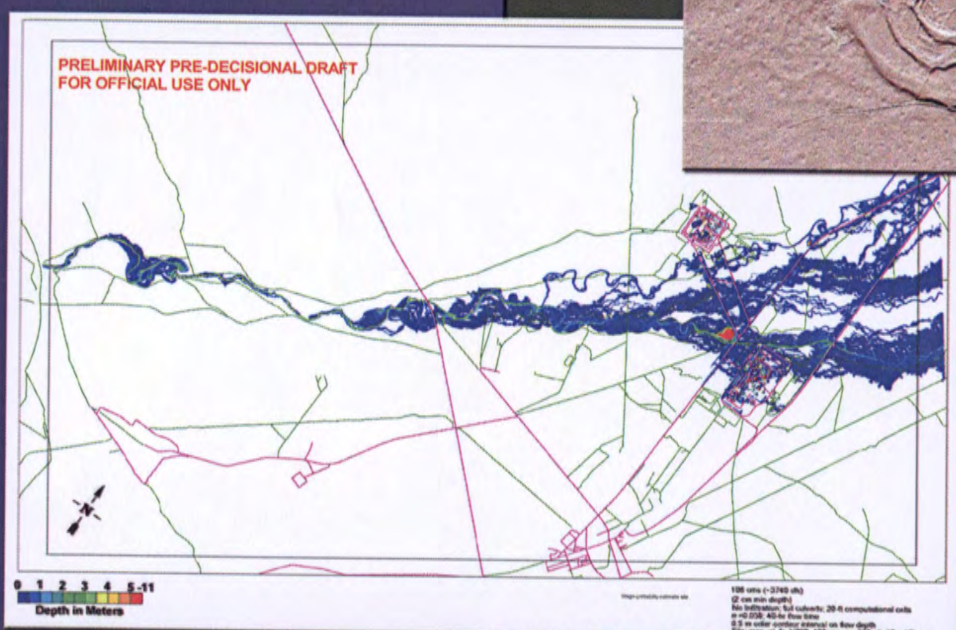
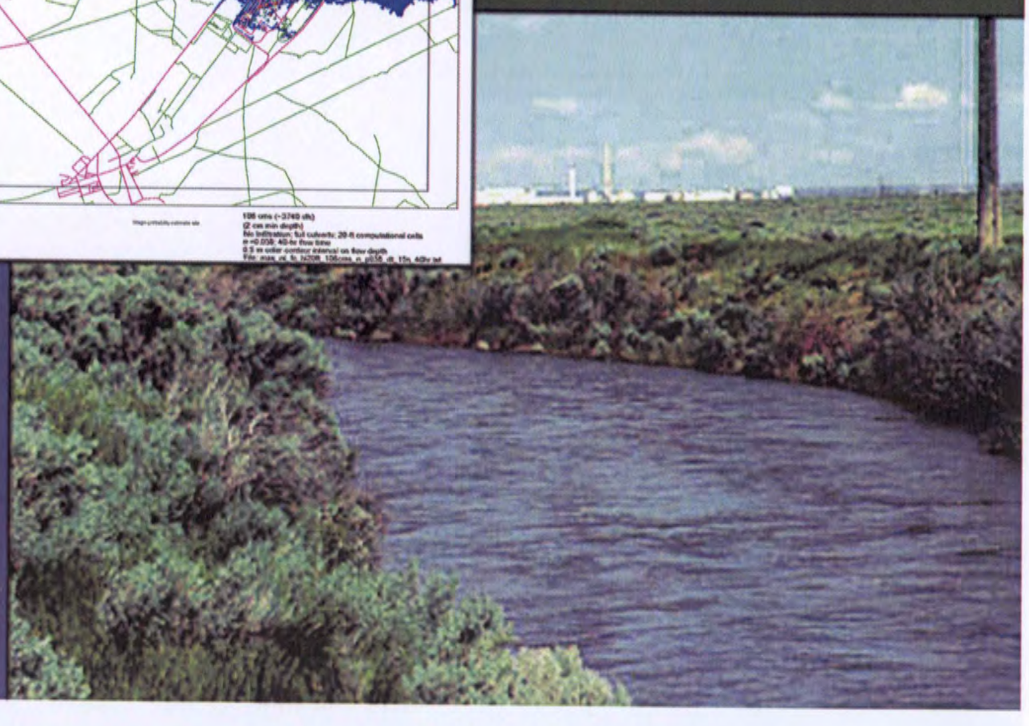
STOLLER-ESER-109

May 2005

Final Quality Assurance Report for the INL Flood Hazard Study - Reducing Uncertainty for Paleohydrologic Bounds and 2-D Hydraulic Routing





Final Quality Assurance Report
for the
INL Flood Hazard Study – Reducing Uncertainty for Paleohydrologic Bounds and 2-D Hydraulic Routing

TABLE OF CONTENTS

1. INTRODUCTION

As stated in the Quality Assurance Plan for the Big Lost River Floodplain Study (the QAP), "an effective quality assurance (QA) program is essential to achieving the reasonableness, acceptability and defensibility objectives of the current Big Lost River Floodplain Study." This report will serve as documentation of how the various processes used throughout the study meet the stated QA objectives. As part of this process a data assessment based on the data quality objectives detailed in the QAP is also presented.

The Idaho National Laboratory (INL) Flood Hazard Study (FHS) of the Big Lost River is intended to support determination of the design basis flood (DBFL) for use in evaluation of the flood hazard at the Idaho Nuclear Technology and Engineering Center (INTEC) and the Advance Test Reactor (ATR) at the Test Reactor Area (TRA) both located on the INL. The basis of the flood hazard evaluation is determined according to potential risk and the associated performance category (PC). Low hazard facilities are designated as PC-1 while facilities with significant potential for risk to workers and the public are designated as PC-4 facilities. A PC-4 facility must continue to function (including confinement of hazardous materials and provision of occupant safety) given a flood hazard with a combined mean-annual exceedance probability (AEP) of 10^{-5} (1 in 100,000). The ATR is an operating reactor that automatically is designated as a PC-4 facility. Certain activities at the INTEC also require natural phenomena hazards (NPH) characterization up to PC-4.

Following guidance described in DOE standard 1020 "*Natural Phenomenon Hazards Design and Evaluation Criteria for Department of Energy Facilities*" (DOE-STD-1020-01) the QAP was based on the requirements in 10 CFR 830, Subpart A. It also captured the provisions of DOE O 414.1A and EPA QA/R-2. The participatory and peer reviewer process used throughout the FHS followed the guidance detailed in NUREG/CR-6372, "*Recommendations for Probabilistic Seismic Hazard Analysis: Guidance on Uncertainty and Use of Experts*." Although written for seismic hazard analysis much of the guidance is applicable to any NPH characterization. NPH characterization efforts at DOE facilities require independent peer review to ensure the quality of deliverables (DOE-STD-1020-01).

1.1. Project Description

The Big Lost River (BLR) is the major surface water feature on the INL. The BLR drains an area of approximately 3,652 km^2 (1,410 mi^2). Due to diversions of the BLR for irrigation before reaching the INL boundary, flow in the river onsite is often intermittent or non-existent. However, of the eight operational facilities, five are located on or near the current or historic floodplain of the BLR. As stated above, two of these facilities are characterized as PC-4 facilities. Federal regulations require a determination of the potential impact from a flood stage with a combined mean annual exceedance probability of 10^{-5} (a return frequency of 100,000 years) for any PC-4 facility.

The overall objective for this project is to develop probabilistic estimates of flood stage at regulated locations at INTEC and TRA following DOE-STD-1020, 1022, and 1023, as well as other related guidance and requirements. Flood discharge probability estimates developed from studies of the BLR will be used to define a range of discharges and associated flood hazard risks. Representative discharges will have been input into two-dimensional (2-D) flow models that are used to predict the resultant inundation at the key INL facilities.

Variations in stage at the facilities result from assumptions and variables including:

- The use of differing hydraulic models (i.e., TrimR2D versus RiCOM);
- Variations in hydraulic model parameters;
- Assumptions and models of flow at bridges and culverts (i.e., open versus closed);
- Assumptions regarding infiltration (i.e., none or full [10-15%]); and
- Assumptions regarding the duration of flow (e.q., in order to maintain the wetted surface each successive flood discharge was built on the results of the previous discharge model run).

A logic tree approach is used to combine the flood stage variations developed through the modeling efforts with input discharge probability distributions to arrive at final stage-probability estimates for the regulated locations at INTEC and TRA.

A second objective of the study is to reduce the estimates of uncertainty associated with the conclusions developed by prior paleoflood studies at the INL (Ostenaa et. al., 1999). Studies in support of this objective include geomorphic mapping, trenching, and hydraulic modeling of a study reach of the BLR immediately downstream of the INL Diversion Dam. These studies are the basis for the paleohydrologic analyses used to estimate the low probability flood discharge for the BLR.

1.2. Project Organization

To maintain a proper level of QA during the FHS a formalized project organization was established. A project manager was designated who had overall control and supervision of the various project task leads. The project task leads (PTL) were assigned by each of the organizations participating in the FHS. There was unconstrained contact between the PTLs, but all contacts were also copied to the project manager.

1.2.1. Project Manager

The Project Manager (PM) maintained overall responsibility for each of the subtasks conducted as part of the FHS. Mr. Robert Creed with the Department of Energy – Idaho Operations Office (DOE-ID) served as the PM for this project. He had the ultimate authority for approval of all project activities.

1.2.2. Project Task Leads

Each participating organization assigned an individual to serve as PTL. For this FHS the PTLs and their responsibilities were as follows:

Mr. Dean Ostenaa, Bureau of Reclamation (BOR), was responsible for conducting fieldwork, interpretation of paleohydrologic and geomorphologic data, and preparation of the final FHS report. He was supported by Mr. Dan O'Connell who was responsible for the 2-dimensional (2-D) modeling and inundation mapping. Both Mr. Ostenaa and Mr. O'Connell developed the final stage/probability plots.

Mr. Charles Berenbrock, United States Geological Survey (USGS), provided support for the 2-D modeling effort by developing rating curves for the bridge and culvert hydraulics of the FHS reach.

Dr. Fritz Fiedler, University of Idaho (UI), also supported the BOR hydraulic modeling by providing infiltration loss estimates. Three estimates (high, low and expected) were to be developed based on existing data collected through literature review of relative studies and masters' theses and the INL soils map. Dr. Fiedler provided oversight of university students working on the project.

Dr. Glenn Thackray and Dr. Paul Link, Idaho State University (ISU), supported the field effort of the BOR. In this capacity they provided supervised oversight of a graduate student (Ms. Val Sheedy) for soil lithologic mapping, description and collection within the trenches excavated in the diversion dam study reach. Dr. Link also provided information on sediment providence through radio-spectral analysis of detrital zircons recovered from various layers within the trenches.

Mr. Chris Martin, S. M. Stoller Corporation, provided support to all organizations throughout the FHS with QA oversight.

2. PEER REVIEW

DOE requires that FHS work for all facilities must undergo an independent peer review to evaluate design assumptions and philosophy. Reviewers are recognized experts in the major technical disciplines employed as part of this study including paleohydrology, hydraulics, and probabilistic risk assessment. Reviews have been assigned, based on expertise, as follows.

2.1. Participatory Peer Reviewers

Dr. P. K. House of the University of Nevada provided critical review of the paleoflood and geomorphic aspects of the work. Dr. Nikolas Katapodes of the University of Michigan provided review of the hydraulic modeling, and Dr. Kevin Coppersmith of Coppersmith Consulting provided review of the probabilistic aspects of the project.

All three reviewers participated in a project "kick-off" meeting held November 1-2, 2001. All project participants were introduced and made presentations to the participatory reviewers on their proposed approach to their part of the project. Participatory reviewers continued to provide guidance and direction throughout the project via direct site visits and/or document reviews. Their input was crucial to development of the draft final report for assuring critical issues were properly addressed and resolved in that report.

2.2. Final Peer Reviewers

Dr Manu Lall of Columbia University provided final peer review with emphasis on the uncertainty analysis. Dr. Klaus Jorde of the University of Idaho provided final peer review with emphasis on the flood hydraulics and geomorphology, while Dr. Victor Baker of the University of Arizona provided final peer review emphasizing the paleohydrologic information/interpretation and geomorphology.

In their positions as recognized experts in their respective fields, Drs. Lall and Baker also participated in detailed reviews of the 1999 BOR report.

All three final peer reviewers participated in a peer review meeting held October 5-7, 2004. At this meeting the process used for data collection, modeling and stage-probability curve generation were presented. Dr. Baker was unable to attend the meeting due to an unfortunate injury and subsequent surgery the week of the meeting. Minutes from the meeting were collected and distributed to all three reviewers (see Appendix A).

Because of Dr. Bakers previous field and paleohydrologic modeling experience at the INL, his physical absence at this meeting did not limit his ability to provide a rigorous review. To assure information consistency for the peer review Dr. Baker was also provided a compressed presentation by Mr. Ostenaa and Mr. O'Connell while attending a conference in Denver. The PM and QA oversight were in attendance at this meeting via conference call, to answer any questions related to overall project objectives or QA. At the completion of the meeting each of the peer reviewers were asked to provide a thorough review of the draft final report, and provide comments on the appropriateness of how the models were applied and methods used.

3. RESOLUTION OF DATA QUALITY OBJECTIVES

Detailed data quality objectives (DQOs) were developed for this project and are explained in the QAP. The decisions and inputs outlined below are based on factors encountered during performance of the project. The following sections present details on each of the DQO steps to be applied to the FHS at the start of the project. It was recognized early on in the development of the QAP, that the final products of this project are dependent on scientific data and methodologies. Strict adherence to QA/QC requirements typically intended for engineering design, construction, and analytical (laboratory) data were seen as creating an unnecessary documentation burden on project participants without adding value to the QA/QC process. Therefore, certain modifications of procedures normally consistent with those types QA/QC requirements were undertaken for this project.

3.1. State the Problem

Part of the NPH analysis requirements of DOE is determination of the flood hazard at designated facilities based on their performance category. Flood hazard evaluations are based on the DBFL for that facility. This DBFL value is obtained from plots of return period versus water surface elevation (UCRL-15910). The return period is the hypothetical probability of a flood of given discharge occurring in a given time period. Differing DBFLs need to be determined for each performance category facility as a function of combined mean annual exceedance probability. DOE further requires that uncertainties associated with the DBFL be formally characterized. The goal of the current project is to utilize peer reviewed methods to quantitatively evaluate flood stage probability at key PC-4 INL facilities consistent with DOE and related requirements.

A related follow-on task to this project will be the determination of a total flood hazard. This flood hazard will include additional discharge inputs from events such as failure of the Mackay Dam, failure/storage by the INL diversion dam, over-land flow (i.e., rain fall on frozen ground), and surface water/groundwater management of the BLR.

3.2. Identify the Decisions

This step lays out the principal study questions, alternative actions, and corresponding decision statements that must be answered to effectively address the above stated problem. The primary decision supported by the FHS is to determine if and what type of mitigation would be required at INL facilities given the probability distribution of flood stage at those facilities. In addition to this, another goal is to show what impacts these peak flows could be expected to produce.

3.2.1. Principal Study Questions.

The purpose of a principal study question (PSQ) is to identify key unknown conditions or unresolved issues that, when answered, provide a solution to the problem being investigated, as stated above. The PSQ's for this project are summarized here and detailed in the QAP.

PSQ-1: Using 2-D flow modeling, what are the variations in flood stage at the INL facilities that result from differing flow models, model parameters, infiltration scenarios, and culvert/bridge flow models for a range of discharges on the Big Lost River?

The discharges to be modeled ranged from approximately 355 cubic feet per second (cfs) to the current RCRA value for the 100-year flood of 24,720 cfs. The significance of 355 cfs is that this is the maximum gauge measured flow downstream of the INL diversion dam since 1984. Other modeled discharges are presented in Table 1.

Two different 2D flow models were used to generate the inundation maps of the regulated INL locations. TrimR2D is a finite-difference model that used a 20-foot grid. RiCOM is a finite-element model that was used a variable mesh based on 5- to 20-ft grid spacing. Results of model comparisons and quality control tests are contained in Appendix C of the final flood hazard report from BOR.

Two Manning roughness numbers (n) based on sand bed streams ($n = 0.030$) and gravel bed streams ($n = 0.038$) were modeled as bounding parameters. Only slight differences were observed.

Model runs were made to test the sensitivity of flows to various bridge/ culvert ratings and infiltration rates. As a result of this sensitivity testing four options for bridge/culvert and infiltration were developed for use in the final modeling. The options were: full bridge/culvert conveyance, full infiltration of 15 percent throughout the study reach; full bridge/culvert conveyance, no infiltration throughout the study reach; partial bridge/culvert conveyance, full infiltration of 15 percent throughout the study reach; and partial bridge/ culvert conveyance, no infiltration throughout the study reach. Partial culvert conveyance was defined as only the structures within the Big Lost River channel remained open (the Highway 20/26 bridge, Lincoln Boulevard culverts, and the INL railroad bridge). Also some limited testing was performed for complete blockage of the Lincoln Boulevard culverts.

It was found that conveyance through the small culverts had little effect on modeled inundation. This appeared to be related to the placement of culverts in low spots, so when they were closed the water overtopped the roads at those locations and flowed on in the same patterns.

Because of the very flat nature of the Snake River Plain in the central portion of the INL most of the flood flows are controlled by small topographic features (old levees, abandoned channels, etc). These features include common items such as road embankments, drainage ditches, and concrete barriers used for security purposes. These small features have little impact on the flood hazard uncertainty. However, because of the "mobile" nature of items such as concrete barriers, they can have a large impact on facility inundation.

Table 1 **Discharge and modeling scenarios used to construct the stage - probability estimates.**

Modeled Discharge[1] m^3/s (ft^3/s)	Infiltration[2]				Potential Significance of Modeled Discharge
	None		Full		
	Full Culverts	Partial Culverts	Full Culverts	Partial Culverts	
13 (~460)	T	T	T	T	Approximate maximum Big Lost river gaged flow downstream of the INL diversion since 1984.
25 (~885)	T	T, R	T, R	T	Approximate maximum INL diversion dam release capacity.
63 (~2225)	T	T, R	T, R	T	Estimated maximum Big Lost River historic flood (1965) upstream of INEEL Diversion Dam.
87		R	R		Revised Big Lost River 100-yr flood (this study).
97		R	R		95% bound on revised Big Lost River100-yr flood (this study).
106 (~3740)	T	T, R	T, R	T	Revised USGS Big Lost River100-yr flood (Hortness and Rousseau, 2002).
110		R	R		Revised Big Lost River 500-yr flood (this study).
130		R	R		Data for stage-probability curves.
150 (~5295)	T	T, R	T, R	T	Preferred discharge for Big Lost River 10,000-yr paleohydrologic bound (Ostenaa and others, 1999). Preferred discharge for late Holocene Big Lost River paleofloods (this study).
176 (~6215)	T	T, R	T, R	T	USGS 100-yr flood downstream of INEEL Diversion Dam (Kjelstrom and Berenbrock, 1998).
200 (~7060)	T	T, R	T, R	T	Data for stage-probability curves
250 (~8830)	T	T, R	T, R	T	Preferred discharge for Big Lost River 10,000-yr paleohydrologic bound (this study).
300 (~10,595)	T	T, R	T, R	T	Data for stage-probability curves.
400 (~14,125)	T	T, R	T, R	T	Data for stage-probability curves.
700 (~24,720)	T	T, R	T, R	T	Adopted INEEL interim 100-yr flood; Estimated dam break flow at INTEC for Mackay Dam 100-yr flood failure (Koslow and Van Haaften, 1986).

Notes:

1 Steady-state discharge input at upstream end of reach near INEEL Diversion Dam

2 Entries in table indicate flow model used for each scenario: T - TRIMR2D with 20-ft rectangular grid as input topography; R - RICOM with 5-, 10-, and 20-ft variable grid as input topography. Limits of 5-ftmesh were defined by extent of inundation from TRIMR2D model of 100 m3/s with no infiltration and partial culverts; limits of 10-ft mesh by extent of TRIMR2D 200 m3/s inundation for same scenario.

PSQ-2: How appropriate is the characterization, and range of uncertainty, of the paleohydrologic bounds and paleoflood inputs to flood frequency analyses used in prior INL paleoflood studies?

The paleohydrologic bounds from the previous study have been extensively revised as discussed in Section 3 of the final report. Major changes in the discharge estimates for the bounds resulted from the recognition of inaccuracies in the input topography used in hydraulic modeling for the previous study (Described in Appendix A of the final report and reproduced here in Appendix B), and new modeling, using new, detailed topographic data, was completed for this study.

In the previous study, the criteria for determining modification of a surface were subjective and very general in nature. The present study provided a very thorough, objective development for the hydraulic bounds used to determine geomorphic modifications. The framework for development of these bounds is detailed in Appendix D of the final report.

The characterization of soils and geomorphology performed as part of the previous paleoflood studies was limited to cut-bank and surface geomorphology. A detailed geomorphic map was completed and extensive trenching work allowed for a three dimensional view of soil characteristics in key locations that provided additional evidence to constrain the paleoflood bounds. These results are discussed in Section 2 of the final flood hazard report and trench logs and field/laboratory data presented in Appendix B of that report.

In addition, although the 1999 study underwent a rigorous peer review before going final, it did not include participatory peer reviewers during project performance.

PSQ-3: What are the flood frequency inputs to be used in the probabilistic flood stage logic tree?

A revised flood frequency analyses based on updated paleohydrologic bounds is presented in Section 4of the final flood hazard report.

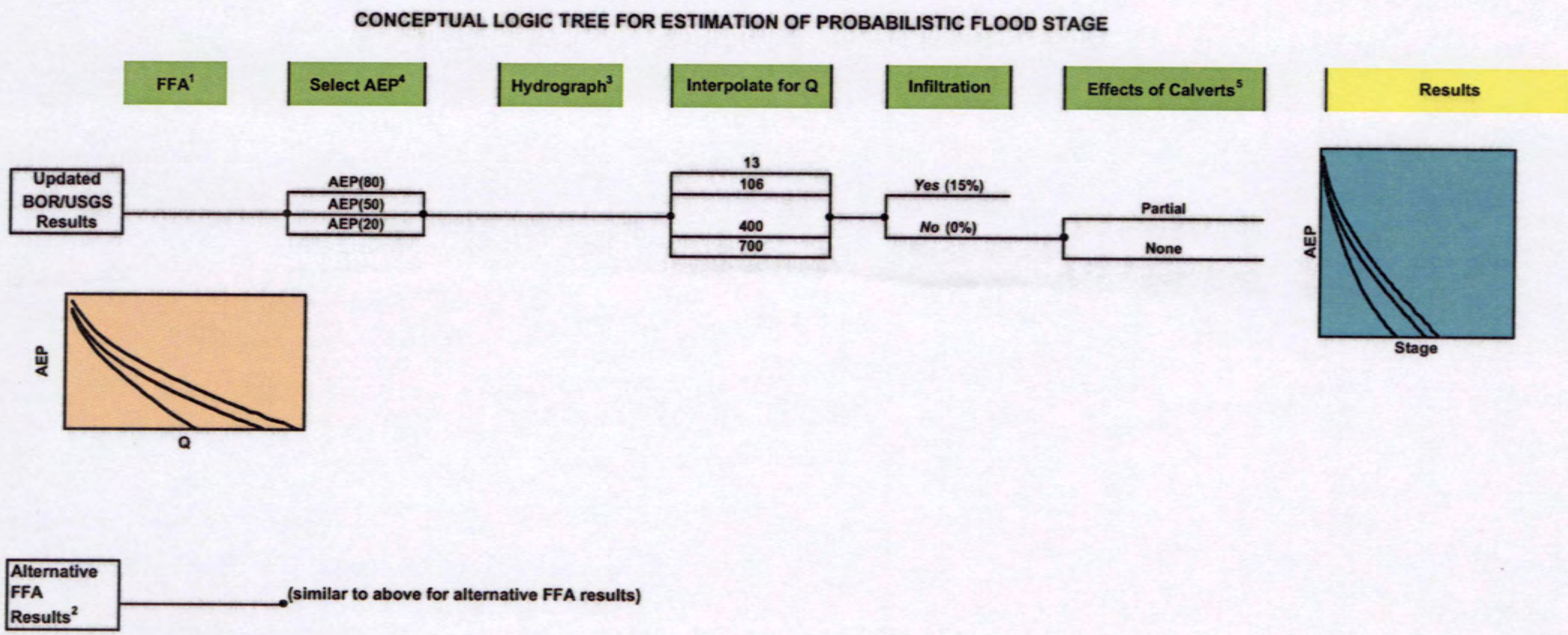
PSQ-4 What is the character of the logic tree to facilitate calculation of the probability of flood stage at INL facilities?

The initial intent for the development of a "standard" logic tree with appropriately weighted branches was seen as becoming more impractical as the project progressed and new constraining data were compiled. As a result the final "logic tree" is a straight line (see Figure 1), with the various PDFs and discharges input for the starting AEPs.

3.2.2. Alternative Actions.

Alternative actions (AA) are possible actions resulting from resolution of the above PSQ's. The types of actions considered depend on the answers to the PSQ's.

