been *other* deviations, perhaps climate-driven, from the erosional/depositional sequence assumed for the South Plateau in the LEM? If so, the paleo period of active erosion may need to be shortened to *less* than 8600 years. The following paragraphs from West Valley EIS scoping comments, while not directed to quite the same geomorphic process or soil type, provide an example of *cyclical* erosion and deposition on time scales measured in centuries. Whether this applies to the South Plateau may be better understood after its peat beds are dated by C-14 or other method. Mapping of both the South Plateau peat and the 400-year-old fill near the Erdman knickpoint – thus delineating each of their contacts with the underlying till – would also be useful.

And to the extent that there are choices to be made for either paleo drought or the downcutting of Erdman Brook, these should be defensibly weighted and incorporated into a probabilistic "period-of-active-paleo-erosion" distribution. Task 4 provides guidance on such weighting.

VEC.178. …See, for example, studies of gullying in western Iowa as reported by Bradford and Piest ("Gully Wall Stability in Loess-Derived Alluvium," *Soil Science Society of America Journal* **41**, 115 (1977)) and by Bettis in the following article entitled "Gully Erosion," taken

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[94] For example, Tucker et al., op. cit., 81.

from the Iowa Department of Natural Resources/Geological Survey website at www.igsb.uiowa.edu/Browse/gullyero/gullyero.htm. The Bettis article, quoted below [in part], is based on work done jointly by Bettis and Dean Thompson (Natural Resources Conservation Service) and is adapted from *Iowa Geology 1983*, No. 8, published by the Iowa Department of Natural Resources:

> Western Iowa, a 10,811 square mile area encompassing all of thirteen and portions of nine other counties, has a national reputation for high sediment loads in streams and severe gully erosion problems. Estimates indicate that 5,000 to 10,000 acres of potential cropland are lost or removed from production annually as a result of gully growth in this region. Large amounts of time and money are spent on maintaining drainage ditches and stream channels which become choked with sediment eroded from gullies. Bridge failures resulting from gully widening are also a common and costly problem for counties in western Iowa. Numerous other problems directly or indirectly associated with the growth of gullies plague residents of this region.…
>
> Occasionally, buried tree stumps, logs, or charcoal are found enclosed in these old alluvial fills where they are exposed along modern gully walls. These organic remains have been radiocarbon-dated and a chronology of gully cutting and filling constructed. More than 100 such radiocarbon dates indicate that the six major alluvial fills recognized in western Iowa valleys represent regionally synchronous episodes of gully cutting and filling during the last 12,000 years. Four of these episodes occurred during the last 4,000 years, and the deposits associated with them are rather well preserved and understood.
>
> About 3,500 to 4,000 years ago, deep gullies much larger than today's dominated the landscape in small western Iowa valleys. In many cases these gullies occupied the entire valley floor. Beginning shortly after 3,500 and continuing until about 2,000 years ago, gully growth stopped and alluvium accumulated in the gullies. By 2,000 years ago the gullied areas were completely filled with silty sediment washed from the adjacent valley slopes, and marshy areas occupied the central portion of the former gullied areas.
>
> Sometime during the 200-year period between 2,000 and 1,800 years ago another gully cycle began. Gullies extended up all moderate-sized valleys and some of their lateral tributaries. Gullying did not extend into small drainages at the upper end of the drainage network as it had during the previous cycle. In extent, depth, and width of gullying, this cycle is analogous to modern gullying in the area. Shortly after 1,800 years ago alluvium again began to accumulate in the gullies, eventually filling them by about 1,000 years ago.
>
> The third gullying cycle began about 800 years ago. In this cycle, gullying was restricted to moderate-sized and larger valleys and did not extend as far up valleys or into smaller valleys as it had during either of the previous episodes. These new gullies were restricted to central portions of the area gullied during the previous cycle. Further, these gullies were not as deep or as wide as earlier gullies had been. Shortly after the gullies developed they began to fill with alluvium. Sediment accumulated until the gullies were completely filled and portions of the surfaces bordering the gullies were buried a few feet. Counts of

growth rings in trees growing on alluvium filling these gullies indicate that sedimentation may have continued until about 100 years ago.

The most recent western Iowa gully cycle began around 100 years ago. Numerous accounts in local histories, original land surveys and early reports of the Iowa Geological Survey indicate that until about 1860 gullies were not widespread in the area. By 1900 reports of problems arising from gully growth, such as the need for bridges at crossings, became common and indicated that the historic period of gully growth was in full swing. In some valleys, gullies have formed and been filled several times during this historic cycle, a process which also occurred during the prehistoric episodes but is too obscure to be interpreted from the geologic alluvial-fill record.

The geologic record contained in western Iowa valleys shows that major gullying is not new to the area. Several episodes, some more widespread than that which affects the area today, occurred prior to Euroamerican settlement and the spread of modern agriculture. Gullying is part of the natural process of landscape evolution in western Iowa. The modern gullying which causes so much concern is also part of this natural process. No doubt, landuse changes accompanying the spread of agriculture and urbanization have aggravated and possibly accelerated the growth and extension of gullies in western Iowa. However, the geologic record suggests that the area was “due” for an episode of gullying prior to the 1850s. Gullies grew and filled several times in the past when humans were not significantly influencing runoff or vegetation patterns. This indicates that human activity affects gullies in this area but does not cause them.

Recognition of the fact that gullies are “native” to western Iowa is important because it indicates that gullies are not a unique phenomena resulting entirely from human modification of the landscape. Through recognition of gully-prone valley sections and the promotion of landuse aimed at preventing or lessening the factors causing gullies in those areas, we can avoid gully growth or lessen its impacts. During the last 12,000 years, gullies and the erosion resulting from their growth have molded the western Iowa landscape into that which we see today. This process is active and will continue to be so far into the future. Currently our knowledge of the factors contributing to gully initiation is very incomplete. Somewhat better understood are the processes and factors involved in gully growth and degradation. These are areas of urgent research needs. Through a better understanding of the processes affecting gully growth and filling, we can lessen the impact our activities have in promoting the gully problem and plan around those portions of the gully network which are too costly or not likely to be controlled.

VEC.179. Despite the differences between the unconsolidated material found in western Iowa (loess) and at the West Valley site (till with intervening beds of recessional sands and gravels), the above-quoted article by Bettis offers important lessons for the understanding of gully advancement at the West Valley site. First, the Bettis article underscores the importance of field work, radiocarbon dating, etc. Gross oversimplifications such as “an initial period of rapid growth followed by decrease in rate of growth, attainment of a maximum length” are no substitute for careful field work. Second, the Bettis article shows that gullies in western Iowa have *not* undergone a continual progressive advance during the post-glacial period of the past 12,000 years. Advancement has been cyclical, interspersed with periods of non-advance and infilling with sediment, on time scales measured in centuries. These long-term cycles in which gully advancement waxed and waned, probably in response to climate variation on a regional or continental scale, may likewise have occurred at the West Valley site. The question needs to be addressed at the West Valley site, partly as a check on the realism and calibration of any erosion model that simulates gully advancement. Third, the Bettis article emphasizes the wide variety of observed gully sizes ("Some gullies are several miles long while others are as short as 100 feet") and indicates that gully size is partly correlated with the aforementioned long-term cycles ("About 3,500 to 4,000 years ago, deep gullies much larger than today's dominated the landscape in small western Iowa valleys"). This observation raises the question of whether "deep gullies much larger than today's" will form in the future at the West Valley site in response to climate conditions similar to those that formed the deep Iowa gullies about 3,500 to 4,000 years ago. This question needs to be addressed. Without a dependable answer, there's no reasonable way to assess the effects of gully advancement on site integrity.

TASK 12: Assess the LEM models' assumed base level and its relationship to glacial rebound and available downstream drainage pathways

Context: The EWG erosion modeling by Tucker and Doty covers the Franks Creek watershed which has its base level at the Franks-Buttermilk confluence (currently about 1181 ft elevation[95]) but does not extend as far downstream as the Buttermilk-Cattaraugus confluence (currently about 1100 ft). Wilson and Young, in their EWG characterization, emphasize that downstream elevations govern upstream elevations in the Cattaraugus/Buttermilk/Franks hierarchy when they speak of:

- "a starting point from which to accurately infer the postglacial incision history of the Buttermilk Creek basin, and therefore the base level lowering history for Franks Creek at its confluence with Buttermilk Creek."[96]
- "Buttermilk's main-stem channel [being] adjusted to Cattaraugus Creek terrace levels as evidenced by Buttermilk terraces gradually fusing with Cattaraugus Creek terraces. Buttermilk Creek could not incise more deeply than Cattaraugus Creek."[97]
- The fact that the shale-sandstone bed of their "zone 6" (Zoar Valley) – where top of bedrock reaches 1320 ft between the Waterman Brook confluence and South Branch confluence – "was the control for initiation of incision of Cattaraugus Creek below the Buttermilk confluence."[98] Accordingly, such elevations "were initially [postglacially] controlled by the top-of-bedrock at 1320 feet downstream at the shoulder of Zoar Valley near Gowanda, NY. The Franks Creek/Buttermilk Creek confluence elevation was approximately 1340 ± 5 feet."[99]

It's difficult to tell from the available documentation whether the paleo and predicted base levels used by Tucker and Doty in their EWG erosion modeling are consistent with Wilson and Young. Logically, the paleo base levels (i.e., elevation of Franks-Buttermilk confluence during the past 13,000 years) would be defined by Wilson and Young's Fig. ES-1[100] where the vertical-axis values ("Elevation Relative to Modern Channel") are to be added to the current 1181-ft elevation of the confluence.[101] But Tucker and Doty's documentation contradicts this by saying, for the paleo calibration period, that:

> We selected the Franks Creek watershed because the majority of the WNYNSC facilities are located within it and data are available near its outlet for constraining its post-glacial downcutting history (Wilson and Young, 2018). We placed the watershed outlet at the junction of Franks Creek and Quarry Creek instead of the junction of Franks Creek and Buttermilk Creek. This reduction in watershed extent, to include only those parts that

[95] Tucker et al., op. cit., at 145.
[96] M.P. Wilson and R.A. Young, *Phase 1 Erosion Studies, EWG Study 1 – Terrain Analysis*, Final Report (Feb. 2018), Vol. 1, at 54.
[97] Id. at 36.
[98] Id. at 37.
[99] Id. at 36.
[100] Id. at v or 116; reproduced in Tucker et al., op. cit., Fig. 4.10.
[101] Tucker et al., op. cit., at 10 and 51-55; also Fig. 6.4.

contain streams impinging on the Site, has the benefit of substantially decreasing model computation time.[102]

Taken literally, this says that Tucker and Doty used the Franks Creek watershed for their calibration runs, and that their model drain ("outlet") wasn't at the lowest topographic elevation but, instead, was at the Quarry-Franks confluence. This makes no sense, so they probably mean that they didn't use the Franks Creek watershed for their calibration runs but, instead, used only the upper part of the Franks watershed (upstream from the Quarry-Franks confluence). If the latter, then there is an apparent mismatch between the modeled watersheds they used for calibration and prediction (partial Franks watershed for calibration runs, full Franks watershed for prediction runs). If true, this is a questionable modeling practice. In part, it leaves the longitudinal profile of lower Franks Creek uncalibrated for the erosion-prediction runs that include lower Franks Creek.

Future base levels (i.e., elevation of the Franks-Buttermilk confluence for the EWG models that run 10,000 years to predict future erosion) are likewise not clearly stated in the documentation but are apparently defined by the S1, S2, and S3 scenarios presented by Tucker and Doty on page 146 and in Fig. 11.1. The figure is said to show "time-versus-elevation plots of the river outlet," evidently referring to the Buttermilk-Cattaraugus confluence as the "river outlet." Here again, vertical-axis values are "Elevation Relative to Modern Channel."[103] These values are apparently shifted upstream by Tucker and Doty, from the Buttermilk-Cattaraugus confluence to the Franks-Buttermilk confluence.[104]

Assuming, despite the foregoing contradiction, that 1) the full Franks Creek watershed is used for all calibration runs and prediction runs, and 2) base levels remain consistently at the Franks-Buttermilk confluence and are tied to the downcutting plots (Wilson and Young Fig. ES-1, Tucker and Doty Fig. 11.1), then the methodology – if actually used by Tucker and Doty – has merit. Using these base-level values doesn't require a full understanding of the longitudinal-profile histories for Cattaraugus and Buttermilk but appears well-supported for the Franks outlet.[105]

Note that Tucker and Doty's S1, S2, and S3 scenarios require future downcutting of Cattaraugus Creek. This isn't directly addressed but appears reasonable.

Where Tucker and Doty go clearly wrong is not in the base levels *per se* but in their erroneous assumption that the prolonged paleo base level of about 1280 ft elevation (essentially constant

[102] Id. at 41.
[103] Tucker et al., op. cit., at 146.
[104] Id. at 54 describes their analogous shift from the abandoned meander to the Franks-Buttermilk confluence, done for purposes of their paleo base levels. Over relatively short distances, such shifts appear reasonable.
[105] And note that Wilson and Young's work, particularly as summarized in their Fig. ES-1, helps constrain the longitudinal-profile histories for both Cattaraugus and Buttermilk. For purposes of the EWG work they consider differential glacial rebound to be minimal (see their page 86; compare Newman et al. 1981), which is reasonable if the differential rebound is on the order of 1 to 2 feet per mile (see Holcombe et al. 2003 and Tarr 1896) but would need to be applied to Cattaraugus Creek, for example, from its Buttermilk confluence to its mouth.

for 4000 years, from about 10,000 to 6000 years ago) was due to erosion-resistant bedrock rather than a prolonged paleo drought. See Task 4.

VCT.[3]. … Any modeling also needs to ensure, in accordance with principles of mass balance, that a reasonable sink exists for water discharged from the model outlet. While this is not likely to impose a substantial constraint on modeling, and certainly would not be a constraint under today's drainage conditions where the Atlantic Ocean is the sink for water discharged from Cattaraugus, Buttermilk, and Franks Creeks, modelers should be aware that Holcombe et al. (2003) consider Lake Erie to have been a closed basin during part of the postglacial period. At times when the lake is considered a closed basin, the discharge flow rate of water from the outlet of a landscape evolution model should not be a disproportionate share of the flow that could reasonably be accepted by the closed lake basin….

<u>TASK 13</u>: Assess whether stream capture of Franks Creek by piping/sapping has been evaluated

Context: Tucker and Doty provide some evaluation of Franks Creek capture or piracy.[106] The adequacy of their evaluation should be assessed, with particular attention to Franks Creek downcutting that brings its streambed onto the permeable Kent Recessional. This will tend to divert part of the surface-water flow from Franks Creek into an underground pathway through the Kent Recessional which will re-emerge from the west bank of Buttermilk Creek as surface flow. Such flow through an underground pathway may become a self-enlarging flow process of piping and sapping that progressively destabilizes the overlying glacial fill and soil – either adjacent to the South Plateau or further downstream on Franks Creek. Such a process will tend to occur sooner at downstream locations and may already be occurring there, which may offer guidance on the potential for piping/sapping to contribute to stream capture. Work done by Tucker and Doty has already shown the effects of timing and may also help to identify locations most sensitive to capture by piping/sapping.

VSC.110. The work done on stream capture in the EWG erosion modeling report is rudimentary[107] and thus unable to provide a realistic assessment of the likelihood of stream capture.

VSC.111. Stream capture, including stream capture initiated by seepage and piping, needs to be characterized and incorporated into any erosion modeling that will support the Phase 2 decision.[108]

VEC.187. As indicated above in comments 92, 95, and 96, the phenomenon of stream capture or stream piracy is a serious issue at the site. Over some period of time, the capture or piracy of Franks Creek by Buttermilk Creek is likely to occur somewhere near the current confluence of Franks and Erdman. The question is *when*. A likely contributing factor in this piracy/capture process is the relatively permeable layer of Kent recessional/lacustrine/overbank deposits that lies beneath the Lavery Till. This layer can act as an essentially horizontal conduit for groundwater transmission (e.g., see 2008 Draft EIS, page E-15). The importance of such a groundwater pathway (and the reason for the emphasis given to it in comment 92) is illustrated by D.T. Pederson, "Stream Piracy Revisited: A Groundwater-Sapping Solution," *GSA Today*, September 2001, page 7 and esp. Fig. 3. As Pederson explains, "The principal fact favoring stream piracy by groundwater sapping is that the groundwater divide does not correspond to the surface-water divide when there is a difference in elevation of streams in the adjacent drainages..." As he shows, the higher-elevation stream (in this case, Franks Creek) tends to provide some of the groundwater flow needed to sustain gully growth toward the drainage divide from the lower-elevation stream (in this case, Buttermilk Creek). Such a process eventually breaches the surface-water divide, causing the so-called piracy which gives the higher-elevation stream (Franks) a comparatively short, steep channel into the lower-elevation stream (Buttermilk). Such a result would be disastrous at the West Valley site, since the already-steep gradient of Franks Creek would suddenly become much steeper in the immediate vicinity of the

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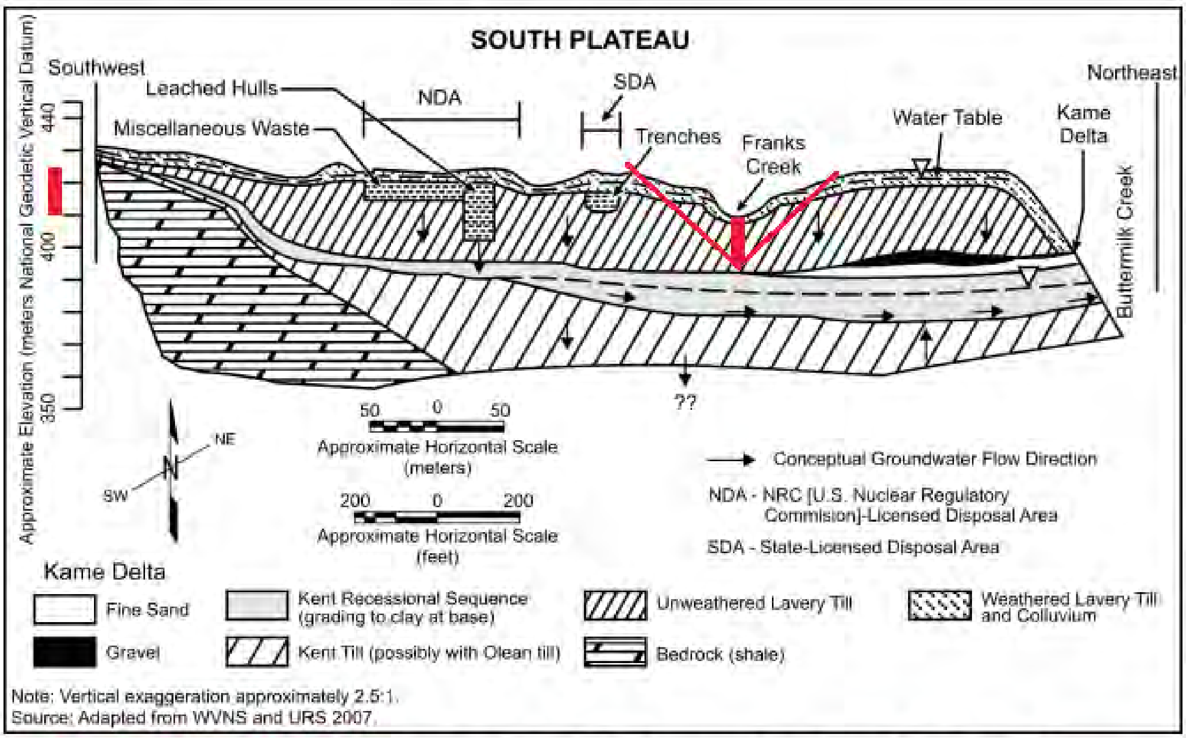
[106] Tucker et al., op. cit., at 462-465.
[107] Id.
[108] Vaughan EIS comments §§ 187-88.

North and South Plateaus, thereby accelerating further downcutting. Given the importance of this process, it should be incorporated into any erosion model or erosion-prediction method for the West Valley site. The erosion model used in the 2008 Draft EIS does not appear to incorporate or address stream piracy or capture – but it must do so. An erosion model that fails to include this process cannot be considered realistic.



Adapted from 2010 EIS, **Figure E–6 Geologic Cross-section through the South Plateau**

Vertical red bar shows 50 foot (15 m) Franks Creek downcutting alongside SDA at about 42.4495°, -78.6490°, more or less as predicted by EWG erosion modeling by Tucker & Doty.

Diagonal red lines show 21° stable slope angle, adjusted here to match vertical exaggeration.

This figure is approximate and illustrative, not exact.