AA-1: There were three possible alternatives to PSQ-1: 1) the PDFs for flood stage at the facilities was not sensitive to variations in modeling or inputs, 2) the PDFs of flood stage show a broad range of uncertainty due to variations in modeling or input uncertainties, or 3) modeling and analyses show that well-constrained results can be obtained only for a limited range of discharges and/or probabilities.



Notes: [1]*FFA - flood frequency analysis*
[2]*Could include regulated flow frequency and/or upstream dam failure probability estimates*
[3]*Hydrographic Shapes are based on assumption of natural, unregulated flow*
[4]*Flow simulations have been run for Q values = 13, 25, 63, 106, 150, 200, 250, 300, 400, 700 ṁ/s*
[5]*For "partial" effect, main stem of river stays open; for "none," all culverts are assumed open*

Figure 1 Logic tree for probabilistic inundation modeling at the INL.

The results presented above for PSQ-1 support alternative 1 for AA-1. That is the PDFs were essentially insensitive to variations in models and inputs such as for bridges/ culverts and infiltration as compared to uncertainty in topography.

AA-2: AA for PSQ-2 are 1) characterizations of the paleofloods and paleohydrologic bounds contained in earlier reports are adequate, or 2) the paleofloods and paleohydrologic bounds need to be revised to include other values of age and/or discharge.

As discussed in the resolution for PSQ-2 above, alternative 2 for AA-2 was selected. Quality checking of topographic data determined that the topography used in the original studies was inadequate. Updated modeling based on new, more detailed topography led to increases in discharge values associated with the paleohydrologic bounds. In addition, the characterizations of previous paleohydrologic bounds were largely subjective in nature. For this study it was deemed necessary to establish a strong formal method for assigning bounds. This was done through a review of available literature on the movement of various particle types and the development of probabilitistic thresholds for erosion based on stream power and shear stress results from the updated hydraulic modeling as described in Appendix D of the final flood hazard report.

AA-3: AA for PSQ-3 are 1) the flood frequency characterization of the existing paleoflood study (Ostenaa et al., 1999) adequately represents the necessary inputs for describing flood frequency, 2) the flood frequency characterization in the paleoflood study needs to be weighted and used as one of several branches in a logic tree that contains other flood frequency relations, or 3) the flood frequency relations in the paleoflood study need to be revised as a result of revisions to paleofloods and paleohydrologic bounds developed from PSQ-2.

Initially it was felt that alternative 1 would be used for AA-3. However, significant differences were noted in initial flow modeling. These differences were found to be associated with errors in the initial 1993 topographic data. For this reason, alternative 3 was selected as the final outcome for AA-3.

AA-4: AA for PSQ-4 are 1) the logic tree is a single stem in which all probability information is derived from a single flood frequency relation from the existing paleoflood study, 2) the logic tree is moderately complex due to the need for including more than one flood frequency input and a moderate range of sensitivity in modeled stage/discharge at INL facilities, or 3) the logic tree has many branches and high potential variability in the weightings due to the large diversity of opinions on flood frequency and the broad range of uncertainty associated with hydraulic modeling of the stage/discharge relationships at the INL facilities.

As proposed by the BOR, the logic tree is essentially a straight line process as described by alternative 1. The stage probability values are derived from a single line process for the case of unregulated natural flow; however, the AEP and discharge assigned for other cases (i.e., Mackay Dam failure) will dictate what the final range of stage probability curves look like.

3.2.3. Decision Statements.

The decision statements (DS) combine the PSQ and AA into a concise statement of action. The DS for each of the PSQ's are stated below.

DS-1: Determine the probability distribution for flood stage at key INL locations at TRA and INTEC.

These are developed in Section 5 of the final flood hazard report and detailed in Appendix F of the final report.

DS-2: Determine probabilistic characterizations of paleofloods and paleohydrologic bounds in light of new field evidence and 2-D flow modeling for the Diversion Dam study reach of the BLR.

A framework for evaluating paleohydrologic information with the 2-D flow modeling results is developed and detailed in Appendix D. Geologic and hydraulic modeling results from these new studies in the Diversion Dam reach are discussed in Section 2 of the final report and revised probability distributions are developed in Section 3 of the final flood hazard report.

DS-3: Determine flood frequency inputs for the logic tree assessment of flood stage probability at the critical TRA/INTEC sites.

A revised flood frequency analyses is contained in Section 4 of the final flood hazard report.

DS-4 Determine the structure and weightings of inputs and branches in a logic tree for calculation of flood stage probability at the TRA/INTEC facilities.

This has been developed in section 5.3 of the final flood hazard report.

3.3. Identify Inputs to the Decision

This step identifies the informational inputs that are required to answer the decision statements made above.

3.3.1. Inputs for PSQ-1.

PSQ-1 will be answered by compiling the results of a suite of flow simulations from hydraulic models into probabilistic descriptions of flood stage versus discharge. Model inputs include topography of the BLR and facility area based on the INL 2-ft contour map, discharge input into the hydraulic model at the INL Diversion Dam, model parameters such as friction, viscosity, scenarios for infiltration, and scenarios for flow at culverts and bridges. The effects of variations in model inputs will form the basis for developing probabilistic descriptions of the uncertainty in flood stage at INL facilities over the range of modeled discharges.

One of the major efforts for resolving this PSQ centered on the INL 2-foot contour map and the 1993 aerial data from which it was derived. Initial hydraulic modeling results from the paleoflood study reach based on topographic data acquired in 2000 were found to be significantly different than modeling results from previous studies. Extensive analyses of the topographic data used in each of the modeling studies was undertaken in an effort to characterize the accuracy of each to the topographic data sets used in the flood hazard analyses. Grids derived from the 1993 INL 2-ft contour map appeared to be significantly warped in some areas. In 2002/2003 reprocessing of the 1993 data for the diversion dam study reach and the regional study reach was undertaken to correct for earlier processing errors.

3.3.2. Inputs for PSQ-2.

The inputs to PSQ-2 will be answered through a combination of direct field methods and hydraulic modeling. Field studies and hydraulic modeling for PSQ-2 will utilize 1-ft contour interval base maps and images derived from aerial photography flown in August 2000 of a 3 mile long study reach along the BLR located downstream of the INL Diversion Dam. These products will be used as base maps for detailed geomorphic mapping of the study reach and site maps for trenching investigations.

Trenching studies at three sites provided soils and stratigraphic data, which were used to corroborate geomorphic surface ages and identify deposits and ages of past floods. Samples were collected from the trenches for soils and radiocarbon analyses to develop a framework for estimating age(s) and age uncertainty distributions. Soil trench work followed the procedures included in Birkeland (1999) and Appendix B of the QAP. Hydraulic modeling of the study reach provided discharge estimates for paleohydrologic bounds and paleofloods that were identified based on the trenching results. Uncertainty characterizations of the discharge estimates were developed similar to PSQ-1. The resulting age and discharge characterizations were compared to the characterizations of paleohydrologic bounds and paleofloods developed in previous paleoflood studies.

3.3.3. Inputs for PSQ-3.

Inputs to PSQ-3 will be subjective weightings of the inputs to the logic tree based on participatory and final peer reviewer input, empirical data, and the professional judgment of project participants.

Paleohydrologic inputs for flood frequency are displayed as probability density functions (PDF's). The primary functional shape developed was a triangular distribution, although some PDF's developed trapezoidal shapes based on the data available for each of the inputs. Flood frequency inputs are portrayed as continuous frequency distributions and associated uncertainties for a range of annual probabilities and discharges. Flood frequency inputs from the previous study were given a weight of zero because hydraulic modeling results were based on inadequate topographic data, thus only the current study results are used. Other flood frequency studies by USGS have been revised such that continuous frequency distribution data is not available. The only available inputs are singular estimates for a 100-yr flood, which has statistical overlap with results of the current study and therefore was not explicitly used.

3.3.4. Inputs for PSQ-4.

The principle inputs to PSQ-4 include the probability distributions for stage/discharge from PSQ-1 and the flood frequency inputs and weightings from PSQ-3.

PSQ-4 inputs were compiled from the hydraulic modeling using the reprocessed topographic data generated in PSQ-1, and the PDFs developed from PSQ-3.

3.4. Define the Boundaries of the Study

This step defines the spatial and temporal boundaries for the problem. Without establishing firm boundaries one can never be sure if the problem has been solved.

Hydraulic modeling studies for PSQ-1 will include a three to four mile wide area along the BLR, extending from the vicinity of the INL Diversion Dam downstream to the intersection of the BLR channel and the INL Railroad that leads to the Naval Reactors Facility (NRF). This area includes the natural channel of the BLR, artificial channels constructed by settlers, channels constructed for flood management at INL, large areas of floodplain and alluvial surfaces flanking the BLR, and the areas of key facilities at INTEC and TRA (see Figure 1-2 of the final flood hazard report). Temporal boundaries for these studies are restricted to the period after 1993, the date of aerial photography flown to develop topographic base maps used in the hydraulic modeling, to present.

Studies for PSQ-2 will focus on a study reach of the BLR extending approximately 3-miles downstream from the INL Diversion Dam that was used in the previous paleoflood study, and where detailed topographic mapping has been done based on the August 2000 aerial photography. Trench sites were selected within this area based on the results of prior studies and the constraints imposed by cultural resource issues.

Temporal boundaries associated with paleohydrology will be restricted from the present to the late Pleistocene (< 100,000 yr. bp). Specific timeframes were developed as field work progressed, based on evidence for the largest observable flood and paleohydrologic bounds.

The spatial boundaries for flood frequency inputs to PSQ-3 and PSQ-4 potentially include the entire BLR drainage basin if data from sites upstream of the INL are considered in the flood frequency analyses. Temporal limits are the Late Pleistocene (last 10,000 years) for paleohydrologic data, and 1905 for the earliest gaging information, to the present.

DOE requirements for NPH characterization specify the development of a combined mean annual exceedance probability from all flood modes. In this study only the hazard associated with natural, unregulated flooding of the BLR is being analyzed. Other flood hazard modes, which are potential contributors to the mean flood hazard, include local run-on/run-off, effects of the INEEL Diversion Dam, and failure of the Mackay Dam.

3.5. Develop a Decision Rule

The Decision Rule (DR) brings together the outputs from steps 1 through 4 into a single statement describing the basis for choosing among the listed alternatives.

DR-1: Based on the flow simulation results conducted using the 2-D models, determine the characteristics of the probability distributions for flood stage/peak discharge at the key TRA/INTEC locations.

Stage-probability results for these sites are discussed in Sections 5.3 to 5.5 of the final flood hazard report. A full set of curves for each site is contained in Appendix F.

DR-2: Based on trenching studies and hydraulic modeling studies of the Diversion Dam study reach, compare the updated characterizations of paleohydrologic bounds and paleofloods for the BLR with those used in flood frequency analyses of the prior studies.

Characterizations and discussions of comparability are developed and detailed in Sections 2 and 3 of the final flood hazard report.

DR-3: Based on the evaluations conducted for DR-2 determine the inputs and weightings for flood frequency in the logic tree that will be used to evaluate the probability of flood stage at key TRA/INTEC sites.

As work progressed it became evident that the discharge AEP was the controlling variable in the analysis of flood hazard. As a result the logic tree developed into a single branch with the starting discharge AEP dictating the additional variables used. Variation due to other factors was determined to be less than the uncertainty associated with topographic resolution in the hydraulic models.

DR-4: Based on the results of DR-1, DR-2, and DR-3, calculate the stage/probability curves for the INL facilities.

A detailed discussion of the development of the stage/probability curves is provided in Section 5 of the Final flood hazard report. Appendix F provides additional curves for each of the modeled discharges at each critical facility.

3.6. Specify Tolerable Limits on Decision Errors

This step sets out the acceptable limits on decision error. These limits are typically used to establish performance goals for the data collection design. Because of the empirical nature of the data it is difficult to place limits on the data use. Discussions will be carried out between all parties concerned, with considerable input from the peer reviewers, to determine what qualifies as sufficient supporting data. The emphasis in all studies is to develop a probabilistic description of each of the inputs and modeled parameters in order to allow the propagation of potential uncertainty through the full study.

Peer reviewers provided comments and recommendations for follow-on work/additional research. Although this work is outside the scope of the present project, certain aspects would serve to potentially further constrain uncertainties or provide additional support for various actions.

3.7. Optimize the Design

The limits of the area selected for flow simulations to develop stage/discharge probability information have been defined by the limits of inundation shown by previous studies, critical facility locations, and sites where flood frequency inputs are developed and applied based on previous work. The study area encompassed an approximate three to four mile wide strip, centered on the present Big Lost River channel, and extending from the INL diversion dam to the railroad bridge east of INTEC.

The areas and trench sites for the evaluation of paleohydrologic bounds were based on the results of previous studies. This detailed study reach encompassed the Big Lost River channel and immediate overbank areas from the INL diversion dam to approximately three miles downstream at the old pioneer diversion. This area was selected based on multiple areas of channel control due to basalt outcrops.

Trench locations for paleohydrologic/geomorphologic study were selected using information and comments from the previous BOR study. The "Big Loop" site was chosen to evaluate the many earth mounds in the area, some of which occur in paleochannels of presumed Pleistocene age. Excavations through the mounds and channels showed that soils and stratigraphy were generally continuous across the channels, providing no evidence of post-late Pleistocene Big Lost River flow at these sites.

Trench excavations at "the Saddle" were carried out to determine constraints on flow through the saddle. This location included a 900 meter (2953 foot) long trench that yielded definitive evidence that water had not flowed through the saddle in Holocene times. Trenches on terrace sites adjacent to the channel showed evidence of a paleofloods about 400-600 years ago, but this evidence was limited to lower sites that did not reach a stage required for flow through the Saddle.

The final trench location was on a terrace on at sharp bend in the river. Previous modeling showed that as flood stage increased, this area became subject to increasing erosive forces. It was anticipated that trenches at this site would provide additional constraint on flood frequency. However, due to cultural resource restrictions, the project was unable to complete a continuous trench and unable to extend trenches to critical locations near the banks. A series of smaller disconnected trenches were dug which exposed a consistent soil and stratigraphic sequence, although absolute correlation could not be established due to the disconnected nature of the exposures. There was some evidence of flood erosion/overtopping, and a stage limit for this evidence at the site, but positive evidence of paleoflood stratigraphy was limited.

4. DATA QUALITY ASSESSMENT

A data quality assessment (DQA) is a formal process for examining the data results to see that they meet the users end needs as stated in the project DQO's. Therefore, data quality assessment can also be thought of as reconciling the reported data to the project DQO's. The EPA provides guidance for this reconciliation through EPA QA/G-9, "*Guidance for Data Quality Assessment*" (EPA, 1998).

The DQA process is the scientific and statistical evaluation of data to determine if the data produced are of the right type, quality, and quantity to support their intended use. Like the DQO process, which it supports, the DQA is an iterative process, consisting of five steps:

- Review the DQO's and sampling design.
- Conduct a preliminary data review.
- Select a statistical test.
- Verify the assumptions of the statistical test.
- Draw conclusions from the data.

As stated in the discussion on DQOs certain exceptions were made for this project as it is not a typical engineering design. The data for this project consists of empirical data such as model simulations, field observations, and narrative descriptions of the methods used to create the graphical end-products (stage/probability plots). In line with those changes the DQA that follows will present narrative explanations to the various questions posed under the DQO process rather than actual numbers.

4.1. Review the Data Quality Objectives and Sampling Design

The main objective of this activity is to provide a context for analyzing the data in later steps. The goal is to familiarize the data analyst with the main features of the program used to generate the data. The data analyst will review all documents associated with the data being reviewed. This will include the QAP, associated DQO's, and the draft and final reports. The DQO's, through the established principal study questions, will guide the assessment by providing an understanding of what the end use of the data is.

For PSQ-2, paleohydrologic bounds and development of paleohydrologic inputs for use in flood frequency analyses were expected to generally follow the conclusions reached by the previous study (Ostenaa et. al., 1999). These inputs are characterized with a best estimate or best estimate range, and an associated range of uncertainty. The study reach used for modeling is termed the INEEL Diversion Dam study reach. This is an approximate three mile stretch of the Big Lost River from immediately downstream of the INEEL diversion dam to the old Pioneer diversion structure. This stretch has been used by other researchers in the past because of the fixed channel constraints provided by frequent basalt outcrops.

Criteria for paleohydrologic bounds in the diversion dam study reach were developed based on data available in the literature, the framework developed in Appendix D of the final flood hazard report, and the site specific sediments evident in the trenches. The first step in the criteria development was to designate different geomorphic surfaces. These ranged from the upper-most Pleistocene gravel surface to the present channel bottom and most recent accretionary bars and terraces.

The next step in the process was to perform detailed soil mapping of the trench faces for the identification of specific facies that could be extended across the area or that represented a specific event. Standard geomorphic relationships (truncation of soil horizons, interfingering of different soil compositions, soil development/calcium carbonate leaching) were observed and recorded. As part of this task radiocarbon age dates were obtained for different well defined zones. Sediment provenance studies were also performed as part of this step.

The final step in this process was the application of the erosion thresholds developed in Appendix D and the field observations for whether a particular geomorphic surface had been modified. As such, PDFs for the various geomorphic surfaces observed were developed consisting of a minimum required value, a best/most probable value and a maximum value. The minimum value represented the minimum value of stream power per unit area or bed shear stress to cause modification of the surface. The best/most probable values represented the value that corresponded best with the observed field data, while the maximum value represented the stream power/shear stress value at which there was virtual certainty that a given geomorphic surface would be modified. These values were derived from field observations of material composition and literature values on their movement. Independent estimates from multiple subareas on each geomorphic surface were combined subjectively into a single probability statement for each paleohydrologic bound.

For PSQ-1, the 2-D flow modeling and a range of discharges determined from previous flood hazard studies were simulated. A sufficient number of simulations were conducted to adequately portray the variability in the model results that result from variations in the inputs and model used. Sensitivity testing of the models was used to determine the total number of simulations required for the analyses as described in Appendix C of the final flood hazard report.

Discharges were modeled with four adjustable variables related to infiltration (full or none) and bridge and culvert conveyance (full or partial). Full infiltration assumed an infiltration capacity of approximately 15%, while none assumed no infiltration loses. This is detailed in Appendix D of the final report. Full conveyance of culverts assumes that all culverts are 100% open. Partial conveyance assumes only the bridges/culverts in the Big Lost River channel are open to flow. Appendix C of the final report also provides details on bridge and culvert conveyance.

For PSQ-3, potential flood frequency inputs from the current and previous studies were weighted according to participatory and final peer reviewer input, empirical data, and the professional judgment of project participants for input into the logic tree assessment for PSQ-4. Inputs from previous studies were not used due to incomplete results or discrepancies in the previous data determined through the present study.

The stage-probability plots for the selected areas of INTEC and TRA were developed from the inundation flows performed under PSQ-2. The probabilities were assigned based on revised flood frequency analyses completed for the present study.

For PSQ-4, a logic tree for flood frequency and hydraulic modeling scenarios was developed (Figure 1). Based on the modeling results, the primary variable for the logic tree is the input discharge AEP. Examples of final AEP/stage diagrams derived from the logic tree for selected discharges are shown in the final flood hazard report.

To derive a set of AEP/stage curves using the conceptual logic tree developed for this study one starts by selecting an AEP of interest. The hydrograph shape for that AEP is propagated downstream to the point of interest. The discharges associated with the AEP are interpolated and constrained by known AEP relations (see Figure 2-12 of the final flood hazard report). Finally infiltration and bridge/culvert effects are applied. The final product is a set of AEP/stage (depth) curves for the different infiltration and bridge/culvert combinations with 95 percent upper and lower bounds. Depending on the distance downstream of the INL diversion dam, different discharges may appear as inflection points on the graph.

4.2. Conduct a Preliminary Data Review

In this step the data analyst will normally perform a preliminary data evaluation, calculate some basic statistics, and graph the data. The goal here is for the data analyst to get a feel for the "structure" of the data. Basic statistics are often used to provide a numerical determination of whether the data will meet the project DQO's. Because of the empirical nature of this projects data this evaluation is done less mathematically.

Outputs from the 2-D hydraulic modeling for each simulated discharge are displayed as plots of inundation extent displaying water surface and additional contour information for depth, stream power and shear stress.

Paleohydrologic inputs for flood frequency are displayed as PDF's. The primary functional shape developed was a triangular distribution, although some PDF's developed trapezoidal shapes based on the data available for each of the inputs. The peer reviewers were fully presented with all the assumptions made during PDF determination and asked to carefully evaluate those assumptions during their review.

Flood frequency inputs are portrayed as continuous frequency distributions and associated uncertainties for a range of annual probabilities and discharges. Differing weights of previous frequency analyses will be required because not all previous frequency analyses extend over the same ranges of discharge or annual probability.

Logic trees were to be portrayed in graphical form with simplified titles for each branch to show the relative weights and relationships of each of the elements used in the calculation of flood stage probability at the facility sites. In the final report the logic tree turned out to be a single linear process with results dependent on the initial AEP used.

One of the major setbacks to the completion of the project was the unplanned requirement to reprocess the input topographic data. The accuracy of topographic data used in the flood hazard studies was called into question when initial hydraulic modeling results based on newly acquired topographic data from the paleoflood study reach downstream of the INL Diversion Dam were found to be significantly different than modeling results from previous studies. Extensive analyses of the topographic data used in these modeling studies was undertaken in an effort to characterize the accuracy of each to the topographic data sets used in the flood hazard analyses.

Each group of topographic data used in these analyses has been compared to an independent, more accurate, set of GPS field survey data. Aerial photography flown in 2000 was used to generate 3-ft grid data for detailed hydraulic modeling of a paleoflood study reach downstream of the INL Diversion Dam. More than 800 points were surveyed with GPS in this reach to evaluate the accuracy of the topographic grid. National Map Accuracy Standards (NMAS) and American Society for Photogrammetry and Remote Sensing (ASPRS) accuracy measures indicate that the grid data is sufficient for mapping with contour intervals of approximately 1.3 to 1.5 ft or larger. For the same reach, grids derived from the 1993 INL 2-ft contour map did not meet standards for 4-ft contour mapping and appeared to be significantly warped in some areas.

For the main Big Lost River corridor between the INL Diversion Dam and areas downstream of INTEC and TRA, comparisons indicated that the original INL 2-ft contour map was close but generally does not meet ASPRS standards for class 1 mapping as a 4-ft contour map, the standard cited in Federal Emergency Management Agency (FEMA) Bulletin 37 for flood hazard mapping. The original 2-ft map also does not meet NMAS standards for a 4-ft contour map based on the present comparison data. The 2003 BOR 5-ft grid developed from the 1993 photography and original Aerographics control data meets both ASPRS and NMAS standards for approximately 3-ft contour mapping. Measured accuracy values on the 2003 BOR 5-ft grid are generally 25 to 50% better than values measured on surfaces derived from the 1993 INL 2-ft contour map. The level of accuracy achieved by the 2003 BOR 5-ft grid appears to be limited by the underlying accuracy of the original control network used for the 1993 photography.

4.3. Select a Statistical Test

The main objective of this step is for the data analyst to select an appropriate statistical hypothesis for testing the data. In a classical sense the data being tested are analytical values. These lend themselves directly to statistical comparisons. However, the primary data produced by the FHS study are not measured values but are empirical in nature. The data produced often consists of outputs derived from models or scientific judgments. Unlike analytical values this type of information does not center on an "expected" value. As such, most of these outputs do not lend themselves readily to statistical testing. The emphasis in this study is to develop probabilistic characterizations and to draw upon inputs from expert peer reviewers to assess and test whether sufficient basis exists for drawing the presented conclusions.

Expert peer reviewers were consulted at key phases during the study in order to assess the data and confirm underlying assumptions. Expert peer reviewers in hydraulics, paleoflood hydrology, and probabilistic characterization were included as study participants (see Section 2.1). Both participatory and final peer reviewers received detailed presentations, including a visit to the field area/trench sites, on the proposed methods and the final outcomes.

Multiple sensitivity analyses were performed on both the TrimR2D and RiCOM codes. Details of these analyses are presented in Appendix C of the final report.

4.4. Verify the Assumptions of the Statistical Test

The goal of this step is to determine whether the underlying assumptions of the statistical test are still valid given the data collected.

Participatory peer reviewers provided comments on the validity of data and assumptions throughout the project. Documentation of participatory peer reviewer comments are maintained by the PM. Comments provided by the final peer reviewers on the draft final flood hazard report covered application of the assumptions made for this project and are included in Appendix C.

4.5. Draw Conclusions from the Data

Once the statistical test has been chosen and it has been determined that the underlying assumptions are valid then the chosen statistical hypothesis is run and tested. Based on the results of the hypothesis test the data analyst draws a conclusion from the data.

Based on expert peer reviews, the draft report was revised and comment/response documents were prepared to document how peer reviewer recommendations were implemented into the final study report. Recommendations not implemented are documented and described, along with rationale for not implementing recommendations.

Conclusions drawn from the paleohydrologic data are detailed in the final flood hazard report. Draft final report peer reviewers provided comments (Appendix C) on the application of the paleohydrologic data to the final decisions. They also provided comments on the use and results of the 2-D hydraulic model inundation maps.

The results for PSQ-4, calculations from the logic tree, were to provide the principal conclusions from the study. As work progressed it became evident that a formal logic tree approach was not necessary as the discharge AEP was the controlling variable in the analysis of flood hazard. As a result the logic tree developed into a single branch with the starting AEP dictating the values of the follow-on variables. The peer reviewers felt that formal weighting of the AEP for other factors was inappropriate given the increasing uncertainties with the longer AEPs and the detailed bounding constraints developed in the report.