Fig. xx: Cross-section of South Plateau, adapted to show how predicted Franks Creek downcutting approaches the point at which its streambed becomes the permeable Kent Recessional.

VEC.188. Two important factors in stream piracy or capture are the length and permeability of the groundwater pathway between the higher and lower streams. Permeability will be enhanced, promoting greater groundwater flow, as Franks Creek downcuts into and thereby increases its communication with the Kent recessional/lacustrine/overbank deposits beneath the Lavery Till. The length of the groundwater pathway will be reduced, again favoring greater flow, as the actively slumping west bank of Buttermilk Creek migrates westward in response to slope instability, as outlined above in comment 186. Separately or in combination, both of these ongoing processes will tend to hasten the process of stream piracy or capture.

TASK 14: Assess LEM equations and algorithms

Context: No prior detailed comments on equations and algorithms; this is new work in 2022.

Task 14A: For long time periods such as 10,000 years, do probabilistic methods of random sampling from a given rainfall frequency-intensity distribution introduce substantial bias relative to the given distribution – such that the distribution itself can and should be used directly (non-probabilistically) in order to avoid or substantially reduce such bias?

The duration of a 10,000-year LEM run is 3.65 million days. For a given rainfall frequency-intensity distribution, the probability on any given day of a given 24-hour rainfall depth is known; hence, *on average* during the 3.65-million-day model run, there must be:

- 90 storms of at least 100-year and no more than 1000-year intensity;
- 9 storms of at least 1000-year and no more than 10,000-year intensity;
- 1 storm of at least 10,000-year intensity;
- etc.

as already noted in Task 2. In other words, there must be *on average* during the 3.65-million-day model run:

- 90 days of at least 100-year-rainfall and no more than 1000-year-rainfall depth;
- 9 days of at least 1000-year-rainfall and no more than 10,000-year-rainfall depth;
- 1 day of at least 10,000-year-rainfall depth;
- etc.

For any one of their model runs, Tucker and Doty sample up to 2000 (or as few as 100) 24-hour rainfall depths from their given rainfall frequency-intensity distribution.[109] A robust alternative method would be to use the above-listed "on-average" values directly. This would be robust because of the large number of days (3.65 million) in each of the model runs. What could change probabilistically in this alternative method is the *sequence* of dry days, wet days, very wet days, etc.

As a test of Tucker and Doty's sampling method, it would be relatively easy and straightforward to use their sampling method repeatedly (*e.g.*, millions of times, using Fortran), taking up to 2000 (or as few as 100) samples each time and adjusting as needed for the wet-day fraction, to see whether the accumulated results either match or under- or over-predict the known current rainfall frequency-intensity distribution for the West Valley site. In this comparison, the "known current rainfall frequency-intensity distribution for the West Valley site" would be, for example, NOAA's Atlas 14 or online Precipitation Frequency Data Server. (In such a comparison, what parameter would be used to judge whether the results match? Would an *erosion or erodibility* parameter be needed? The *total sampled rainfall* during the 3.65-million-day model run would

[109] Tucker et al., op. cit., at 60, 147, and 149. They use a Weibull distribution for their *wet-day* probability density distribution of daily precipitation intensity. They adjust this by a *wet-day fraction F* to make the distribution representative of *all* days within their modeling run. See also Task 2 above for Tucker and Doty's sub-time-steps which result in up to 2000 (or as few as 100) samples being taken per run.

not be a meaningful parameter,[110] but comparing the full-breadth distribution curves should work and would not depend on an erosion or erodibility parameter.)

Task 14B: Look at the fundamental equations and algorithms used by Tucker and Doty, and compare them (for example) to Kwang and Parker (2017), Lague (2014), work by Alan D. Howard, work by Mudd et al., etc.

The general form of Tucker and Doty's erosion equation (see their Equation 11.3) is:

$$E = KA^{0.5}S$$

where E is the erosion rate, K is an erodibility constant or erosion coefficient, A is the upstream drainage area, and S is the downstream drainage gradient or channel bed slope.[111] An equation of this general form is widely used and accepted. See, for example, Kwang and Parker (2017), Lague (2014), work by Alan D. Howard, work by Mudd et al., etc. See also Equation (2) in Task 20 below, where the volumetric rate of gully advance is expressed in terms of a similar power of the upstream drainage area ($A^{0.52}$). However, despite the wide acceptance of such an equation, Tucker and Doty's use of it doesn't make sense, at least not without further explanation.

A more specialized understanding of this type of erosion equation is needed to sort this out. In the meantime, one speculative possibility is that an erosion equation such as $E = KA^{0.5}S$ has been found empirically to be a good representation of the erosion rate in many different settings, *where no separate account is taken of the runoff-to-rainfall ratio and other parameters* that Tucker and Doty take into account separately. As noted, this is speculative.

The difficulty with $E = KA^{0.5}S$ is easily demonstrated by looking at Tucker and Doty's modified form (their Equation 11.7) by which they express the *instantaneous* erosion rate E_i as:

$$E_i = K_q\, r^{0.5}A^{0.5}S$$

where K_q is a new erosion constant and r is the runoff rate averaged over the upstream drainage area A. For the sake of illustration, units of measurement could be defined such that K_q, A, and S are all equal to 1, so that the instantaneous erosion rate E_i is proportional to (and equal to) the square root of the runoff rate r:

$$E_i = r^{0.5}$$

As a good approximation, the runoff-to-rainfall ratio is about 0.6 (see Task 5). Thus, during the August 2009 storm, rainfall was about 5 inches/day and the runoff rate r was about 3 inches/day. We can also consider two lesser storm events in which rainfall was about 1 inch/day and 3 inches/day, and the runoff r was about 0.6 inch/day and 1.8 inches/day, respectively. For each of

[110] Assume, as quasi-realistic values to illustrate this point, that average annual rainfall is about 40 inches (hence 400,000 inches in 10,000 years). Similarly, assume that 24-hour rainfall depths are about 5 inches for a 100-year storm, about 8 inches for a 1000-year storm, and about 15 inches for a 10,000-year storm (hence the total for the 100 intense storms listed above in the bullet points would be on the order of 650 inches). An intense-storm total of roughly 650 inches, or somewhat more than 650 inches due to climate change, is a tiny fraction of 400,000 inches.

[111] See Tucker et al., op. cit., 147.

these storms, the following table shows the erosion rate predicted by Tucker and Doty's equation ($E_i = K_q\, r^{0.5} A^{0.5} S$). For each storm, it also shows the *erosion rate per inch of rainfall* and the *erosion rate per inch of runoff*:

Storm rainfall (in/day)	Storm runoff r (in/day)	Predicted erosion rate E_i	Erosion rate per inch of rainfall	Erosion rate per inch of runoff
1	0.6	0.7746	0.7746	1.2910
3	1.8	1.3416	0.4472	0.7453
5	3.0	1.7321	0.3464	0.5774

This makes no sense. The table, based on Tucker and Doty's equation, shows both the *erosion rate per inch of rainfall* and the *erosion rate per inch of runoff* decreasing as the size of the storm increases. Compared to the 5 inch/day storm in August 2009, a much smaller 1 inch/day storm is shown as being more than twice as erosive per inch of either rainfall or runoff. Such a result is the opposite of physical reality. As stated in Task 6, five inches of drizzle over a prolonged period is far less erosive than a five-inch downpour. Thus, there appears to be a serious fundamental problem with Tucker and Doty's landscape evolution model. Note that the above table isn't an exact representation of their calculated erosion rates because it doesn't include additional factors such as the soil infiltration capacity I_c or erosion threshold ω_c – but such factors appear to have too small an effect to resolve the problem identified here.

A more general form of the widely accepted erosion-rate equation used by Tucker and Doty is:

$$E = KA^m S^n$$

where m is an exponent (not necessarily 0.5) and n is another exponent (not necessarily 1).[112] Kwang and Parker (2017) provide a good overview of this numerical relationship, known generally as the stream power incision model (SPIM), and they offer a strong warning that:

> The most common choice of exponents satisfies $m/n = 0.5$. Yet all models have limitations. Here, we show that when hillslope diffusion (which operates only on small scales) is neglected, the choice $m/n = 0.5$ yields a curiously unrealistic result…

Tucker and Doty use $m/n = 0.5$ (typically $m = 0.5$ and $n = 1$, but also $m = 1/3$ and $n = 2/3$ in their shear-stress model). It is not clear whether they are using a hillslope diffusivity coefficient that might avoid the unrealistic result pointed out by Kwang and Parker, nor is it clear whether such a limitation on stream power incision models is connected in any substantial way to the problem identified above.

[112] See Tucker et al., op. cit., 66-67, Kwang and Parker (2017), Lague (2014), etc.

TASK 15: Account for climate change, and, as a starting-point, ensure that current and paleo rainfall are properly represented

Context: How climate change is incorporated into models and other probabilistic predictions remains a major unanswered question. See also Task 17 and the "rainy day normal" (RDN) discussion in Task 20.

Questions of *regionalization* and *statistical downscaling* arise for both current climate and climate change. See Task 16 for the latter. The problem of *regionalization* has been described as follows:

> Frequency analysis is a problem in hydrology because sufficient information is seldom available at a site to adequately determine the frequency of rare events. At some sites no information is available. When one has 30 years of data to estimate the event exceeded with a chance of 1 in 100 (the 1 percent exceedance event), extrapolation is required. Given that sufficient data will seldom be available at the site of interest, it makes sense to use climatic and hydrologic data from nearby and similar locations.
>
> The National Research Council (Ref. 104, p. 6) proposed three principles for hydrometeorological modeling: "(1) 'substitute space for time'; (2) introduction of more 'structure' into models; and (3) focus on extremes or tails as opposed to, or even to the exclusion of, central characteristics." One substitutes space for time by using hydrologic information at different locations to compensate for short records at a single site.This is easier to do for rainfall which in regions without appreciable relief should have fairly uniform characteristics over large areas. It is more difficult for floods and particularly low flows because of the effects of catchment topography and geology....[113]

The need to "substitute space for time" for quantifying extreme rainfall at a given location has also been described thus:

> It turns out that the most extreme point rainfalls of record are almost entirely from unofficial sources. This should be expected since there is practically no chance that the most extreme rainfall of a storm would occur over a preselected gage site.[114]

VEC.171. Information on the predicted increase in extreme weather events as a consequence of climate change can be found in a recent U.S. federal government report. This report, entitled *Weather and Climate Extremes in a Changing Climate*, edited by T.R. Karl et al., U.S. Climate Change Science Program, Synthesis and Assessment Product 3.3, June 2008, is available online at www.climatescience.gov/Library/sap/sap3-3/final-report/sap3-3-final-all.pdf. In part, the report reviews the expected increase in heavy precipitation events associated with climate change (as might be expected, since warmer air masses can carry more water vapor). See esp. Fig. 3.5

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[113] Stedinger, Vogel, and Foufoula-Georgiou, Chapter 18, *Frequency Analysis of Extreme Events*, (https://engineering.tufts.edu/cee/people/vogel/documents/frequencyAnalysis.pdf), § 18.5.

[114] Ho and Riedel, "Seasonal Variation of 10-Square-Mile Probable Maximum Precipitation Estimates, United States East of the 105th Meridian," NOAA/NRC Hydrometeorological Report No. 53 ("HMR No. 53"), April 1980, p. 3.

(page 100) and its caption, which indicates that “Daily total precipitation events that occur on average every 20 years in the present climate would, for example, occur once every 4-6 years [in the last decade of this century] for Northeast North America. These results are based on a multimodel ensemble of global climate models.” As indicated on p. 102, “precipitation intensity (i.e., precipitation amount per event) is projected to increase over most regions...and the increase of precipitation extremes is greater than changes in mean precipitation...” See also Fig. 3.6 (p. 102), which shows projected changes in the intensity of precipitation, and its caption, which notes that “the lightest precipitation is projected to decrease, while the heaviest will increase...” For more detailed discussion, see p. 102 ff. of the report. This trend toward more frequent heavy precipitation events has clear implications for erosion of the West Valley site and cannot be omitted from DOE’s erosional analyses.

VEC.172. It is not clear from the 2008 Draft EIS whether its assumptions about *current* climate, especially its assumed intensity-frequency relationship and assumed probable maximum precipitation (PMP) for storms, are reasonable. Given the 2008 EIS’s reliance on current climate as the basis for both future climate and paleoclimate, the assumed current climate is a distinct and important issue. As the basis for precipitation used in the West Valley erosion model, the 2008 Draft EIS relies on an MIT M.S. thesis by Hawk (1992). See Appendix F of 2008 Draft EIS, pages F-42 and F-43. This thesis, entitled “Climatology of station storm rainfall in the continental United States: Parameters of the Bartlett-Lewis and Poisson rectangular pulses models,” was published under the same title in 1992 by Hawk and Eagleson as an MIT/NASA report (available at http://ntrs.nasa.gov/archive/nasa/casi.ntrs.nasa.gov/19930015334_1993115334.pdf). Hawk’s general method is recognized in the scientific community as a stochastic technique for generating artificial rainfall records for such purposes as evaluating probable maximum precipitation (PMP) events and design floods. The question here is whether DOE’s CHILD erosion model (and the Buffalo, NY, data fed to it from Hawk’s thesis) produce a rainfall intensity-frequency relationship and a PMP that are reasonable for the present climate of the West Valley site.

VEC.173. The rainfall intensity-frequency relationship and the PMP used in DOE’s erosion model need to be reasonably consistent with Figure 1 of *Estimating Bounds on Extreme Precipitation Events: A Brief Assessment* (Washington, DC: National Academy Press, 1994) and with the the 2-, 5-, 10-, 20-, and 100-year and PMP storms that are assumed in the 1999 Draft EIS and mentioned (but not actually used) in the 2008 Draft EIS. The values used in the 1999 Draft EIS and listed in the 2008 Draft EIS, page F-19, including a PMP of 24.9", are taken from the U.S. Department of Agriculture (USDA). Figure 1 of *Estimating Bounds on Extreme Precipitation Events: A Brief Assessment* is said to involve a “very conservative version” of the PMP. As stated on page 11 of the National Academy Press book:

> A very conservative version of PMP could be created through the use of Figure 1. For every point on the Earth's surface, we could assume that in any given amount of time the greatest precipitation would be bounded by the greatest precipitation accumulation observed anywhere in the world for that duration. However, designing all structures to survive such conditions would be prohibitively expensive.

At the West Valley site, the assumptions fed to an erosion model should not be censored or biased by concerns about a "prohibitively expensive" result. Any debate about cost should arise at a later stage, not at the point of data input to a model. Any assumptions fed to an erosion model should be those that are reasonable and reasonably conservative. As it happens, neither the 24.9" PMP from USDA nor Figure 1 of *Estimating Bounds on Extreme Precipitation Events: A Brief Assessment* is excessively conservative for the West Valley site. Both depend substantially on a PMP rainfall of 34.5" (thirty-four and one-half inches) that fell during a 24-hour period at Smethport, PA, on July 17, 1942. Smethport is relatively close to the West Valley site (about 40 miles), and both are in generally similar topographic settings on the Allegheny Plateau. A storm of the intensity of the Smethport event would unquestionably cause severe erosion. "Hillsides in the Smethport area were reported stripped of vegetation to the bedrock," according to C.C. Burt and M. Stroud, *Extreme Weather: A Guide & Record Book*, W.W. Norton, 2007, page 119 (and see also p. 117). Nor is Smethport the only example of heavy rainfall in nearby areas. Portions of Erie, PA, suffered a 20" one-day deluge in July 1947, according to the Pennsylvania State Climatologist website (http://pasc.met.psu.edu/PA_Climatologist/fod/paex.html). In summary, any rainfall intensity-frequency relationship generated by DOE's erosion model must be reasonable for the West Valley site and must generate a relatively high PMP storm under current climate conditions, and this relationship must be progressively modified to reflect the predicted increase in extreme weather events due to climate change.

CSC.63 and VSC.92. Paleoclimate needs to be reconstructed based on the best available evidence and needs to be adequately and transparently incorporated into EWG and PPA erosion modeling.[115]

CSC.64 and VSC.93. The period of approximately 4000 years of minimal Buttermilk Creek downcutting (between about 10,000 and 6000 years before present), as identified by the EWG report by Wilson and Young,[116] needs to be linked to causal factors such as reduced rainfall or other evidence-based factors.

VEC.204. The relatively intense rainfall event which delivered a total of approximately 5 inches of rain to the West Valley site between August 8 and August 10, 2009, has important implications for the site's susceptibility to erosion, long-term site integrity, storm return intervals, climate-change-induced changes in storm frequency and intensity, and the need for reliable data collection.

VEC.205. Several very obvious erosion effects occurred on and near the site in short periods of time (e.g., several hours) as direct results of the rain event and associated runoff, as I observed during a site inspection several days later (August 19, 2009). For example, knickpoints on both Erdman Brook and Franks Creek migrated several feet upstream, with associated enlargement of their plunge pools. The Quarry Creek ravine underwent substantial scouring and sidecutting in several locations near the old Rock Springs Road bridge abutments. This caused the root systems of large trees growing on the banks to be partly undercut, caused other large trees on the banks to fall into the ravine due to more extensive undercutting and slumping, caused or enhanced the

[115] Vaughan EIS comments §§ 166-68.
[116] Wilson & Young, op. cit., esp. Figs. 4.10-3 and 4.10-4.

slumping of other blocks of earth on the sloping ravine banks, caused large clayey clasts ranging up to 12 or more inches in diameter (apparently rip-up clasts plucked from the ravine banks by the flowing water) to be deposited within the ravine as the peak flow receded, and apparently caused large quantities of sediment to be carried downstream beyond the ravine during the storm event, both in the form of particles carried as suspended sediment and in the form of rip-up clasts (ranging up to 12 inches and more) that were carried as bed load by the flowing water. On the high bank of Buttermilk Creek where persistent slumping has occurred for decades and has been extensively studied, a large landslide carried thousands of tons of Lavery Till and Kent recessional sediments down the slope toward (and partly into) Buttermilk Creek. The immediate cause was apparently the erosional removal of some of the relatively uncohesive Kent recessional sediments from beneath the Lavery Till, which caused blocks of the unsupported till to break off and roll downslope into jumbled piles – but it is unclear whether the initial erosional removal of Kent recessional sediments was a result of undercutting by high water in the creek(s) below (meaning Buttermilk Creek and flow from "Heinz" Creek which enters Buttermilk opposite the landslide face), or as a result of groundwater emerging from the base of the Kent recessional bed, or as a result of surface water cascading down from the top of the bank and impinging on the Kent recessional bed at the height of the storm. This is one of several storm-related issues that needs to be studied and resolved.

VEC.206. The August 2009 storm event was not a unique or highly unusual occurrence for the West Valley site. During the past 50 years, the five storms shown in Table 1 on the next page have delivered roughly equivalent rainfall (storm totals of roughly 5 inches in each case) and have caused roughly similar high flow in Cattaraugus Creek. The August 2009 storm is not demonstrably larger than the others listed in this table, and the rainfall it delivered to the West Valley site is not demonstrably larger than about 5 inches.

VEC.207. Information discussed here and presented in Table 1 suggests that the return interval of the August 2009 storm is about ten years. Climate change, to the extent that it increases the frequency and/or intensity of severe storms (e.g., see comments 169-171 above), will reduce the return interval to less than 10 years.

Table 1
Storms of approximately similar magnitude experienced at West Valley site in past 50 years

Date of storm	Associated hurricane or tropical storm, if any	Estimated peak flow (cfs) at USGS Cattaraugus Creek gage at Gowanda	Recorded rainfall (Buffalo)	Recorded rainfall (elsewhere)
Sept. 27-28, 1967	[none]	28,800 (Sept. 28)[3]	4.40" NWS[5]	--
June 21-23, 1972	Agnes[1]	25,300 (June 23)[3]	3.88" NWS[6]	--
Sept. 14, 1979	Frederic[2]	26,700 (Sept. 14)[3]	4.89" NWS[7]	--
June 26, 1998	[none]	28,000 (June 26)[3]	0.30" NWS[8]	3.25" WVDP[10] 8" Ashford[11]
Aug. 8-10, 2009	[none]	32,500 (Aug. 10)[4]	2.78" NWS[9]	3.45" Eden[12] 7.75" Perrysburg[13]

Table 1 Notes:

1. See NWS 1972 N. Atlantic Hurricane Tracking Chart (online) for track of Agnes, which passed over central New York (not directly over WNY) as a tropical storm. See also Bailey, Patterson, and Paulhus, Hurricane Agnes Rainfall and Floods, June-July 1972, USGS Professional Paper 924 (Washington, DC: U.S. Geological Survey, 1975); U.S. Army Corps of Engineers, Buffalo District, Report of Flood, Tropical Storm Agnes, June 1972, NTIS Report AD-A100 811/9/HDM, 249 pages, August 1973.

2. See NWS 1979 N. Atlantic Hurricane Tracking Chart (online) for track of Frederic, the extratropical stage of which passed over central New York (not directly over WNY).

3. From http://nwis.waterdata.usgs.gov/ny/nwis/peak?site_no=04213500&agency_cd=USGS&format=html.

4. Real-time data retrieved August 2009, for site 04213500, USGS stream gage at Gowanda: http://nwis.waterdata.usgs.gov/ny/nwis/uv?cb_00065=on&cb_00060=on&format=html&period=30&site_no=04213500.

5. See www.erh.noaa.gov/buf/f6/bufSep67.html, which shows 0.99" on Sept. 27 and 3.41" on Sept. 28, 1967.

6. See www.erh.noaa.gov/buf/f6/bufJun72.html, which shows 1.75" on June 21; 1.43" on June 22; and 0.70" on June 23, 1972.

7. See www.erh.noaa.gov/buf/f6/bufSep79.html, which shows 4.89" on Sept. 14, 1979.

8. See www.erh.noaa.gov/buf/f6/bufJun98.html, which shows 0.30" on June 26, 1998.

9. From Preliminary Local Climatological Data (form F-6) for Buffalo NWS, retrieved August 2009 from www.weather.gov/climate/getclimate.php?wfo=buf, which shows 0.26" on August 8; 1.63" on August 9; and 0.89" on August 10, 2009.

10. West Valley site rain gauge record, as provided in 1998 by John Chamberlain.

11. Rain gauge maintained by Dr. Tim Siepel at his house in Ashford, NY, personal communication.

12. From http://newa.nrcc.cornell.edu/newaLister/, daily data retrieved August 2009 for Eden, NY, showing 0.08" on August 8; 1.91" on August 9; and 1.46" on August 10, 2009. Cornell's NEWA website also lists weather stations in Dunkirk, Fredonia, and Gainesville, NY – but none in Cattaraugus County.

13. NWS Cooperative Weather Observer in Perrysburg measured 0.48" from 7:00 AM on August 8 to 7:00 AM on August 9, and measured 7.27" from 7:00 AM on August 9 to 7:00 AM on August 10, 2009.

* * *

VCT.[3]. … [A]ny modeling of past erosion at the West Valley site (e.g., for model calibration purposes) needs to use realistic sequences of assumed precipitation that are based on, and consistent with, available paleoclimate information. Likewise, any modeling of future erosion at the West Valley site needs to use realistic sequences of assumed precipitation that are based on, and consistent with, a good understanding of climate change. Two potentially useful sources of paleoclimate information, not intended to be exhaustive, are Noren et al. (2002) and Holcombe et al. (2003). Based on sediments deposited in lakes in Vermont and eastern New York, Noren et al. (2002) identified four periods of intense storminess that occurred about 11,900, 9,100, 5,800, and 2,600 years ago. Interspersed between the second and third of these storm periods was the middle Holocene climatic optimum (9,000 to 6,000 years ago), during which "warmer temperatures and greater aridity" characterized the climate of the Lake Erie region, according to Holcombe et al. (2003). Such paleoclimate information provides guidance needed for modeling of past erosion at the West Valley site….

<u>TASK 16</u>: Assess whether MACA method of statistical downscaling of GCMs is protective (*e.g.*, in view of Waverly, TN, in 2021; Liguria, Italy, in 2021; Smethport, PA, in 1942)

Context: Given the sporadic and apparently increasing occurrence of "trains" of intense storms (e.g., the August 2009 storm(s) that delivered about 5 inches of rain to the Cattaraugus Creek basin, including the West Valley site) and atmospheric "rivers," is it reasonable and protective to use statistical downscaling of Global Climate Models as Tucker and Doty[117] have done? Such statistical downscaling is based on localized weather records of limited duration (limited by the period of record), which appear poorly suited to understanding the future frequency and intensity of sporadic rare events at a given location. In other words, is it reasonable and reasonably prudent to assume, in the absence of *major* orographic/topographic influences, that such "trains" or "rivers" will respect and spare localities with historically lower-than-average rainfall? Or will such intense weather systems simply "barge through" and inundate locations along their pathway with minimal regard for past rainfall history?

CSC.56 and VSC.84. Erosion modeling for the West Valley site (both EWG and PPA modeling) needs to recognize and incorporate rapidly moving, organized thunderstorm systems, sometimes called *derechos* or mesoscale convective system (MCS)-organized convective storms. Two examples are the July 1942 storm in Smethport, PA (~30 inches rainfall in 4.75 hr), and the July 1996 Redbank storm near Brookville, PA (~5 inches in 4 hr), that have been reviewed and analyzed by Smith et al.[118] Both locations are on the western margin of the central Appalachians, less than 100 miles south of the West Valley site. While the orographic relief of these Pennsylvania sites is not identical to that of the West Valley site, there are similarities not only in topography but in the occurrence of "trains" of storms that stream generally eastward along a relatively stationary track for many hours, delivering exceptional rainfall accompanied by intense lightning. The Redbank storm, for example, "consisted of a system of multicellular thunderstorms that moved rapidly from Lake Erie across western Pennsylvania," involving "multiple storms that tracked over Redbank Creek," with cloud-to-ground lightning flash densities ranging up to 2-3 strikes per square kilometer during the storm. This type of powerful "training" storm system was apparently involved in both the Smethport storm[119] and the August 2009 West Valley derecho or storm.[120] Storms of this type need to be incorporated into EWG and PPA erosion modeling.

CSC.57 and VSC.85. An article by Prein et al.[121] finds that MCS-organized convective storms with a size of ~100 km are poorly represented in traditional climate models yet are increasing in frequency and intensity. For the West Valley area, these authors show a 50% to 70% increase in

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117 Tucker et al., op. cit., at 150-52.
118 J.A. Smith et al., "Extreme rainfall and flooding from orographic thunderstorms in the central Appalachians," *Water Resources Research* **47**, W04514 (2011).
119 Id.
120 C.O. Szabo, W.F. Coon, and T.A. Niziol, *Flash Floods of August 10, 2009, in the Villages of Gowanda and Silver Creek, New York*, USGS Scientific Investigations Report 2010-5259.
121 A.F. Prein et al., "Increased rainfall volume from future convective storms in the US," *Nature Climate Change* **7**, 880-86 and Supplementary Information (Dec. 2017), esp. Supplementary Fig. 2(e).

the frequency of MCSs (expressed as track density difference) relative to current conditions.[122] This trend, discussed by Feng as a "near doubling" of severe storms,[123] needs to be incorporated into EWG and PPA erosion modeling.

CSC.58 and VSC.86. Climate experts at the August 2012 WVDP climate workshop noted that "Climate Scientists have high confidence that extreme precipitation intensity will increase in the future due to the increases in ocean temperature as greenhouse gas concentrations increase in the atmosphere ...[and] that maximum water vapor concentration in the atmosphere will substantially increase during the 21st Century in western New York. For a high greenhouse gas emissions scenario, these increases were in the 20 to 30 percent range by 2100. Although other factors (frequency and intensity of meteorological systems that cause extreme precipitation) could have enhancing or moderating effects on future design storm values, there are no comprehensive studies that assess the magnitude of such influences. As a first order approximation, design storm precipitation totals…may increase by approximately 25 percent by 2100."[124] They also noted that, "During the early part of the 21st century, the frequency of extreme precipitation events has increased by as much as 74% across the Northeastern United States compared to the late 1950s to early 1960s."[125]

CSC.59 and VSC.87. Evidence continues to grow that intense storms will become more frequent, and that their intensity will increase. For example, a recent article by Prein et al.[126] shows hourly extreme precipitation in the West Valley area increasing by 35% to 49% as a result of climate change in both winter (Dec.-Jan.-Feb.) and summer (June-July-Aug.), where "extreme" precipitation, defined as the 99.95th percentile of hourly precipitation, corresponds to the maximum precipitation that occurs on average once every season.[127] The same article shows the exceedance probability of hourly extreme precipitation increasing by about 130% (winter) and 165% (summer) in the West Valley area, relative to a 2000 to 2013 control period.[128] Such effects of climate change, including larger temperature fluctuations and the resulting changes in both direct rainfall and runoff from snowmelt, and also including periods of increasing drought interspersed with increased storminess, need to be adequately and transparently incorporated into EWG and PPA erosion modeling.

VSC.88. It is not that clear that the sensitivity analyses for the EWG erosion modeling runs cover the intensity-frequency increases for intense storms.[129] Sensitivity analyses for these

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[122] Id., Supplementary Fig. 2(e).
[123] Z. Feng, "Near doubling of storm rainfall," *Nature Climate Change* **7**, 855-56 (Dec. 2017).
[124] Enviro Compliance Solutions Inc., "Climate Guidance for Phase 1 Studies" (Nov. 2012), pp. 9-10.
[125] Id, p. 2.
[126] A.F. Prein et al., "The future intensification of hourly precipitation extremes," *Nature Climate Change* **7**, 48-52 (Jan. 2017). The authors are using a pseudo global warming (PGW) approach to "perturb the lateral boundary conditions of ERA-Interim with a high-end scenario (RCP8.5) 95-year ensemble monthly mean climate change signal from 19 Coupled Model Intercomparison Project Phase 5 Models" (CMIP5).
[127] Id., Fig. 1 and related text.
[128] Id., Fig. 2 and related text.
[129] Tucker, QPM presentation, op. cit., slide 6, does not show such a sensitivity analysis. As discussed elsewhere in these comments, it's clear that the EWG erosion modeling (as reported by Tucker et al., op.

intensity-frequency increases, and for the incorporation of such increases into models employing relatively long (e.g., 10-year) time steps, need to be defensibly and transparently incorporated into the SEIS process.

CSC.60 and VSC.89. The EWG erosion models are employing unrealistically and unacceptably low levels of future climate change. The Multivariate Adaptive Constructed Analogs (MACA) climate scenarios that are being used to represent climate change in the EWG erosion models[130] are adding relatively little intensity and frequency to the current level of intense storms. The EWG models are assuming increases of approximately 9% in mean annual precipitation, 1% in mean wet day frequency, and 12% in mean wet day intensity,[131] and the models' three "future climate scenarios" assume increases in the neighborhood of 8% to 12% in mean wet day precipitation.[132] These trivial increases are inconsistent with the increases outlined in the preceding paragraphs. Incorporation of climate change in the SEIS process must be more than a token effort; it needs to reflect current science. Modeling runs that do not adequately represent climate change need to be re-done.

cit.) does not treat precipitation and runoff (from both rain and snow) in a realistic manner. The modeling thus provides no clear and direct basis for understanding the effects of precipitation and runoff on model results (or conversely, for understanding the sensitivity of model results to precipitation and runoff).

[130] Tucker, QPM presentation, op. cit., esp. slides 19-20.

[131] Id., slide 19, where values interpreted from the currently available version (a paper copy of the slide) are 1250/1150 = 109%, 0.48/0.475 = 101%, and 7.0/6.25 = 112%.

[132] Id., slide 20, where values interpreted from the currently available version (a paper copy of the slide) are 6.72/6.25 = 108% and 7.0/6.25 = 112% for RCP-4.5 and RCP-8.5, respectively.

TASK 17: Assess whether worsening of climate beyond 2100 is properly addressed in LEM and can be plausibly represented in any model

CSC.61 and VSC.90. The EWG erosion models assume no further climate change beyond year 2100.[133] This is inconsistent with the August 2012 WVDP climate workshop where it was noted that, "Although, as a first-order approximation, design storm values may increase by approximately 25 percent by 2100, this approximation certainly does not represent an upper limit beyond 2100."[134]

[133] Id., slide 20.
[134] Enviro Compliance Solutions Inc., op. cit, p. v.

<u>TASK 18</u>: Assess 1996 DEIS / EID methodology; compare to landscape evolution modeling

Context and terminology: Erosion modeling to support the 2010 EIS was done with Landscape Evolution Models (LEMs) developed by Greg Tucker and Sandy Doty based on their CHILD landscape evolution modeling. Tucker and Doty's involvement continued past 2010 as they were appointed to the West Valley Erosion Working Group (EWG) and, in that capacity, continued to develop generally similar LEMs that are here called the "EWG erosion models." Their earlier LEMs such as CHILD and SIBERIA, while now superseded, sometimes provide useful information on modeling procedures that were carried over into the current models but are not clearly explained in current documentation. Prior to the SIBERIA modeling, HEC-based modeling was used to support the 1996 DEIS. The HEC-based modeling was supported in turn by Environmental Information Documents (EIDs) such as those prepared by Beyer (1993) and O'Connor (1993) on behalf of DOE and NYSERDA. As noted above, the newer results from the Tucker-Doty LEMs are very different from the earlier modeling results that supported the 1996 DEIS; hence the need to understand these differences as we approach the supplemental "Phase 2" EIS process and the associated site cleanup decision that the Tucker-Doty (EWG) erosion predictions are meant to support.

<u>TASK 19</u>: Obtain approximate comparative results from other methods such as WEPP

Context: Different erosion modeling methods, if valid, should produce roughly similar results.

VEC.150. As an alternative to the short-term calibration method used in the 1996 Draft EIS (based on longitudinal profile surveys of Franks Creek), the 2008 Draft EIS uses a Water Erosion Prediction Project (WEPP) method to predict sediment yield and associated downcutting rates at the West Valley site. WEPP is generally recognized as a versatile, physically based, distributed-parameter, continuous-simulation erosion model that can be applied to both hillslopes and channels. As presented in the 2008 Draft EIS (pages F-20 through F-26), the WEPP modeling shows a relatively small *average* elevation change of 408 mm per 1000 years associated with its predicted annual soil loss and sediment yield (6.1 MT/hectare, as shown in Table F-13). However, since soil loss and sediment yield at the West Valley site are concentrated mainly in the stream channels, the elevation change within channels is much greater than the average sitewide elevation change. Consistent with this general truth, the 2008 Draft EIS reported (page F-23) that "WEPP predicts that the average annual sediment yield of the watershed through creek channels is approximately 22,317 metric tons (24,600 tons) per year, equivalent to an average downcutting rate of 98,000 millimeters (320 feet) per 1,000 years." This average downcutting rate of 320 feet per 1000 years is orders of magnitude greater than the rate predicted by the CHILD erosion model on which DOE relies in the 2008 Draft EIS, implying a severe mutual miscalibration between WEPP and CHILD. Despite the generally good reputation of WEPP as a modeling tool, the 2008 Draft EIS rejects the WEPP downcutting rate of 320 feet per 1000 years and favors the minimally erosive results of the CHILD model. The 2008 Draft EIS neither addresses nor resolves this major discrepancy between the model predictions; it simply dismisses short-term models such as WEPP by stating that "they are not generally used for long-term projections."

VEC.151. While I recognize the risk of extrapolating short-term model results for purposes of long-term projections, I also recognize the need for long-term models to be properly and defensibly calibrated. A long-term erosion model such as CHILD *cannot be considered properly calibrated* unless it can be run for relatively short periods (e.g., 10 years or 35 years) and can generate results compatible with either *short-term downcutting* of the type and magnitude observed in the Franks Creek longitudinal profile surveys or *short-term modeling results* such as WEPP's prediction of 320 feet average channel downcutting per 1000 years. No credibility can be given to a long-term erosion model that dismisses any link to reality.

VEC.227. The FEIS fails to respond to my comment 150 [VEC.150, above] in which I expressed concern about the "major discrepancy between the model predictions" that were generated by CHILD and WEPP and reported in the DEIS. Response 110-92 misrepresents my comment by saying that the WEPP results in the DEIS "are not used for the calibration of the landscape erosion models." This is not a response; it merely echoes what I said, without addressing the issue I raised about the large discrepancy between the two recognized models. Despite its claim that "the revised Appendix F presents a more sophisticated erosion model calibration and analysis," response 110-92 provides no evidence that the calibration is either sophisticated or reasonable. With respect to the measurements or studies cited in Appendix F "that are helpful in judging the reasonableness of the CHILD predictions," I request a meeting or technical workshop session at which the cited studies can be reviewed. Participants at such a meeting or workshop should include, among others, Dr. Greg Tucker of Colorado and Dr. Chris Renschler of UB. Their collective expertise includes CHILD, WEPP, and SWAT.

VEC.228. The FEIS fails to respond to my comment 146 in which I said:

> Both the 1996 and 2008 methods of model calibration are logical and potentially useful, at least in theory. Indeed, if carried out properly, the two calibration methods should agree with each other within some margin of error, and the error bounds of each method should be reasonably comprehensible, perhaps even predictable....

Responses 110-88, 110-89, and 110-90 provide three seemingly contradictory answers, yet none of the three addresses the fundamental scientific issue raised in my comment 146:

In response 110-88, the FEIS says that the 1996 analysis and the current analysis "take very different approaches to model calibration" and that the nature of their predictions "is so fundamentally different...that comparisons are not meaningful." In my opinion, the FEIS statement that "comparisons are not meaningful" is absurd; it essentially says I shouldn't have raised the point made in my comment 146.

In response 110-89, the FEIS says that "The two different erosion models are fundamentally different" and that "There is no reason to conclude that the two models do not yield comparable results." In my opinion, this makes more sense than response 110-88; it at least entertains the possibility of comparing model results and/or model calibrations.

In response 110-90, the FEIS says that "A calibration based on topography changes over a few decades (regardless of whether or not it involves changes over 10 years or 30 years) is clearly weaker than a calibration based on topography changes over thousands of years." With two caveats, I agree. One caveat, as stated in my comment 146, is that the two calibration methods should agree with each other within some margin of error, and the error bounds of each method should be reasonably comprehensible, perhaps even predictable. The other caveat is that "a calibration based on topography changes over thousands of years" must be based on a reasonably well-known interval of thousands of years in order to avoid the type of miscalibration discussed above in my comment 224. See further discussion below.

VEC.229. The FEIS fails to respond to my comment 146 in which I said:

> Additional studies conducted between Phase I and Phase II of phased decisionmaking should include *a defensible demonstration that the two calibration methods yield compatible results*....

Studies and/or comparisons of this type are needed to support Phase 2 decisionmaking. Defensible comparisons to other models such as WEPP or SWAT may serve as one form of testable verification for the CHILD erosion model and its calibration (see comment 227 above), yet comparison to real-world erosion at the West Valley site remains necessary as well. There are various ways this might be done. Possibilities, not meant to be exhaustive or prescriptive, include running the CHILD model for 10 years with 1980-1990 site-specific weather data, or running the

CHILD model for 30 years with 1980-2010 site-specific weather data, to see whether the CHILD results are predictive of observed changes in the longitudinal profile of Franks Creek. This is merely a suggestion, but – regardless of the chosen method – it is important to move forward with some type of study that allows verification of the CHILD model and its calibration against observable data. A meeting or technical workshop may be a useful first step toward such study.