The final outcome is a detailed set of AEP vs. stage diagrams for different discharges at each critical facility location. The development of these diagrams is detailed in Section 5 of the final flood hazard report. This section also addresses constraints on extending the curves beyond and AEP of 2×10^{-4}. Appendix F provides a complete set of diagrams of the final probability/stage curves at the selected facilities at INTEC and TRA.

5. CONCLUSION

The overall objective of this project was to develop and document a relationship between AEP and stage/discharge for PC-1 to PC-4 facilities at the INL. Following guidance described in DOE standard 1020 and using the peer review process detailed in NUREG/CR-6372 as guidance, the design process and final curve development underwent a rigorous review by experts in the fields of hydraulics, uncertainty analysis, and paleohydrology.

This report documents the QA used in each step of the development process throughout the study to meet the stated DQOs. As part of this process a data assessment based on the DQOs detailed in the QAP is also presented. Assessment shows that all DQOs for the project were met. Peer review comments supported the documented process and generally recommended additional activities to provide further constraints on uncertainty in areas such as infiltration losses. The peer reviewers concurred with the final analytical process and outcomes as being reasonable and well thought-out.

The final outcome of this project revealed that the anticipated multi-branch logic tree approach was unnecessary. A single controlling variable (AEP) was found for the analysis of flood hazard at the INL. The initial discharge AEP determined the values for the additional variables used in the analysis. Plots of inundation depth for AEPs out to at least 1 in 100,000 years (10^{-5}) were generated for each critical location at the INL. Peer reviewers agreed that formal weighting of AEPs for other factors in the logic tree approach was uncalled for.

6. REFERENCES

Berenbrock, C., and Kjelstrom, L.C., 1998, *Preliminary Water-Surface Elevations and Boundary of the 100-year Peak Flow in the Big Lost River at the Idaho National Engineering and Environmental Laboratory, Idaho*, Water-Resources Investigations Report 98-4065, DOE/ID-22148, 13 p., 1 folded plate.

Kjelstrom, L.C., and Berenbrock, C., 1996, *Estimated 100-Year Peak Flow and Flow Volumes in the Big Lost River and Birch Creek at the Idaho National Engineering Laboratory*, Water-Resources Investigations Report 96-4163, 23 p.

Ostenaa D.A., Levish, D.R., Klinger, R.E., and O'Connell, D.R.H., 1999, *Phase 2 Paleohydrologic and Geomorphic Studies for the Assessment of Flood Risk for the Idaho National Engineering and Environmental Laboratory, Idaho*, Report 99-7, Geophysics, Paleohydrology, and Seismotectonics Group, Bureau of Reclamation, Denver, CO, 112 p., 1 folded plate, 4 appendices.

Hotness, J.E., and Rousseau, J.P, 2003, *Estimating the Magnitude of the 100-YearPeak Flow in the Big Lost River at the Idaho National Engineering and Environmental Laboratory, Idaho*, Water-Resources Investigations Report 02-4299, DOE/ID-22181, 36 p.

APPENDIX A

MINUTES FROM OCTOBER PEER REVIEW MEETING

Big Lost River Flood Hazard

October 5, 2004
Stoller Corporation
Idaho Falls, Idaho

Attendees:	Dan O'Connell	Bureau of Reclamation
	Robert Creed	Department of Energy-Idaho
	Scott Jensen	BBWI
	Dr. Klaus Jorde	University of Idaho
	Dr Paul Link	Idaho State University
	Dr. Upmanu (Manu) Lall	Columbia University
	Chris Martin	S.M. Stoller Corporation
	Kathy Morgan	S.M. Stoller Corporation
	Dean Ostenaa	Bureau of Reclamation

Bob welcomed the group and thanked them for the contribution that would be given to the report. He stated that this is a diverse group that reflects multi disciplinary nature of the project. The site with an operating nuclear reactor not only is it multi disciplinary but each of those disciplines has to be pursued in a very serious and rigorous manner to data and analysis supporting the conclusions that we reach on this project. I want to stress that this is not the first review that we have had here. We have had a number of peer reviews and most importantly during the last segment of work we had what we refer to a participatory peer review as recommended by NUREG 6372. That nuclear regulatory commission guidance on the use of experts on the seismic hazards analyses is basically the template for the review structure and process that we have been following.

Bob continued with a review of maps of the INEEL, Mackay Dam, the flow path of the Big Lost River, Big Lost Sinks, and Little Lost Sinks. Internal drainage system, one of the important conclusions you draw from the drainage system is that over geologic time this basin has never filled up with enough water to cut a path to the Snake River. It provides a first order very broad constraint on the consistency of flows.

For the validation review, a couple of key questions we want to ask are

1. Do the processes, all of the processes--review, data acquisition, analytical--support the probabilistic models?
2. What changes, if any, need to be made to our probabilistic models to optimize our defensibility and acceptability?

Bob emphasized that extensive reviews have been preformed and he has sent comments and comment response documents to illustrate that point. So in the classical sense this is not a peer review requiring verification and validation in a formal sense. Having said that, however, Dr. Jorde and Dr. Lall are world class experts in their fields and if they see something that looks like it really has to be addressed in technical perspective, we will certainly considered that very seriously. If you feel like it is so bad that we can't proceed with the final probabilistic model. We obviously have more work to do.

Bob commented that here is what we have to have out of this.

- ✓ 100 and 500-year flows that are equal and arguably exceed FEMA techniques and requirements.
- ✓ DOE flood model that goes out to 100,000-year return period
- ✓ Lay the foundation for subsequent work.
    - Consideration of how do we assess and modify the INEEL Diversion Dam
- ✓ QA quality assurance and quality control has to be documented and robust.

He continued while we have a note taker here and I appreciate that. Almost every meeting I run like this I demand that people put things in writing if they think it is a serious issue. So in spite of having a note taker, if you have an issue that has to be addressed we have to see it in writing. Ultimately, we are going to transfer a nice, wonderful, incredible, extensive, and defensible data set to the INEEL. That will be Scott's job or he will have a hand in it somehow. Do the INEEL A & E standards still exist?

Scott replied yes.

A map was reviewed and the chance for flows to come out of Mackay Dam if it were to fail and directions of flow of Big Lost River were discussed.

Bob stated that one of the important issues is the amount of irrigation and diversion that comes off the Big Lost River. That is reflected by the amount of land in production; he continued by saying that the 1999 report was unique and groundbreaking successful study. The study resulted in four (4) peer review publications thanks to the hard work of Dan and Dean.

Bob added the reason Dr. Baker and his graduate student were involved. In 1993, Sarah Rathburn published a paper saying there was a 4 million cubic feet per second 20,000-year-old glacier outburst flood. Being a geologist, I ran into the field and tried to find evidence for it and it really wasn't there. That stimulated a lot of interest on my part to review the geology that constraints or characterizes flood hazards.

Bob continued saying the other thing that happened here that we ought to talk about is that there has been a discussion between the USGS, Bob, and Dean about what the 100-year flood should be. Their initial 100 flood estimate, Berenbrock and Kjelstrom was 7,320 cfs at Arco and after going through the diversion dam reach and with attenuation it ended up at 6,220 cfs on the INEEL and again being a geologist I took a look at the maps and the geomorphology out there and thought that is just crazy, again stimulating the need for more detailed work. Joe Rousseau and Hortoness produced a 100-year flood report that is much more realistic. The number of assumptions that went into this were extremely conservative and probably had no basis in reality; definitely had no mathematical basis, to assert of a flood with the AEP of 1 in 100.

In spite of the work done in 1999, the flood hazard work remained controversial and a lot of people with a lot of interest in how the work was done, who did it, and how much money they received. That led to some identification of issues and uncertainties that were marginally significant in my opinion. Nevertheless, because we have an operating reactor and because we are interested in promoting the site as a place for future work we need to get a real good defensible handle on the flood hazard regardless of perception of the uncertainties raised by others we really have to nail this thing down. The work you will see over the next few days resulted from the attempt to address some of these issues. We also would like to extend the flood hazard to the 100,000-year return period, which we all recognize is extremely problematic. We are not talking about a flood that is going to happen every 100,000 years. We are talking about mitigating an event at a facility with a risk such that we cannot afford to have a flood with an annual exceedance probability of about 1 in 100,000. Climate change all that other stuff—qualitatively and heuristically we all think of that stuff but in terms of the functional mitigation requirements we are not going to be worried about that. Now in some other cases, we are talking about storage of spent nuclear fuel, there are some requirements to look at "is this site safe over the next 10,000 years." That is not were we are going. We need to provide inundation maps across the INEEL and some other things. Because of the serious, rigorous nature of the implications of the products of this process and work, we are taking a hard look at every piece of data that goes in to it. One of the things we looked at was the contour data and one of things that came out of one of the previous peer reviews that should give you some comfort about the nature and scrutiny this has gone through was that our contour data was bad. One of the things we will deliver to the INEEL is reprocessed the contour data.

Bob explained Diversion Dam has two culverts that will pass 900 – 1200 cfs. Water that is diverted goes into the spreading areas. In my opinion these spreading areas have infinite capacity because there is a slope off to the southwest that will allow water to go forever. Water not diverted to the spreading areas continues along the Big Lost River pass the Test Reactor Area, INTEC to the Big Lost Sinks, and Little Lost Sinks. What is not shown here are the various man structures, pioneer diversion, and other things that complicate the story somewhat.

Dr. Jorde asked if there was any flow below 900-1200 cfs goes into those sinks.

Bob replied no; those culverts can be closed. If those culverts are totally open the most water that will go through here at bank full discharge or culvert capacity. That would be all of the water backed up to the top of the diversion dam so you have a head on the water and it is forcing water through the culverts you can get 1200 cfs if the diversion dam does not fail. That's one of the things that will come out of this once we know the flood loads. I can go to management and say look if you want the diversion dam to withstand the 500-yr flood here is what we have to do and try to get it certified for a flood control structure.

Dean added as a footnote there is a gauging station around the diversion dam since about the mid 1980s and since that time the largest flow that has gone down the Big Lost River, downstream of the diversion channel is about 800 or 900 cfs. Out of the last 20 years probably about 10 of those years there has been no flow at all downstream of the diversion dam. The last time there was flow downstream of the diversion dam was in 1999 or some time in the late 1990 maybe 1997.

Bob continued we are also working on an integrated groundwater and surface water management strategy; where we have to figure out what to do with the water. The two end scenarios are (1) Keep the culverts wide open all of the time and take our chances with water percolating into INTEC perched aquifers which contain strontium at 90,000 curies per liter. (2) Close the gates and let water go nowhere but into the spreading areas and take our chances that it will not migrate into the subsurface to the RWMC and send nasties down to the aquifer that way.

(3) And the third thing to keep in mind is that if you keep this channel flood dry you have a nice infiltration capacity if you do have to release water from the diversion dam. This is a real classic optimization problem. I've talked to Fritz Fiedler about how unique is it and is it worth doing as a research thing but convincing management is difficult. There is a big picture framework that goes beyond flood hazard stuff.

Bob commented this is the 1997 flow looking downstream of the head gates. This flow was 800 cfs. The capacity of the diversion dam based on geotechnical work done by Clint Kingsford based on standard penetration test and tensiometer is that the diversion dam will take 6000 cfs with a factor of safety of 1.9. So for practical, functional purposes the diversion dam is safe at less up to that number.

Bob explained the diversion dam itself is built reasonably well. As you get down farther it is actually a dike system.

Scott continued the original dike was built when the diversion dam was built. Then the dike was raised a little bit during the winter after the Mt Borah earthquake. The river flowed all that winter because of the earthquake. There was an ice dam that kept raising the water level that winter and so the dike was temporarily raised.

Dean interjected the discharge was only 50 cfs. It was really, really minor but it was the ice damming issue, it kept freezing....

Scott added that spring they raised the dike by two or three feet for several miles.

Bob said while we have wonderful geotechnical data from two points along the diversion dam, there is no guarantee that factor of safety applies along the whole reach of the diversion structure.

Dr. Jorde asked is that is flow over a spillway or is that uncontrolled flow over a dam?

Bob replied the dam overtops at about 7000 cfs. This is one foot of free board.

Dean said tomorrow we will go to that spot and have a talk in more detail about it.

Bob stated he has some preliminary 2-D routing of flows against the upstream side of the dam that shows where all of the water will go if the velocities might....

Bob showed a gauge record that emphasized what Dean stated about getting pretty good flows and then nothing for years. In terms of flood hazard and risk, what are we looking at in terms of real data at the INEEL. If you just look at that it is difficult to convince yourself that any of this stuff is worth doing. Nonetheless, requirements are there for us to do this study.

Bob said this is the Arco gauge and I am assuming you are going to show more of this stuff.

Dean replied no; actually I wasn't going to go back through all of the hydrology, so this is good.

Bob okay, I explained to Dr. Jorde this estimated point, notice this is the highest point on here. This is based on a slope conveyance method when the Arco gauge got wiped out. I think there is significant uncertainty associated with that. I was telling Dr. Jorde I look at the actual surveyor's notes, maybe you can remember this better than me, he had one paragraph of measurements that came out 5000 cfs and he said, no this can't be right. He redoes it and comes back with 2500 cfs. Who know what that number really is, but the USGS makes a big deal of this point. The Arco gauge data—there are some zero years in there. The scatters are about what you would except for a heavily regulated system.

Bob continued with information about the diversion dam. Then he showed an image with a better view of where RWMC is located and showing how close it is to a diversion structure. Bob explained a hydraulic gradient and in the saturated part of the aquifer at about 650 feet below ground surface. The gradient is in the southwest direction. So for water to get to RWMC from the spreading areas it has to percolate, then hit something, move to the east, and then percolate down and back out. The USGS claims they put tracer in the spreading area and then found traces of that tracer by RWMC. The report was published last year.

He stated this is an extreme flow model if you will. This is Mackay Dam failure with a probable maximum inflow at Mackay Dam with a full reservoir with an over topping failure. You can see what that inundation would look like. This was completed in 1986, well reviewed, and a credible report for its time. The problem is there is only three survey cross sections in the whole report. There are significant uncertainties in where these boundaries ought to be. Nonetheless, at this flow they have computed an elevation that at INTEC of 4917 ft that turns out to be incredibly critical. If you go six inches higher than that, bad things happen. The report authors assumed a 40 percent infiltration based on the Teton Dam failure.

This is the 100-year flood at INTEC based on the USGS old number. This is a very preliminary estimate although our critics would like to make us live with this number. The Idaho Nuclear and Technology Engineering Center is partially inundated with this flow. The cross sections actually straddle the INTEC. What they have had to do for the facilities located between those cross sections is interpolate between them. Which is a big no-no for this type of routing nonetheless it is what people are stuck with for a while.

Bob explained this is why we are doing this:

- ✓ Laws that require 100-year flood plain litigation and executive orders,
- ✓ DOE requires assessment of the 100- and 500-year flood plain critical actions, and
- ✓ FEMA requirements.

Bob stated he was able to get headquarters to get away from the FEMA guidance. The way 10 CFR 1022 was originally written says you have to use a Federal insurance agency/FEMA type flood hazard analysis. When this regulation came up for review; I reviewed it and provided input until they changed this to allow DOE type flood plain characterization. This is what we are doing now and it is a lot different than the FEMA flood hazard characterization. This is what is causing us all of the grief. The RCRA requirements require that a FEMA type flood frequency determination as delineated in Bulletin 17B to determine the 100 year flood. The problem is Bulletin 17B, and it says this on page 2, does not apply to regulated systems. So we are put in a box. On one hand we are told to use FEMA guidance and on the other hand FEMA says don't use us. In the laws and requirements, there is no out. The reason why these reviews are so important and why they are going to be rigorously documented and why our conclusions have to be robust is when stakeholders drag me in front of the Citizens Advisory Board or wherever and say your 100-year flood is not a FEMA flood therefore it is invalid go back and start over. I can say well it is not a FEMA flood but look at the pedigree of this thing, look at the amount of peer review, look at the quality of research, and look at the quality of the reviewers. Even if you look East Texas State University folks that provided the database for Bulletin 17B statistical model validation LP3 if you look at the appendixes for that stuff it all says this applies to natural systems. Everything about it says don't use for regulated systems. It puts the engineers in a box because you have all of these laws and professional practice states use FEMA but how many systems do you have in the United States that are unregulated systems now.

What we are doing is not an Ad Hoc analysis it is firm, consistent, and robust support for this methodology of using the DOE standards. Our argument for not using a FEMA type analysis besides a regulated system that interferes with using FEMA is we have our own DOE orders and standards that tell us how to do flood hazard analysis at the INEEL. One of the DOE site characterizations standards actually states what to do. It recommends peer review, probabilistic modeling. There are very few PC4 – operating reactor facilities. Hazard category 1 states that this thing has significant risk for people off site. This would be significant risk for workers on site, so presumably something like high level waste due to the long half life of things and other things that would qualify as a PC4. Next slide…This is what we started out to develop and this is what you will see over the next few days, the end products that we are after. Here is the ….I mentioned that DOE allows you to develop probabilistic flood hazards. This is actually out of DOE guidance or position paper something like that and this is how they use an example of how you do this you come up with a mean flood hazard curve based on things like dam failure, river flooding, levee failure, run on/run off, tsunami, etc., and you come up with mean flood hazard based on all of those inputs. Right now we are going to produce this curve right here, all of these others will get dumped in there sooner or later. Next…

Dr. Lall commented hopefully not a tsunami

Bob stated you have no idea how much I want to get this over with. I'm getting……my reputation is….I don't even worry about it anymore. In terms of INEEL hazard categorization history for floods, this chart summarizes things pretty…..do you have anything like this? This is why I embarked on this thing. This shows various types of INEEL flood hazard estimates that have been conducted in the past. Here I've labeled something engineering time estimate which is your classic LP-3 let's take the stream gauge data and use it. You can most of those are for the 100- and maybe 500-year floods. Here we have had other hydrologic estimates done at the INEEL. The most famous one, of course, is the Rathburn one which gives us the giant numbers here and here is the 1999 Bureau of Reclamation paleohydrologic estimates to give you an idea of where that 1999 analysis fits with respect to the historical data. You can see it tends to be lower that's good news and it's bad news. It's good news because it's defensible and obviously we like lower numbers for reasons I'll get into later.

Dr. Lall said your RCRA flood is a billion year flood according to that slide.

Bob continued saying in the 1999 report we do have some questions about these numbers here for the 20,000 or I should say 10,000; no, 50,000- and 100,000-year floods. Again that why we had to get Baker involved because he was responsible for these two big outliers here and I wanted him engaged as an advocate and peer reviewer for this stuff. He was very opened minded and receptive of the whole thing and one of the things this illustrates is how rigorous and systematic paleohydrologic data can reduce the conservatism you tend to find in the engineering type estimates. The uncertainty in our 100,000-year flood model is what is so vague. This is our current RCRA flood at 24,480 cfs. The new USGS flood is much more reasonable and will have error bars that over lap with what Dean will come up with. This is an outrageously high number—if this is our 100-year flood how do we scale this out to 10,000 and 100,000 years. Nobody is going to let us make it flat, nobody is going to let us drop it.

Our end goal is to develop site wide inundation maps and stage versus frequency diagrams. Uncertainty in hazards assessments can easily result in, and as it has in the past, in a net risk increase to the public. Why? Because the resources allocated to poorly defined hazards could result in fewer resources available for well-defined risks. The risks need to be independently and rigorously and defensibly determined. It shouldn't this is the risk you should spend money on because I have the most political clout or that I scream the loudest it should be the risk that you most credibly and defensibly come up with. Hazards and risks have to be defined in a rigorous, systemic and consistent context to ensure effective resource allocation.

Bob continued saying standard engineering techniques that have been used by most previous studies are not appropriate for the regulated Big Lost system or DOE return period requirements. You just can't take LP3 analysis and scale it out to 10,000 years. People will do it but it just doesn't make any sense; people that work with this stuff often will tell you that. The DOE standards provide an alternate methodology for standard engineering studies. In terms of process and requirements we have firm regulatory and legal support for what we are doing here in terms of procedures. NUREG-6372 is the template that we are using for the peer review. This is the SSHAC report on guidance for use of experts in seismic hazard analysis. Some of you may know that a few years ago there was this problem with using experts in seismic hazard analyses and they had no way to levelize out the various opinions, ranges of opinions, and outlier opinions, part of it was due to faulty peer review process. And part of it was due to taking what people were saying when they had a peer review they would equally weigh everyone's opinion.

Dr. Lall asked what do you mean by mean probabilistic flood hazard. There is a couple of ways one could look at this. One is that I could come up with six different scenarios for failure each has some probability. The other is I list my tree of options and say these are your scenarios.

Bob replied the DOE guidance is to construct a plot or graph of various things and come up with a mean out of all the data.

Dr. Lall stated that this could be approached from a reliability analysis prospective and say here are the different modes of failure. These modes of failure can occur in combination. The main point is you stop short of calculating a mean. What you successfully communicate is perhaps you would use when you compute the mean.

Bob stated that when the mean is computed none of the data would be filtered out. The primary function of the probabilistic model is to help us quantify and identify significant sources of uncertainty. I challenged Dan and Dean to come up with a model that is robust enough that we can plug in these other failure modes, Mackay Dam failure and diversion dam failure as we go along and use of the logic tree seems to be the best way to do that.

Bob then showed a model that illustrated the utility of paleohydrologic data. Then reviewed the current 100-year plan illustration, showed Trench T-6, and gave an example of the stage versus frequency curve that DOE requires.

Bob finished by reminding everyone to submit issues in writing.

Chris Martin then stated that everything that happens would be documented. The technical approach that was used to collect field data would be reviewed as well as the conclusions to determine if the conclusions are supported by the technical aspects. Is what they did right as compared to what they are saying they came up with. The other part of it is looking at 2-D routing models; doing the same thing basically. Technical approaches of how they did the modeling, conclusions of the modeling, and comparing. Are the conclusions they drew appropriate for the model they used. We are not going to do a review of why TRIMR2D and RICOM were chosen over other models.

Chris reviewed the plans for Day 2. It will basically be a field day; looking at the diversion dam and trenches. At the trenches, the logging results will be reviewed and Dean will spend time discussing the features that haven't been modified by flow as opposed to those that have. Looking at constraints of the river, there are two or three areas where the river is bisected by basalt flow so it is constrained and will give a good idea of the flow.

Bob added another important aspect of the field day is for Dean to point out some of the issues that we have had to resolve. We did pay serious attention to the peer reviewers' issues, that guided the very rigorous and systematic way our data collection and analysis was done. We were very responsive to those issues and concerns.

Chris reviewed Day 3. Thursday will be spent revisiting the field day, 2-D modeling, and answering any questions that Dr. Lall and Dr. Jorde may have after visiting the field. Then end by discussing the probability curves and come to some agreement on weightings for the logic tree based on best estimates and data that is available. So we can begin work on the probability plot.

Bob stated what we want to emphasize is a mean centered approach for parameterization and let the probabilities fall out on the curves.

Chris mentioned that we are not trying to say this is the value, we want to say based on the information collected these constraints are the best that we have so our value is somewhere within there. Then we know within a 95 percent certainty that that is an accurate number. So we have good, defensible, appropriate bounds for that number.

Dean stated that he would discuss the topographic data, which was a major issue, geomorphic analysis, and trenching. Dan will discuss the 2 dimensional modeling result from the paleoflood studies, which is a limited flood reach study near the INEEL diversion dam. Then Dean will discuss the paleohdyrologic bounds criteria. That is how do we reach conclusions from the marriage of the geologic data and the hydraulic modeling data.

Dean stated that he wanted to make sure everyone understood the diversion dam because it is a huge issue with any regards to the flood conclusions. Tour the facilities and look at the channel (when in the field.)

Then Dean reviewed a map the showed locations of Mackay Reservoir, Leslie, Howe Ranch, Moore, Box Canyon, Arco, and the paleflood study reach. At these locations, data has been collected for as much as 100 years. Howe Ranch records date from 1904. The basin is about 200 square miles in the Pioneer Mountains, which is unregulated, and this is where all of the water comes from. The Big Lost River drainage at the Arco gauge is 1800 square miles and the largest flood in 50 years of record was 2500 cfs. The largest flood in the Howe Ranch gauge in 100 years was 4000 cfs or about 50 percent larger than the largest flow recorded in Arco in the shorter time. The largest flow in the Howe Ranch has been in the last 50 years and should be represented in the Arco but it is not.

Paul asked a question about the gauging of Antelope Creek.

Bob stated that there is no gauge on Antelope Creek, as it doesn't see much flow; it has the largest drainage area but no large flows.

Dean added the primary source of all the closed basins is snowmelt in the surrounding mountains. In the case of the Big Lost River there is a number of limestone bedrock units, which provided ample opportunities for infiltration of water. The conclusion of the hydrologic studies is that it is a losing system; flows decrease downstream systematically. A little bit of the analysis of that is included in the 1999 report and that was picked up on and elaborated in the recent USGS report.

Bob mentioned that in the 1996 USGS report the assumption was yes there are loses but those loses are exceeded by the contributions from the 22 sub basins. The argument was that the same time the Big Lost River is peaking each of those sub basins is peaking and more than off sets any infiltration loss.