<u>TASK 20</u>: Assess erosional sidecutting caused (or initiated) by gully initiation and growth

Context: Gullies are an integral part of site erosion. NYSERDA, as quoted above in the Introduction, explained the importance of gullies in relation to the landscape evolution modeling that was the direct predecessor of the current EWG erosion modeling and shows similar results:

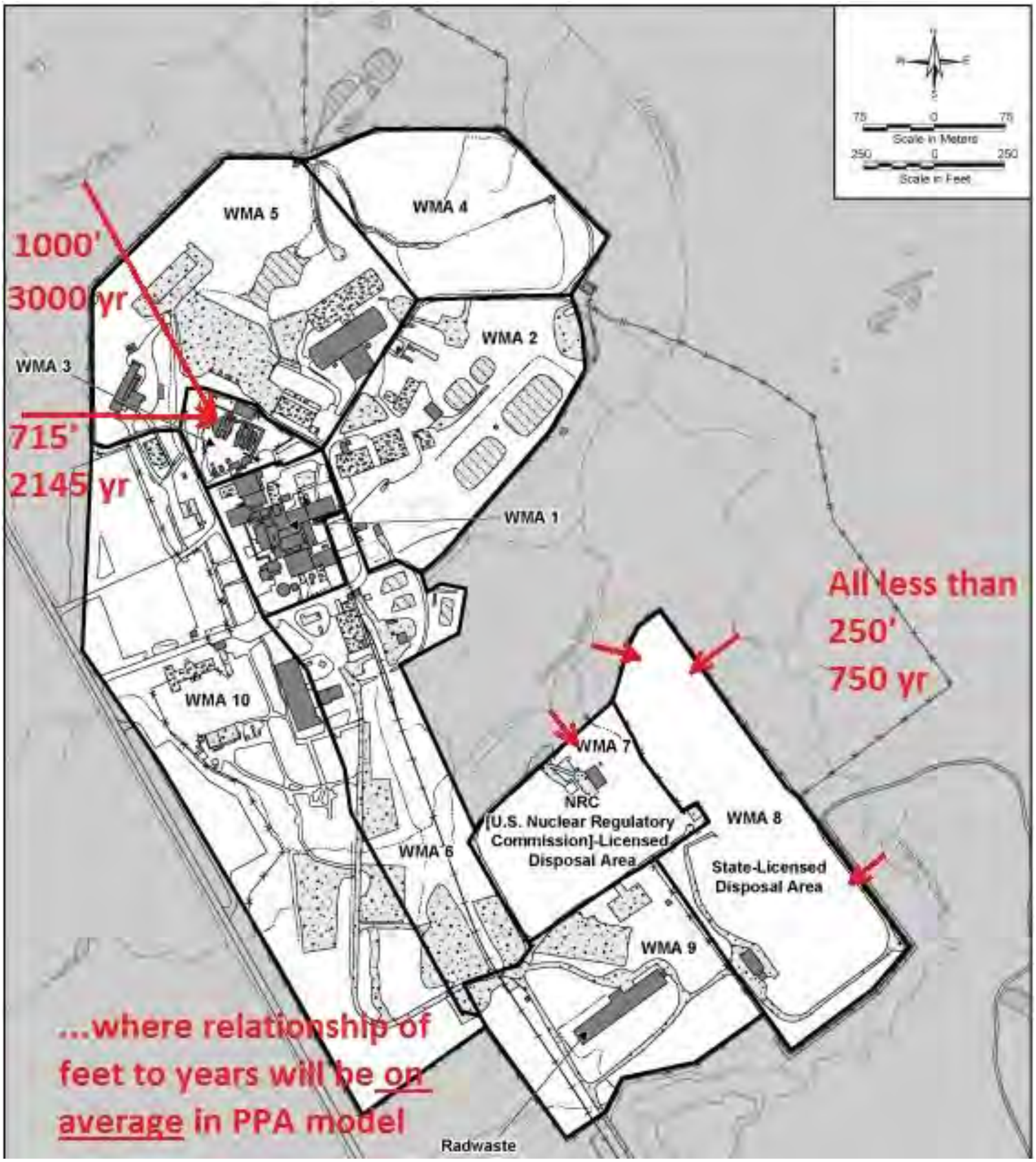
> With the exception of one modeling scenario, the simulation results show no gully erosion of the South Plateau over the next 10,000 years. Even more astonishing, these results show streams surrounding the South Plateau filling in with sediment over the same time period. These results are wholly inconsistent with what is being observed at these locations today. The streams themselves are actively downcutting dramatically in some locations, and the stream valley walls contain actively eroding gullies. The modeling results for the North Plateau predict tremendous downcutting (up to 30 meters or 100 feet) on Quarry Creek, which borders the WVDP to the north, yet relatively little gully erosion protruding into the plateau. Again, this predicted landscape is not representative of observed site or regional topography. Where local streams have incised the landscape, deep gullies extend many hundreds of feet into the landscape on either side of the stream. These discrepancies suggest the modeling results are neither meaningful nor reliable.[135]

As of 2018-2019, Neptune and Company was investigating headward advance rates of gullies.[136] At the 11/14/18 West Valley Quarterly Public Meeting, at the end of a Probabilistic Performance Assessment (PPA) presentation on radionuclide distributions in the Waste Management Areas, Neptune responded to an off-topic question about how they will calibrate erosion rates for the PPA model. The answer appeared to be that Neptune will mostly *not* be using the Erosion Working Group's erosion modeling results. Instead, Neptune has been analyzing airphotos dating back several decades in order to determine the headward advance rate of gullies that are on or near the WVDP. Neptune has apparently determined that the average headward advance rate is about 0.1 meter per year, i.e., about 10 centimeters or 4 inches per year. On average, this means that onsite gullies advance about 1 foot per 3 years, or 250 feet per 750 years, where Neptune appeared to be treating such "on average" rates as rates measured in a straight line (shortest path) from a gully-initiation point along an existing stream to the nearest boundary point of a burial ground. Neptune apparently sees gully advancement as the main erosion threat to the SDA and NDA burial grounds.

Red arrows have been added to the site map below to illustrate such a gully advance process. Each of the four arrows pointing to the burial grounds is less than 250 feet long, suggesting that (on average) a gully would be expected to invade the burial grounds in less than 750 years. The gully would take longer to reach the center or far side of the burial grounds, but it would have penetrated at least some of the burial holes or trenches in less than 750 years, on average.

[135] NYSERDA statement in 2010 EIS, page ix.
[136] Note that some scientists use the term headward *retreat* as a synonym for headward *advance* of gullies.

Other arrows point to the High Level Waste tank farm which is about 700 to 1000 feet from the nearest points on Quarry Creek. If Neptune uses the same logic for erosion risk to the tank farm, gully advance would take about 2000 to 3000 years, on average, to reach the tank farm.



WVDP map with arrows added to illustrate 0.1 m/yr gully headward advance rate

It's not yet clear how Neptune will represent such gully advance rates probabilistically.

The effect of climate change on the rate of gully headward advance is of interest and can be understood *approximately* from the relationships expressed by Vanmaercke et al., "How fast do gully headcuts retreat?", *Earth-Science Reviews* **154**, 336–355 (2016). See especially the relationships shown in their Fig. 7(b) between RR_V (the *volumetric* rate of gully headward advance, in m^3/yr) and RR_L (the *linear* rate of gully headward advance, in m/yr)[137] and in their Equation (2) which relates RR_V to RDN (the "rainy day normal," in mm/day):

$$RR_V = 5.68\, {RR_L}^{1.12} \tag{1}$$

$$RR_V = 0.001\, A^{0.52}\, RDN^{4.97} \tag{2}$$

The variable A in Equation (2), representing upstream drainage area, can be considered constant for the present purpose. (The purpose here is to find the *difference* in gully headward advance rate due to climate change, *at any given gully head location*. The upstream drainage area at any given gully head location remains essentially constant regardless of whether the headward advance rate is increased by climate change.)

Equations (1) and (2) can be combined to determine the approximate relationship between RR_L (which is the gully headward advance rate that we and Neptune are interested in, expressed in m/yr) and the "rainy day normal" RDN which is expressed in mm/day:

$$5.68\, {RR_L}^{1.12} = 0.001\, A^{0.52}\, RDN^{4.97}$$

Thus,

$${RR_L}^{1.12} = \frac{0.001\, A^{0.52}\, RDN^{4.97}}{5.68}$$

$$RR_L = \sqrt[1.12]{\frac{0.001\, A^{0.52}\, RDN^{4.97}}{5.68}}$$

Thus, replacing $\sqrt[1.12]{A^{0.52}}$ with the constant *k*,

$$RR_L = k\left(\sqrt[1.12]{\frac{0.001\, RDN^{4.97}}{5.68}}\right)$$

$$RR_L = (0.000445\, k)\, RDN^{4.4375} \tag{3}$$

where:

$$k = \sqrt[1.12]{A^{0.52}} = A^{0.4643} \tag{4}$$

Equation (3) shows approximately how much the linear rate of gully headward advance RR_L (in m/yr) will change as a result of changes in the "rainy day normal" RDN (mm/day), which is

[137] Their Fig. 7(b) shows this relationship for *x* and *y*, representing RR_L and RR_V, respectively.

defined by Vanmaercke et al. as the total mean annual precipitation depth, divided by the average number of rainy days per year.

The "rainy day normal" RDN can be expected to increase due to climate change because:

- Annual rainfall remains approximately the same but the average number of rainy days per year decreases due to climate change (partly as a result of increased frequency of both intense rainfall events and drought), or
- Annual rainfall increases due to climate change but the average number of rainy days per year remains approximately the same, or
- Some combination of these two trends.

Increases in RDN as a result of climate change may possibly be in the neighborhood of 25% to 45%. A 25% increase might be inferred from what Cornell climate scientist DeGaetano has written about both nationwide trends and West-Valley-specific trends:

DeGaetano, for example, notes that "Heavy-rainfall events have become more frequent since the middle of the last century…", and this trend is now generally accepted in climate science. For the West Valley site, he and others have suggested that, "As a first order approximation, design storm precipitation totals…may increase by approximately 25 percent by 2100."[138]

A 45% increase in RDN might be inferred from the German Climate-Signal-Map "Increase in the occurrence of extremely wet days per year."[139] While neither the 25% value nor the 45% value is presented precisely as a rainy day normal or RDN value, defined as total mean annual precipitation depth divided by the average number of rainy days per year, either may serve as a preliminary illustration of the effects of climate change on the rate of gully headward advance. For this purpose and others, *we need better characterization of the effects of climate change on the rainfall intensity-frequency distribution*. Tucker and other erosion modelers on the Erosion Working Group consistently refused to take this task seriously, but it is a crucial part of understanding the erosional effects of climate change.

If a 25% increase in RDN is assumed, then Equation (3) shows that RR_L, the linear rate of gully headward advance, will become about 2.7 times its current or recent historical value. Neptune's current or recent historical value of 0.1 m/yr would become 0.27 m/yr in accordance with Equation (3). In Figure 1, the arrows indicating less than 250 years before a gully breaches the burial grounds would need to be relabeled to show less than 100 years, and the arrows indicating ~2145 yr and ~3000 yr before a gully breaches the tank farm would need to be relabeled to show ~800 yr and ~1115 yr, respectively, if the 25% increase in RDN became effective immediately.

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[138] CTF memo, "Actions Needed Related to Potential [Climate] Change Impacts," July 27, 2015, at 6, wherein the two direct quotes are from A.T. DeGaetano, "Time-Dependent Changes in Extreme-Precipitation Return-Period Amounts in the Continental United States," *Journal of Applied Meteorology and Climatology* **48**, 2086-99 (October 2009), at 2086-87, and DeGaetano et al., *Climate Guidance for Phase 1 Studies, West Valley Demonstration Project* (November 2012), at 9.

[139] Climate Service Center 2.0, Hamburg, Germany, December 2014, https://www.climate-service-center.de/imperia/md/content/csc/projekte/klimasignalkarten/csm_extremelywetdays_dec2014.pdf.

More realistically, Neptune's current or recent historical value of 0.1 m/yr would gradually increase over time to become 0.27 m/yr, as a result of which the time period before a gully breaches either the burial grounds or the tank farm would be intermediate between the above values.

If a 45% increase in RDN is assumed, then Equation (3) shows that RR_L, the linear rate of gully headward advance, will become about 5.2 times its current or recent historical value. Neptune's current or recent historical value of 0.1 m/yr would become 0.52 m/yr in accordance with Equation (3). In Figure 1, the arrows indicating less than 250 years before a gully breaches the burial grounds would need to be relabeled to show less than 50 years, and the arrows indicating ~2145 yr and ~3000 yr before a gully breaches the tank farm would need to be relabeled to show ~415 yr and ~600 yr, respectively, if the 45% increase in RDN became effective immediately. More realistically, Neptune's current or recent historical value of 0.1 m/yr would gradually increase over time to become 0.52 m/yr, as a result of which the time period before a gully breaches either the burial grounds or the tank farm would be intermediate between the above values.

All of the above values are approximate and could/should be improved based on climate science and erosion research generally, and on site-specific characterization of the West Valley site. Additional resources that may be useful include Piest and Bowie (1974), Allen et al. (2018), Vandekerckhove et al. (2003), New et al. (2002), and Gado et al. (2017).

VSC.108. The work done on gullies in the EWG erosion modeling report is rudimentary[140] and thus unable to provide a realistic assessment of the risk that gullies pose to site integrity.

VSC.109. The formation of new gullies, and the headward advance of new and existing gullies, need to be characterized and incorporated into any erosion modeling that will support the Phase 2 decision.

VEC.98. The [1996] Draft EIS (page 4-33) states that initiation and growth of gullies "appears to be the quickest mechanism for eroding into the north or south plateau and ultimately disturbing the site facilities." However, the Draft EIS does not use this information in any meaningful way. At the bottom of page L-5, the Draft EIS states that "Methods for predicting the long-term erosion rates of gullies are not available; therefore, gully advance for the 1,000-yr period was not predicted." Thus, as already noted, the Draft EIS bases its erosion models on ravine widening alone; it fails to integrate gully initiation and growth into these models. This omission is unacceptable, especially since the gully processes appear to be the quickest mechanism for eroding into the north and south plateaus and disturbing waste facilities.

VEC.100. Monte Carlo methods provide one way of developing an erosion model that integrates gully processes with ravine widening. See Appendix D for a computer program...[141]

VEC.175. On page L-5, the 1996 Draft EIS stated that, "based on the gully head advancement rates that were estimated for the SDA, NP3, and 006 gullies, the existing gullies in the Project

[140] Tucker et al., op. cit., at 25ff. and 95.
[141] Details available from Vaughan upon request.

Premises are considered a threat to the integrity of the existing facilities over the next 1,000-yr period..." Although the 2008 Draft EIS omits the phrase about "a threat to the integrity of the existing facilities over the next 1,000-yr period," it recognizes the general severity of the gully advance problem and provides the same underlying information as the earlier Draft EIS: More than 20 major and moderate-sized gullies have been identified... Some of these gullies have formed from natural gully advancement processes and others are the result of site activities.... Several of the gullies are active and migrating into the edge of the North and South Plateaus. (Page 3-36)

Major erosion processes affecting WNYNSC include...gully advance... (Page F-3) Gully advance is the third type of erosion process that results from local runoff and reflects soil characteristics. Gullies are most likely to form in areas along streambanks where slumps and deep fractures are present, seeps are flowing, and the toe of the slope intersects the outside of the meander loop. Gully growth is not a steadystate process; it occurs in response to episodic events, such as during thaws and after thunderstorms in areas where a concentrated stream of water flows over the side of a plateau, as well as in areas where groundwater pore pressure is high enough for seepage to promote grain-by-grain entrainment and removal of soil particles from the base of the gully scarp (a process sometimes known as "sapping"). Sapping causes small tunnels (or "pipes") to form in the soil at the gully base, which contributes to gully growth by undermining and weakening the scarp until it collapses. Surface water runoff into the gully also contributes to gully growth by removing fallen debris at the scarp base, undercutting side walls, and scouring the base of a head scarp. Although human-induced changes to the surface water drainage pattern can control the growth of some gullies, other natural processes that induce gully formation, such as the development of animal trails or tree falls, cannot be readily controlled. (Page F-4)

VEC.176. On pages F-14 and F-15 (esp. Table F-6), the 2008 Draft EIS lists *the same gully head advancement rates cited in the 1996 Draft EIS* (page L-5) for the SDA, NP3, and 006 gullies but offers no explanation why these rates would not constitute "a threat to the integrity of the existing facilities over the next 1,000-yr period..." as identified in the 1996 Draft EIS. The 2008 Draft EIS notes that remedial work has slowed the advance of the SDA gully, but it neither lists the slowed rate of advance nor discusses whether the slowed rate would revert to the original rate of 0.4 meters/year in the absence of long-term remedial measures.

VEC.177. Appendix H of the 2008 Draft EIS improperly dismisses gully head advancement as a serious threat to site integrity. The approach taken in Appendix H is flawed because it is based on two unreliable procedures or assumptions. First, the 2008 Draft EIS constructs and conducts modeling that "considers only erosion of the Low-Level Waste Treatment Facility on the North Plateau and of the SDA and NDA on the South Plateau" (page H-65), thereby eliminating from consideration any possible threat to the Main Plant Process Building, Vitrification Facility, and Waste Tank Farm. These important facilities were eliminated from consideration based on the the landscape evolution model used for the 2008 Draft EIS, which predicted very little erosion in those areas. However, as discussed elsewhere in these comments, the landscape evolution model is badly miscalibrated and cannot be used as a basis for ruling out erosion impacts to facilities such as the Main Plant Process Building, Vitrification Facility, and Waste Tank Farm. Second, section H.3.4 of the 2008 Draft EIS imposes an unsupported assumption that gully behavior consists of “an initial period of rapid growth followed by decrease in rate of growth, attainment of a maximum length” which can be expressed by a negative exponential relation termed Graf’s Law (page H-73). The 2008 Draft EIS cites only a single reference (Nachtergaele et al., “Medium-term evolution of a gully developed in a loess-derived soil,” *Geomorphology* **46**, 223 (2002)) for this overly simplistic idea which greatly limits, at least on paper, the ability of gullies to reach critical parts of the site. Even after introducing this simplistic idea, Table H-70 on page H-74 shows relatively high modeled doses from the NDA (either 170 mrem/yr or 45 mrem/yr) due to gully impacts, yet these doses would likely be much higher if DOE had not relied inappropriately on the Nachtergaele study. Quite simply, the Nachtergaele study and its finding of “an initial period of rapid growth followed by decrease in rate of growth, attainment of a maximum length” have little or no relevance to the West Valley site. The Nachtergaele study involved only a *single gully* that was monitored for 13 years (certainly not a robust basis for understanding the longterm behavior of gullies at the West Valley site!), and the Nachtergaele study area is a poor match for the West Valley site in terms of both the unconsolidated material being cut by gullies (loess vs. till/sand/gravel) and the topographic relief of the site. These factors are discussed below in more detail. [See VEC.178-182.]

<u>TASK 21</u>: Assess erosional sidecutting in relation to downcutting, stream-meander processes, and the slope stability angle of about 21 degrees

Context: Streams and rivers, unless structurally controlled or put into manmade channels, don't run straight; they meander. The slightest deviation from straightness initiates a self-perpetuating process, as the flowing water impinges on the outer bank (the so-called "cutbank"), thus removing sediments from that bank by plucking or abrasion, producing a progressively widening curve in the channel. As the flowing water erodes sediment from the toe of the sloping bank, the top of the bank retreats at about the same rate due to the 21-degree slope stability angle, thus approaching and eventually undercutting any facilities located atop the bank (e.g., waste burial grounds). The current EWG erosion models correctly account for the slope stability angle, but only in relation to erosional *downcutting* at the toe of the slope. They don't do so in relation to meander processes where the cutbank is progressively eroded by *sidecutting* at the toe of the slope.

VSC.70. Among the acknowledged limitations of the current EWG erosion models, one is sufficiently severe that it should immediately disqualify the models:

> None of the erosion models used in this study accounts for lateral erosion by streams, which can lead to valley widening. For this reason, the model projections do not address the possibility that the streams bounding the Site – Franks Creek, Quarry Creek, and Erdman Brook – could undergo valley-floor widening and thereby drive additional back-wearing of their valley walls.[142]

VEC.185. It is unclear whether and how the CHILD erosion model in the 2008 Draft EIS treats the ongoing evolution and/or adjustment of unstable hillslopes steeper than 21 degrees. This angle is widely recognized as a threshold beyond which slopes at the West Valley site are unstable or "potentially unstable." For example, see 2008 Draft EIS, page F-13; also A. Napoleon et al., *The Real Costs of Cleaning Up Nuclear Waste: A Full Cost Accounting of Cleanup Options for the West Valley Nuclear Site*, 2008, pp. 101-102. It appears that the CHILD erosion model assigns a value of 30 degrees to the threshold slope gradient parameter S_c (see 2008 Draft EIS, page F-46), where S_c is said to be equivalent to the parameter S_{0max} which represents a "maximum stable slope angle" in the SIBERIA model (see page F-42). In the event that S_c represents a "maximum stable slope angle" beyond which no soil creep or landsliding occurs in the CHILD erosion model, the assignment of a 30-degree value to S_c is patently wrong and must be corrected.

VEC.186. Figure 6.7 on page 102 of A. Napoleon et al., *The Real Costs of Cleaning Up Nuclear Waste: A Full Cost Accounting of Cleanup Options for the West Valley Nuclear Site*, shows the predictable evolution (i.e., westward migration) of the actively slumping west bank of Buttermilk Creek toward the incised confluence of Franks Creek and Erdman Brook and toward the nuclear waste facilities that lie beyond the confluence. This westward migration of the top of the west bank of Buttermilk can be predicted from slope stability criteria alone; it requires neither downcutting nor sidecutting of the actual channel of Buttermilk Creek (and thus would be

[142] Tucker et al., op. cit., at 215.

additive with any effects of further downcutting and sidecutting of that channel). The erosion model used in the 2008 Draft EIS *must be able to demonstrate* the above-described evolution (i.e., westward migration) of the west bank of Buttermilk Creek over some reasonable period of time. If the model cannot predict such westward migration of the actively slumping west bank of Buttermilk Creek, its inability to do so would be a clear indication that the model is either defective or operating with incorrect parameters. This is one of several crucial tests that the model must pass.

<u>TASK 22</u>: Assess erodibility of buried SDA and NDA waste and of existing backfill around tanks

Context: Sean Bennett of the EWG has measured the erodibility of native materials such as till and bedrock. Such measurements, or well-founded estimates based on comparable materials, are also needed for the buried waste mass in each SDA waste trench and NDA burial hole, and for the existing backfill around tanks 8D-1 through 8D-4 and their respective concrete vaults. Such information will aid in understanding the rate of erosion and rate of downstream radionuclide movement *once a trench, burial hole, or original tank excavation has been breached* by either gully-head advance or erosional sidecutting. While radionuclide escape from tanks and their vaults may be retarded somewhat if tanks/vaults are filled with grout, such information is needed to determine the rate at which ongoing gullying or sidecutting will progressively expose the vault walls and eventually reach the poorly protected underbelly of each vault. The contents of the SDA trenches and NDA burial holes, when progressively breached, are likely to be more erodible than the till and other glacial fill through which erosion had to proceed to reach them. Thus, the rate of erosion and radionuclide loss *within* each breached trench or hole is likely to be faster than the rate at which erosion *approaches* the trench or hole. To the extent that grout might be an effective SDA or NDA barrier (and would not be bypassed around its edges), this should be assessed as well.

<u>TASK 23</u>: Assess Cattaraugus-Buttermilk-Franks stream profiles (not concave-up; stable?) and also the effects of the currently proposed lowering of Springville Dam

It is well known that the longitudinal profiles of rivers, creeks, and streams evolve toward concave-upward configurations – or, stated otherwise, headwaters tend to have steeper gradients than the downstream reaches of a given watercourse.[143] In effect, concave-up is the stable profile into which rivers, creeks, and streams evolve, and watercourses not yet showing such a longitudinal profile will predictably evolve toward concave-up.

Cattaraugus, Buttermilk, and Franks Creeks do *not* exhibit concave-up profiles. For the latter two, this can be seen in the figure attached to Tasks 7 and 24, and the profile for Cattaraugus Creek is roughly similar. Such profiles are not surprising, since all three are likely young creeks (post-glacial, <14,000 years). For Cattaraugus and Franks, the transition toward concave-up during the next several thousand years – especially the steepening of headwaters and greatest deepening/downcutting in the middle reaches of these creeks – will inevitably deepen the ravines adjacent to the onsite waste burial grounds, and, probably on a longer time scale, lower the Buttermilk-Cattaraugus confluence. Such trends can be predicted, at least qualitatively, without resorting to modeling methods. Both trends contribute to the erosion risk facing the SDA and NDA burial grounds, especially in view of the downcutting-sidecutting relationship outlined in Task 21.

A related issue is the current plan by the U.S. Army Corps of Engineers, NYSDEC, and Erie County (NY) to reduce the height of Springville Dam, located on Cattaraugus Creek about 2 miles downstream from the Buttermilk-Cattaraugus confluence. This dam, almost 100 years old, acts as a durable (unchanging) knickpoint as long as it remains in place. As such, it stabilizes upstream water levels, including the Buttermilk-Cattaraugus confluence. If the dam is lowered as planned, its lowering will gradually (years or decades) lower upstream water levels, including the Buttermilk-Cattaraugus confluence, by several feet. In the longer term (centuries or millennia), the dam will likely be gone, and in any case its continued presence cannot be assumed due to nuclear regulatory policies that limit long-term reliance on institutional controls. If/when gone, the dam can no longer impede Cattaraugus Creek's long-term natural downcutting toward a more stable concave-up profile. Such downcutting will invalidate the erosion modeling assumption that the base level at the Buttermilk-Cattaraugus confluence remains constant over the millennia being modeled in the EWG modeling runs.

[143] For example, S.M. Mudd et al., "How concave are river channels?", *Earth Surf. Dynam.* **6**, 505–523 (2018); B.J. Zaprowski et al., "Climatic influences on profile concavity and river incision," *Journal of Geophysical Research: Earth Surface* **110**, Issue F3 (2005).

TASK 24: Assess beaver dams, formerly protective against erosion but limited by stream gradient, so may be unable to be equally protective in the future

Context: According to L.M. Gordon et al., "Hindcasting, forecasting, and controlling erosion at the Western New York Nuclear Service Center," NYSGA 2013 Field Trip Guidebook:

> Near the headwaters of many of Buttermilk's tributaries, there is a sharp transition from a deep V-shaped valley to a more broad U-shaped valley, which coincides with a change in the longitudinal profile of the stream to a gentler grade (Figure 8). In Erdman Brook and Frank's creek, this transition has generally mirrored the location of large knickpoints (Trip Stops 2 and 3, Figures 11 and 12). Upstream of the transition/knickpoints, the floodplains occupy a wide, flat valley bottom, and in many cases (typically in wetland areas), a defined stream channel is not evident. While the V-shaped reaches are incised in Lavery till, the upland U-shaped reaches have been filled with 1 m to 3 m of fine-grained sediment in the recent past, evidently by beaver dams (Figures 11 and 12). Beaver dams are common in the area and effectively result in deposition of large amounts of sediment in these upland stream reaches. Beaver dams/ponds also serve as a natural means of erosion protection, providing grade control and energy dissipation. In order to monitor and manage the streams in a stable condition, beavers (and their dams) have been removed from Frank's Creek and Erdman Brook since the development of the Center (1960s). In the absence of beaver dams to hold the deposited sediments in place, knickpoints moving upstream out of the V-shaped reaches have encountered the highly erodible deposits, and over the past ~50 years have incised more than 100 m of both Erdman Brook and Frank's Creek. As these knickpoints have moved closer to the radioactive waste disposal areas, the state and federal agencies managing the site have taken steps to control the erosion.

Steps taken by agencies to control erosion cannot be assumed to be available beyond 100 years due to regulatory limits on how long institutional controls can be assumed.[144] Thus, there are two issues here: 1) Can beavers resume their role of minimizing erosion? 2) Is the LEM's failure to distinguish between U- and V-shaped valleys a serious problem or inaccuracy in erosion modeling? The former is addressed here; the latter is addressed in Task 7.

[144] For SDA waste burial ground, see 2021 NYSDEC letter described in Task 33; also federal limits noted in Task 34 on how long institutional control can be assumed.

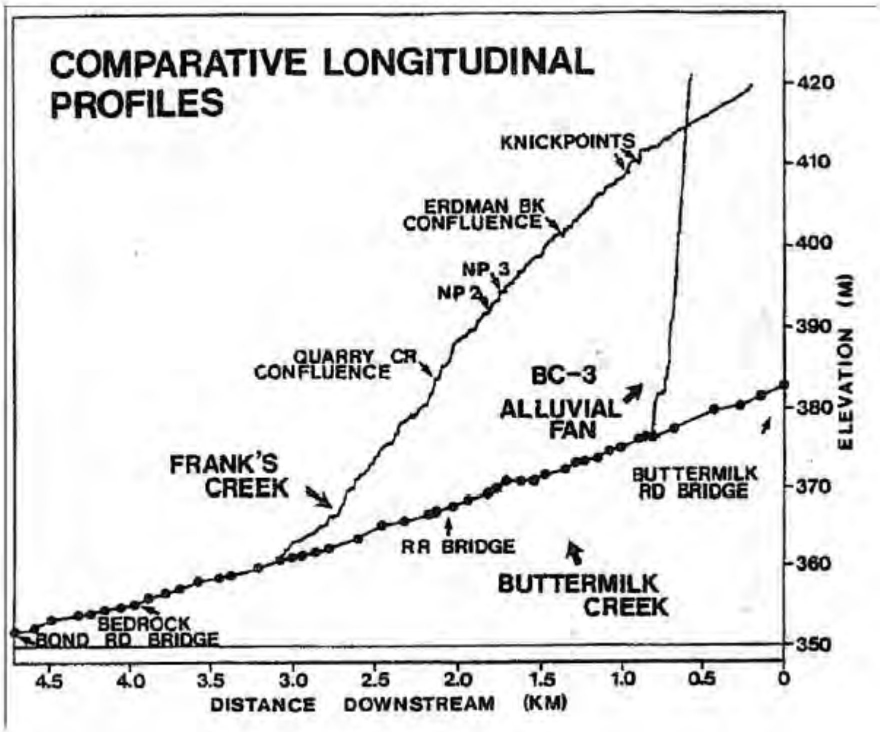

Figure 8 - Longitudinal profiles of Buttermilk Creek, Frank's Creek, and a small valley wall alluvial fan (Boothroyd et al., 1982)

Beavers prefer to build their dams on low-gradient streams, typically less than 6% and preferably lower.[145] The overall gradient of Franks Creek in the above figure ("Fig. 8") is about 2.2%, which is well within the gradient preferred by beavers. However, the upper reach of Franks Creek (above its confluence with Erdman Brook, thus adjacent to the SDA waste burial ground) will steepen over time as a result of the upstream knickpoint migration in combination with the gradual transition of Franks Creek to a concave-upward profile. As can be seen from the above figure ("Fig. 8"), the profile is not currently concave-up but, over time, will trend toward the more stable concave-up configuration that will inevitably steepen the upper reach adjacent to the SDA waste burial ground. What remains unclear is the length of time before geomorphic steepening of Franks Creek will prevail over the erosion suppression carried out by beavers. This may be too speculative to be a productive task for experts.

[145] E. Wohl et al., *Managing for large wood and beaver dams in stream corridors*, Gen. Tech. Rep. 404. Fort Collins, CO: USDA/USFS Rocky Mountain Research Station; also B.J. Dittbrenner et al., 2018, *Modeling intrinsic potential for beaver* (Castor canadensis) *habitat to inform restoration and climate change adaptation*, PLoS ONE 13(2): e0192538 (2018), https://doi.org/10.1371/journal.pone.0192538; also M.M. Pollock et al., *Hydrologic and Geomorphic Effects of Beaver Dams and Their Influence on Influence on Fishes*, American Fisheries Society Symposium 37 (2003), https://www.researchgate.net/publication/231218389.

<u>TASK 25</u>: Recognize and assess the bedrock valley groundwater system, its approximate flow & storage properties, and its relationship to (and groundwater divide(s) from) surficial North & South Plateau units, till sand, Kent recessional, and other onsite water-bearing units

VEC.232. The FEIS has started to respond in a meaningful way to my comments 11, 50, 51, etc., regarding the bedrock-valley aquifer and its relationship to surface recharge and surface contamination – but more work is needed to support Phase 2 decisionmaking. The information outlined on pages 3-64 and 3-71 to 3-72 of Volume 1 and in Appendix E of Volume 2 is a good start, but a more comprehensive characterization of the water budget and flow pathways is needed. A study completed in 2001 in a roughly comparable bedrock valley (Yager et al., Simulated effects of salt-mine collapse on ground-water flow and land subsidence in a glacial aquifer system, Livingston County, New York, USGS Professional Paper 1611) would serve as a good example. A meeting or technical workshop may be a useful first step toward such characterization or study.

VSC.22. For any alternative that leaves or stores waste on any bedrock portion of the site which serves as a recharge area for the underlying bedrock-valley aquifer(s), (e.g., west of Rock Springs Road and some portions of the site east of Buttermilk Creek), the SEIS process would need to include studies to characterize the underlying bedrock-valley aquifer(s), and the Draft SEIS would need to assess impacts to such aquifer(s).[146] Current characterization of this/these aquifer(s) is too sparse to support waste storage or disposal within bedrock portions of the site that serve as recharge areas.

VSM.8: The CSM's [Conceptual Site Model's] treatment of groundwater hydrology on p. 96 and elsewhere is overly simplistic; it provides no meaningful assessment of subsurface flow into the bedrock valley and along its thalweg. The CSM should acknowledge my report on the bedrock valley[147] and provide at least a defensible qualitative treatment of such flow. Such a treatment should, in part, reconcile the mismatch between CSM Fig. 44 and CSM Fig. 51, where the flow arrows in the latter figure show flow through fractured/decomposed bedrock into the thalweg (and show no exit flow, implying lateral/northward flow along the thalweg?) while flow arrows in the highly generalized Fig. 44 show no such flow.[148] Ideally, the CSM would go beyond my 25-year-old report on the bedrock valley and address newer information such as pumping test data collected during installation of the site's current potable-water system. Such data appear to show a large flow rate of groundwater diving into the bedrock valley, with the source, sink, and other details of this flow remaining poorly characterized.

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[146] See R.C. Vaughan, "Geologic and Hydrologic Implications of the Buried Bedrock Valley that Extends from the Western New York Nuclear Service Center into Erie County, N.Y.", in *Geology Reports of the Coalition on West Valley Nuclear Wastes* (East Concord, NY, 1994), available online at http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf, at pp. 180-207 of the pdf file. See also **VEC.50-56**.

[147] R.C. Vaughan, "Geologic and Hydrologic Implications of the Buried Bedrock Valley" report, op. cit.

[148] Disagreement between Figs. 44 and 51 is noted, but the uncertainty is not resolved, on pp. 112-13 of the CSM.

<u>TASK 26</u>: Resolve mismatch between DOE's potable-water-system modeling and the SEIS characterization of the bedrock valley system; establish a plausible water budget

VWL.1a. Assessment is needed for the *hydrology of the aquifer system* that is tapped by the two groundwater wells from which much of the proposed withdrawal will occur. The aquifer system tapped by the two groundwater wells is poorly understood; it needs better characterization in order to protect local groundwater resources and to assess the effects of the high proposed rate of withdrawal on adjacent areas such as the North and South Plateaus of the Western New York Nuclear Service Center (the West Valley nuclear waste site) where the waste tank farm, waste burial trenches, etc., are located. As described below in my new comment no. 3, the supporting documentation for the U.S. Dept. of Energy's permit application does not provide the needed assessment. The needed hydrologic assessment should at least consist of:

i) Identification and quantification of the source(s), i.e., the recharge area(s) and recharge rate, for the aquifer tapped by the two groundwater wells.

ii) Identification and quantification of the sink(s) to which water in the aquifer is flowing, including documented identification of any locations where such water "daylights" or discharges to surface waterways, along with associated flow rates, and including a reasonably documented water budget. Such identification and quantification should include and compare three different flow conditions: The baseline case of no water withdrawal from the two groundwater wells, an extreme case at the maximum rate of withdrawal allowed by the requested permit, and an intermediate rate corresponding to the maximum rate of current withdrawal.

iii) Characterization of the water in the aquifer tapped by the two groundwater wells, including age-dating the water by tritium analysis and geochemical characterization by major ions and stable isotopes (deuterium and oxygen-18).

iv) Based on the above information, a determination of whether the requested groundwater withdrawal is sustainable, or, alternatively, whether the aquifer will be progressively depleted by the withdrawal.

v) Groundwater head mapping of the aquifer, not only in the immediate vicinity of the two withdrawal wells but upgradient and downgradient from the wells, under three different flow conditions: The baseline case of no water withdrawal from the two wells, an extreme case at the maximum rate of withdrawal allowed by the requested permit, and an intermediate rate corresponding to the maximum rate of current withdrawal.

vi) Determination of whether the aquifer is confined or unconfined, and determination and quantification of whether the groundwater head varies seasonally.

vii) Determination and quantification (including flow rates) of hydrologic connections between the aquifer tapped by the two groundwater wells and the Kent Recessional, Till Sand, and other permeable units that lie beneath the North and/or South Plateau of the Western New York Nuclear Service Center. As part of the information needed to support this determination and quantification, the groundwater head maps created in accordance with 1(a)(v) should include the North and South Plateaus.

VWL.3. While the supporting documentation for the U.S. Dept. of Energy's permit application may appear at a quick glance to provide a good assessment of the hydrology of the aquifer system tapped by the two groundwater wells, it does not do so. It relies on unsupported assumptions that are contradicted by other site-specific work, and thus does not provide a defensible hydrologic assessment. The aquifer system tapped by the two groundwater wells remains poorly understood; it needs better characterization in order to protect local groundwater resources and to assess the effects of the proposed withdrawal rate on adjacent areas such as the North and South Plateaus of the West Valley site where the waste tank farm, waste burial trenches, etc., are located.

VWL.3a. The primary defect in the hydrologic documentation submitted with the U.S. Dept. of Energy's permit application is its adoption of unrealistic values for both *thickness* and *hydraulic conductivity* of the weathered bedrock aquifer. The deviation of the weathered-bedrock hydraulic conductivity by two orders of magnitude from generally accepted (and model- and data-supported) values is acknowledged but not discussed or justified. The deviation of the weathered-bedrock thickness by a factor of 20 from generally accepted (and model- and data-supported) values is neither acknowledged nor discussed. When these unrealistic assumptions are fed into the groundwater model submitted with the U.S. Dept. of Energy's permit application, they "show" a modeled well yield that is orders of magnitude greater than could be obtained with the generally accepted model input values.

VWL.3b. The generally accepted thickness of the water-transmissive fractured bedrock is 10 feet, but the thickness assumed for the hydrologic model submitted with the U.S. Dept. of Energy's permit application is 200 feet. The generally accepted hydraulic conductivity of the water-transmissive fractured bedrock is about 0.3 in/day or 0.03 ft/day, but the conductivity assumed for the hydrologic model submitted with the U.S. Dept. of Energy's permit application is about 100 times greater, 3 ft/day. These discrepancies can be seen by comparing text and figures submitted with the U.S. Dept. of Energy's permit application with text from the Dept. of Energy's environmental impact statement (EIS) documents:

> "For purpose of groundwater modeling, a value of 3 feet/day was selected to represent the hydraulic conductivity of the bedrock. This hydraulic conductivity value is approximately two orders of magnitude higher than the hydraulic conductivity value selected for bedrock in other modeling completed for the WVDP (Table E-3, Appendix E – Geohydrological Analyses, Final Environmental Impact Analysis, January 2010)."
>
> GEI report dated July 31, 2014, "Pumping Test Data Analysis: Water Supply Well #1 and Well #2 West Valley Demonstration Project," submitted with the U.S. Dept. of Energy's instant permit application, Appendix E, pdf page 348.

> "It was assumed that predominant groundwater flow occurs only within the upper 200 feet of fractured bedrock."
>
> GEI report dated August 19, 2014, "Geohydrologic Assessment of New Bedrock Groundwater Supply Wells West Valley Demonstration Project," submitted with the U.S.

Dept. of Energy's instant permit application, Appendix F, pdf page 408. See also ibid., Figs. 4, 5, and 6, showing the model's assumed 200-foot-thick fracture flow zone and its assumed hydraulic conductivities.

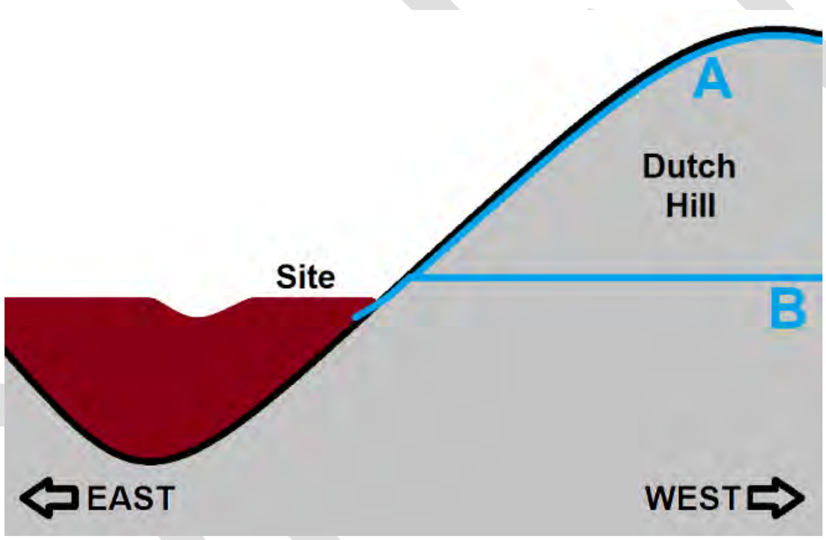
"… Regional groundwater in the bedrock tends to flow downward within the higher hills, laterally beneath lower hillsides and terraces, and upward near major streams. The upper 3 meters (10 feet) of bedrock has been both mechanically and chemically weathered and contains abundant fractures and decomposed rock, which makes this layer more hydraulically transmissive than the underlying competent bedrock. Hydraulic conductivity in the weathered zone has been estimated at 1×10^{-5} centimeters per second (0.3 inches per day). Wells completed in this zone yield 40 to 60 liters per minute (10.6 to 15.9 gallons per minute) and correspond to the regional bedrock aquifer. The hydraulic conductivity of the underlying competent rock has been estimated at 1×10^{-7} centimeters per second (0.003 inches per day). The difference in conductivities between these two zones suggests preferential flow through the weathered portion, which would be directed downslope within the weathered zone toward the axis of the buried valley underlying the WNYNSC (WVNS 1993d, WVNS and Dames and Moore 1997)."