Dr. Lall asked this is a snow basin? I don't know what the snow pack looks like but it seems reasonable that all of the snow pack will be consistent with elevation pack. Dean replied that no that there is a very a strong elevation gradient increasing to the north.

Dean continued that as a result of numerous studies is that there is convergence now as opposed to divergence of opinion. People are agreeing with the data and that is the stream flow decreases downstream on the Big Lost River and should be characterized as such.

Dean then reviewed a USGS geologic map of the site. The Big Lost River flows down through the site. The major facilities we are interested in are INTEC and TRA lie on either side of the river on this big area of Pleistocene alluvium and Holocene alluvium flanked on either side by basalt flows of different ages. This region of basalt has been mapped as being 600,000 years of age. Other flows are mapped as 200,000 to 400,000. These flows in the bottom part are generally 100,000 or 200,000 so both settings of the Big Lost River are on the Snake River Plain. The Snake River Plain is in one sense an aggrading system, that is periodically there are basalt flows that have erupted from volcanoes on the plain and built the surface up. Over all the plain is slowly subsiding and is seems to be in some kind of quasi-equilibrium. The streams that come out of the mountains carrying sediment are bringing that sediment out on to the plain and defeating the basalt flows that are trying to block them off. Paul has done a lot of work on the Big Lost River Trough. It is a persistent low where sediment from the Big Lost and Little Lost and Birch Creek has been infilling over the last several hundred thousand years.

Even though this system is an aggrading system it is also a stable system for purposes of looking at short term things in that we have the Big Lost really locked into the basalt flows.

Bob commented that another important point is that there is no short wave length structures or faults to screw up our assumptions about the slope change with time.

Dean continued we have historical flood data that we can use and that will be the best. We have a huge effect with the gauging records for regulation and floods attenuate downstream both due to that regulation as well as natural factors.

The geological evidence of floods confirms downstream of attenuation of peaks. The geologic framework within which the river flow decreases in size as one moves downstream. On the site we have documented positive evidence of paleofloods, that is a prehistoric flood from the geologic record that is

50 – 100 percent larger in peak discharge than the largest historical floods in the Arco gauging record. At the same time this geologic data provides us with very long-term limits, thousands of years in length for the limit of the upper bound of flood magnitude in those regions. In a

heuristic framework for estimating floods hazards at INEEL all the key facilities are built on late Pleistocene geomorphic surfaces and deposits. That is those are surfaces that are 10,000 to 20,000 years in age and have generally impacts of that surface morphology, which is an indication that no real large flows or frequent flows have been coursing through those facilities under somewhat natural conditions. Now we have to be a little careful with this, we'll revisit this in the context of the site.

Dean then reviewed prior study findings with the group and stated that this is an historic plot 2500 with positive geologic evidence of a flood which we characterize as being about 3500 cfs about 400 years ago and geologic evidence that limited the largest peak discharge in about 10,000 years to about 150 cubic meters. Then we did a flood frequency analysis incorporating this data that was significantly different that a lot of the previous characterizations. The fundamental concept that we are using for our geologic study is a paleohydrologic bound.

Dean presented some of the factors that are really important in these concepts. From a geologic perspective, think about what you see in a river after a large flood. There is some level in the river usually marked by floating debris and other obvious flow indicators that represents the high water mark of that flood. Below that level there will be obvious geomorphic evidence of that flood; like channels on stream terraces, erosion of soils, and deposition locally. Those features tell that those sites were under water so the stage had to exceed all of those features. Maybe at similar elevations or slightly higher or slightly lower there will be slack water that is fine grain deposition. Then down the main channel there will be gravel bars and obvious fluvial bed forms. These will all be fresh; there will be little or no soil development on those.

Dean continued over time all of these features are modified by weathering and soil development. It shows a stable landform surface with soil that reflects the amount of time since that last flood. The basic idea with the paleohydrologic bound is that if we can differentiate between the upper limit were we see positive evidence for past floods, if we can differentiate between that versus a level in the landscape where we see positive evidence of long term stability then we have a proxy for the paleostage that hasn't been exceeded over the time in which this soil has developed. Now notice that I've drawn the Paleohydrologic bound way up here, well above that level. This is the problem of resolution and how we go about characterizing in terms of discharge and stage of where to place this, because we need to have some kind of criteria for sufficient evidence to know that the flood hasn't occurred since this surface stabilized and the soil developed. Generally the answer is no because just barely having water on it isn't enough in most people's opinion to identify. The flow needs to be enough across the surface to have changed it in some way. So there would erosion or deposition but some geological record that geologists can go out into the field and identify and agree on. There will always be some variation and one of the things we have been working on in this current study is refining the basis for reaching a conclusion on where to establish a paleohydrologic bound.

Dr. Jorde asked where in this section do you determine age. Dean replied we'll come back to that.

Dean showed the results from the 1999 report illustrating how the bounds were applied and said this is information about the section of the site called the saddle. It is a low spot in a general ridge and at 100 cubic meters there is no flow and at 150 cubic meters the flow overtops and develops high power in the area down stream of the saddle. This is an area that from geomorphology and soils we said this is a Pleistocene surface or more than 10,000 years old and

geomorphically it appears to be unmodified. These numbers are grid cells and each cell is six feet; so 50 units here is approximately 300 feet.
Dr. Lall asked now where the colors are deep blue and have low stream power these are areas where you would have sedimentation or these are areas where….is the topographically high….they're all topographically low right?

Dean responded well no, some are high…it is a thin shallow flow here on a high surface. This is deep stagnant flow….

Dr. Lall interjected so let's take the thin shallow flow on the high surface. What would be happening here is sedimentation, would that be a zone where sedimentation just doesn't even get up.

Dean replied that's the 64 dollar question with regards to the bounds and that….as I'll go on to say later this is one warts on the whole concept that we did not, you know, elucidate, I think, in enough detail, but in a general sense what one would expect is as sediment accumulation occurs at very low power and stagnant sites. The question is what is the quantifiable measure of low, is it one or is it fifteen. Likewise erosion occurs at high (power.) We have had many debates about what is high. We show this scale at 0 – 100 the actual numbers in these plots actually go to 300. In the 1999 study, we defined high power as 50 – 100 watts per square meter and made some very general arguments as to why that should be appropriate.

The paleohydrologic conclusions to that report were based on a limited number of bank exposures. We really didn't have very explicit criteria for developing the bounds. The use of paleohydrologic bounds were introduced into the flood frequency game in the '90s and are continuing to evolve. There are issues with the characteristics of the eolian deposits and transient nature of some of the subtle landforms like around the saddle and other places. These are issues that we are trying to explore in the geological studies that we laid out. After the 1999 report, we didn't realize how important an issue topography was until we got well into this study.

The flood frequency that we determined for the 1999 report that was reached from previous estimates and that was controversial. It did not fit with previous engineering paradigms that had been applied at the site and other perceptions of what the flood hazards should be. In the context of developing defensibility for regulatory purposes we needed to develop a higher comfort level throughout the site and for everybody who is going to use that data.
Bob added this is germane to the discussion of driving towards a mean to develop a frequency curve and letting the frequency curve tell you what hazards should be for a certain risk. Versus driving observances based on your professional opinion and what you think the hazards should be. That is the root cause of the culture clash.

Dean stated that beginning in late 2000 we started out on a course that we are trying to complete now. The first major task was to evaluate paleohydrologic bounds. The plan involved doing more geomorphic and trenching studies. It was recognized up front that a more detailed topographic base for that reach and obtaining a highly detailed photogrammetric base map of the study reach. Also do more detailed 2-D modeling to update and reevaluate the results of the 1999 study. In 2001, two-dimensional flow modeling to evaluate facility inundation and the final aspect is to develop the framework for estimates of flood stage probability at INTEC and TRA. Then consider the effects of infiltration, bridges and culverts on the site scale modeling. After we generated the grid, we realized a 2 foot grid was not going to cut it, so we developed a

5 foot grid from the 1993 photography and there were major holdups due to the verification of the photography for stage probability estimates at the facilities sites for scenario and inputs.

Dr. Jorde asked when you say a 5 foot grid what is that?

Dean explained it is a 5 foot rectangular developed from photograph, TRIM notes……xyz

Dr. Jorde inquired what vertical resolution does that have as compared to the 2 foot contour lines.

Dean responded I'll go through that….that's what we are trying to get through here

Then Dean addressed the topographic issues. The topographic resolution and accuracy are significant contributors to stage probability uncertainties other than input discharge. The topography upstream and near the facilities is complex with many small channels and low thresholds that affect the distribution of flow likewise large grids. Therefore, 1-D models are not adequate to resolve the small details and variation of flow distribution across the site. On the regulatory side, that topography needs to be able to be shown to meet some accuracy standards that are embedded in the regulatory standards.

He said the original data for the 2000 study called for one-foot contour map for the diversion dam study reach. This was the base map for detailed geomorphic studies and as details were updated for hydraulic modeling. The inundation modeling used going to existing INEEL 2 foot contour maps that had been produced from aerial photography in 1993. When we initially tried to grid those maps we found numerous artifacts and poor resolution of the topography. We made an early change in plans to develop an updated photogrammetric grid from the existing photography and control.

Dean continued the August 2000 data was 1:4000 scale aerial photographs and the flight was one month after a major range fire at INEEL. About two-thirds of the study area was burned crisp. Then Dean reviewed the photographs pointing out the two flight lines, Pioneer Diversion Dam, and the old target range at the INEEL. All of these maps and photographs can be viewed in Appendix A. The 1993 photography is 1:10,000 scale and the original product was a two-foot contour map and black and white photographs. Documentation has not been found for quality checking, control, and accuracy of that mapping. The 1999 report used a six-foot grid that we derived from the 2 foot contour map as input for the 2-D modeling so all of our prior conclusions are contingent upon the quality of that data. Time was then spent reviewing the maps.

Dean explained they got involved with the quality issues when, after receiving the 2000 data and beginning to run the initial hydraulic modeling we found that the saddle didn't overtop until 250 cubic meters instead of 150 cubic meters. The grid inputs were reviewed and it was discovered that in the model the sill elevation in the 2000 grid was two feet higher than in the sill elevation on the grids of the 2000 contour map. It looks like what happened was because they were contours only they didn't close it at the saddle and it interpolated a low spot. As the model results were reviewed there were discrepancies at several other sites. It wasn't immediately obvious that there was a consistent pattern throughout. Dean stated that they planned to do an accuracy assessment of the diversion dam study reach because it was not known which map was more accurate. They preformed a GPS survey of widely dispersed ground points through out the study reach. Elevations were also where the differences were contained. It was found that X Y characterization wasn't the problem as features matched. So elevations were where the differences were contained. In the Paleoflood reach, GPS data points were surveyed. See Appendix A.

Bob asked if that was it was on the 93 data.

Dean responded no, this is in the diversion dam study reach.

Bob inquired so is this the '93 data or your....

Dean replied no, these are point locations shown on the 2000 flight lines but they can be applied to the data set.

Dr. Lall asked so the way these are collected is a human walks around with a GPS or....

Dean said we take a GPS instrument on a rod....so that we stop and do a 30 second observation

Dan added we had a base station....

Dean agreed we had a base station running and so we are collecting the RMS on the GPS data is in the report and it is down in the centimeter level or something like that.

Dr. Lall stated what I am curious about is for other applications for example is it feasible to put a GPS on an ATV and drive around randomly and basically sample the whole....

Dean responded we did that and it works. The accuracy is degraded because you are obviously measuring a slough but for relatively flat terrain that is not....and we did that, well I'll tell about it. It can be done and it is done. Can we jump back to Appendix A, 2.12.A Back up one since we're in here, there is I believe residuals control maps so these are the observational residuals. This is residuals and control panels so this is what we are starting with here in the....we are unabashed about mixing units so....

Dan said we are....expedient unit system down.

Dean explained here is the histogram of the elevation differences of the check points. The blue data, what we did was we took the GPS elevation here and we subtracted the elevation of the projected surface at that XY location. So the blue histogram shows the elevation difference of the 2000 BOR surface from the actual GPS observation. You can see it is a real nice tight peak here. We had 800 points that we checked and the statistics on that are .4 feet, median .2, standard deviation is .4. The red...well the green and the gray, let's go to the gray was the same data projected to the aerographic's 2 foot contour surface note that RWMC is about 4 times higher deviant values....different is negative instead of positive which means sort of the average surface was biased in a different direction. That is probably a data....but overall distribution is much broader and the data issue between the 2000 BOR surface and aerographics is probably due to stuff that we did when we did the 2000 work because the concept of the 2000 work when we started it was that it didn't necessarily have to be tied hard into the INEEL data coordinates system which is a unique system because every on site is done in state plane NAD 27 vertical 29 and it's Idaho east, the INEEL kind of straddles a couple of state plain zones so there is some interesting projection issues when you get into the ....

Dean oh, I forgot, one more. We have a scatter plot...this again illustrates some of the datum issues here again the blue data are the points comparing to the 2000 BOR surface. The gray data shows much greater scattered are the points related to the 2 foot contour map there is zero and you can see in general that 2000 surface is just floating slightly above....that means the GPS.....that means that our grid is actually a little bit low relative to real world coordinates/INEEL datum. Aerographics 2 foot contour map seems to be a little high relative to the datum on the upstream part. Then on the downstream part it looks like there is a big area that is somewhat low so what we, and that started to go a long ways towards explaining what we thought we were seeing when we started looking at the hydraulic modeling results in that some of our sites we were getting somewhat conclusions that matched and some of sites we were high and some we were low and we didn't know what to make of that and I think our conclusion in the end is that it's two fold. There is a resolution problem with the two foot data it doesn't resolve the capacity of the channel as well as the newer data. Also, the two-foot contour map has some warps and folds in it that is probably a result of the way it was produced and stress on the control points. The summary of the paleoflood reach was that 888 GPS points were used for comparison; also compared to National Map Accuracy Standards and American Society of Photogrammetric Class 1 Standards. The standards are all referenced in FEMA flood plain mapping. The accuracy and RMS values are summarized in this table.

Reference note: Appendix D Table 2-1 page 25.

Dean continued saying the goal is to have the maps meet standards. The table shows that when comparing the accuracy and RMS values for Z elevation, the 1993 two-foot contour did not meet standards for a four-foot contour map. The 2000 data meets two-foot standards but doesn't make one-foot standards in the paleoflood reach. The five-foot grid from the 1993 data reprocessed exceeded the standards for a three-foot contour map but it still isn't adequate to make a two-foot contour map in the paleoflood reach.

Dean commented that after this research they realized they had much bigger problems throughout the study area so they expanded the scope of the checking especially because they wanted to use this data for inundation modeling also. So check data was done all through the area around INTEC and TRA. There were elevation checks on 500 points. Not much check data was gathered in the paleoflood area and areas away from the channels. The quality checking on the data could go further.

Dean then stated there were still problems with 1993 data. The accuracy of the control data was questionable; approximately one-quarter of the control points were surveyed again. So conclusions, using 500 points and same methods, were the 1993 two-foot contour map was below standards for two foot and mostly below standards for four-foot contours. The reprocessed five-foot data met standards for a three-foot map but it did not make it to a two-foot map. After reviewing the control data, there seems to be underlying accuracy issues with the control network. That is what is limiting the accuracy.

Dean went on to explain the reprocessed Box Canyon data from the 1993 report. It was decided to reprocess the whole Box Canyon area anticipating future needs for modeling. Map contours are 10-foot contours that repeat in 100-foot repetitions and elevation increases gradually. Upstream of the diversion dam there was no new hydraulic modeling but it was included to have a consistent map to the area. A GPS survey of break lines through the deepest part of the canyon to improve the accuracy by adding 8000 ground points. Through most of Box Canyon the existing two-foot contour map only goes to the boundary of the INEEL site and most of the canyon actually lies just off of the INEEL site. All of the topographic data there was derived from the USGS 40-foot contour map. There was not adequate topographic data. So any estimates of conveyance of large dam break flows from Mackay Dam would have been subject to the limitations imposed by that topographic data set. Topographic constriction in the Box

Canyon area will be highly critical to estimates of a dam break or other floods that source upstream of the INEEL site. Dean concluded the topographic data by stating that included in the new data set is much better resolution of the topography in the spreading areas as well.

Dr. Lall asked about the erosion and sediment transport data. It was determined that the data does exist but is not used.

Dan answered that the front dynamics are so complex and there is no way to characterize and parameterize the sediment distribution. Bob also commented that what needs to be determined, knowing that it is not tractable, is the impact it would have on the uncertainty.

Dr. Jorde suggested running the model to look where critical reaches are and where erosion might take place. Also look at the potential for erosion, change the contours in the model to see if that makes a big difference. For example, once the saddle gets over spilled and the water starts channel formation in the saddle then look at data. Or put in the channel assuming a certain erosion rate, change the contours in the 2-D model, or run the model with the original contours for a period of time and after a day or whatever, then change the contours.

Bob stated a couple of issues he could see with that is the basalt that is near the surface and knowing how much it will cut down through the basalt. There is no way to how deep the basalt is (beneath the sediment) until it is probed with bars.

Dr. Jorde commented that you could identify where reaches are in terms of stream power that have a potential to be eroded.

Dr. Lall asked how changes in the cross section changes against the carrying capacity changes to get the baseline argument out. The second question was; at certain sections where you now targeting aggradational deposition, the density of the flow is very different than what has been assumed and he asked what is the implication of that?

Bob said that the system has been in place for a couple of thousand years so all the dynamic effects have been integrated to give us the current system. This system will probably not have any significant changes in terms of sediment transfer. The real question for the purposes of the review is given these issues and concerns what does it do to the uncertainties.

Dr. Jorde stated that the might be a possibility of doing a very limited of additional modeling. Bob agreed that it was a possibility after the final model was agreed on.

Dean then showed a slide that dealt with channel power in the Big Lost River for set of flows. He sampled a narrow strip of values only in the channel and then projected them. This was more or less an upstream to downstream plot, right to left. It showed the variability of power. It showed that in the upstream part the power increased with discharge in an extreme way, however in the central part as discharge increases power went down. That was in the big loop area, upstream of the saddle and saddle constriction. There were several places like that. So the reach as a whole, there were alternating domains of significant erosion with severe aggradation on an alternating basis. At 70 cubic meters it has uniform transport capabilities through the reach. At the more extreme floods, the channel has no ability to convey those floods so they stagnate in places.

Dr. Jorde asked how many points in the channel at one given flow. Dean replied there are thousands in this plot as every six foot cell was sampled. At this point Dean explained the sampling.

Then Dean reviewed the maps with the group.

Bob then stated that Dr. Jorde had raised a good procedural issue and then commented that this issue would not be resolved by Thursday. So two items needed to be addressed the first is how do we account for the uncertainty. And the second would be recommendations for future work. He said that any recommendations would be welcomed and a proposal would be submitted.

Dean went through the geomorphic studies. He stated that the goal is once again improving the defensibity of the geologically based conclusions of the study both stated and unstated. The stated being what can be formally characterized and unstated being the fundamental differences of opinions. He reviewed a map that showed the study reach caught between two basalt flows and the Pioneer Diversion Dam.

Dean stated they used the new aerial photography, did a lot of fieldwork, and mapped in greater detail. Also, there were a series trenches dug so the detail of the soil could be studied. Through the trenches landscape stability was studied, sedimentology to the deposits, and the power criteria for evaluating bounds was determined. The base for the geologic log of the trench walls was a digital photo mosaic at a field scale of 1:13. The logs depict both stratigraphic and soil contacts. These sites were selected; BLR 8, Saddle, and Big Loop.

Big Loop Area

- Pleistocene age of small channels on surfaces (T1, T2, T3)
- Characteristics of the earth mounds
- Pleistocene loess and soil variability
- Sedimentology of Pleistocene units.

Saddle Area

- Short trenches by main channel (T4, T5)
  - ✓ Quantify limits through high power on Holocene terraces
- Long Trench downstream (T6, T7)
  - ✓ Address NPH concerns
  - ✓ Quantify power limits
  - ✓ Confirm age
  - ✓ Discharge bounds
  - ✓ Sediment

BLR 8

- Upstream trenches (T8)
  - ✓ Quantify power limit (400 year)
  - ✓ Holocene flood power limits
- Downstream (T9)
  - ✓ Slack water stratigraphy of Holocene floods
  - ✓ Origin of mounds
  - ✓ Eolian sand

Dean said they were seeking positive evidence of floods and bounds; also wanted age and origin of Pleistocene channels and looking for more positive evidence of the Holocene floods and are there other deposits of floods that were similar or larger than the 400-year flood. Information sought is this one flood or many floods, more confirmation on the bounds, additional dating on the bounds, better estimates of the discharge, and perhaps identify additional bounds.

Pleistocene surfaces were subdivided in two or three geomorphic areas called, from oldest to youngest, P-1, P-2, and P-3. Dean stated that in general the channel network on these surfaces is not following the course of the present Big Lost River. The P-1 surface lacks any channels and is topographically the highest portion. P-1 and P-2 have large earth mounds all over them and P-2 has channels. Parts of the braid plain (P-3) shutdown and became more restricted. There was a big change 10 to 15 thousand years ago and the channel gravel supply was shut off. The last phase of deposition on the braid plain was probably low flow, fine grain filling of the braid channels as the river incised to the present course.

The H-2 is most extensive, the H-1 preserved only as small terraces along the back edge of the H-2 surfaces. The 400-year flood is associated primarily with the geomorphic expression is the H-2 surfaces. The H-1 and H-2 date back to early Holocene. There are four or five radiocarbon ages in these deposits that range in age from six to ten thousand years old. Then H-3 and H-4 that are low terraces and sills associated historic and channel forming floods along the Big Lost River. H-4 is a post INEEL diversion dam unit that are small accretion terraces, forming now in the channel. Dean stated these are the basic units that are the framework of the geomorphic map.

The discussion then moved to Pleistocene gravel. These gravels have buried 95,000 year and older basalt flows. There is likely multiply ages of gravel. These surfaces preserved the primary deposition morphology and sedimentary structures in those gravels, as expressed in the trenches, correlate very well to the channel morphology on the surface. These gravels were deposited as sheets a meter and a half thick overlapping beds in broad fans. The last stage of the fluvial activity on the braid plain involved deposition of fine grained deposits in the channels with the peak of loess deposition 10-12 thousand years ago as the braid plain was drying up. The current channel geometry is strongly constrained by basalt out crops. Basically the river started to incise and the fundamental channel was locked in at several key locations. The stratigraphy in this section is totally over printed by bioturbation and soil formation. There are carbonate accumulations that indicate that they must be at least early Holocene if not late Pleistocene on the order of 8-12 thousand years.

Dean stated that the group would look at the channel on field day.

Dean pointed out another one of the channels margins; the key element being that where the braid plain gravels interfinger with the channel; so the channel and the main gravel sheets are the same age.

Soils were then discussed. One of the things that became apparent when looking at the details in the trenches was massive translocation of silt from the loess worked way down deep into these gravels. That is the indication of stability. When asked why the soil wasn't clay, Dean replied that some clay is present but the soil is mostly fine silt. Most of the soil has five to ten percent clay.

Dean stated that one of the questions that is important when looking at the soil is how do you know that the upper horizons are younger deposits and do not represent the same amount of time that the underlying materials do. He said he would come back to that later.

The reason he brought that up was he is sort of embedding that in concepts of the erosion criteria. Then he moved to the key soil areas of Pleistocene were in the long trenches in T-7. Stating that there is hundreds of meters of exposures in those areas. Also stage 2 and stage 3 carbonate development, we have white coatings on the pebbles this is evidence of the carbon flux and large amounts of time on the order of thousands to 10 thousand to 20 thousand years to develop.

Dr. Lall asked about carbonate and the water moving through the channel.

Dean answered that some of the channels are in these gravels. If water flowed through the channels intermittently, what would happen is the flux of water through there would be such that it would dissolve most of those carbonates it would flush it from the profile. The reason the carbonate exists here is because it represents the wetting process. Water, snow, or whatever falls and tries to percolate down and then carbon evaporates and becomes carbonate or super saturate and precipitates out. There is a depth constraint associated with average depth of wetting that is in this case about 40 centimeters in these soils. You will not find any carbonate above that depth because the average soil moisture conditions are high enough that the carbonate doesn't precipitate. The carbonate gets flushed to a certain depth.

Bob commented that there is a place by INTEC at Monroe and Lincoln Ave there is a 30-foot deep borrow pit right next to the river that never gets water because of the vertical permeability in this stuff is so great.

Dean went on to discuss evidence of stability and disruption. There is strong carbonate involvement—evidence of stability. Then in many place there is wedge like forms in places where silt is going down into the gravel. Those are of two origins. One is those are root cast where the sagebrush put a tap root down and wedged its way into the gravels. Then when the sagebrush dies and the roots dissolve the silt works its way down into the gravel. The other origin is that they are frost wedges. After the frost action, little critters churn the soil.

T-8 is a good example of carbonate pipes, differential carbonate accumulation, and different stratigraphic units. There is carbonate at the interface of the gravel and the loess.