West Valley FEIS (DOE/EIS-0226, January 2010), section 3.6.2.1, Hydrostratigraphy of the North and South Plateaus, Bedrock Unit, page 3-64. See generally ibid., Chapter 3 and Appendix E.

VWL.3c. As stated above in 3(a), "When these unrealistic assumptions are fed into the groundwater model submitted with the U.S. Dept. of Energy's permit application, they 'show' a modeled well yield that is orders of magnitude greater than could be obtained with the generally accepted model input values." Granted, the modeled well yield corresponds to a measured yield obtained from the pumping tests submitted with the U.S. Dept. of Energy's permit application – so the yield itself, aside from the question of its sustainability, cannot be called unrealistic. What is unrealistic is what the model purports to show, namely, sources and sinks that are identified and quantified. The aquifer characterization is deficient; it needs to include age-dating, geochemical characterization, whether the yield is sustainable or will gradually deplete the aquifer, etc.

VWL.3d. Such uncertainties are illustrated by the diagram below, which shows two possible configurations of the aquifer tapped by the two groundwater wells. The hydrologic documentation submitted with the U.S. Dept. of Energy's permit application assumes a shallow aquifer on the east-facing slope of Dutch Hill, as sketched below in blue and marked "A", but, alternatively, the aquifer may exist primarily within bedding-plane openings in a permeable sand- or siltstone bed, indicated by the blue line marked "B", that lies within the gray shales of Dutch Hill. One candidate for such a bed outcrops in Quarry Creek, where it forms the caprock of the waterfall west of Rock Springs Road.[149] Whether the aquifer is configured primarily as "A" or "B" needs to be assessed. These two possible configurations are very different with respect to source, recharge, age of the water, sustainability vs. depletion, etc.

VWL.3e. Both possible configurations remain uncertain with respect to the aquifer's discharge or "sink" within the bedrock valley. The documentation submitted with the U.S. Dept. of Energy's permit application offers the vague suggestion that "Groundwater in bedrock is expected to ultimately discharge farther downgradient where bedrock is exposed in the direction of Buttermilk Creek and Cattaraugus Creek" – but additional field work is needed to determine whether and where the natural discharge flow from the aquifer returns to Buttermilk and Cattaraugus Creeks (or whether it goes mainly to the thalweg of the bedrock valley), and to create a defensible water budget. Such work is needed in order to understand the downgradient loss (i.e., impact) of the water withdrawn from the two groundwater wells.



[149] This sand/siltstone bed may be laterally continuous. See comment no. 102 of my scoping comments submitted to U.S. Dept. of Energy on May 23, 2018. Copy available upon request – or may be available from U.S. Dept. of Energy.

TASK 27: Assess relevance of temperature excursions in Kelview and other nearby gas wells

The temperature log for the Kelview gas well (API no. 31-009-11723, located near the intersection of Rock Springs/Schwartz and Dutch Hill/Edies Roads) shows a large downward temperature excursion at about 620 ft depth (about 770 ft elevation). The log shows the temperature at that depth falling abruptly to 47.3° from a nearly uniform temperature of 55°-56°. Such temperature excursions are typically caused by a fluid such as water or gas flowing into a well from a fracture that intersects the well. Given the Kelview well's proximity to the West Valley site, the fracture and its connection to other fluid-bearing fractures should be investigated and characterized. A mile further west, another well (Blesy, 31-009-11689) shows a small temperature excursion at 808 ft depth (497 ft elevation) and should be included in the fracture characterization. Known faults in the immediate vicinity, such as the deep "Intersection Feature" fault identified in Bay Geophysical (2001) and the three faults seen in outcrop slightly above 1100 ft elevation,[150] should also be included in such characterization.

[150] See R.C. Vaughan et al., "Confirmation of Anomalous Westward Dip Between Springville and West Valley, N.Y.", 1993, included in *Geology Reports of the Coalition on West Valley Nuclear Wastes*" (East Concord, NY: CWVNW, 1994), where faults are noted in text and mapped in Fig. 1.

<u>TASK 28</u>: Assess relevance of the Cattaraugus Creek sole source aquifer

See 52 Federal Register 36100-02, September 25, 1987 (available online at https://www.govinfo.gov/content/pkg/FR-1987-09-25/pdf/FR-1987-09-25.pdf, pages 36100-36102) for details; also https://www.nrc.gov/docs/ML1408/ML14086A522.pdf for EPA's fact sheet and map for the designated sole source aquifers, including the Cattaraugus Creek Basin Aquifer, that are within EPA Region 2.

<u>TASK 29</u>: Assess stratigraphy of sand/siltstone bed at Quarry Creek waterfall, its hydrology and lateral continuity, and water encountered at 147 ft depth in well 4113

The Miller gas well (31-009-04113), drilled in 1911 on what is now the WNY Nuclear Service Center, hit fresh water at 147 ft depth (1463 ft elevation).[151] Based on unpublished stratigraphic research by Vaughan, this is the same horizon as the fractured sandstone/siltstone bed that forms the capstone of the Quarry Creek waterfall upstream from Rock Springs Road, and also the same horizon seen in other local outcrop, all designated "Ledge A" by Vaughan. Regardless of Vaughan's interpretation, and regardless of whether or not there is hydrologic continuity between the Miller well and the Quarry Creek waterfall, there is a water-bearing bed at *each* of these two locations that needs to be understood in relation to site hydrology, especially in relation to the aquifer that now provides the site's potable water, the exceptionally high flow rate of groundwater within this aquifer (see Task 26), and the groundwater divide between this aquifer and the groundwater system(s) of the North and South Plateaus. See also Task 25.

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[151] W.L. Kreidler, *Selected Deep Wells and Areas of Gas Production in Western New York*, NYS Museum Bulletin, Vol. 390, 1963.

__**TASK 30**__: Assess the pervasive near-vertical bedrock fractures & low RQD in onsite borings (*cf.* my 2010 CTF presentation, etc.)

VEC.83. The EIS should acknowledge, compile, and address the evidence for the degree of fracturing of bedrock under the West Valley site. Near-surface bedrock in Western New York is often fractured, but rarely to the degree found under the site. The main evidence is the frequency (nearly 100%) with which test bores that penetrate more than 10 feet into bedrock have encountered vertical or high-angle fractures. It is unusual for vertical or high-angle fractures in bedrock to be so closely spaced that virtually every vertical bore encounters them. Borings that encountered vertical and/or high-angle fractures include the NX#1 core well (31-009-06740) that was drilled as part of the 1969-1971 ORNL injection-well test program; the Dames & Moore 74-DMB36, 74-DMB37, and 74-DMB42 wells; and the Dames & Moore HLW-1 well.

VEC.84. The Dames & Moore report on the HLW-1 well, for example, contains the following description: "Weathered bedrock was encountered in boring HLW-1 at approximately 110.5 feet below ground surface. More competent rock was encountered at 118.5 feet. Rock core obtained from 118.5 through 149.5 feet possessed low RQD values (i.e., 0 to 16%) and was described as very broken to broken showing slight to moderate degrees of weathering." (From page C-5 of Geotechnical Investigation, High Level Waste Transfer System, West Valley Demonstration Project, Dames & Moore, August 24, 1992.)

VEC.85. A summary of the evidence for, probable cause of, and future implications of the highly fractured bedrock should be given in the EIS. This is part of the site description and site evaluation needed for an informed decision.

<u>TASK 31</u>: Assess the Clarendon-Linden Fault and the branch of that fault known as the Attica Splay, esp. in relation to the Sardinia Feature reported in Bay Geophysical (2001)

Context: The 2010 EIS, page 3-27, acknowledges that "The Attica Splay is the most active portion of the Clarendon-Linden Fault Zone," refers to "A seismic reflection survey completed in June 2001 (line WVN-1…)," and notes that "The Sardinia faults [i.e., the faults called the "Sardinia Feature" on line WVN-1] may represent the southwest continuation of the Attica Splay into southeastern Erie County. A thin band of northeast-trending lineaments that extends from Batavia, New York, and past Sardinia into Erie County may represent the surface expression of the Attica Splay…" See next page for the map (Fig. 3-12) from the 2010 EIS that shows the Clarendon-Linden Fault, the Attica Splay, associated lineaments, etc. Additional discussion in Section 3.5 of the 2010 EIS focuses on future quakes at/near Attica rather than reactivation of the Attica Splay in the vicinity of Sardinia and West Valley.

CSC.45 and VSC.72. Evidence of two deep-seated faults – one at Sardinia and one at the north end of the US 219 bridge over Cattaraugus Creek near Springville – was released in 2001 in the Bay Geophysical seismic study,[152] but no follow-up work has been done to identify or clarify the strike of these faults, their geographic extent, their surface expression (if any), and their likelihood of reactivation. Such follow-up investigation is needed in the SEIS process in order to understand long-term seismic risks to site stability and containment integrity.[153]

CSC.46 and VSC.73. The role of the seismically active Attica Splay of the Clarendon-Linden Fault needs to be understood.[154] The Sardinia fault identified by the Bay Geophysical seismic survey is particularly relevant because it is aligned with, and may be part of, the seismically active Attica Splay. The SEIS process needs to investigate and determine whether the Sardinia fault connects with the Attica Spay at/near Varysburg and also needs to investigate and determine whether it extends southwestward toward the West Valley site – and if so, how closely it approaches the site.

VSM.4: The CSM's [Conceptual Site Model's] treatment of fault systems and seismicity on pp. 73-81 fails to acknowledge and consider the evidence of two deep-seated faults – one at Sardinia and one at the north end of the US 219 bridge over Cattaraugus Creek near Springville – that was released in 2001 in the Bay Geophysical seismic study.[155] While no follow-up work has been done to identify or clarify the strike of these faults, their geographic extent, surface expression (if any), and likelihood of reactivation, they pose long-term seismic risks to site stability and containment integrity. As such, they need to be acknowledged in the CSM and included in the probabilistic performance assessments and Supplemental EIS process.[156] The Sardinia Fault identified in the Bay Geophysical seismic study is particularly relevant because it is on strike

[152] Bay Geophysical, *Seismic Reflection Survey to Identify Subsurface Faults near the West Valley Demonstration Project*, report prepared for West Valley Nuclear Services Company (Traverse City, MI: Bay Geophysical, 2001).
[153] Vaughan EIS comment § 57A.
[154] Vaughan EIS comments §§ 57A, 82, and 88.
[155] Bay Geophysical, op. cit.
[156] Vaughan EIS comments §§ 57A, 82, and 88.

with, and probably part of, the seismically active Attica Splay of the Clarendon-Linden Fault. The Attica Splay is known to extend at least to Varysburg, NY, and a line projected along the fault trace passes approximately through Sardinia at a distance of 18 miles beyond Varysburg, as is evident from CSM Fig. 32. Beyond Sardinia, such a line continues south-southwestward and passes three or four miles east of the West Valley site. Whether the Attica Splay extends this far south remains unknown, but the apparent association between the Sardinia Fault and Attica Splay demonstrates a need for either additional geophysical investigation or a probabilistic representation of the fault's uncertain extent and seismicity.

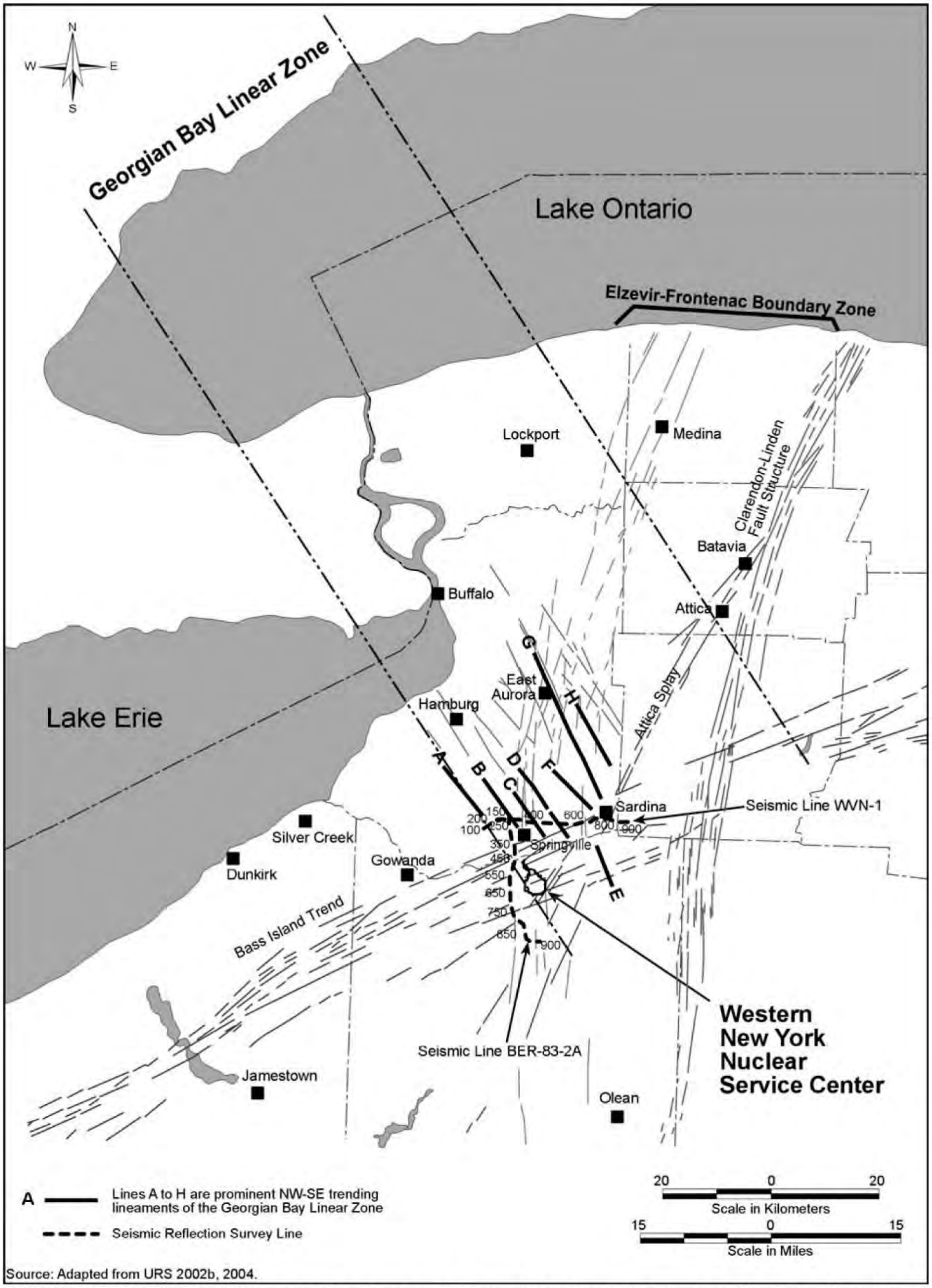

Figure 3–12 Selected Lineament Systems and Major Structural Features in Western New York

Source: 2010 EIS

<u>TASK 32</u>: Assess earthquake & liquefaction risk, and how such processes interact with and augment erosion and slumping/landsliding

Context: Earthquakes pose a risk to slope stability, thereby impacting the same valley walls, ravine walls, and gully walls that are subject to erosion and slumping. Investigation by Tuttle looked for evidence of liquefaction from past earthquakes, and, finding no clearcut examples of such liquefaction, assigned an upper limit to the magnitude of quakes that have affected the West Valley site and may affect it in the future.[157] There is, however, an important caveat or logical flaw regarding Tuttle's work. To the extent that her work looked for evidence of liquefaction in the aforesaid valley walls, ravine walls, and gully walls that are subject to erosion and slumping, she may have been a few centuries or millennia too late to see such evidence – meaning that the evidence may have "peeled away" as part of the erosion and slumping that are pervasive in the hilly terrain of Western New York. Thus, Tuttle's work remains relevant within its limitations but is not a definitive work on seismic magnitude and liquefaction.

CSC.47 and VSC.74. Earthquakes pose a risk to slope stability. Extreme examples were seen in the 1964 Alaska earthquake,[158] while quakes of lesser magnitude will have similar but less dramatic effects on unstable or quasi-stable slopes. Relevant slopes at the West Valley site include the same valley walls, ravine walls, and gully walls that are subject to erosion and slumping. Thus, given the fact that seismic events will accelerate the overall loss of site integrity by causing large-scale landsliding, slumping, and mass wasting,[159] and given the acknowledged lack of any seismic component in the EWG erosion modeling runs,[160] *those erosion modeling runs need to be re-done with intermittent (probabilistic) seismic "jumps" incorporated into the models.*

CSC.48 and VSC.75. An example of how seismic effects on slope stability can be modeled can be found in Appendix C-5 of an engineering report for the proposed expansion of a hazardous waste facility.[161] Applying such a model to slopes on the West Valley site would require site-specific values for soils and glacial fill materials, and would also require site-specific seismic information based on characterization of the Sardinia Fault, its relation to the Attica Splay, and other fault structures in the vicinity of the site.

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[157] M.P. Tuttle et al., *Paleoliquefaction Study along the Clarendon-Linden Fault System*, Final Report, Palisades, NY (May 10, 1995); M.P. Tuttle et al., "Seismic Hazard Implications of a Paleoliquefaction Study Along the Clarendon-Linden Fault System in Western New York State," *Geological Society of America, Abstracts*, Vol. 28, No. 3, p. 106 (February 1996); M.P. Tuttle et al., "Paleoliquefaction Study of the Clarendon-Linden Fault System, Western New York State," *Tectonophysics* **353**, 263-286 (2002). See also discussion in 2010 EIS, pages 3-43 and 3-47.

[158] For example, see W.R. Hansen, "Effects at Anchorage," in *The Great Alaska Earthquake of 1964* (Washington: National Academy of Sciences, 1971), available online at http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf , at pp. 30-140 of the pdf file.

[159] Vaughan EIS comments §§ 103-04.

[160] Tucker et al., op. cit., at 216.

[161] Arcadis, RMU-2 Engineering Report (Rev. Nov. 2013), http://modelcity.wm.com/RMU/06-RMU-2%20Engineering_Report_Revised_November_2013.pdf .

CSC.49 and VSC.76. Soil liquefaction may in some cases contribute to seismically induced slope failures; however, in other cases a slump-prone slope may fail in an abruptly accelerated episode of slumping without observable liquefaction. In any case, liquefaction of onsite soils adjacent to existing slopes needs to be investigated in the SEIS process and incorporated into landscape-evolution modeling.[162]

VCM.5: Soil liquefaction is improperly dismissed in the CSM [Conceptual Site Model] (p. 81) without due consideration of the evidence. The CSM fails to acknowledge and consider the report[163] wherein I review the evidence of unstable soils and liquefaction and, based on this evidence, conclude that "at least one sediment layer is somewhat susceptible to seismically-induced liquefaction, and...results are not yet available for the lacustrine sediments that lie beneath a large portion of the site."[164] (The CSM cites a critique[165] of my report but not the report itself!) A better evaluation of soil liquefaction is needed before it can be ruled out in the CSM.

VCT.[1]. Effects on slope stability are a primary reason why seismic activity needs to be well understood at the West Valley site. The site is highly susceptible to both erosion and slope failures (for example, see [the 1996 Draft EIS], including Figures 4-13 and L-1; Albanese et al. 1984; Boothroyd et al. 1979; Boothroyd et al. 1982); however, seismically-induced slope failures have not been adequately addressed (see Vaughan 1996) and cannot be reliably addressed without a better understanding of faults, seismicity, and recurrence intervals in the immediate vicinity of the West Valley site. The connection between slope failures (i.e., landslides of various types) and earthquakes is well-known (Sidle et al. 1985; Keller 1985). The 1964 Alaska earthquake produced slope failures on a massive scale (see photos in Hansen 1971, attached hereto as Appendix C), but smaller seismic events may also cause failures on susceptible slopes. Sidle et al. (1985) note that, "For most landslides that were initiated by earthquakes, the direct physical and mechanical influence of the ground motions appeared sufficient to generate failures on slopes that were in a delicate state of balance." The "delicate state of balance" criterion seems to be met at the West Valley site, given the many slope failures that are presently occurring at the site without additional seismic enhancement (DOE and NYSERDA 1996, including Figures 4-13

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[162] See especially R.C. Vaughan, "Geologic and Hydrologic Implications of the Buried Bedrock Valley…", op. cit., available online at http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf , esp. pp. 203-207 of the pdf file.