Earth mounds are spatially stable and persistent. Dean explained that in hundreds of meters of trenching they had not found abandoned or fossil mounds in the subsurface and thinks they are similar in age to the gravel surfaces. The mounds are currently highly active sites of bioturbation. They have surface and subsurface relief. Mounds are formed on gravel bars and channel bars and channel centers. They are a well-mixed loose deposit. A preferred host stratigraphy was not evident. Dean stated that ice wedges and frost features could not be identified. As to original size and form, some of the gravel was missing. There is evidence of lateral expansion and growth that is evidence of persistence and stability. These probably originated as some type of permafrost feature at the end of the Pleistocene. There was probably a little crater there that as the braid plain dried up and the permafrost ended it filled with fine grain material and made the best place for plants and little critters to move into.

Dean continued in general the carbonate profile tends to dip at these higher permeability and looser mound soils so the wetting front goes deeper. This is an area with spectacular basal carbonate and actual stratigraphic truncation at some of the mounds. Some of the mounds have been fairly stable and show evidence of long term stability and non-erosion on the Pleistocene surfaces that would support the ideas of paleohydrologic bounds. The mounds are highly erosable. They have loose silty fine sand. The trench data suggest they have long-term spatial persistence. There are limited areas of P3 surfaces that have no mounds consistent with areas that have been overtopped by floods.

Dean then moved to discussion on the Big Lost River incision. The incision below the braid plain must have been 8 – 12 thousand years ago. There are similar fine grain deposits in channels on the braid plain as well as along the incision channel. T-6 is the best place to see this sequence.

Dean reviewed slides of Trench T-4 that showed the surface was very finely stratified and at this site there is a thin, white bed that is called the "white flood". That is a deposit of a fairly young flood that overtopped the surface relatively recently but presumably prehistoric. Modeling suggests that prehistoric floods did not reach this surface. That deposit is only present at this site. There are six radiocarbon ages that are 400 – 600 years. It has weak soil development but is recognizable. There is some stuff at the top that is so finely stratified that it cannot be resolved very well and may be the product of younger floods. Then there is a soil underlying this deposit that is often slightly truncated and the carbonation indicates it is several hundred to a thousand years in age. Radiocarbon age from this is always older than 600 years and often times as old as 3000 years old. One was before the flood and 20 centimeters below the ground surface but in the position that roots and things could get into the profile. Then it was buried by the flood deposits about 400-500 years ago. The lower part of the trench around T4 20 is about 7500 years old.

Dean explained at the BLR 8 site ideally a single continuous trench should have been dug. However because of the constraints on cultural resources, three short trenches were dug. The stratigraphy of the trenches is unique. The trench T-8a closest to the river that should be flooded most frequently has an eroded strip of carbonate soil and fine-grained deposits. These deposits look very similar to the deposits in T-6. They clearly show a truncated section in that the carbonate layer is right at the surface. This is positive evidence of a flood that has stripped 20 to 30 cm of material from the profile. This is inferred to be a relatively young flood since it takes thousands of years to form this carbonate layer, but not too young early Holocene. Trench T-9 is where they had hoped to find a great record. There is a strange channel deposit to the left of this trench. This is 1 to 1 ½ meters above the Big Lost River on an H-3 terrace. The only problem with this site is it goes under water at 50 cubic meters. Up the trench the radiocarbon ages are right at 600 years. There is evidence of a big paleoflood about 1200 years ago and it left a little deposit.

Dean summed up the flood evidence in this area. Positive evidence of large floods is very limited. Deposits in T-4, T-5, T-6, and T-9 are typically one-half a meter thick. Radiocarbon age is 400 – 600 years and the distinctive soil morphology as well. We can recognize that stratigraphy all up and down the river. That relationship was identified in the 1999 study and was the primary basis of the conclusion for the existence of a paleoflood; stratigraphically the same as the 1999 study. The new data found in the trenching is the thin bed near the surface of T-4 not recognized in T-5, T-6, or T-9 must be very young based on the soil development. One thing discussed in the 1999 report that has been confirmed through these studies is that there probably are some other floods that are slightly smaller than the paleoflood about 400 years ago that produced very thin deposits. There is a suggestion of these floods but no formal record in the stratigraphy. There is clear evidence of the upper limits of erosion at T-6 and T-8. In trench T-1 there is a channel with very anomalous soil section. It is unresolved whether the lack of carbonate represent young flow that has flushed the carbonate out or if it is a function of grain size in the channel.

Dean stated that they (the USGS) were not seeing a huge difference in the water surfaces. Locally some differences in detail.

Dr. Jorde stated that he was looking at the profile talked about this morning. When we go to the field and dig a trench what exactly are we looking for? You go out there you dig a trench and you identify different layers and processes that you can identify from that trench and you take samples and get the radiocarbon age then sort of fit that into what process that has created that kind of finding. You are not always looking for the same kind of sequence or things that could be totally different depending on how the process has been that shaped that particular site. Is that sort of the process?

Dean replied yes, it is an iterative process. The cycle he was trying to make is if you look at the lower part of that cross section. Think of that as I'm walking out the day after the flood but over time if there is not another flood they change. The key with regards to geological recognition is being able to set to document the timing and origin of the different reaches and figure out what stage you are presently looking at. Am I looking at something very young or very old? Is this a deposit that is old but intact or this is a deposit subsequently modified.

Dr. Jorde commented it could have been flood with high scouring potential that has removed some of that old surface and not deposited anything on top of that. Or it could also have been a flood that had a high stage but no scouring potential so it hadn't removed anything but has deposited something newer on top of that old. So there are all kinds of different processes that could have contributed to the sequence of layers that you found out there.

Dean as the geologist/geomorphologist the first order when you walk in, you could say okay I recognize this geomorphic landscape. What kind of hydraulic environment do I have during a hypothetical series of floods here? These often times turn out to be complete washes because of the hydraulics and vice versa. We had a tug-of-war in terms of the process of how we go to site selection. Whether we invest in modeling or geomorphology first, we have to go step by step.

Dean reviewed the circle that was used as a template to use as representation for the mounds. There are distinct spatial patterns to the distribution of the mounds. On one sense at the loop there is a big area between two channels that seems to have much fewer big mounds. That is trench T2. There are other areas that have no big mounds. The soils don't support the idea that this was the result of the shallow flows.

Dr. Jorde asked what are the mounds?

Dean answered that the mounds were topographic bumps. They are 2 to 3 meters in diameter and stand about one-half meter high. There's extensive geologic/geomorphic literature on these and a debate that has raged for decades.

Dr. Jorde asked if they were Martian mounds (joking)

Dean said there is actually one paper that says they are due to strong ground shaking. That would screw up Paul's seismic hazard analysis. The other very strong camp is that these are due to biological activity. That somehow these are places where plants got started they originated as small plant mounds. The other point of view is that they had their origins as permafrost features. That they actually originated during the colder glacier climates of the late Pleistocene. The counter side to that is that there are similar looking features along the lower elevation slopes of the Sierra Nevada and parts of the central valley of California; although, it could have gotten cold enough there during the late Pleistocene.

Bob stated that the mechanical process they looked at in Wyoming, was that the hypothesis was that these are transient permafrost conditions. The ground is freezing and thawing over hundreds of years. The contraction and expansion process associated with that, you get the mechanical action that causes the gravel distribution that causes the frost wedging. One of the indicators for these things is frost wedges on the margins and near the bottom of these things where the solid chunks of material is coming and going with respect to the bottom and causing these little thrust features. We don't see those. If we do they are bioturbated, because one place where that might be there.

Dr. Lall stated you geologists are funny and imaginative.

Dean said he wanted to show the fan shape nature of the geomorphic surface. On this map you can see this kind of conical shape here. This is the Big Lost River braid plain fan as it makes the turn and heads downstream and both at this scale and in detail this thing really is fan shaped. That means when we start to do the flow modeling this is not a surface created by the present river. It is something created by some paleo-river that doesn't exist any more. That means when we do flow modeling here water is going to get to this point and instead of going downstream and being captured in some canyon its going to take off and go due south or follow the contour off this fan. The steepest gradient; at the diversion dam we have a critical junction with respect to the flow diversion to RWMC spreading areas and the river. One of our unfortunate choices we made in terms of modeling was we put all of the water in right here and forced it this way, which really wasn't happening in nature. There was something going in this direction as well and until we got into this phase one of the things I didn't fully appreciate with regards to what must have been the Pleistocene-Holocene transition, was the presence of four old irrigation channels that went right to the point where the present diversion dam road embankment goes through. At the place where they constructed the diversion channel, there must have been very low sill at that site. Otherwise they could build a small diversion structure here and capture the water and then send if off down there.

Dr. Jorde asked where it would go if you sent it down there.

Dean replied it goes to a different part of the fan. The topographic divide of the fan is about here.

Dr. Jorde said no, just to change the diversion structure....

Dean said that was what Bob was saying right now for the regulatory purposes the diversion dam is not a certified flood control structure. What the logical out come for it is it becomes a trade off if you want all of the water to go to spreading areas then you are making a conclusion about managing the water in the spreading areas and the simple effects subsurface stuff that is part of this here. There is a whole ecosystem associated with the Big Lost River Sinks.

Dr. Jorde said yes he understood that but you could still provide enough water for that but as soon as flow go over a certain threshold point....;

Except that some years no water ever gets there!

Dr. Jorde said but these are not highly specialist ecosystems. These are ecosystems that get flooded only every so often and there is probably highly specialized species in there that can only survive in very strange ecological conditions.

Chris interjected there was talk a few years ago about putting a head gate on the diversion channel to the spreading area so you could control direction the water went. You know, on those few years when you did have flow. It has to been a flow of 450 cfm before anything goes down the river. I don't remember the numbers but I know there's a certain valve it all goes down the diversion channel up to a certain point.

Dean stated the diversion channel is a little higher so I think it is the other way. In order to get over and spill into the diversion channel it has to be above that valve, if the gates have to be open.

Scott said that there is a certain flow, I don't know what it is, but big enough flow here going down the river and when you get to the highway at INTEC and TRA there is no water.

Dean said that information is in the infiltration document. There is a series of hydrographs in there for the low flows that shows the lag between the diversion dam and the gauge at Lincoln, which is down by the facilities. You can see in that data how much channel loess is going on in the river and what it takes to fill the surface reservoir there.

Scott added I'm sure if there is enough years of no flow then it takes more.....

Bob said what happens is there are a couple swales in there and that water ponds but in reality it backs up over here.

Scott said he was one of the few people that could say he had been in that river with waders on. Usually in most of the places you can walk down the river.

Dean said when he first came up here he thought he was going to need his canoe so he went back to Denver and got it. When he got back, someone had turned it off.

Dr. Lall asked if someone had looked at snow pack at all.

Multiple voices here so answer could not be transcribed.

Dan then reviewed the hydraulic modeling. He said a lot of effort over the years had gone into trying to find field data to test the codes. The two codes he focused on were TRIMR2D and RICOM. TRIM actually focuses on 2-D and there is a 3-D version

Bob said the one of the other issues is FEMA table of acceptable routing codes.

Dan continued these are part of the validity for a variety of purpose. 1-D flood routing, 2-D routing, and debris flow routing. They have a series of categories. TRIMR2D is not one of them. I sent you the rules. It was a surprise to me that that is the way the process works. One of the people we collaborated with is Roger Denlinger and he has a debris flow code. One of the FEMA debris codes doesn't work but it is certified. I think it is certainly doable but we had an interest for a while in programs within the Bureau and this program was to find a way to test these codes under realistic conditions and quantify their performance. In Appendix C part B section 1 is a synopsis of some of the testing we have done it is not an exhaustive documentation of all the testing we have done but it covers the most interesting. As it turns out there is very little flow velocity data available for large floods. Probably for obvious reasons, because of the time people were using these things and using technology involving dropping a bird in the water. Through some efforts by USGS in 1993 there is some direct measurements of velocities across the cross sections that is well stage for very large flows and there was also extensive effort by USGS and others to map high water marks along both banks and through a restriction. Fairly complicated geometry. That was one of our showcase examples because it actually had flow meter data.

Dean interjected that was one of the peer review publications that sort of got sidelined.

Dan said RICOM is a finite element code which allows us to use a more general computational mesh…as that section discusses it tends to more accurately represent the effects of advection.

Bob added that one of the things he would like Dr. Jorde to think about is, given all of the background and validation….how do you think this code compares and whether it is usable as a FEMA equivalent type code.

Dr. Jorde stated I know some of the FEMA things and I know some of the issues that have been discussed and I know some of the models, which are not FEMA certified, and some of the ones that are FEMA certified and I wouldn't want to get into that.

Dan continued saying some of the questions related to these two particular codes is because of the big separation of the massive amount of ways you can gain computational efficiency with how well they perform the transcritical flow situations. Which is a fairly uncommon aspect of flow there are a couple of places you can get transcritical flow. Here are a couple of constrictions the general….of flow. We did some testing up river for high water marks from the 1997 flood but there are no direct velocity measurements to verify that we could reproduce the stage and the high water marks through a reach probably had true numbers exceeding 2….a drop over 100 meters and some other cases to. That section contains some discussion of the accuracies of the codes and the bottom line is RICOM is the more accurate code. Roger has spent over seven years developing a completely alternative numerical formulation applying a volume method that couples mass momentum….he wanted a completely different independent numerical approach to check things and basically he is getting the same type results as RICOM. RICOM and his code are pretty much the same hydraulic jump geometry, transcript of flow solutions and trends basically smoothing the hydraulic jump. It is not accounting for the full effects of backwater quite as much and it tends to block these little paths relative to the other two. So water surface right around constrictions is a little different in TRIM and RICOM and there are some examples of that in this section. The saddle constriction that releases water to the south.

Just to give you background on how…these are staggered codes so basically we calculate water surface elevations and the center of an element and then we have lots of calculation on this site. Written many papers back to the early 1980s and so basically what is required is a topographic representation of the terrain and all I really do literally is take the results of the topography that was produced from the GIS system. I just take those elevations for the finite difference grid that is really straightforward. There is a uniform code thing which is what he did and then what TRIMR2D does is simply sub samples out datapoints are staggered between the elevation points and the differences between the elevation points is where the velocity is calculated and it calculates the gradients basically between the sub sample elevations. For instance for the paleoflood reach we have a six foot computational cell we started with a three foot posting. Probably one of the best ways to is to show you the most complicated one we have produced and then I'll talk about aspects of some of the testing. This is….I'm going to zoom basically on a test grid we setup which we extracted the inundation grid but it also contains…this is part of the paleoflood grid. So you have the diversion dam here and this is the channel the colors go basically below its points or the outlet here.

Bob stated let me back up here and rephrase what you are doing and see if I got it right. You are going with a finite difference grid first and then based on the results of this thing it is faster computation you go to a finite element model.

Dan said yes there are two basic grids he has to talk about. I have to talk about the...we have about 11 meters of relief across this whole section. But we have the paleoflood reach where we did hydraulic modeling to support all of the geomorphic investigations and what we needed to do there is calculate the flow with a stage of velocities then drive the quantities like shear stress and stream power so that they could lay that over on the geomorphic sites and understand how those quantities change with discharge and so these tests calculates....for peer reviewers. One of the things we wanted to do was a convergence test that is how much resolution you need to have in the grid that is a discreet representation of topography in the channel to produce the same results or in other words the same inundations levels and velocities. So this is this area's test grid which represents about 1 kilometer from the diversion dam downstream with the test area we selected because it has some of the narrowest channels geometries at this section here below Juniper Bends where there is several...1 foot wide at the bottom slots in basalt and so a question in doing any of these things is to get enough resolution to accurately represent the volume available in the channel. What we did is a success of refinement, Matt (Jones) went through with the highest resolution grid we had which is the paleoflood reach 1 to 4,000 and he produced basically a 1 ½ foot posting and a 3 foot posting so those will get sub-sampled at 3 foot and 6 foot cells and then I calculated for the same discharges in the two grids and then compared the estimated stages and velocities. Those were actually in Appendix C Part B Section 2 in response to Nic's questions and recommendations on average the differences between going 3 foot cells in this reach to 6 foot cells they are basically unbiased in terms of stage. There is only a couple of millimeters difference in stage and pretty small standard deviations. So we felt that the 6-foot cells were adequate to represent the stage and that the same thing applied for the velocities were unbiased. That represents were the convergence testing for the purposes of working in the paleoflood reach the 6-foot cells were adequate. Now we also used 5 foot cell grid because as we found the flows got larger than 200 – 250 cubic meters per second we ran out of ...we hit the bottom of the grid. The way the grids were constructed for the paleoflood reach is that there is no flow allowed through the top, bottom, and left boundaries. In other words, the only flow is allowed to leave the grid...

Bob asked what do you mean you hit the bottom of the grid?

Dan answered the grids of course are finite and my case the bottom is the south side of the grid and the top is the north side. So the two flight lines that covered the paleoflood grids get close to the old meander channels....

Dean interjected that basically discharge is at 200 – 225 the water goes through these low spots in the basalt flows and then wants to go down the gradient to the fan and we didn't include that much topography in the detailed reach. So that is why we extracted a bigger chunk of this from the 1993 data to run the bigger flows.

Dan added that in hindsight there would be much difference, as we had to demonstrate that because most of that flow stays out of the channel for the purposes of the paleoflood reach. So it escapes and even when it was confined it wouldn't sneak back into the channel but we couldn't establish that without doing the calculations.

Dean said there are plots you will see if you'll look in the 11x17 part of the Appendix D that are whole series of sets of plots in there for different discharges of the paleoflood reach and you will see the difference in the grid. There is a much bigger area included for those largest flows.

Dan continued the other modifications made to the grid points I obtained from the GIS system for the parametery was a rotation or translation. I rotated them to try to minimize the width of these things in terms of total number of cells required for computation, then extracted a sub set of the total available topographic points. Then I always set these things up with TRIM so that I turn the bottom the grid so outlet flows normal to the cells and by doing that, we minimize the boundary condition effects. The way the boundary conditions are by fixing the water surface elevation of the outlet. So what I do is I generally calculate flow look at the velocities coming in and then lower the water surface maybe 5 or 10 centimeters so that is accelerated a little toward the edge so I don't over inundate the inside. Then lower 5 centimeters at the end relative to what the ....fixed, the same velocities all the way down just I make sure I don't over inundate upstream but it is well below critical. This tends to give sort of a natural boundary condition but what I found in grids when you intersect at an angle you start getting more interaction of the boundary condition at the back of the grid. This issue is easily eliminated in RICOM so you can literally just construct elements. So all of the calculations on the paleoflood reach were done with fixed finite difference grid, no variable mesh they are either 5-foot cells or 6-foot cells depending upon the two-grid sets or topographic data. So what we did was set up inverse transformation to preserve the most precision in the calculations we strip off the values and just operate off a local datum. So I subtract off lowest elevations and in the finite difference grid it is trivial, it is just grid indices there is no need to calculate the distances between points. So I set up an inverse transformation matrix so I plug this back into INEEL stage plain, which is restoring the elevations and the rotations.

Dan said he could literally take the flow quantities and dump them back into the GIS system and then overlay them on the renderings. Then there are the trench locations and so forth on here. In this case it would be a stream power plot and you can directly over lay the stream power for different discharges on to the terrain and all the reference sites....

Dean stated for all the geomorphic mapping is over lain and various other things. These plots are all in Appendix D the 11 x 17 part.

Dan said it's really not very complicated to set up. He said for the inundation modeling it is a huge area. It is almost 19 kilometers long and almost 6 kilometers wide and the channel is dinky. I mean the channel...through most of the site the channel is less than 60 feet wide. So we started by using 20-foot cells because it's the fastest code and that would give us an idea of inundation and that is what a lot of these plots are. We took the 5 ft posting data from the 1993 topography sub-sampled to 10 feet, then TRIM sub-sampled to 20 feet postings and through the entire site that being from the diversion dam to downstream of INTEC and again I did a rotation and a translation to put it into a local Cartesian system. Then the appeal of the finite element code was that we could use something like a variable mesh and really put a lot of resolution into the primary channel which maybe narrow and really I wanted to do this if nothing else to test whether we converged in terms of getting enough representation of volume available and to see if the inundation estimates changed the relationship with respect to discharge. What I did was I used the 20-foot grid spacing estimate at the inundation of 106 cubic meters per second to define the area that are inundated as active channels and used a 5-foot cells in all those and that is what this is representative of. Then I used the inundation at 700 cubic meters per second, which is the largest flow we were intending to simulate to define the area that had 10-foot cells. So that was at least twice the resolution of the 4-foot cell instead of 1 compared to TRIM even at 700. Then I had to devise a strategy to put the triangles in quads to smoothly mesh between the finer to coarser zones. So here what you are seeing is the fine cells represents the inundation at 106 and then you go out here and this area out here would never be inundated even at 700. Then these were inundated at something greater than 106. By doing this we tend to follow the banks instead of having a stair step pattern because we can do triangles in addition to squares which can align the grid more naturally with channel shape. Because we are using 4 x smaller cells, our time step had to drop by a factor of 4, which immediately meant that RICOM was going to have to run 4 times slower. TRIM was running these flows at somewhere at ½ to ¼ real time. To run these RICOM flows meant it was taking me almost a day per CPU per hour of flow. This is a relatively modest slope terrain that takes a long time for some of the water to work its way through some of these secondary channels used in the flow diagrams. I ran out to 40 hours of load time at TRIM although it looked like at converged at 20 it wasn't as expensive to check that. These other ones I ran at 20 hours in RICOM so about 300 to 400 hours of CPU time per flow.

Bob asked the question, this is on your machine or have you got....

Dan responded I have a cluster of 11 dual processors Opterons. This next grid has 7.2 million elements and 6.7 million modes and vertices and almost 14 million sides, that is a loose code for elements of this size, and it required about 1.8 gigabytes of memory per processor. We did a lot of testing, the verification testing and everything but ultimately it required running full calculations all the way out to verify this. Frankly, you can't verify if you do not run the problem full scale. Nick Katapodes said there is probably nobody in the world that is quite doing what you guys are doing. Basically I have to do all of the calculations and I do them again. I have three different compiliers that I tested with this at this level of optimization because I didn't want to get burned by...well compiliers have bugs from time to time. So that I could demonstrate that the codes behaved up to a certain level of optimization very comparably. We are looking at steady state flows and with any of these iterative solvers you will never get the same flow with the same initial conditions unless you turn off all of the optimizations. There is no point in doing transient behavior because literally I predict, looking at steady state flow. So what we do is we monitor hydrographs along different parts of the channel and some of the overbank areas and we look until they go to steady-state hydrographs.

Dan continued with as far as infiltration there are two things that had to be implemented in the grid that were not used in the paleoflood were infiltration and culverts. I did some testing on Fritz's grid and I could make the water disappear before it reached the end of the grid. We realize it is the scaling of Fritz's data that is not consistent with the historical channel loses between the diversion dam and Lincoln Avenue. You can do a simple calculation of taking an infiltration rate in terms of meters per second and calculate how many square meters it takes to dispose of it. As Fritz alluded to in his report, we realized that some calibration was necessary to come up with effective infiltration parameters that were consistent with the historical data.

Bob stated that the accepted infiltration rates are 10 – 40 percent. That is what is reasonable at the INEEL.

Dan noted that in the channel it ended up at 4×10^{-5} meters per second in the channel and 10^{-7} out of bank. We simply added a GIS layer in the flow code; each cell had an index to look up one of these discrete infiltration rates for that cell.

Dr. Lall said the infiltration rate does not depend on the depth of water.

Dan answered no. Fortunately even though the perception was very high they are relatively very low in terms of the time steps used.

Dan was asked what he meant about the boundary depth and he replied the turbulent roughness depth you have a depth averaged flow, but some part of that is turbulent flow.

Bob asked what difference does turbulence make to infiltration.

Dan replied it means we decouple the flow into two flow depths. We have the flow depth for the logarithmic depth average flow and then we have part of the flow depth devoted to roughness and that would be the turbulent depth.

Bob said infiltration should not depend on turbulence.

Dan said that it doesn't but what he is saying is the way we realize the flow is we separate into a zone with a logarithmic velocity profile which is our 2-D depth average part of the flow and then we decoupled that with basically a scale of resistance term.

Bob said does that 10-40 percent number he was telling about is what people will want some comfort about. You can compare hydrographs or do whatever you want to do, but people are going to want to know percentages.

Dan replied that what we did we calibrated to those high hydrograph loses in the channel and that was really following up on Fritz's recommendation. The fact that his values were so high that 10 meters per second was not going to make it to Lincoln Avenue. That indicated we had to reevaluate it.

There was multi person discussion on Dr Fiedler's infiltration information.

Dan said that the general perception was that the out of channel area should have a lower infiltration rate than the channel area because of the loess and other factors.

Dean added that was embedded in his conclusion.

Bob stated one of Fritz's important sources of data was the large-scale infiltration test and they pumped 3000 gallons a minute into this.

Dan continued that is an important point to consider because in all these runs when we did infiltration our infiltration out of channel is lower than the infiltration, in the channel based on the relative ratings in Fritz's report. So you rescaled his in channel and rescaled the out of channel the same way? There is no data to test that.

Dean added he would have to say the final numbers we ended up with are consistent with those large-scale tests over all. In effect what we ended up doing was weighting some of the more recent INEEL site data a little heavy than Fritz had done. You should reread that carefully. It is the first piece of Appendix D.

Paul said he did not have Appendix C or D.

Scott said it would depend on what the local soils were like especially if you wash off the silt and clay and get to the gravel then the infiltration would be really high.