[163] See R.C. Vaughan, "Geologic and Hydrologic Implications of the Buried Bedrock Valley that Extends from the Western New York Nuclear Service Center into Erie County, N.Y.", in *Geology Reports of the Coalition on West Valley Nuclear Wastes* (East Concord, NY, 1994), available online at http://www.westvalleyctf.org/2008_Materials/2008-01-Materials/Core_Team_Issues-Vaughan_with_Appendices.pdf, at pp. 180-207 of the pdf file. See also Vaughan EIS comments §§ 50-56.

[164] R.C. Vaughan, "Geologic and Hydrologic Implications of the Buried Bedrock Valley" report, op. cit., p. 11 (*Geology Reports*, p. 72; pdf page 191).

[165] Z.Z. Zadins, *A Hydrogeologic Evaluation of "Geologic and Hydrologic Implication of the Buried Bedrock Valley that Extends from the Western New York Nuclear Service Center into Erie County, NY,"* Dames & Moore technical report, prepared for DOE and West Valley Nuclear Services Co. (August 1997).

and L-1; Albanese et al. 1984; Boothroyd et al. 1979; Boothroyd et al. 1982). The main impact of seismically induced slope failures at the West Valley site under loss-of-institutional-control scenarios would be an intermittent acceleration of the backcutting of ravine edges. Such accelerated backcutting of ravine edges would expose buried wastes more rapidly than under normal erosional conditions. Slope failures elsewhere in western New York State, including locations close to the West Valley site, may be instructive. See discussion in Vaughan (1994), including information on unstable lacustrine sediments (similar to quick clays) observed north of Springville, NY, by Owens et al. See also Gephart-Ripstein (1990) for a review, based on anecdotal and historical sources, of earthquakes and slope failures in Wyoming County, NY. Some of the earthquakes cited there are apparently not listed in standard modern earthquake catalogs. The slope failures reviewed by Gephart-Ripstein (1990), typically involving 6 to 20 acres, are not explicitly linked to earthquakes yet are of interest because some are located along the Tonawanda Creek valley between Attica and Varysburg, NY. This creek valley, apparently structurally controlled, follows the Attica Splay of the Clarendon-Linden Fault (see Fakundiny et al. 1978; Fakundiny and Pomeroy 2002). Despite the lack of explicit linkage to seismicity, the slope failures may be fault-related if the fault serves as a conduit for fluid flow.

<u>TASK 33</u>: Assess how the SDA will be evaluated against criteria (mostly 6 NYCRR 382-83) as required by DEC in 2/3/2020 Daniel Evans letter

Context: DEC's requirements for SDA closure, developed by Tim Rice and sent to NYSERDA in a 2/3/2020 letter signed by Daniel Evans, notes that:

> Regulatory requirements for closure of a radioactive waste disposal site are fundamentally different from license termination or remediation of a licensed facility in that disposal site regulatory requirements are based on the expectation that such a facility is intended to be the final disposal location for the waste contained therein, and will be closed in-place rather than remediated and closed through a license termination process.

This means that a disposal facility licensed under either DEC's 6 NYCRR 382-83 regulations or the similar federal regulations (10 CFR 61) has been given a license *based on those regulations being met.* A facility licensed under those regulations is meant to be safely closeable-in-place. But the SDA burials weren't licensed under either 10 CFR 61 or 6 NYCRR 382-83. Thus, DEC has no expectation that the SDA is intended to be the final disposal location for the waste contained therein, and no expectation that the SDA will be closed in place rather than remediated and closed.

DEC expects that unrestricted release of the entire West Valley site will be one of the alternatives considered in the SEIS. If unrestricted release is the preferred alternative, then the SEIS will need to show that NRC's LTR unrestricted release standard (25 mrem/yr) is met.

However, if unrestricted release of the entire West Valley site is *not* the preferred alternative, then DEC says that the assessment of SDA closure alternatives (in the SEIS) will need to consider the NYS radiological performance assessment (PA) criteria such as those listed below. Even though DEC's current regulations aren't directly applicable to pre-existing disposal sites such as the SDA, DEC says the requirements of 6 NYCRR 382-83 could be incorporated into a 6 NYCRR 380 permit[166] as deemed appropriate by DEC. Specifically, DEC says that the following PA requirements from 6 NYCRR 382-83 "should be utilized to evaluate the SEIS closure alternatives for the SDA:

- 382.12, protection of individuals from inadvertent intrusion into the waste mass;
- 382.14, stability of the disposal site after closure;
- 383-3.4(h)(2)(i), the analysis period must include dose assessments for a period of 10,000 years unless justification for a shorter time period is provided for consideration;
- 383-3.4(h)(2)(ii), the assessment *must not* rely on an institutional control period of greater than 100 years; and

[166] As noted in DEC's letter, the SDA currently has a 6 NYCRR 380 permit from DEC for maintenance of the disposal site.

- 383-3.4(h)(2)(iv), requirements for the analysis of long-term post-closure site stability, including the need to provide reasonable assurance that there will not be a need for ongoing active maintenance of the site."

Furthermore, if unrestricted release of the entire site is not the closure alternative selected through the SEIS process, then "the long-term dose attributable to the SDA[167] to site occupants, inadvertent intruders or offsite members of the public *must not exceed*" the 6 NYCRR 382.11 requirements (25-75-25 mrem/yr) or the NRC's TEDE requirement (25 mrem/yr) which DEC considers equivalent. ALARA ("As Low As Reasonably Achievable") requirements also apply.

While not specifically listed in the DEC letter, the following requirements from 6 NYCRR 383 may also warrant consideration for the SDA:

- 363-5.1(a) requirements relating to bedrock and unconsolidated deposits;
- 363-5.1(f)(1) requirements relating to areas having an active or substantial probability of mass movement where the movement of earth material at, beneath, or adjacent to the landfill may result in downslope transport of soil or rock by means of gravitational influence (i.e., slumping, landsliding);
- 363-5.1(h) requirements related to fault areas; and
- 363-5.1(i) requirements applicable to seismic impact zones.

[167] This would apparently allow 25 mrem/yr from the SDA plus 25 mrem/yr from the rest of the site, totaling up to 50 mrem/yr.

<u>TASK 34</u>: Assess how the Project Premises will be evaluated against criteria (10 CFR 61.50 and 61.51) as required by Stipulation of Compromise Settlement

Context: DEC's closure requirements for the SDA (see Task 33) are NYS requirements that don't apply directly to DOE. However, since DOE and NYSERDA are said to be committed to a coordinated site closure process based on uniform (or at least compatible) requirements, these may affect DOE as well.

And it's important to note that one of the provisions of the Stipulation of Compromise Settlement also applies. It requires DOE to perform certain evaluations as part of the EIS process:

> 8. While this agreement will not in and of itself subject the Department of Energy to formal NRC procedures, nor to actions required by law for licensed activities, it is hereby agreed that every good-faith effort shall be made to evaluate the site and the design(s) relative to the provisions of 10 C.F.R. §61.50 and §61.51….

Referring back to Task 33, note that some of DEC's SDA requirements are similar to the 10 CFR 61 provisions that DOE has agreed to use for evaluating the site and design(s):[168]

- 6 NYCRR 382.12, protection of individuals from inadvertent intrusion into the waste mass. This doesn't fall within the 10 CFR 61.50 and 61.51 provisions covered by the Stipulation but is instead matched to 10 CFR 61.42, *Protection of individuals from inadvertent intrusion*. It's generally consistent with NRC radiation protection requirements.

- 6 NYCRR 382.14, stability of the disposal site after closure. This is linked to both 10 CFR 61.44, *Stability of the disposal site after closure*, and 10 CFR 61.50, *Disposal site suitability requirements for land disposal*. See, for example, 10 CFR 61.50(a)(10) which says that "Areas must be avoided where surface geologic processes such as mass wasting, erosion, slumping, landsliding, or weathering occur with such frequency and extent to significantly affect the ability of the disposal site to meet the performance objectives of subpart C of this part, or may preclude defensible modeling and prediction of long-term impacts."

- 6 NYCRR 383-3.4(h)(2)(i), the analysis period must include dose assessments for a period of 10,000 years unless justification for a shorter time period is provided for consideration. This doesn't fall within the 10 CFR 61.50 and 61.51 provisions covered by the Stipulation, but NRC generally requires 10,000-year assessments. Both DOE and NYSERDA have been using a 10,000-year analysis period.

- 6 NYCRR 383-3.4(h)(2)(ii), the assessment *must not* rely on an institutional control period of greater than 100 years. This doesn't fall within the 10 CFR 61.50 and 61.51 provisions covered by the Stipulation but appears elsewhere in 10 CFR 61. See 10 CFR

[168] In this context, "site" means the portion of the site under DOE's control, and "design(s)" means plans (such as grouting?) that might be carried out with the intention of improving site stability.

61.59, "...institutional controls may not be relied upon for more than 100 years following transfer of control of the disposal site to the owner."

- 6 NYCRR 383-3.4(h)(2)(iv), requirements for the analysis of long-term post-closure site stability, including the need to provide reasonable assurance that there will not be a need for ongoing active maintenance of the site. This matches parts of 10 CFR 61.51, *Disposal site design for land disposal*, which the Stipulation says must be evaluated. See 10 CFR 61.51(a)(1) which says that "Site design features must be directed toward long-term isolation and avoidance of the need for continuing active maintenance after site closure." See also 10 CFR 61.51(a)(5): "Surface features must direct surface water drainage away from disposal units at velocities and gradients which will not result in erosion that will require ongoing active maintenance in the future."

Thus, most of the evaluations that NYSERDA must conduct under DEC's SDA-closure requirements are paralleled by evaluations that DOE must conduct, either under the Stipulation or under generally-accepted methods of evaluating site-closure impacts.

TASK 35: Assess possible lateral movement of bedrock (neither demonstrated nor investigated at this site) and its possible relevance to site stability/integrity (*cf*. Fakundiny, Brennan, possible evidence in onsite injection wells)

VEC.203. The West Valley site needs to be monitored for possible but unlikely changes in its geometry. Any such change – in either the geometry of the glacial fill on which the site is located or the geometry of the underlying bedrock valley – should be regarded as a "low-probability, high-consequences" phenomenon. If such change is happening at all, it would consist of an ultra-slow distortion such as a narrowing of the bedrock valley due to regional compressive stress, or an evolving bulge or pop-ups in shales at the thalweg of the bedrock valley due to local gravitational stress…. Any such change of this type would need to be closely monitored and analyzed before its implications for long-term site integrity could be determined. As noted, any such change is possible but *unlikely*. Despite its low probability, it is widely recognized that both rock and glacial fill undergo distortion under certain circumstances, and there are site-specific factors that make the idea plausible here, including the fact that the ENE-oriented compressive regional stress is perpendicular to the NNW-trending bedrock valley. See also comments 83-84 above regarding the pervasive fracturing and low RQD of bedrock under the site, various comments about whether nearby faults such as the Sardinia and Cattaraugus Creek Features extend beneath the site (currently unknown)… Given the potential implications for long-term site integrity, site geometry needs to be monitored or checked for measurable changes. Possible methods of doing so include InSAR, laser ranging, and geophysical logging/acoustic imaging of one or more of the hydrofracture test wells in WMA 11 to see if well casing has undergone any horizontal offset or kinking due to bedrock detachment.

CSC.50 and VSC.77. Aseismic (non-seismic) horizontal movement of large blocks of either *bedrock* or *overlying fill and soil* may be occurring on or near the site, over and above the known slumping and landsliding.[169] Any such movement of either rock or soil would be a type of topographic instability with potentially serious but currently uncharacterized effects on long-term site stability and containment integrity. The probability of such movement appears low but cannot be ruled out without further investigation. The SEIS process needs to engage in such investigation and needs to treat horizontal movement of either *bedrock* or *overlying fill and soil* as a low-probability but potentially high-consequences phenomenon in accordance with environmental review requirements such as 6 NYCRR 617.9(b)(6)(iii).

CSC.51 and VSC.78. If investigation shows horizontal movement of large blocks of bedrock, fill, and/or soil, the Draft SEIS should quantify and document the rate(s) of movement and associated implications or impacts on long-term site stability and containment integrity. Alternatively, if investigation shows that horizontal movement of large blocks of bedrock, fill, and/or soil can be ruled out, the Draft SEIS should document this conclusion and how it was reached.

CSC.52 and VSC.79. *Horizontal bedrock movement?* Evidence of aseismic horizontal bedrock movement at one location in WNY comes from a paper by the late Prof. Wm. Brennan of SUNY

[169] Vaughan EIS comments § 203.

Geneseo.[170] Brennan reported horizontal offset (partial blockage) in the steel casing of brine wells in the Wyoming valley near Wyoming and Warsaw, NY. The offset occurred at the depth of the thalweg of the adjacent bedrock valley, implying an essentially horizontal detachment surface or decollement in the local shale at the depth of the thalweg, with the movement of the overlying bedrock block driven by the prevailing regional compressive stress. Given the regional extent of this ENE-WNW-oriented tectonic stress, and given the fact that the Buttermilk valley's NNW-SSE alignment is even more favorably oriented (essentially perpendicular to the regional compressive stress), it is reasonable to investigate whether the type of bedrock movement observed by Brennan is also occurring in the West Valley site's injection wells which have remained inactive since about 1970. Some of the West Valley injection wells are known to be blocked by grout, but others are considered grout-free and could/should be checked for offset and/or casing blockage at the approximate depth of the adjacent bedrock-valley thalweg.

CSC.53 and VSC.80. Effects of regional compressive stress in WNY bedrock are well-known to at least two members of the Phase 1 Studies Erosion Working Group (Fakundiny and Young), both of whom have written about such horizontally-oriented stress and its role in causing observable displacement of bedrock.[171] Fakundiny and coauthors have noted, for example, that "Foundation instability, produced by lateral expansion of rock into excavation voids, prevails throughout western New York and the Niagara Peninsula of Ontario, Canada…and is generally thought to be the result of regional stresses acting with a high, horizontal compressive component oriented in a generally east-west to northeast-southwest direction at shallow depths in the earth's crust…"[172]

[170] W.J. Brennan, "Stress-Relief Phenomena Observed During Solution Mining in Western New York," presented at Fall 1996 Meeting, Solution Mining Research Institute, Cleveland, Ohio.
[171] R. Fakundiny et al., "Structural Stability Features in the Vicinity of the Clarendon-Linden Fault System, Western New York and Lake Ontario," in *Advances in Analysis of Geotechnical Instabilities*, (University of Waterloo Press, 1978), esp. p. 121. The decollements shown therein in Figs. 15B (p. 162) and 19-20 (pp. 169-70) may also be relevant. See also A.S. Nieto and R.A. Young, "Retsof Salt Mine Collapse and Aquifer Dewatering, Genesee Valley, Livingston County, New York," in J.W. Borchers, ed., *Land Subsidence: Case Studies and Current Research* (Association of Engineering Geologists, 1998), esp. Fig. 8 and pp. 322-23.
[172] Fakundiny et al., op. cit., p. 121.

<u>TASK 36</u>: Assess possible lateral movement of glacial fill (neither demonstrated nor investigated at this site) and its possible relevance to site stability/integrity (*cf*. soil creep generally, Fakundiny, LiDAR (mis)alignment, possible change in BR&P track alignment)

VEC.203. The West Valley site needs to be monitored for possible but unlikely changes in its geometry. Any such change – in either the geometry of the glacial fill on which the site is located or the geometry of the underlying bedrock valley – should be regarded as a "low-probability, high-consequences" phenomenon. If such change is happening at all, it would consist of an ultra-slow distortion such as…a slow plastic deformation or sagging of the unconsolidated valley fill due to gravitationally-driven creep. Any such change of this type would need to be closely monitored and analyzed before its implications for long-term site integrity could be determined. As noted, any such change is possible but *unlikely*. Despite its low probability, it is widely recognized that both rock and glacial fill undergo distortion under certain circumstances, and there are site-specific factors that make the idea plausible here, including the fact that the ENE-oriented compressive regional stress is perpendicular to the NNW-trending bedrock valley. See also…comment 105 above regarding unlikely but possible evidence of mass movement of valley fill (more likely a map error, but needs to be checked). Given the potential implications for long-term site integrity, site geometry needs to be monitored or checked for measurable changes. Possible methods of doing so include InSAR, laser ranging, and geophysical logging/acoustic imaging of one or more of the hydrofracture test wells in WMA 11 to see if well casing has undergone any horizontal offset or kinking due to bedrock detachment.

CSC.50 and VSC.77. Aseismic (non-seismic) horizontal movement of large blocks of either *bedrock* or *overlying fill and soil* may be occurring on or near the site, over and above the known slumping and landsliding.[173] Any such movement of either rock or soil would be a type of topographic instability with potentially serious but currently uncharacterized effects on long-term site stability and containment integrity. The probability of such movement appears low but cannot be ruled out without further investigation. The SEIS process needs to engage in such investigation and needs to treat horizontal movement of either *bedrock* or *overlying fill and soil* as a low-probability but potentially high-consequences phenomenon in accordance with environmental review requirements such as 6 NYCRR 617.9(b)(6)(iii).

CSC.51 and VSC.78. If investigation shows horizontal movement of large blocks of bedrock, fill, and/or soil, the Draft SEIS should quantify and document the rate(s) of movement and associated implications or impacts on long-term site stability and containment integrity. Alternatively, if investigation shows that horizontal movement of large blocks of bedrock, fill, and/or soil can be ruled out, the Draft SEIS should document this conclusion and how it was reached.

CSC.54 and VSC.81. *Horizontal soil/till movement?* Soils and tills are typically plastic materials that may undergo slow creep toward unbuttressed voids such as valleys, potentially including the Buttermilk valley. Possible evidence of such movement immediately southeast of the West Valley site has been described by Vaughan, EIS comments, § 105 and Figure 4. The work currently being done by Neptune risks missing such movement if any/every horizontal

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173 Vaughan EIS comments § 203.

discrepancy in airphotos (relative to LiDAR maps) is assumed to be from airphoto distortion. The SEIS process should investigate whether horizontal soil/till movement is occurring, document the findings, and address the implications and impacts if any such movement is detected.

<u>TASK 37</u>: Assess site maintenance costs and predicted occupational injuries & deaths for any long-term institutional control; correct the failure of the 2010 EIS to address these

VEC.195. A. Napoleon et al., *The Real Costs of Cleaning Up Nuclear Waste: A Full Cost Accounting of Cleanup Options for the West Valley Nuclear Site*, pp. 97-104 and 136-137, show certain costs that will be incurred in maintaining erosion-control structures needed to protect the West Valley site. As shown there, erosion-control structures will need to be continually rebuilt or replaced on either a 25-year or 50-year replacement cycle. The estimated labor costs of doing so are listed in Table 8.1 on page 137, but Napoleon et al. *do not provide the associated occupational injury and fatality rates* for continual replacement of erosion-protection structures at the West Valley site. (Occupational injuries and fatalities are not fully monetized in labor costs.) Omission of this information on occupational injuries and fatalities is a major lapse in the report by A. Napoleon et al. Since occupational injuries and fatalities are a foreseeable *impact* of the Sitewide Close-in-Place Alternative, DOE needs to supply and assess this information in the course of the current EIS process. Specifically, in any Final EIS issued in the near future and also in any Draft EIS issued for Phase II, DOE needs to address the impact of the occupational injuries and fatalities attributable to ongoing replacement of erosion control structures under the Sitewide Close-in- Place Alternative.