Dean said absolutely and that was one of the cases that we characterized; so there is really two elements to what Fritz gave us. One was a spatial distribution for infiltration rates and we didn't modify that and the spatial distribution was based soil classes and geology. So anywhere there was gravel with minimal loss got higher values.

Dan interjected that basically meant that the higher rates got spread over the whole central area (referring to braid area between facilities.) When I talk about the low out of channel, I'm basically talking about this area upstream of this central area of this central area and everything between the facilities is this area that it was given the same infiltration rate as the channel.

Bob asked again are we going to get a percentage of infiltration that people can use for comparison purposes.

Dan said that yes he had a rate he had used that can be converted into any units.

Bob said that would be very useful. Paul asked if that was percent over some distance.

Dan said between the diversion dam and Lincoln Avenue it is calibrated to 10 or 15 (percent infiltration.) Dean added he thought it was 10. He continued what we tried to do was....what we did in that discussion that is at the beginning of Appendix D was we took the gauge data from the diversion and Lincoln Avenue and plotted all that up and you can make an estimate from the data and we brought in the large scale ring tests and all that. We tried to walk through that and say here is what that data suggests is a reasonable number and it is 10 or 15. But you should look at that and see if that is adequately documented for the purposes that you are trying to talk about.

Dr. Jorde asked if they get a reduction in the percentage of higher flows. That is what you can see in the inundation between Howe's Ranch and Arco. The inundation rates get smaller at higher flows.

Dean answered somewhat, yeah. Dan added and smaller at longer duration flows. Dean then said that they went a little bit on the low side to account for that. I mean you can certainly go to higher values but by doing so you are not accounting the fact that it must reduce flows.

Bob added it is a time average percentage is what it is.

Dan said he didn't think they had tried to represent the whole dry condition.

Dean said that the hydrograph the or flow data that is available the kinds of durations that are present in the flow data are on the order of months and there some persistence of constant infiltration rates over that kind of timeframe.

Dr. Jorde said he was taking of the annual peaks. There is a number of diagrams in the 1999 report that is comparing annual peaks between different gauging stations and attenuation between those peaks.

Dan commented that in those hydrographs the flows where the peaks occurred earlier in the season tend to have a higher infiltration and the flows of the peaks that are 20 or 30 days later have lower infiltration and it seems to be some saturation effect.

Dan went on to discuss differentiation between bridges and big culverts and the rest of the culverts, which are 12 – 24 inch, and there might be a 30-inch. There are major conveyance points, like Highway 20/26, Lincoln, and the railroad embankment downstream of INTEC. He only exclusively dealt with one of those and that is I took them out and gave full conveyance for all three sections. I ran a scenario where I blocked Lincoln Avenue – no conveyance. I wanted to see how sensitive it was to those and there are about 30 culverts throughout the site. We implemented Charles Berenbrocks rating curves in the code and I did a fit to his inlet/outlet, basically stage discharge relations for those culverts. On the 20 foot grid it was necessary to move culverts locations around to get the inlet/outlet elevations and to make sure that they went from one drainage area to another. Most of those are really small discharges anyway. At many sites the ground is so level on each side that once you get flow you can drown the outlet pretty quick and then you don't get much conveyance once it's inundated downstream. So what we wanted to.....look at sensitivity of the estimated inundation to those and because there was so many of them it takes so long to run the codes I simplified it to...you know, they were all operating at full rate of capacity or they weren't, to see, does that change the estimated inundation. Then I separately did a case where we had them operating but we blocked Lincoln, the Lincoln Bridge.

Dean said that Clint Kingsford did a really nice job documenting all of those culverts for this culvert report so we have nice pictures of the various kinds of things that are present out on the site but they range from the not so great conditions with weeds and sand blocking one or both ends to .... The three pipes are Lincoln and this in the INEEL railroad bridge. So this is the one that says what Dan essentially did in the grid was he chopped it out or we blocked it...made a brick wall there.

Dan said you have to understand his motivation. There is a way we implemented the culverts in the codes we are not trying to preserve them as actual culverts and all we do is adjust the stage on each side. We conserved the mass going through but did not conserve the contribution of the velocity. Now if we did a flux and I didn't want to get into that. This is one of the critical points I wanted to bracket here. It conveys or it doesn't convey at all. I wanted what's important first and then figure out how we....

Dr. Lall asked what their assessment of things that are the largest sources of uncertainty with respect to the final product being the inundation map for the higher flows.

Dan said for INTEC it was the performance of the culverts. Bob said that it might be a bit of a problem because if he remembered from his hydrology class, engineers use professional judgment and assume a certain percentage on the capacity of these things. It like going out and looking at it and saying the big flow will plug about 50 percent.

Dan replied that when engineers do their brainstorming for failure modes they never conceive the particular failure mode we encountered on our Morning Glory spillway in North California, which was the fiberglass hull of a boat that got in and plugged the spillway with the bottom facing up. It turned out that they are really strong and you don't want to get really close to that thing in case you successfully dislodge it. So you have a bit of an operational problem. So these guys had risk analogies but what good was that. They had a heck of a time knocking the boat loose before the water overtopped the dam.

Those things look like they are big holes but you drop one Winnebago off at 20/26 and you are going to plug those thing. You have got to look at those things. He thought it was quite plausible that you could block most of the conveyance there, the way those things are laid out and the way the channel approaches there. If in terms of human activities it is not difficult to plug those so I thought it was an important scenario to run. Now we can do partial conveyance, we can do some other scenarios but again I wanted to bracket the range of possibilities to understand the significance of each of these parameters. We can refine and run other scenarios but we have to understand the importance of each of these parameters.

Dean said that overall we found that the little culverts are unimportant, they have no effect.

Bob said he needed some gradational scenarios of that. That's going to be what the engineers are asking.

Dan said when you block the conveyance here now you get the inundation at INTEC at 25 and at 106 when this thing is conveying.

Bob posed the question to Scott about emergency response activities being monitoring the Lincoln Ave culverts.

Scott commented that when there is flow on the river they monitor these culverts and the diversion dam.

Dan said as far as risk reduction; get rid of those culverts.

Bob said all kinds of things are going to be on the table eventually. The other thing that I want to see with these other partially plugged scenarios is the numbers 25, 50, and 75 percent. At 50 percent capacity, what is the risk at INTEC and then compare that to what we need to do at the diversion dam. At 50 percent capacity, pick a number—1000 cfs—is it a problem. Did we design the diversion dam so it releases that amount of water? Those are kinds of scenarios we need to look at to evaluate what to do with the diversion dam.

Dan said first order you could look at is say you have 50 percent conveyance then put something between 50 and 70 acts like 106 we have at full conveyance. This thing will drive you risk because the more frequent floods become relevant at INTEC when something less than full conveyance is available at this part of the channel.

Bob commented again it all goes back to the diversion dam.

Dean interjected there is an effect here and it is kind of significant but it's definitely a second tier effect after the diversion dam

Bob said right now the diversion dam culverts would not pass for control. That is going to be the key structure and what I want to know is what is the safest amount of water and safest scenario in terms of anticipating how plugged those things are going to get. Then having enough scenarios that if I say they are going to be only 25 percent plugged and Scott says wait a minute its going to be 75 percent plugged. I'll have some…

Dean stated that as Dan said what we are doing right now is a vision of this as an unregulated flood frequency…a natural (flow system) without the diversion. So if you put the diversion dam and the capacity in there and start to operate in that mode then all of the floods….we are looking the diversion portion of floods that are in the range of 200 cubic meters upstream of the diversion dam. We are beyond the 500-year flood at this time, and then with the increment that…if the diversion dam is still functioning at 1 in 500 we only have a 1000 cubic feet going down the channel and we don't have a problem. So now the question is as we move up on that scale at the diversion dam to have an effect here we have to get 50 to 100 cubic meters coming down the main channel. We are still sending most of it down to the diversion dam. When we get out to that part of the logic tree you are way out in the one in several thousand kind of probability land and the incremental difference between the plugging or a partial plugging of Lincoln it is not going to move the decimal points. If we are forced to evaluate scenarios where we don't consider the effects of the diversion dam, it doesn't exist anymore. Which is basically the model runs we are giving you, and what the rating curves are based on at this point. It (no conveyance through Lincoln Ave) is highly significant. If we construct the tree and say well in the risk to

INEEL, the scenario where the diversion dam is absent gets only 10 percent weight. The way we construct these results for probability is based on the residual flow down the Big Lost that doesn't go through the diversion dam then there is not going to be a lot of contribution from the differences at Lincoln

Bob commented the only thing beyond the scope of what you are doing is how much water you can get flowing down the channel through local rain.

Scott added all the flow down the river after the Mt Borah earthquake all that ever did is put water in the spreading area. We didn't have any.

Dean stated that there was some backwater onto the fringes of the INTEC fence area; from the ice jams at the railroad bridge. Well, that is what I've been told anyway I never saw it. Again, that is not a discharge causing the problem. It's backwater due to ice jamming.

Scott then we had water at the spring thaw basically all the water ran to the low spots and stood there, ponding, until things thawed.

Dr. Jorde said he didn't quite follow where if those Lincoln Bridge culverts are closed and this would actually be better for which of the sides because I don't really have the geometry in my mind but what my suggestion would have been that you randomly close some of the culverts to see if that has any effects on what you want to know and if there are some major ones then they should all be closed and factor in the cottonwood tree or tumbleweed. Or there may be Winnebago lying around and if we talk about a 10,000-year flood then this is a major disaster so that people will worry about all kinds of things and it is not like everybody is standing around and wondering what these culverts are doing. So this is a major disaster event probably and everybody is busy with something. So I would certainly run a scenario where this is all closed and see and if it has no effect than it was fine.

Dan said it has the biggest effect on the lowest flows because they backwater and spill over and what they tend to do is mimic larger flows in terms of how they inundate parts of INTEC. Once you reach larger flows they couldn't convey through that section anyway and at 200 you are already inundated...so the difference between blocking and having 100 percent conveyance becomes smaller and smaller as the discharge goes up. It is just overwhelming the conveyance in general and its spilling all over the landscape at about the same pattern it just happens to end up at the same place downstream at Lincoln Avenue. It gets behind Lincoln and spills over in the same place. So the differences are primarily from flows below 200 cubic meters but in terms of risk the more frequent events become relevant when it's blocked and that is where most of your risk is in the more frequent events. I make the suggestion that is the most critical risk on the site because that is the most likely to occur probabilistically to get loaded. Your probability of seeing 200 – 400 cms is pretty low.

It turns out that there is not much sensitivity on the performance of the culverts on the inundation related to those flows. It is really related to the inundation between 25 – 100, which are more likely to happen or happen more frequently.

Bob said actually they will not have the same capacity to carrying material into the culverts either.

Dan replied no but you don't want 25 having the same inundation capacity at INTEC of 100-200.

Dean stated that we need to get those stage probabilities credible.

Bob asked why would the culverts plug at low flows. You're still within the banks.

Dan said yes you are still in the banks but at 25 we have enough depth to go up to the top of the culverts right there

Bob questioned those calculations. Then said whatever 1500 cfs is.

Dan said if you look at 800 in the diversion as being bank full. Twenty-five is pretty much bank full.

Bob stated he was just telling that is the question he would get asked. If these are rating curves, why are you giving me this full or closed scenario.

Dan replied because everything is between those two values.

Bob answered that the other thing is this is a steady state analysis. The hydrograph and transient analysis the amount that you can convey through those culverts in a certain amount of time in respect to the hydrograph is important.

Dr. Lall said it seems like your solution is ......if you started plugging it up it seems like it would very rapidly towards the fully plugged solution. So I don't think I would even bother making the runs; I would just explain it.

Bob stated that one of the things he has to worry about is the utility of this document is a legacy document. We could all sit here and say yeah, this is right thing to do but two or three years from now I may be looking at this and having to defend it. I'm not sure I'll be able to reconstruct these arguments; I'm a bit worried about that.

Dan said there is the logistical constraint about the fact that these calculations took several weeks, I had to start by bracketing things. I would like to run more scenarios...

Bob asked why Dan didn't use the INEEL super computer.

Dr. Jorde added that he would like to see all of the simulations you have done. So if you had address this then I am not asking you to do more. All I have had time to look through is all of these rating curves and I said that might not be relevant but if that has been included in the runs you have all ready done then that is fine. I was just a little worried about those rating curves.

Dan asked if he had done a good job in explaining how the grids are generated and used. The group agreed he had. Then discussion moved to the bounds.

Break

Dan started the discussion of paleohydrologic bounds as another source of information that you can put in; particularly over a long time period that helps constraint the uncertainty in flood frequency analysis. So we want to include that, but we also want to include the uncertainty in characterizing the bounds

Dan commented that the bounds from the hydraulic perspective. There are two things you need for a bound. You need the convergence of a geomorphic configuration that reveals something about the age of stable deposits. Then you need a corresponding hydraulic situation where generally the stream power and something that could erode or remove those deposit increases with discharge in some regular way across the geomorphic marker. So you have some geomorphic unit that you can establish that has been stable for some period of time and then you hydraulic configuration where as the discharge increases the power increased to a point where that stable configuration that will be catastrophically removed and without any doubt for some discharges exceeding a certain amount. So you can have a place with where there is a beautiful geographic record and the hydraulics don't cooperate and you just can't develop that power or you can have a beautiful hydraulic situation where you get a nice systematic variation of hydraulic power with discharge but you don't get a good preservation of critical stable surfaces. So we need both a surface that is stable, that is dateable, established, that will persist for discharges below a certain discharge and can be removed for discharges above a certain discharge. I didn't know if we had ever laid out the framework.

Dean said that bounds are a geomorphic concept that we have. That we see in the geologic records limits on the flood record but in order to make it useful for flood frequency we need more. We have to find a way to marry that to the hydraulic results. This is a major weakness of previous studies. We had a very subjective and non-quantified criteria for how we went about that process. If you think about in the context of a 1-D process at a cross section and if we assume that the over bank section of the cross section is our geomorphic surface then what depth of inundation do we need on that over bank in order to be confident that we would have a geological record produced by that flow. Obviously subjective.

As we try to utilize the spatial aspects of two-dimensional modeling, we had to extend that line of thought; well if we have geological information in an area how do we take that geological information and merge it with the spatial variability that we see from the hydraulic modeling. Once we do that what are the criteria here; is it some depth, is it some set of power values, or shear stress values. Those are the kinds of things that we can possibly use and how do we do that and what should be the track through that data that allows us to reach those conclusions. So now we are back to this….

Chris commented that you have a stable terrace surface; it's a perfect Paleo-thing but your paleohydrologic bound is 4 million cfs. You don't have a realistic hydraulic flow that you can say that terrace hasn't been effected by something.

Dan answered there is geomorphic sensitivity and resolution and there is a hydraulic sensitivity and resolution. We have had places where it turned out that all through out the entire reach the whole thing just backwatered and there are very few places that developed power. So that whole reach wasn't particularly diagnostic. So what you want is coalescence of hydraulic resolution resolving power and geomorphic resolving power. So an example of that would be on the inside bend of a meander that you get older and older soils in that bend and at successively larger and larger discharges and they start to overtop that bend and eventually they cut a channel or straight path through that bend and remove that material with clear power. So you are looking for the sublime combination of geomorphology and hydrology.

Dean continued that the problem is that playing this game of extreme floods here and trying to relate the geomorphic record to that. What we step into very quickly when we start looking at long timeframes of low probability events is we are tying to characterize the probability of a hypothetical flood that has never occurred in the system. So almost by definition that flood is beyond the capacity of the river channel in which it is trying to flow and sometimes you find that there is somewhat non intuitive hydraulic effects that begin to emerge because you have to much water for the system and the role of hydraulic backwatering on bends and subtle constriction becomes very important at a lot of site. While a geologist would say 'Well I have this nice meander with good progression of soil this is obviously a time transgressive site and I can get half dozen bounds out of this site." But that has developed as the river has incised it's not the past record of successively smaller floods. It is a record of abandonment due to incision, so what we are doing with the proxy record here is we are going to overwhelm this system and say well at successively higher levels it's becoming a bound at these discharges. But what can happen, and we have seen this, is that it backwaters downstream and you reach some point it ceases to erode as it successfully inundates higher and higher on the surfaces.

Chris commented than you can have a huge flow and not anything is going to happen to the surface.

Dean said, right, in the lore of floods as you talk to people everybody knows of places where there was a huge flood and meters of inundation and absolutely no record produced. That is the criticism and comments people have made to us in the context in talking about the paleohydrologic record. People will stand there and say I've seen lots of floods that didn't leave a record how do you know as a geologist that if a flood happened here that it would leave a record. That is a question that we are trying answer with this whole issue of how do we specify a paleohydrologic bound. How do we come to a quantifiable criteria for deciding whether a discharge and stage associated with paleohydrologic bounds should be up here because that is a sufficient space to assure us that we have erosion of this stable surface or does that bound have to be instead have to be up here at the top of the slide to be highly confident that we have erosion.

Dean continued what we are really trying to do is exploit spatial information that is we are using geomorphic mapping at reach-scale. We want to move beyond the framework of making a conclusion based on single point or single cross-section data. We have two-dimensional hydraulic modeling that gives 6-foot spatial resolution. While the hydraulic variability of flows, we can express that has power, velocity, shear stress, or whatever. Likewise we have detailed geomorphic mapping that tells us the spatial distribution of different geomorphic elements of different ages in the landscape and we need to be relating those two data sets and making a conclusion that says here is the spatial variability of the hydraulic parameters within the areas that characterized as this single geomorphic unit and then making a conclusion of a reach-scale for a single unit.

Dean added they are trying to use empirical data on shear stress and power as a quantitative definition for erosion limits. We want to use restrictive spatial areas to define those areas in which to evaluate erosion based on geomorphic mapping. We need to make the criteria that are very specific to soils and vegetation for the site. So the criteria that we come up with for the conditions at INEEL is not going to be necessarily applicable to some other river; some other terrace; or some other place. It has to be conditioned to what you have. He said the next step is to sample the inundation, stream power, and shear stress from the hydraulic model and apply the erosion criteria within each of these sample areas that defines both our best estimate as well as some uncertainty for each area and combine the results from both of the sample areas. Note that many of the sites are going to basis to high values because of the effects that Dan was talking about. What we are going to find here is that even though we have a 1000-year-old geomorphic surface that might be present all the way down the river and making flotation's at some locations when we impose a higher flow on that and say well what does it take to modify that existing surface that existing surface at this particular location. We find that when we have flows that greatly exceed that there are some sites that will never be eroded or that we can never be confident that we can produce a record because at higher flows they go with the backwater and maybe the record that would be produced there would be a depositional record but in the context of a confidence of a traceable geologic record we probably can't make a positive statement about those.

Bob asked if they could change channel depth until it worked. Dean replied that would be a subjective.

Dean continued by saying there are deposits that are consistent along the river and those are the products of a flood of a certain size.

Bob commented that he knew exactly what Dean was saying….

Dean interjected that there are some sites that no matter what you do you can never make a confident statement at that site and that doesn't mean that the whole thing goes out the door. It means that some sites have higher resolving power than other sites do.

Bob added that he also thinks there is an assumption about stable channel geometry and other things that you have to replay….that had some positives.

Dr. Lall asked with regard to data that you gave earlier about the possibility of succession if it reaches above the meander level or anything of that sort. It seems like if there were a way to fingerprint similar data elements across the reach scale you could resolve the issue of where the channel geometry which is stable for the paleomarker you are looking at.

Dean said what we can say with regards to stable channel geometry is that in general the Big Lost River channel as we would look at it right now as been stable for at least the last 10 – 12 thousand years because its incised into a 10,000 year old deposit—Pleistocene outwash—there is a pretty much fixed width narrow channel with only very narrow terraces along most of that length. So the period of record for most of the reach this channel geometry has been pretty stable. Now the one place where it obviously had not been stable is in the big meander belt up here. You can see for example, downstream of the diversion dam this whole stretch is pretty incised in Pleistocene deposits and basalt down to about right here. Then it starts to bow out a little bit and it has meandered all over the place all through this area.

Dr. Lall asked if those are two channels or are they new things?

Dean said these are old meander scars on the Pleistocene deposits and are not related…this is what we addressed by the trenching. There has been no Holocene post Pleistocene flow across this area. These are independent of the river. So the channel for the Holocene is this section through here and potentially there is uncertainty in the channel through here but the whole reach under large flows backwaters. So it becomes sort of a big pond, as you'll see from the power plots at higher flows. Then here is passes through the constriction and from here on down all through this reach we have a channel that is locked into basalt and Pleistocene gravels again with very narrow Holocene….

Dr. Lall interjected basically at this end you can define bounds.

Dean replied at this end you can define bounds and at this end you can define bounds and at this reach you can't, because here the stable channel assumption is violated. So the geomorphic bounding provides you with sort of your first order definition of presence or absence of first order channel stability. Now there is more details in there about…at the 20 percent cross section change and things like that. So what are we using for erosion criteria data? Okay, let's back up—all of this is covered in the report in Appendix D the small part. Then there is some discussion in section 2.4; I believe. What I really tap into here is 100 years of engineering hydraulics. It is essentially a miracle; we have several different classes of information that we need to look at somewhat independently though in the probabilistic framework. There are all sort of basics of how do you design your irrigation canals. The first people who came out at INEEL had to figure out how steep can the canals at INEEL be in order not to blow out when we start to run water down them. There are a couple of examples on INEEL actually that when they tested the canals they were too steep and they actually blew them out. They put grade control structures in these things. This is level one channel stability type of analysis. There is a lot of literature on this and I'm not sure we've really begun to tap this but there are really…I've separated this for the purposes we are trying to use here there are three types of basic characterizations.

1. Design criteria elements that have been put out by some people doing channel design and stream restoration kind of criteria. The way to think about those is that they, in a probabilistic sense, have to be considered as bounding values because generally they do incorporate some level of conservatism in the way they are presented. They have a factor of safety built in, they are engineering applied type of numbers.
2. There is a huge body of literature on sediment entrainment and transport limits. There are ranges of values that one can derive, different formulation for problems. In general, those types of values give you some measure or better degree of particle motion in a channel and those are going to be within your range of values either upper bounds or lower bounds but probably the majority of those are skewed towards insipient kind of motion.
3. Geomorphic study limits. Vic Baker is one of the people who got that literature going in looking at paleofloods. He put some of the earlier compilations together on that. These are generally post flood assessments that people have done. Where they have gone out after a flood and measured a cross section or two and from that tried to estimate the hydraulic parameters that are associated with some geomorphic change in those rivers or channels. What they represent is a significant exceedance of the threshold for erosion or destruction. The treatment of those data in the literature is generally highly subjective. When we look at values from that data set we probably should be thinking of those as lying above our threshold value. They obviously represent some kind of exceedance. So in a probabilistic framework those have to be upper tail types of values. Ultimately that is where we are trying to go here is into some kind of probabilistic characterization.

Dean continued this is an example of motion kind of criteria from Andrews 1984. He defined based on dimensionless shear stress three domains of sediment transport based on his preferred data points; a domain of no transport, marginal bed load transport, and general bed motion. Other people have come up with slightly different values using this same kind of approach but you can associate these ranges with a set of shear stress numbers.

This is the heart of this data and there is discussion of the data in Appendix D. What we have tried to do for both stream power and shear stress data is compile all the different data sets and try an arrange them in a way that we can make some useful conclusions for applications in the paleo bounds. I am going to start on left side here for with the design criteria limits. What this comes from is highway design Dr. Lall and stream restoration Dr. Lall. What people have done for a stable channel design is say what is the stream power, if you plant your Class C Turf in your ditch and maintain it, what stream power will have to be exceeded before you erode that turf. Or if you put 153 millimeter D50 rip rap in your channel here is the range of values we would expect to be associated with the erosion of the with an appropriate factor of safety built in.

I have a couple of tables in the appendix there that describes what Class C Turf is and what those consist of. I will tell you right now there is nothing that I know that looks even remotely like Class C Turf because this isn't Kentucky. We are well below these kinds of characterizations.

Here is some data for power and there is some similar data for shear stress. Entrainment and transport limits there is not a lot of data in that in the realm for stream power most of that have been done in realm of shear stress. This is for really insipient motion far below the similar values for design criteria. So these have the correct relationships relative to one another. You would expect these to be less. The first motion is a lot less than the factor of safety associated with design criteria for total destruction. Geomorphic study values: Here we have as I was describing geomorphic estimates from the people who have published in various places for channel stability on some rivers. What is called channel changes and erosion thresholds, channel bank erosion and widening, minimal change in course gravel channel? Local scourer, major changes. Here we have power values estimated to be associated with the Missoula outburst flood in sites that have streamlined hills of the loess plains of eastern Washington. These are large-scale, meters of water features. These are a couple of sites that Dan and I have worked on the Santa Inez River. This river was one of the early paleoflood studies that we did where a large flood overtopped some terraces that were several hundred years old. This was the foundation of our paleohydrologic bound idea. We went back in the last couple of years to try to calculate stream power at those sites associated with that flood and these are the over bank subdivision values in the cross sections that I got for a couple of different sites. Again these must be maximum values because we greatly exceeded whatever the threshold values were. So our objective here was then to take this type of data and come up with a criteria that we can use for the classes of materials that we choose to define for the Big Lost River. When we look at the sites here I've boiled it down to two categories of material; the erosion of the loess and eolian soils on the terrace surfaces. That is the upper 10 – 20 centimeters of fine grain silt and very fine sand that exist on those surfaces. Generally unvegetated things like that. So that is the low end of that distribution. There isn't a whole lot of data here but as you can see that there is a design criteria limit for sandy loam soils at least in this realm. This is erosion of those types of material and we have this trapezoidal distribution where we say we have a best estimate that is somewhere up here and 2 – 5 range and in the upper limit high confidence that major erosion will occur by the time you reach 10. Likewise the second category involves eroding into the deeper parts of the soil horizons and into the coarser grain deposits on terraces. So now we need more transport capacity, we need more power. The size fractions we are talking about typically in these trench deposits and the gravels the E50 in those, is generally going to be in the 2 – 10 centimeter range. This is what we tried to construct for that; this is the realm we talk about trying to be in for erosion on the terraces. The third category and this really comes back to the geomorphology approach off into how stream power is implemented because most of the geomorphic data really is derived from the cross section channel estimates or channel averages. We want to reduce this to a 1-D problem and what kind of average channel values would we expect are going to be associated with significant channel changes with geomorphic change in the Big Lost the low end of that might be in the 50 range but the bulk of the data say its got to be a much larger value up here in the 200 – 300-400 range so we skew our distributions in that directions.

Dean presented a similar approach on shear stress. We have some design criteria type limit lots of entrainment limits from several different sources more uncertainty here and a limited amount of geomorphic data. Again we define that same kind of criteria; a shear stress limit or surfacial erosion of the A-horizon and B-horizon. There is a triangular distribution that ends right here and peaks right there. Imagine that the triangle exists and then another bound which is really which is highly unconstrained here and the shear stress side from a geomorphic approach. I will move these into the kind of form for a probability distribution diagram so the upper one is for shear stress and the lower one is for power. These are the same things now we just turned them on their sides. In the probability distribution what we have...what we are trying to say with the lower left corner of these is this is possible from the exiting data. These are our best estimate kind of region and this is highly certain. These are intended to be probability distributions and the same kind of terminology applies. Now I'll show you a couple of examples from the results.

Bob asked how come you have make those independent grain size.

Dean asked what he means

Bob said you made them huge bins of grain sizes one is eolian fine grain size the other is everything else. It includes outsized boulders and sand.

Dean replied because there is an averaging and generalization problem there. Actually we can make a very strong case that all the surfacial materials almost everywhere are fine sand. I've got lots of particle size analysis to support that. There is nothing coarser than that at the INEEL and that what this is intended to be; specific to the Big Lost River.

Dean continued we need to look at the maps. What we have tried to do in defining spatial areas where we can pull the grain size distributions out of. Where did those power boxes come from....those are the big blue boxes. I was going to turn around and point that out on this map and it didn't plot.

Dean said to restrict the spatial expanse of the stream power data that we look at from the hydraulic modeling what we did was define a bunch sample areas which are outlined in the blue box and given these descriptive names like FP-5 and FP-6 and the boundaries of these things correspond to the limits of geomorphic units that are outlined on the plates. FP-8 is within H...an over top Pleistocene unit, FP-10 is also on a Pleistocene unit. We've got FP-15 here; we'll pick one we can see. Then we have another one FP-17 that is restricted. FP-11, which is a small one on the only the Holocene, terrace at T-6 and the other one I was going to look at was at BLR-8. We have a couple of restricted ones at BLR-8 that are limited to segments of Holocene terraces, for example, at FP-17 is just a small area on a small Holocene terrace and FP-16 is a Pleistocene. So each of these is a sampling of geomorphic units and we also have a whole series of sampling areas that range down the channel based on kind of similar channel characteristics. Some are constriction sites and some that are backwater sites and for our purposes right now we are trying to look at very restricted areas. So an example I have in the screen here we are going to look at that. FP-13; these are all of the figures in Appendix D. Some of what we are running here are discharge versus the percent of the area that has exceeded some given power routing. If we read deep into the details of the legend here the first thing we had the inundation. For example, here at a 100 cubic meters only 30 percent of the area is inundated but by the time we go to the next sample point at 130 a 100 percent of that area is inundated for all flows of 130 and larger. So one criteria obviously needed for having any effect due to a flood is you need to inundate most or all of the area if you want to have high assurance. The next....according to the colors here we have different values of stream powers associated with those distributions that I drew. The red ones are generally the soil erosion values and the green ones are associated with erosion of the gravel type flows and the magenta and orange ones are the thresholds in the channel modification scheme of things. So what you can see here is that for just using one of these red lines is at this site at least very rapidly as discharges increases we reach very high percentages of the site are exceeding this threshold. Same thing for stream power down here ....plot the data. We have a couple of different cases embedded and values as well. This is portraying the variability and the hydraulic inputs as well as what happens to the stream power for each site. So we do that for each of the different areas and you can see there are slightly different patterns that emerge as you go to other sites.

Dr. Jorde are there some curves and diagrams, which are not in the legend like those dark yellow ones.

Dean said there are right there.

Dr. Jorde pointed out there is only one in each of those diagrams. You had three of the curves but there is only one of those...

Dean replied that the filled symbols are from the 6-foot grid and the open symbols are from the 5-foot grid. The dotted lines and the stars are 030 versus 038. That is how we get more than four lines. I was trying to keep the colors associated with the threshold values and use the symbols to depict different scenarios from the models.

Dr. Jorde asked why did you step here from your characterizing two different threshold and two different probabilities like where the maximum probability was reached for the sand fraction and for the gravel fraction where you reduced basically the sediment to two different grain sizes and the you have a starting probability and maximum probability and maximum or lowest to maximum and highest or whatever. So why did you step back to size classifications.

Dean replied that what he was trying to do was get the discharge value. So those are criteria that we described that you could apply for the site but now you have to decide for an area. How much of the area do I want those criteria to be exceeded over?

Dr. Jorde commented that he totally understood what the diagrams are but he wasn't quite sure why you first you elaborated on how you reduced it to those two different classes and here you have again...

Dean said that out of those two classes you have five values or six actually and that is what we have. Actually we have the channels so we have six or eight different values. Each in separate line in color corresponds to a ...I've got to find a simple way to say this. So that way when you come up with an area then you say well if I'm going to require 100 percent of the area has to exceed that threshold that is one point of view. Then I have to go up here to get a discharge or maybe at the end you would say if 20 percent of the area exceeds the threshold then I can pick my discharge from down here off those curves. That leads us to what I call a detectable geomorphic change. To achieve virtual certainty is it necessary to achieve the upper bound. As long as we have sample area that are large compared to the geomorphic features that we can see and resolve in the area of interest. For the BLR bound these areas are large, they are huge compared to the things we are mapping out there. I've got a table in here where I tabulate the number of acres and square meters; however you want to measure it. There are a number of cells in the hydraulic model that are associated with each of these. Its hundred and thousands of square meters...given the size of these things and scale of features that we can resolve from the geomorphic mapping my judgment on this is if we are exceeding these threshold values over 50 percent of the area, the area would not look the way it does in the present landscape. If 50 percent of that area had been eroded, what we would map would be only half as big.

Dr. Jorde asked what do you mean with your sample area here. Isn't that related to those trenches, which relates to the blue boxes?

Dean answered that what he is talking about right now is the blue boxes.

Then Dr. Jorde asked you mean the trenches represent the boxes and then the sample area here is the boxes or..

Dean said we start with the geomorphic map

Dr. Jorde asked are you only looking at the surface. So right now you are only looking at the surface of the boxes

Bob said that the trenches served to tie the geomorphic record to the boxes. Is that right? We get age dates from the trenches and other places, which give us the timelines for the boxes

Dean said that this is a sampling problem in a sense. The trenches are point samples and in our geologically backwards-interim problem here we do geomorphic mapping and we define area that we think are similar. Then we say well to prove that our geomorphic observations and conclusions are correct we are going to dig one trench or two soil pits in the area and we will do detailed description there that are representative over this geomorphic region. Then get detailed data from those points that we can then extend over a geomorphic region. What we are trying to do these sampling area and apply them to the bounds its like well we have a two-dimensional hydraulic model where we clearly had spatial variability in those types of flows so let's restrict our sampling of those results to specified surfaces that have a geomorphic context and draw some conclusion from those. As I say my call on this given the size of the areas that we defined this that generally 50 percent is actually a conservative number for producing a detectable change in most of those boxes and in fact for some of the larger area like on the big Pleistocene P-2 surfaces most of the flows are concentrated in Paleo-topography of the braid channels. Much smaller percentages are going to be appropriate because if we ever concentrated highly erosive flows in those channels they would look geomorphically very different. So to require that we have 100 watts of stream flow over the entire Pleistocene surface to have a Pleistocene bound is not a realistic or appropriate criteria.

Dean went on so now what we have just talked about is that we can reach a conclusion of each sample area. FP-13 where we would say based on the erosion from the PDF. 120 cubic meters would be a starting point where we might see something happening that is one watt per meter at 5 watts per meter we are typically over 50 percent of the area or at 140 roughly and at the top end that PDF we are 175. So this is the range of values we would prefer for that site for soil erosion. At FP-18 it is much lower and at FP-19 there is either no inundation or backwatering going on and maybe more uncertainty here but it is a much higher range before you begin to see soil erosion.

Bob asked how come the soil won't be bounded?

Dean replied because you would need lower values. Sand is more erodable than gravel. Eventually at a big enough flow where we sampled things will start to erode.

Bob commented that a 20-foot depth erodes at the same the same as a 5-foot depth.

Dean said there is more velocity. This is the combined result of shear stress and power.

Dr. Jorde asked what are the black lines and the blue ones.

Dean answered here is all of our sites and some sites are better than others; some sites provide more definition; oh, black is soil and blue is gravel. Again the gray shading is the region we decided the bound for the soil erosion might occur and here is the region where we could bound the terrace erosion.

Dr. Jorde asked why is the…it looks like some of the black and blue lines are very close to each other are they the same site or are they different sites

Dean said they are the same site. So vertical is at a site…

Dr. Jorde asked if that site had fine sand and gravel

Dean commented that yeah,

What about 18 were there is a big gap?

Dean said he would have to look at the data to see why there is a big gap. Obviously, something…we get a little bit of power right away and then it takes a long time before anything starts to happen. These sites that are way up here flooding are obviously backwater. If we looked at those data you would see why they just never produce power. All of these sites produce a little power then they tail off and they don't do anything and so we really can't make a conclusion about any about the erosion of any deep gravel sites at any of these locations. Conceptually what we want to take away from this is; that when we get to the stage of saying what is our reach scale framework for defining bounds; we would say well the bounds, in terms of a probabilistic concept, are going to be defined on the lowest side of discharge by the best site. The best site or the lowest site is providing the most fidelity and resolution in terms of the bound, even though there are lots of other sites where nothing happens until much higher discharges in the context of the bounds. These are all sites that are a thousand years old. This is the hydraulic variability along the river. So as long as I have confidence that I can identify all those thousand year sites and that they really are thousand year sites then this is a good representation of what the bound could be. Now if I wanted to add some confidence that I could explicitly incorporate some more sites.

Dr. Lall commented let me see if I understand this assertion here. The argument you are making is that if you were to find a site at which the lowest possible flow was the bound then clearly that had to be the bound because they are all the same there is no uncertainty they are all the same quality. That argument is fine with one qualification, which is, the stream discharge we expect to change along the stream and here you have a chronology of the stream. Somebody could argue that at FP-1 the paleo-bound is lower and in normal stream not at Big Lost because flow increases downstream. Here is the reverse so it is the same argument that the fact that the bound that is the smallest on the side where you expect the lowest flow is the same as the flip situation for a regular stream…right and how much loss are we talking about.

Dean stated that it would be a 4-kilometer long reach and we should be okay with the assumption of constant discharge for the scale of the reach we are talking about.

Dr. Lall added the second thing that occurred to him was this business of taking the reach with the lowest and tightest bound. The lowest we have discussed so let's take the tightest now. It seems like suppose you are taking the medium of the bars. Would you be able to combine all of them with a greater combination based on the position associated with each one of them that would bring all of that junk in that you didn't want to bring in without raising the question of you made a choice off of flood plain and bound and so forth?

Dean replied I know what you are saying but the problem I see is that with the concept of the bound….we are implicitly talking about something that we think has never happened. When we are characterizing real data, real events; the statement we are trying to make is that for a given timeframe this has never happened and so the features that are portrayed on here are not the result of an event that has some sort of sampling variability. So each one of these sites has unique threshold for destruction and some have higher thresholds and some have lower thresholds.

Dan added that some sites are subjected to more power than other sites at lower discharges. So that gets back to the hydraulic resolution and the sublime combination of the greatest hydraulic sensitivity along with the inadequate geomorphic record. You would never want to go and just combined them all because you would be plotting different sensitivities. You would want to go to the best site, which has the best sensitivity. However, Murphy's Law says there will always be a very small number of those sites. So then you would have the statistical problem of small samples. When you want more samples or assurance then you have to deal with probably varying levels hydraulic sensitivity so while you have higher level of assurance you have higher levels of bias.

Bob added if you are going to see this anywhere this is where you will see it. Dean agreed.

Dean continued saying the finality in the bound depending on the geomorphic ability or to be able to go out and find these critical places.

Dan said the first thing that should be done is to generate a grid and run the discharges while they are still trying to find out logistically where they can do geomorphology…. then you target the sublime combination of hydraulics and geomorphology

Dean replied to do the modeling correctly requires lots of money.

Dr. Lall asked in many cases where apparently the flood leaves no marks and it probably should have so if I go to FP-18 as your target and I say that perhaps the mark that is left here was an erosive mark rather than a deposit. The argument here is that there is high sensitivity because there is high stream power. So the cross section actually has changed dramatically and you don't know that.

Dean answered that this is where you have think subjectively and put other data points into the mix. He has come to the conclusion that it definitely would not be a good idea to try to combine these statistically. They are not all equal; they were never equal so that can't be the right approach. In fact, some of them may be so far off the scale you just throw them out, because they should have no weighting in this. So there is a subjective weighting scheme that has to be imposed here somehow.

Dr. Lall then asked the statement you are making is that given a bunch of assessments of what the bound is likely to be and each one has a different precision. The first statement you are making is I'm just going to take FP-18 and that range is that range and it might not work. The other thing we could do is that we can say we have all of our other estimates of course they have a different range and I have a certain acceptance probability on which I will accept one those as a relevant bound. Then I could do integration over that particular constraint. It could be that essentially what you are doing here is that you are refining your estimate of FP-18 where you are bringing in each of those values but with a different acceptance probability and ending up with markers there.

Dan said if you combined them in different ways and come up with a distribution based on those subjective valves.

Bob interjected that in ways you would have to reject some of them because they will be backwater areas

Dean said that is why some of these never go anywhere...

Bob added that you are also taking into account that you have looked at FP-18 and you have some confidence determined that erosion has not occurred versus each of the other sites, there is always a chance that you don't see it.

Dean said some of the sites we have trenches on. So we have high confidence in our geological observations of the stratigraphy. At this site, it is strictly a geomorphic site or surface site and we interpret it to be the same as this site. But that is a geomorphic interpretation, it could be something else. If all of your confidence is coming from geomorphic sites, where you don't have any hard information then you have to be more lax with the upper bounds than if the hardest information you have is coming from a site where you have real good geomorphic data then have more confidence here. You have a trench that confirms that nothing has then you are closer to the virtual certainty side of things than if it is strictly a geomorphic observation.

Dr. Jorde asked what is in the hardest information. Do you say uncertainty the smallest one because you used the lowest value for the bound?

Dean answered he was just hypothetically saying it could be the hardest because you have the most detail geological information at that site. That is independent of the uncertainty in discharge.

Dan added that you could bring the hydraulic quality rating to if it happened to be a trapped bedrock section of the channel.

Dean said there is the potential to make this much more formal but at this stage the process is going to be somewhat inherently subjective because we start back at the beginning here we have subjective definition of the erosion criteria which are arrayed from that data. Then the next subjective step is the geomorphic mapping and the selection of the subareas as a subset from the geomorphic map areas. I considered early on in this that what I should do it take my geomorphic map units and import them into a GIS program and call those sampling polygons and let the whole thing be totally automated in that way. Then I stopped to think about how I do mapping and I realized that my geomorphic units as portrayed at least at this level on the map include terrace risers and the terrace tread is one thing and there is the transition up to the next flow. That riser has to go into one unit or the other in the geomorphic map scheme we have implemented and if I put it the lower unit I am going to bias things in one way and if I put in the upper unit I am going to bias it in the other way. So that is when I said I need a subjective sub-sample of geomorphic unit, sampling areas, to restrict that. Then we come to this stage and we take all of the uncertainties from one individual area.

Dr. Lall interjected that he thought this was very clever. I think I have figured out how to present a question to you. If you delete 12, 17, and 18 from the story, now its not so clear what you want to say. So that is why you need grid to equal out stabilities.

Dean said his conclusion in that case would be at that time you get ruled by what is at FP-7. What I need to draw out of this is a 3-point distribution for discharge associated with a bound. So I will have a lower possible value for my paleohydrologic bound that is somewhere down at this level based on this site and I will have an upper value where I'd say well subjectively I should be up here and that is I have ...I'm exceeding virtual certainty at nearly...several sites, possible at all sites, and best estimates at some sites.

Dr. Lall stated that now you are going where I am suggesting it needs to go but we can be sharper than sticking it at the very top.

Dean agreed with Dr. Lall. Then added that is how this block box is drawn right now. This is the heart of the whole uncertainty in the paleohydrologic bounds game. This is where we have had the biggest controversies. Inevitably it is in the translation of the geologic observation into discharge parameters based on the geologic plus hydraulic information. How do you make that judgment and part of it is cultural because as geologists you reach one conclusion from the geomorphology and then you have to interface with people who are knowledgeable with hydraulics and look at things from the hydraulic uncertainty view points but may have a few different view of what the uncertainties geology can be or should be or are. We need a way to walk them through because this is not simple. I think there are beginnings of a pathway here. In my mind it needs saying a bound is defined by 1 meter of water over the surface or a bound is defined by 50 - 100 meters of power somewhere around the area of the geomorphic site or at a point at the geomorphic site.

Dean continued by saying we are not trying to make a single valued estimate. We are trying to come up with a number that is possible as a low flow value based on data and good judgment; best estimate to high or virtual certainty associated with the upper bound. That is the way the input needs to be framed for flood frequency analysis.

Bob added part of the reason for this review is if Dean tells us that is his best estimate. What is your professional judgment going to be? We don't have time to generate a rigorous objective quantitative model analysis but the graphical and data and discussion leads us to conclude that we are probably on the right track. Although if there can be some improvement we definitely want to hear about it.

Dean then reviewed the catastrophic channel change. Here is the PDF. The blue one is for stream power; the green for shear stress, and the possible best estimate virtual certainty. On the shear stress side, I chose to make a boxcar PDF for the probability because there is nothing up there to hold that together. That was my conclusion from the earlier plot.

Bob asked shouldn't those probabilities sum to 1 not....

Dean and Dan replied they are relative. The relative value of 1....versus the other.

Dean said he was very leery of this but he was doing it because it was embedded in the geomorphic literature. It is because this is very subjective based on geomorphic perception on the significance of the extent of change. It's the classic thing if you got 1000 geologists you get 10,000 opinions applied to the space of this because there is multiple mechanisms of erosion that come into play when we start talking about channel change including the issues of bank erosion and mechanisms associated with that versus the mechanisms that are associated with over topping in a shallow flow on surfaces. It is a complimentary way to think about what might be happening in the larger scale sense and I bring this in when we say well when we say from other criteria for given geomorphic surfaces that we are expecting to see major destruction of all the Holocene deposits in the channel or along the Big Lost River. We probably should be thinking, from a channel change criteria, we would reach the same conclusion. For most geomorphologists.....when all the Holocene stuff gets wiped out most of us would say that is pretty major. I don't see an easy way to implement that criteria real formally into a specific bound because we can't say that the channel has a timeframe necessarily associated with it.

Dr. Lall stated that this is related to the problem of setting of records in a stationery process. So any record that I have will be beaten, right—just like the Olympics? So in a way what you are saying is that thing that you that you are wrestling with as the minimum bound right now. Sorry, the one that is not the most minimum, what is the chance that you have some, forget the precision stuff for now, so you found your record at FP-7 this connotates the lowest possible flow but you haven't explored FP-12 yet because you didn't know it ......so now you want to know the probability that FP-7 will be beaten given that it's the 7th guy you have explored. So what I have to do in my head now is think about if the fact FP-1, 2, 3, 4, 5, 6, 7 also were not known precisely then how do I figure out the same record setting probability for different levels. So then I can determine the position of the latest record. There has got to be a paper somewhere that somebody has written on something that gets me started at least so I will get started on that.

Dean stated he understood what Dr. Lall was saying on that. Dean then reviewed a chart that he had used earlier. We can say here that certainly when we reach discharges on the order of 250 cubic meters and above we are passing the higher, relatively high end of stream power numbers i.e., about 300 watts per square meter is an average channel value, that people have commonly sited as thresholds for major change in channels. The range that I put out there was in the range of 50 – 300 was sort of our bounding values. Basically once we go beyond 70 cubic meters here along many, many sections of the channels we are getting pretty high power and we should expecting to see some changes happening in the river. That is about all we would use because the channel it is not a timeline. I think we can associate time with in the context of the bounds, we can't use this except as a very uncomplimentary approach. For example, what I would say here is that if the power distribution in the channel was consistently stayed down here for every discharge we ran I think we would be on weak grounds saying that we expect erosions of a Pleistocene surface at some higher discharges. This is consistent with the idea that we see erosion at 200 where out on the Pleistocene we're seeing major changes.

Revised Paleo-bounds; Dean said he thought this was the last slide in the series. Four hundred year flood deposits. Basically, our conclusion was that there was a discharge of 130 at 110-150 cubic meters not exceeded in past 400-600 years. We also tend to find an older flood deposit bounds that mostly underlie H2 surfaces. One hundred and fifty cubic meters this is erosion into the gravel on these surfaces basically at 130-175 cubic meters not exceeded for the past 800-1200 years. The early Holocene where as we take out on the road sites like trench T-6 surfaces; 225 and the Pleistocene, the flow across the Pleistocene surfaces. That starts and sort of starts and starts to have some power in the small channels at 225 or so. It seems like there is many, many, channel sites that have flow at 250 that have moderate power in that clutch that we would see a record and in a sense it becomes unbounded because you could never ever get really high power on the Pleistocene surfaces no matter what discharge. This is a classic case where we have lots sampling areas that encompass huge expanses of real estate but all we have to do really to make a bound here is take out a big chunk of one surface. We need high power on one of the Pleistocene surfaces that exits and not over large expanses of trench we need a sizeable chunk to be different than it presently is to be able to make the statement that this flood has not occurred in that timeframe.

Dr. Lall stated the way you have this list here ....my record setting self before you brought this up because now that you put the list up, I see it totally clearly that is exactly what I'm talking about because if the record is 400 years what's the magnitude of the maximum record that is likely to be expected in that range and as you increase it the record has to go up for any distribution that we look at.

Dean said now just for comparison, okay, in the 1999 study we had a number something like this associated with the Pleistocene bound and the 400 year flood number was I think 100 cubic meters with 90 – 110 range so the differences in these discharge values from 1999 to present doesn't have to...uh, first order has to do with the differences in the hydraulic result because..the topography..because these geomorphic sites didn't change. So I am talk about over topping the same surfaces in the same kind of sites we have actually got more data now... we can add these intermediate bounds really because of the data we got from trenches we have more definition of the geologic record in between that we didn't have previously. What happened was because we, a different input topographic set, discharge values increased that's first order. Second order is we have probably flared the uncertainty out a little bit especially on the Pleistocene bound because of thinking about what is a defensible upper limit for having "virtual certainty" or high confidence that we are really going to have erosion. Our confidence that we can define that has decreased in the intervening time. That would be a fair statement.

The meeting moved to Jakers

Big Lost River Flood Hazard

October 6, 2004
INEEL Site
Idaho Falls, Idaho

Attendees:	Dan O'Connell	Bureau of Reclamation
	Robert Creed	Department of Energy-Idaho
	Scott Jensen	BBWI
	Dr. Klaus Jorde	University of Idaho
	Dr Paul Link	Idaho State University
	Dr. Upmanu (Manu) Lall	Columbia University
	Chris Martin	S.M. Stoller Corporation
	Dean Ostenaa	Bureau of Reclamation
	Dr. Glen Thackery	Idaho State University

Dean began the day with the following observation.

Trench T-1

This is stable, unmodified soil on the Pleistocene surfaces. This is pretty typical of what we see time and time again when we dig soil pits and exposures here. We see that anywhere from 25-40 centimeters that is probably the range of loess in the eolian deposits. This is mostly silt and fine sand in the upper horizons and then this is the threshold at about 30 centimeters, typically, we start to see carbonate accumulation in whatever unit is present here. If there is thicker loess the carbonate will be accumulating in the loess, where the loess is a little bit thinner the carbonate will be accumulating in the gravel. Here, if you look, the carbonate was beginning right at the base of the loess. So that there is very weak evidence for directly saying that the loess and the gravels are somewhat contemporaneous in terms of the surface age. Elsewhere you can make a little bit better case for it because carbonate actually ends in the loess and is pretty well developed. Here we have strong carbonate accumulation over an interval like this and then it tends to decrease below a depth of about one meter below ground surface. Like Bob said this is real typical to see carbonate accumulating on the base of all the clasts. You see the concentration; you see kind of bands; all of which governed by the local permeability and gradation of the gravels. In aggragate, these kinds of levels of carbonate accumulation in the soil are widely described for this kind of climate as being typical of what one sees in the soils of Pleistocene age about 12,000 to 25,000 years.