VEC.196. Calculation of the occupational injuries and fatalities attributable to ongoing replacement of erosion control structures under the Sitewide Close-in-Place Alternative is a straightforward procedure based on the number of man-hours needed for such ongoing replacement and based on standard rates of occupational injury and death. The number of man-hours needed for ongoing replacement of erosion control structures under the Sitewide Close-in-Place Alternative is implicit in Table 8.1 of Napoleon et al. (see p. 136: "We gathered the costs of erosion controls from industrial sources at a cost per unit component..."), but the number of man-hours is not given explicitly. DOE either needs to derive the number of man-hours in consultation with Napoleon et al. or needs to make its own calculation of the number of man-hours (while ensuring that its own calculation is reasonably consistent with Napoleon et al.). Once the number of man-hours is obtained, it can be multiplied by a standard injury rate or standard fatality rate for an industry such as Heavy and civil engineering construction (NAICS code 237) to obtain the occupational injuries and fatalities attributable to ongoing replacement of erosion control structures under the Sitewide Close-in-Place Alternative. Examples of standard injury and fatality rates, compiled by the U.S. Bureau of Labor Statistics (BLS) for the years 2006 and 2007, are shown below:

From www.bls.gov/iif/oshwc/osh/os/ostb1757.pdf:

TABLE SNR05. Incidence rate and number of nonfatal occupational injuries by industry, private industry, 2006:

Industry	NAICS code	2006 Annual avg. employment (thousands)	Incidence rate per 100 full-time workers	Number of cases (thousands)
Heavy and civil engineering construction...	237	966.3	5.1	48.8
Rail transportation.........................	482	-	2.2	5.3
Truck transportation.......................	484	1,415.4	5.7	84.0

From www.bls.gov/iif/oshwc/osh/os/ostb1909.pdf:

TABLE SNR05. Incidence rate and number of nonfatal occupational injuries by industry, private industry, 2007:

Industry	NAICS code	2007 Annual avg. employment (thousands)	Incidence rate per 100 full-time workers	Number of cases (thousands)
Heavy and civil engineering construction...	237	1,001.0	4.7	46.2
Rail transportation.........................	482	-	2.2	5.4
Truck transportation.......................	484	1,456.6	5.5	83.8

From http://stats.bls.gov/iif/oshwc/cfoi/cftb0214.pdf:

TABLE A-1. Fatal occupational injuries by industry and event or exposure, All United States, 2006

Industry	NAICS code	Total fatalities (number)	...............Event or exposure...............					
			Transp. incidents	Assaults, violent acts	Contact with objects and equipment	Other categories		
Heavy and Civil Engineering Construction...	237	224	123	3	47	21	24	6
Rail Transportation.........................	482	19	15	--	3	--	--	--
Truck Transportation.......................	484	553	448	14	46	15	23	6

From http://stats.bls.gov/iif/oshwc/cfoi/cftb0223.pdf:

TABLE A-1. Fatal occupational injuries by industry and event or exposure, All United States, 2007

Industry	NAICS code	Total fatalities (number)	...............Event or exposure...............					
			Transp. incidents	Assaults, violent acts	Contact with objects and equipment	Other categories		
Heavy and Civil Engineering Construction...	237	219	99	--	58	31	28	3
Rail Transportation.........................	482	16	11	--	--	--	--	--
Truck Transportation.......................	484	583	465	21	42	24	25	5

VEC.197. Occupational injuries and fatalities attributable to ongoing replacement of erosion control structures, calculated as outlined above, need to be added to the worker injuries and fatalities for the Sitewide Close-in-Place Alternative that are currently listed in Table 4-19 (page 4-56) of the 2008 Draft EIS.

VEC.233. The FEIS provides only a limited conclusory response to my comments 195-196 regarding occupational injuries and fatalities incurred in maintaining erosion control structures. Response 110-122 says that such injuries and fatalities “represent less than 1 percent of the total impacts listed in Table 4-19,” but neither the response nor the referenced portion of the FEIS (pages 4-62 to 4-63, particularly Table 4-19) provides the basis for the “less than 1 percent” response. Part of what is needed, for example, is the annual number of man-hours in NAICS code 237 (and the basis for estimating that number) for workers engaged in maintenance of erosion control structures. I request the information needed to establish occupational injuries and fatalities incurred in maintaining erosion control structures. As appropriate, such information might also be presented at a meeting or technical workshop session.

<u>TASK 38</u>: Assess whether probability distributions are transparent and scientifically supported, including whether important probability distributions being prepared for the SEIS are essentially deterministic estimates dressed up to look like probability distributions. For example, as part of this question, are probability distributions being created from too few data points (*e.g*., only 2 or 3 data points) to be meaningful?

Xx

VSC.29. The PPA [Probabilistic Performance Assessment] computer model runs will require *probability estimates for the input variables* (input parameters) that control or affect the predicted radiological doses. Probability distributions for these variables – potentially including variables such as rainfall, erodibility of till, or abstract variables that represent these real-world variables – are typically based on expert opinion. Scientists working in statistical risk assessment recognize potential problems such as expert overconfidence, lack of calibration, and lack of empirical validation of such probability estimates. Various scientists have recommended procedures that can guard against errors in expert estimates.[174] Such safeguards should be incorporated into the SEIS process, and should be described fully and transparently.

CSC.28 and VSC.32. The variables and assumptions in the model should be described in a transparent manner to the CTF and the public. More information should be presented in the following areas:

- What are the probability distributions for variables under best and worst scenarios that have the greatest influence on the models?
- What is the tolerance of these variables and the strength of the prior data used to support these probabilities?
- Which variables are described with assumptions supported by the weakest or least prior data?
- What degree of influence do these variables have on the final models?
- What procedures were run to describe and adjust for the influence these variables with weak prior data?
- Under the worst case scenarios, what influence do these variables have on the final models?

[174] See K. Shrader-Frechette, "Uncertainty Analysis, Nuclear Waste, and Million-Year Predictions," in S.O. Hansson and G. Hirsch Hadorn, eds., *The Argumentative Turn in Policy Analysis* (Springer, 2016), 291-303, esp. pp. 298-99, and sources cited therein.

<u>TASK 39</u>: Assess how the Bayesian priors are handled

Xx

VSC.30. PPA [Probabilistic Performance Assessment] computer model runs typically use Bayesian methods that require assumptions about the "prior" probability distributions of different variables.[175] Developing these "priors" or "prior distributions" can be procedurally difficult because the supporting data have not yet been applied to the distribution. Safeguards against poorly chosen "priors" should be incorporated into the SEIS process, and the safeguards should be described fully and transparently.

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[175] For example, R.E. Kass and L. Wasserman, "The Selection of Prior Distributions by Formal Rules," *Journal of the American Statistical Association* **91**, 1343-70 (1996); H. Chipman et al., "The Practical Implementation of Bayesian Model Selection," IMS Lecture Notes - Monograph Series **38**, 65-134 (2001), available at http://www-stat.wharton.upenn.edu/~edgeorge/Research_papers/ims.pdf .

<u>TASK 40</u>: Assess whether the SEIS process is unduly reliant on Ralph Wild's reconstructions of the inventories of the buried waste at the site (in both the SDA and the NDA)

Context: See Wild/URS (2002) and Wild/URS (2000), respectively, for these two estimates or reconstructions of what's buried in the two burial grounds. For the SDA, the Wild/URS reconstruction is *the most recent of several* reconstructions of the radiological inventory; likewise for the NDA. The question is whether the probability distribution for each of these burial grounds should be, and can be, based entirely on Wild's reconstruction of the inventory. Alternatively, should each of these distributions be developed not just from Wild's, but from the earlier reconstructions as well – and, if so, should the earlier reconstructions be given a lower weighting than Wild's, thereby creating reasonably supported probabilistic distributions? The distributions will be used for the PPA (probabilistic performance assessment) and SEIS processes. Given the existence of these other reconstructions of the SDA and NDA inventories, there appears to be no rational basis for deterministically adopting the Wild/URS inventories as the only trustworthy sources.

VSC.112A: ... Both the NDA and the SDA, for example, have been subject to several separate studies that have attempted to estimate the quantities of buried waste in each disposal area. Results of these studies tend to be "all over the map" (meaning that the results for either disposal area may vary by a factor of four or more), and new studies typically produce new "guesstimates" without any consultation of prior studies as a means of assessing error bounds. My use of the word "guesstimate" is not meant to be unduly pejorative but reflects the large (and largely unexplained) study-to-study variation and the lack of any clear superiority of new study methods over old study methods. For the NDA, see J.L. Ryan, NDA Inventory, PNL, 1992, p. 42; also the NDA Waste Characterization Report, Rev. 1, WVDP-EIS-021, WVNS, ca. 1996, p. 24; also the 1996 Draft EIS, pages C-41 and C-42; also the NDA Waste Characterization Report [by Ralph Wild], URS/Dames & Moore, August 2000, page ii. For the SDA, see Stiles et al., Profiles for characterization of SDA, PNL, October 1994, page 3.5; also the SDA Waste Characterization Report, Rev. 2, WVDP-EIS-022, WVNS, ca. 1996; also the 1996 Draft EIS, page C-55; also the SDA Radiological Characterization Report [by Ralph Wild], URS, September 2002.

TASK 41: Assess the evidence that much of the transuranic (TRU) and high-level waste at the site came from military/defense sources, and whether such evidence creates a compelling presumption that these wastes can be and should be legally defined as defense wastes

Xx

<u>TASK 42</u>: Assuming that buried wastes remain onsite, and either do or don't leak, assess whether the permanent loss of at least part of the site from productive re-use is treated fairly in the SEIS as an impact to Ashford

CFI.16. The CTF expects that the Preferred Alternative will include the restoration of the Center to alternative uses (such as educational, industrial, commercial or recreational uses) as much as is possible and as soon as possible.

VSC.18. For any adverse impacts from wastes left onsite, the CTF and affected populations should be afforded the opportunity to determine the accuracy of the impact assessments.

CSC.18 and **VSC.20**. As stated in the CTF's 1998 Final Report, the Site Managers should recommend policies that will affect, ameliorate, or replace the losses to the community from the redirection in economic activity at the site, i.e., at the Western New York Nuclear Service Center. Procedures for instituting and implementing such policies should be explored and developed as part of the SEIS process.

CSC.19 and **VSC.21**. Any alternative that leaves waste onsite may have other "non-tangible" impacts in addition to Community Character impacts. Any such "non-tangible" impacts to nearby communities and natural resources (including the Great Lakes, for example) should be identified and vetted as scoping issues for any site closure alternative other than full exhumation.

__**TASK 43**__: Assuming that buried wastes remain onsite and eventually leak, resulting in downstream exposures either above or below 25 mrem/yr, assess whether the *economic and social* impacts (to Ashford, other downstream towns and cities, both counties, the Seneca Nation, and Canada) are fairly and adequately represented in the SEIS.

Context: For example, are there inadequately represented impacts such as an ongoing stigma that result in loss of tourism, reduced in-migration of new residents, reduced real-estate values, inability to enjoy traditional and recreational resources, etc.? All of these can and should be evaluated.

Note that the overall premise of this task ("that buried wastes remain onsite and eventually leak") makes no presumption that such wastes would actually leak but fulfills the need to identify risks and evaluate their impacts within the normal process of environmental review. See also Tasks 45-46 for such impacts and how they're addressed.

VSC.18. For any adverse impacts from wastes left onsite, the CTF and affected populations should be afforded the opportunity to determine the accuracy of the impact assessments.

VSC.20. As stated in the CTF's 1998 Final Report, the Site Managers should recommend policies that will affect, ameliorate, or replace the losses to the community from the redirection in economic activity at the site, i.e., at the Western New York Nuclear Service Center. Procedures for instituting and implementing such policies should be explored and developed as part of the SEIS process.

CSC.19. Any alternative that leaves waste onsite may have other "non-tangible" impacts in addition to Community Character impacts. Any such "non-tangible" impacts to nearby communities and natural resources (including the Great Lakes, for example) should be identified and vetted as scoping issues for any site closure alternative other than full exhumation.

CSC. 69. For any alternative that leaves waste onsite, the scoping process should address attainment of water-resource goals such as "fishable, swimmable, drinkable," other measures of ecological protection,[176] as well as other measures intended to protect public health, safety, and enjoyment of affected waterways such as Zoar Valley and Lake Erie.

[176] Newly completed New York Natural Heritage riparian assessment: See http://buffalonews.com/2018/03/23/watersheds-in-cattaraugus-county-among-healthiest-in-new-yorkstate-data-shows/ and http://www.nynhp.org/treesfortribsny

TASK 44: Assuming that buried wastes remain onsite and eventually leak, resulting in downstream exposures either above or below 25 mrem/yr, assess whether the *health and social* impacts (to Ashford, other downstream towns and cities, both counties, the Seneca Nation, and Canada) are fairly and adequately represented in the SEIS.

Context: The incremental dose-dependent change in cancer risk will undoubtedly be evaluated as an impact in the SEIS. Beyond that, are there inadequately represented impacts such as psychological/cultural/physical/spiritual impacts of living near the site, perhaps expressed in trauma and its coping mechanisms? A recent study on trauma in residents exposed to smog in China found that "people who perceived the situation as less controllable had more tendencies to focus on…rather than move past the emotional impact of smog…"[177] Such a study, looking beyond European-American contexts, may offer a useful framework for the different populations at risk from a leaking West Valley site.

Trauma is a deeply distressing or disturbing experience, or a person's response to such an experience, sometimes affecting a person's ability to cope and function. For people living near and/or downstream from the West Valley site, is it reasonable to assess trauma as a potential impact of a state/federal decision to leave buried wastes onsite?

There are at least three possible variations in the circumstances where this question arises:

a) *What if studies predict that wastes left onsite may cause some people to be exposed to radiation or radioactive contamination at a level that exceeds a nominal limit such as 25 mrem/year? Is it reasonable to assess trauma as a potential impact?* Yes, it appears reasonable in this circumstance. Note that the question isn't whether everyone would experience trauma, but, rather, whether trauma is a foreseeable impact for some people. While some may contend that exposure above a nominal limit such as 25 mrem/yr poses a negligibly small risk compared to other risks (including risks from various sources of background radiation), the risk at a 25 mrem/year exposure level exceeds the "ten-to-the-minus-sixth" or one-in-a-million guidance level for involuntary risks, i.e., risks that people don't voluntarily choose for themselves.

b) *What if studies predict that wastes left onsite will not cause anyone to be exposed to radiation or radioactive contamination at a level that exceeds a nominal limit such as 25 mrem/year? Is it reasonable to assess trauma as a potential impact?* There's no clear answer to this question due to individual differences in believing that the studies are trustworthy, that a 25 mrem/year limit is adequately protective, etc. Concerns about the trustworthiness of studies may be based, for example, on whether the studies' assumptions about exposure pathways are realistic for the individual. Has radionuclide uptake by traditional/ceremonial plants been properly assessed, so that plants gathered from a flood plain may be safely used? Is this a persistent source of worry for the individual? Are fish safe to eat? Are swimming and boating safe in downstream waters? Different individuals will vary in their responses to such studies that predict no exceedances.

[177] Zhuoying Zhu & Yitong Zhao, "Severe Air Pollution and Psychological Distress in China: The Interactive Effects of Coping and Perceived Controllability," *Frontiers in Psychology* 12:601964 (02 June 2021).

One impediment to accepting such studies is the large unresolved difference between dose predictions expressed in the 1994 Draft EIS and dose predictions expressed in the 2010 EIS.

c) *What if onsite/offsite measurements of radiation and radioactive contamination become unavailable in the future and therefore can't inform people whether their exposure from wastes left onsite exceeds, or doesn't exceed, a nominal limit such as 25 mrem/year? Is it reasonable to assess trauma as a potential impact?* This is a crucial issue. The question could be rephrased as, *What if the local/regional population retains at least a vague memory of something dangerous buried at the West Valley site, and this memory outlasts the scientific expertise and the government willingness needed for radiological monitoring and reporting?* It appears reasonable to expect trauma in that circumstance. Some may contend either that people would prudently move away from the West Valley site (see stigma, covered separately in Task 43) or that folk wisdom combined with a corps of elders respected for their wisdom would keep a watchful eye on the site (highly unrealistic as a protective measure). But seriously, is there any reason to think that scientific expertise and/or governmental monitoring/reporting oversight may be lost? Yes, on two counts. Historic and current social unrest, combined with enormous disconnects in social priorities and forms of government across centuries and millennia, make it impossible for society to predict with any confidence that transparent government programs of radiological monitoring will continue indefinitely. Equally importantly, government decisions on radioactive waste disposal are generally not allowed to assume that institutional control will continue beyond 100 years. Institutional control, in this context, includes government programs for radiological monitoring and reporting. Since reliable monitoring can't be assumed to last beyond 100 years, future trauma resulting from uncertainty about local and regional exposure levels qualifies as an impact that needs to be covered (scoped) in the forthcoming EIS.

Note that the overall premise of this task ("that buried wastes remain onsite and eventually leak") makes no presumption that such wastes would actually leak but fulfills the need to identify risks and evaluate their impacts within the normal process of environmental review. See also Tasks 45-46 for such impacts and how they're addressed.

CFI.1. The CTF expects that the Preferred Alternative will protect human health and the environment from all risks associated with the Center. Because proximity to the Center increases potential risk, the CTF believes that special attention should be paid to the long-term health and safety of people residing in the adjacent towns.

VSC.18. For any adverse impacts from wastes left onsite, the CTF and affected populations should be afforded the opportunity to determine the accuracy of the impact assessments.

CSC. 17 and **VSC.19**. The Draft SEIS should include support for assessments of the surrounding communities that focus on psychological/cultural/physical/spiritual impacts of living near the site. Such assessments should be facilitated through the CTF in collaboration with Roswell Park Comprehensive Cancer Center, the SUNY University at Buffalo School of Public Health, and the Seneca Nation of Indians. The schedule for these assessments should be outlined in advance and performed at a minimum of every 10 years. Outcomes should include action-orientated recommendations.[178]

VSC.21. Any alternative that leaves waste onsite may have other "non-tangible" impacts in addition to Community Character impacts. Any such "non-tangible" impacts to nearby communities and natural resources (including the Great Lakes, for example) should be identified and vetted as scoping issues for any site closure alternative other than full exhumation.

[178] J. Johnson, J. Baldwin, R.C. Haring, S.A. Wiechelt, S. Roth, J. Gryczynski, and H. Lozano, "Essential information for disaster management and trauma specialists working with American Indians," Chapter 4 in A. Marsella, J. Johnson, P. Watson, and J. Gryczynski, eds., *Ethnocultural Perspectives on Disaster and Trauma: Foundations, Issues, and Applications* (New York: Springer SBM Publishing, 2008).

<u>TASK 45</u>: Assess whether the aforementioned impacts can be, *and are*, properly represented in the SEIS process as SEQRA "community character" impacts

CSC. 10 and **VSC.10**. Radiological impacts currently recognized by DOE and NYSERDA include impacts to the general population and onsite workers,[179] with population impacts generally being rated against NRC's 25 millirem-per-year exposure standard for unrestricted release of the site. However, for any alternatives in which wastes are left in place, there may be significant adverse impacts to "Community Character" resulting from *radiological releases that substantially exceed background levels but do not exceed NRC's 25 millirem-per-year exposure standard for a maximally exposed individual*. Examples of such impacts are provided below. Note that effects on Community Character are a specific type of impact that must be considered under New York's State Environmental Quality Review (SEQR) requirements.[180] Such impacts would not apply to the No-Action alternative but would apply to the "actions" of any of the other alternatives.

CSC. 11 and **VSC.11**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of Ashford, including such impacts on the town's residents and its prospects for economic development, resulting from the stigma of radioactive waste.

VSC.12. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of Concord, including such impacts on the town's residents and its prospects for economic development, resulting from the stigma of radioactive waste.

CSC. 12 and **VSC.13**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of the Seneca Nation of Indians, including such impacts on the Nation's residents, traditional cultural practices, and prospects for economic development, resulting from any detectable above-background level of radioactive contamination moving along Cattaraugus Creek through the Nation's Cattaraugus Territory.

CSC. 13 and **VSC.14**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of Cattaraugus County, including such impacts on the county's residents, their enjoyment of Zoar Valley, Cattaraugus Creek, and prospects for tourism and economic development, resulting from any detectable above-background level of radioactive contamination moving along Cattaraugus and Buttermilk Creeks. The Draft SEIS should also consider impacts on the county's residents and its prospects for economic development, resulting from the stigma of radioactive waste.

CSC. 14 and **VSC.15**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of Erie County, including such impacts on the county's residents, their enjoyment of Zoar Valley and the lakeshore waterfront, and prospects for tourism and economic development, resulting from any detectable above-background level of radioactive contamination moving along Cattaraugus Creek from Springville to Irving, along the

[179] 83 *Federal Register* 7464 (Feb. 21, 2018) at 7468, column 1.
[180] See NYSDEC, *The SEQR Handbook*, 3rd ed. (2010), pp. 87-89 and 204-05; also *Matter of Village of Chestnut Ridge et al. v. Town of Ramapo et al.*, 45 AD3d 74 (2d Dept. 2007) at 85-87 and 94-95.

Lake Erie shoreline from Irving to Buffalo, and along the Niagara River shoreline from Buffalo to Tonawanda.

CSC. 15 and **VSC.16**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of the City of Buffalo, including such impacts on the city's residents, their enjoyment of the waterfront, and prospects for tourism and economic development, resulting from any detectable above-background level of radioactive contamination moving past and through the city's waterfront.

CSC. 16 and **VSC.17**. For any alternative that leaves waste onsite, the Draft SEIS should address the adverse impacts on the community character of other downstream communities in the U.S. and Canada, resulting from any detectable above-background level of radioactive contamination moving through their waterways or along their shorelines.

TASK 46: Assess whether and how the aforementioned impacts can be, and are, properly represented in the SEIS process as impacts under NEPA

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Many older links shown here are no longer active but are listed as possible identification. See above ("Chronology of the EIS process") for EIS documents.

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