Dr. Lall asked what is the time scale of carbon accumulation that you said typically.

Dean answered in the soils no one is quite sure there. We describe what we call carbonate stages and carbonate morphology. In these it sort of starts in gravels like this with thin coats on the bottoms of pebbles to thicker coats and what we call pipes and knots and at the highest stage you see this horizon is completely formed or cemented with carbonate. It becomes totally white. At first, we called those or designate those Stages 1, 2, 3, 4 with pluses and minuses on them. This level we call Stage 2 and that kind of time scale is generally tens of thousands of years in this kind of climate with this kind of precipitation. Its precipitation/climate driven typically 10,000 or more years would be where you would put that out here, but you can get in probably to Stage 2 in the right settings. At the other end of that, for the first appearance of carbonate in these soils and finest grain deposits it seems to be that we'll get weak, barely detectable to the eye kind of carbonate accumulation in 200 years. Then 300 – 500 later on we can look in at the deposits in the trenches but the first hint of carbonate is in the 400 to 500 year old deposits, from this last big flood that we think we see. Then the deposits that directly underlie that have clearly visible carbonate accumulations generally they are called Stage 1 or Stage 1+ it is pretty obvious all of the time

Dr. Lall answered the original source of the carbonate is just?

Dean interjected airborne; that is what drives soil carbonation because it is infiltrating and being flushed from the surface it is not weathering out of the rocks. The other process we see here in the soils is we see the infiltration of silt in these soils. I don't see a real good place to describe that here. I'll try and find one as we walk down the trench. There are often times a relationship between the silt coats on these clasts and the carbonate front. There have been a lot of rocks from the material pushed down into the profile that is evidence of long-term stability here. One of the things we tried to do in these trenches is map all of this out to document the differences. You kind of view this as a type section of stability and what we are really looking for the on the surfaces is the differences and if it is different then trying to sort out why is it different. Is it a local thing or is it something else.

Dr. Lall added it is amazing the consistency in the thickness of this stuff.

Dean stated we'll see some differences though. Our goal here was to say what are the earth mounds and what are the channels. That is not a channel section so…here is a mound. The first order of things we found here was you could see some of the stripes, which are extenuated by the carbonate here. This mound; the bedding and the gravel kind of dips into this so this one actually looks like it had what some channel straitgraphy association but that is not….we don't see that on everyone but this one did. You can see the gravel has a shape in the subsurface that sort of mirrors and mimics the shape on the surface. It's a pretty mixed deposit. As Bob pointed out, you can track the rocks with carbonate. Here is one that is upside down so obviously by burrowing that has been turned upside down fairly recently. That's evidence of recent burrowing. As we came into the margins of this and we mapped it and if I had acid I could show you that up here the silts and sands don't have any carbonate in that they don't fizz with weak hydrochloric acid. When you get to a certain point down here they do. So there is carbonate in these things at a few percent kind of level and those horizons you can map through this thing and they come and go as they have been real disrupted. Those are evidence that at least portions of the internal structures of this has been stable for time scales on the order of a few hundred years and to a few thousand years between massive burrowing episodes within the structure of those.

Bob added the other thing, and maybe I've pointed this out all ready, is how easily erodable these things are.

Dean stated in general these are way looser than even this stuff. We do the penetration test here and this is always loose.

Bob said that is kind of an over hang…

Dean added when we dug these we had the safety engineers in stitches because they…this piece fell down within 20 minutes of when we dug the trench. Now the whole trench is going to be unsafe for years. These are the working faces that I strung up initially.

Scott said the carbonate really kind of cements it together. It's more stable than it was when you dug it.

Dean added at this height these things are pretty safe.

Bob added sometimes

Dean continued they slough off a little bit

Bob interjected at one of these trenches we will show you where the gravel about no different than that is just…well, it slid out. There are some other things going on.

Dean stated let's move quickly up the trench as you can see there is lots of carbonate underneath here.

Bob asked do you think that's associated with the thickness of the fine grain material in the mound above it.

Dean replied what we see with these is that there is much, the carbonate morphology increases beneath the mound. I think that is probably a result of the fact that there is so much churning in here that there is more vertical permeability in the mound itself and the carbonate is getting flushed deeper into the section and then it hangs up immediately.

Bob inquired so that is indirect evidence of the age to the mound to

Dean stated the volume of the carbonate indicates long-term persistence. These things have been here at least as long as the gravels have been here. Now it doesn't necessarily speak to the origin but it speaks to long-term spatial persistence. This along the trench shows that things are persistent in one spot but they are not appearing and being obliterated in other areas. These are sort of spatially stable systems that exist where we see them and don't exist where we don't see them. The straitgraphy in the gravel we have continuity right across the terrace riser here. That means that the terrace riser is associated with the original deposition of the gravel. It was just sort of the last stage in this system and it's not a younger feature super-imposed on these older gravels because the soils are also consistent through here. Sometimes there is evidence of stability but there is also lot's of evidence of disruption in these upper horizons just from natural causes.

Bob stated sandy homogeneous stuff

Bob said these are things that are easy to put roots in that lets critters go in that provides organic material that lets the plants

Chris added want to grow there

Dean said here is another mound; here you can see there remnants of an old well developed soil in the mound incorporated as blocks and chunks.

Bob interjected I guess that's the bottom of the old channel we were trenching through to look-see and make sure it wasn't....

Dean agreed right; it's actually right at ...if we look on the map here for the main channel axis was sort of right at the mounds basically. The low spot in that channel...

Bob said now that you have seen what a mound looks like you can look around here and start picking them out all over the place on the surface here.

Dean added some of the best developed mounds are from here back to the road back there so as we drive out sort of look around but the size of the mounds on the drive now out to Trench 1 is a little more subdued than most of the areas but they are still out here. If you can kind of take a snapshot in your mind what I want you to think about is the soils and straitgraphy that you see in this trench and try to keep that in your mind and the question is, is this same or different than what we are going to see in the next trench.

Trench T-3

Dean said this trench....let's look quickly at the map here and I'll show you where it is. This is way down sort of on the edge of what I call the P-3 surface; the youngest of the Pleistocene deposits just on the margin. There is another subtle channel here much like these others and the idea was to see whether this looked similar or different than what we saw further up. Have we crossed the boundary or limits of the young flooding or not? Certainly when we looked at the soils at the west end here it seems like at first they are pretty similar accounting for the broad scale differences we see...the variability we naturally see these are pretty much same. In general, for most of the trench that conclusion more or less applies but a little anomalous here. The first thing you see is a couple channels and the fine grain stuff deeper in; but, those channels have...well you can see there are pretty whitened and actually the few stones that are in there have carbonate coats so they appear to be fairly old and in fact when we look at other trenches you will see this is something we see real commonly on these younger Pleistocene surfaces. So these are also kind of typical of the normal straitgraphy of the Pleistocene surfaces. What is sort of anomalous here is right at this one section right here between these two channels there is a real relative decrease in the amount of carbonate in the soils here. Although there is a lot of evidence for silt being translocated into this section. The question gets posed here had there actually been flow through this small channel; like, thin shallow flow through this and is that the signature of that flow on the soils. In looking at the upper straitgraphy here, there is really no clear straitgraphic break that we could identify in those upper deposits when we had them all scraped out and cleaned off that would allow us to say yeah these deposits are younger and something has been cut in here. So the best I can say here is that well this is permissive of having had large flows through here so if enough power develops we should see some deeper erosion. It has been somewhat limited the reason I kind of ...I'll show you a couple of the plots here.

Bob interjected let me rephrase your argument here for you if that's all right. This carbonate layer right here if there is some carbonate loess its got to be very old and you can trace, depending on your faith and skills as a sedimentologist, you can trace it up into here and call that one continuous timeline if nothing else. If there was flow through here, it certainly didn't get down to here. It's either very small or nonexistent.

Dean added I would go step further than that even and say there is continuous traceable straitgraphy close to the string line that is in general laterally continuous and traceable and if there erosion through here its been limited to the upper 20 –30 centimeters.

Bob said so even though this doesn't have any classic carbonate signature that we associate with older deposits we do a little straitgraphy and sedimentology and convince yourself that this is some of the stuff you can connect.

Dean stated everything below 20-30 centimeters is continuous and Pleistocene in age. I would say the question is; has there been remobilization or redeposition of the materials of the upper 20 centimeters in the bottom of the channel because at this site what the flow modeling shows is that at about 130 cubic meters water starts to trickle through this little back channel that's right here and when we go to the other sites, BLR-8 and so on, what we see there is evidence that that is the size of the some of the largest floods/discharge that we are associating with the 130 to 150 cubic meters. Cubic meters is the discharge that seems to best fit the geomorphic evidence though and at 150 it still is possibly really no power through here, but by the time we get to 170 or 200 it actually starts to pick up in these channels here and especially kind of right through here.

As I said this is consistent as the deposits we just looked at; over all less carbonate in here. Stream power? But it is also pretty consistent with what we see on the what we call the younger Pleistocene surfaces so these are the surfaces that were sort of the active elements of the system right at the end of the Pleistocene gravel stage as the braid plain was sort of being abandoned there was small channel networks that filled in with loess. You see these really beautifully in trenches T-6 and T-7 much more legible. Well it could be ponded water in this or it could be stuff that is just…I don't know, we'd have to dig it out.

Bob asked you know what that is its glaze from….

Dean continues it is the crusts from the backhoe spoil of some kind, see. I'd dig another thick fresh face before I said to much about it. We didn't see any thing when we cleaned it off and mapped it originally but I wouldn't…there are places where there is going to be some little laminated stuff at one level where things ponded up a little bit.

Bob inquired is there a relationship between the stability of clays and their age?

Dean replied no, that happens real quickly.

Scott added I was telling these guys that the materials lab had a clay silt sample from spreading area B that they did oven dry to run a permeability test on it and then it just sat there dry after that it was oven dried initially and then wetted. I was just curious so they put it in the compression machine to break it. It broke at around 600 psi just from wetting/compacting it.

Dr. Jorde began in the previous modeling of 150 …..

Dean stated you have to go all the way to 250; like we said yesterday what we found was….

Dr. Lall asked what can you expect to see out of the change I mean how do we know whether or not…

Dean replied that is where the stream power criteria came into to play.

Dr. Lall stated my suspicion is that it would be low, right?

Dean answered well it depends on the…let's back up…when it first starts to trickling…this is 150. It just barely trickling through here the power values are zero to one; very minimal velocity, nothing happening. What happens as the flow/discharge goes up you get more flow getting up on this surface getting up on this surface from multiply sources sort of upstream, this whole thing is sloping back down and everything and gradually toward the outlets the velocities are increasing as its reconnecting with the flow in the main channels. It takes a long time, like at 200, to start to see patchy power developing at various places on these surfaces. It starts clipping through at 225 now, its getting up into the 5-10 watt so with the stream power criteria what we would be saying is that at those levels you are easily going to be moving and starting to erode into these silts and fines.

Dr. Lall stated we won't have any evidence either way because it could just be a sheet of erosion at the top and we don't know where it goes and so since the top layer is maybe the amount of erosion we expected most of the 0-10 cm and at 30 cm you actually don't know.

Dean said predicting the amount of erosion is going to be dependent on the duration of everything. I agree but we'll see evidence of erosion at some these other sites or places where it has thoroughly been eroded. As I say here, this is permissive of having…there are some anomalies in the soil here and I don't think we can make the statement here that nothing has happened but on the other hand there is no clear positive evidence that something has happened here. It's kind of a neutral site.

Dr. Lall asked a question.

Dean replied its ambiguous I think is the case and I guess I use the word permissive because clearly the modeling shows that we do get water in here at flows that we think from other sites are the kind of discharge levels we think we see evidence of at other sites and so the question is what kind of record would we see here and the answer is well unless the flows get up into the 200 or 250 range we probably won't see a lot here okay and we don't. I guess I would say it is consistent with the flows being smaller and but at the same time the kind of scale that we see here certainly at 130, 150 there is a little bit of power and we could arrange stuff and we have a flux of water here that would probably wash some of the carbonate out, the flow has duration here and that's where I'd leave it. If nothing had happened here I guess I would expect that the carbonate through this channel would have been much more consistent over time. The fact that in low spots we see this sort of depletion of the carbonate kind of makes me wonder if we haven't had some flow. Now having said that, it is also possible that one way to get flow in here is that one day we had a huge thunderstorm out here and there is enough surface area here, a couple square miles that this would have to leave on this big surface out here. That is kind of the other wild card end member here that it could all be just a local thing and that this is the kind of record you get intermittently from that but again we had enough…that is a speculative scenario and I think a more direct thing is to say well this is perhaps more consistent with the idea that we had some limited flow through here a few times in the past but nothing too exciting.

Dr. Jorde asked how much of a fine material and silt would have been here at the beginning of the Holocene

Dean replied about this much. I think in general there is a little debate on that but I think the most common view on that is that most of this loess was deposited 10, 12, 14 thousand years ago. There certainly has been some loess deposition and reworking during the Holocene but it hasn't been wide spread you know. So presumably the component of it is getting reworked and that is actively mobile during the Holocene is maybe on the order of a few centimeters. Do you think that that is fair number, Glen. That the Holocene active compound is a few centimeters

Glen added yeah, that's what I would say.

Dean continued in general most of these kinds of sheet type things….I mean locally you can certainly get Holocene loess deposits that are meters thick.

Glen stated and downstream at Burns especially.

Dean said and intermittently things burn and we saw in 2000 after the big burn here; it didn't burn here you can tell…anywhere there is sagebrush it didn't burn okay. We'll be out of the sagebrush.

Chris added "yeah, you can see just across the river where…"

Dean interject "that all burned in 2000 and the whole area over there was a sheet of mobile sand. Locally there are places where in a depression or something you'll get a measurable accumulation but in general on the more flat terrain it's transient. So there is this kind of transient mobile upper layer that is centimeters thick and sometimes it moves through and that is an issue in how we characterize some of these floods here because there is certainly some potential for sort of transient dunes and sand deposits in some key areas and at the saddle that was always a concern that was raised repeatedly that yeah there is a threshold here but how do you know that it wasn't filled with eolian deposits when the flood happened and it never spilled over. So that was an uncertainty associated with that."

Lunch at **Trench T-4** Area

Dean continued "with high power this dam will actually, really start to get cut off…..those are older deposits and a lot of rock. You can see there is…the bigger flow ….The surface here is a higher running…"

NOTE: Tape quality is bad here and sometimes the conversation is indistinguishable. klm

Dean said late Holocene terraces and we see on the right a thin deposit….good evidence of the 400-year flood. We saw a picture of this yesterday it had the big, white holes in it where I had sampled. Those are from a couple hundred to a couple thousand years…some of those things actually have some reach to them but we have to be real careful about that.

Dr. Lall stated there is some debris that is over there so if that was from 1997….

Dean replied there are some measures of calibrations we can do for stream power and that are the basis for all these interpolations.

Dr. Lall said there were rivers in India when I was growing up and boulders this size would move with the daily flow because the daily flow depth would reach this but the bed slope is a lot crazier than what we are talking about here.

Dean explained you know one of the things the USGS did was they dug a trench in the constrictions….made a big deal out of the process….shell casings and stuff a meter or two down….of deposits, our calculations show in these constrictions you know even at 20 cubic meters real common….more than enough energy to…..

Scott stated both by this river there to…

Dean interjected right there, yeah it scours out there. Also they dug a test pit there.

Bob asked where is all of the loess?

Trench T-4

Dean explained down below it. Starting up here and the upper end on the edge of the Pleistocene surface and there is eolian sand that sort of comes in here plus the road but appears as though this end of the trench probably started in an old mound as near as we can figure out towards the end that in this particular setting between the road and the terrace slope really didn't have much morphologic expression in the subsurface context but that seems most likely to be part of a mound. So then we have bedrock so the trench is very shallow across here plus on the wall we really have no useful record.

On this surface there is a small remnant here of one of what I have mapped as a late and an early Holocene terrace what I called H-1 and H-2 on the map. I showed you guys a photo of this yesterday. At that time, there where probably four or five craters in it. This one was there and that one and a couple over here. There were three major units in this section. An upper part at the top that had very weak soil developed in it and very weak to barely detectable carbonate in it. Down in this part of it which is similar to the deposits we've seen in other locations, in the bank exposures and other trenches made us suspect that it was about 400 to 500 years old and we had two radiocarbon ages from this unit that actually confirmed that and two ages of about 500 and immediately below it a slightly older age so it brackets the deposition of this upper ½ meter deposit to that age and younger. It seems straitgraphically although somewhat mixed that like there was one unit that constituted most of this and that is the one that we had ages of around 500 years. There is a couple hundred year range of uncertainty on any individual age in that so I say 500 but the actual dating uncertainty is maybe 400 to 600 is all that we can really claim the resolution is there. Then in the upper most horizons up here there was a thin light colored finely laminated unit about 5 centimeters below the surface horizon here. That is the deposit that I called the "white flood" in the discussion and it's only present in this trench it's not present in that trench over there but that side is about 20 centimeters higher in elevation and it's not present at trench 6 which is also slightly higher. So we think that is a deposit of a flood that occurred probably 100 to 150 years ago. It is bigger than anything in the historical records probably late 1800s or something in that kind of timeframe. It looks like it would be 20 percent bigger than the largest historical flood recorded at Arco. Then the radiocarbon ages from the lower part of this, the oldest age in here; well there was a major break right here you can see the contrast there is a lot more carbonate below that. At the thicker section out here the oldest ages from that were about 2,000 years each from the light deposit was about 7,200 years but we have a major unconformity in the trench here but that seems to correspond to the age of a major fine grained Holocene fill that is present in the other trenches and was fairly extensible in here. Then you have the younger Holocene flood deposits set into that. The way we used this in the Paleoflood story was that what we can identify is the positive evidence of really two floods. One of which was preserved in the surface, which was relatively small, and the other which is thicker but lower in elevation. So at this site all one can really say for that is the minimum discharge associated with deposition of this 400 year flood was such that the stage had to exceed the level of about where my knife is and now we corresponded this site to maybe a discharge of only about 70 cubic meters per second which is only as large as an historical flood. At other sites we have other evidence of erosion and limits on what the high water might be so we say that flood was probably much bigger than that and in the end we end up concluding from adding those lines of evidence, so the dating for the floods comes from a site like this where we have only minimum stage but its erosional evidence comes from other sites which leads us to say well the discharge was probably much larger. So that's kind of positive evidence for flooding. In terms of the bounds, we have to look at it in a different context. If flow gets large, how much flow does it take over the top of this before we would expect to see this change?

Dr. Lall said one question I have is the flow that would have deposited that layer 150 years ago or whatever that would be lower than the over topping flow today. Or would it be higher?

Dean explained it is right at the surface so there must have been really shallow flow over the top of this surface or there was some flow over top and it looks like....

Dr. Lall asked is it higher than what we would get today for the same spot because the cross-sectionly we are marginally larger.

Dean replied no the cross section we presented at the cross section was about the same. The model flows that get up on this surface are about 90 cubic meters to have thin, shallow, a few inches of flow and have very low power. What happens at this site is that once the flow gets very deep you very rapidly get a fair amount of velocity cutting across this and a lot of power and it is somewhat constrained by the Pleistocene banks and the higher banks here. So this actually gets real high power very quickly. So in the context of the bounds what we see here is that we can have a little bit of over topping and there is no suggestion of modification of this would occur and if fact you might get a little deposition of silt. Once water gets very deep here you are going to have velocity, power, and shear stress here at such that what you would expect is erosion ant that is probably the kind of record, that how we produced this truncation in the lower section that at some point in the past whatever was here at that time, we don't know what it was, there was sufficient power to move this out of there and redeposit/rebuild this sequence of deposits which has been slowly building over time. We would say at this site; this is a record of stability at the surface here we have deposits of the 400 years flood and a slightly younger flood. The significant erosion here; we have a stable section here for about 400 or 500 years and so we have a bound because if a flood deep enough to significantly erode this had occurred since that time we wouldn't see it. For this site, this is one of the more limiting sites for the bound because the power grows fairly quickly. This is one of the sites that get high weight in our discussion for the bounds at least for the time period of 400 to 500 hundred years.

Dr. Lall stated but what that still means is that there could have been an event larger, earlier that scoured all this away and then this event in the 150-year time range deposited or 400-year time range deposited.

Dean added oh yeah, this site only tells you a bound for the last 500 years it doesn't tell you anything about longer time periods. That is absolutely right and we don't use it for anything else but that. When I mentioned 2,000 or 7,000 year sections it does tell you the geomorphic history of the river in terms of the infilling and the incision in the time periods that that stuff is operating but we have to go to other sites to get longer records that we might apply.

Bob commented this is a very steep course

Trench T-5

Dean said this is hard to see; these trenches have never been highly photogenic or real clear because the sediments are all homogenous in color most of the time and the light is often difficult, actually today this is as good as this one ever gets. You can see there is an upper section that has got lots of blue flags in it and that appears to a composite of several depositional units that are probably the result of late Holocene floods. We had a couple of radiocarbon ages in it that are in the several hundred to 1500 year age range. We had a couple of reversals there is some bioturbation in this section. In the middle of the between two sets of blue flags you can see there is a slightly lighter band it looks like it is the remnants of a soil that was formed in the lower unit and above that we think is a deposit of this 400 year flood; from a soil standpoint it looks very similar but its not we don't have good confirming evidence here but that seems to be the most consistent interpretation of it. The lower part we have again a truncated much older deposit where it is whiter because there is a lot carbonate in it. We will see at the next trench of what these white deposits probably were, they are remnants of the original early Holocene fill that have been eroded out of here by a later Holocene flood partially represented by the upper deposits but again at this site it was pretty clear that we really didn't have anything here that was…what was missing here was this thin deposits at the top of what I call the white flood unit over there. There is nothing in the upper most soil over here so we think that it is probable that there was something here a little bigger than the historic floods got up kind of a little bit on that surface didn't really leave any record here. But what is here are these deposits that are consistent with the late Holocene 400 year flood and probably for others of likely similar size but there is enough bioturbation that we can't recognize individual flood units although clearly the process says that over the long term pretty much the same thing is happening and getting fine grain deposition on these surfaces. The modeling would say that to get water upon top of this surface we need a discharge of about 100 to 110 cubic meters to get water up here even initially. You can see that this terrace continues on down to the next trench you can see there is a bench here that is distinct its about 1 to 1 ½ meter below the high ground to the left. The high ground to the left and behind the rock is all Pleistocene gravel for 2 or 3 miles. We have an inset terrace here that is all that composed this fine grain deposits. We will get a real good picture in the next trench. Here those fine grain deposits have been eroded out and been replaced in the upper section by things that are younger than last couple thousand years. In the next trench will we see that whole section is intact?

Bob asked did you talk about the basalt and bedrock or is that not relevant.

Dean replied it's relevant. This is typical of the bedrock control we have in these channels. We have very old patches and then channels that are formed in alluvium but there is intermittent but pretty consistent rock out crop along the margins and in the bed of the channel so certainly you can get some scour in between these outcrops but the overall channel geometry…we can say with some degree of confidence that we have a pretty stable channel geometry."

Bob added the issue is how convincing is that to the local people. You can put together all kinds of problematic channel geometries between the basalt here. How important is it to make a 1 foot channel between those two basalt outcrops 100 feet deep then fill it up with sediment as flow declines and does it matter. It doesn't matter does it?

Dean stated in the big scheme of things for the accuracy of estimating large flows I don't think so. That has been our conclusion.

Dr. Jorde commented it matters, it makes a difference in between the control, the bedrock controls but that is not really what you are interested in. You don't want to know what locally is happening right here. I think what you have to do is make a careful argument why this is not needed and why this wouldn't give any additional information."

Bob said from my perspective what about the real impact on the uncertainty we are talking about.

Dr. Jorde stated it wouldn't have any influence on the stages and on the flows. That's what I would conclude and I would have to be, and I haven't seen the entire river so I just have to believe what you say, it has to be actively addressed in the report so people don't question it.

Bob interjected this is just like Dean said you get just enough outcrop to convince us that it is probably not significant but there are also places in between that and nearby where you can make a problematic argument. So the question is how do we avoid that.... and people have done it. They have come out here and trenched and hammered rebar in....

Scott added the further down the river you get actually the smaller the channel gets and less basalt and the finer the materials are."

Bob injected "but for the point that..."

Dean commented "But that is outside of the Paleoflood reach."

Bob stated "That is one of the reasons we are being shown these places, I think, because we have as much basalt bedrock control as we could hope for in combination with the other characteristics. That right?"

Dean replied yeah, we limited the Paleoflood study reach to the section between the Pioneer Diversion and the INEEL Diversion expressly for the reason that there was an, with the exception of the meander section in here, there is abundance of bedrock outcrops and it is the reach where we can make the best arguments for stable channel geometry. When you go just a mile down, once you get to the highway and downstream, it's all alluvial and the channel morphology changes completely downstream of the highway. It broadens out and there is a much more meandering system plus there are lots of modifications in place down there. Large parts of that system are constructed channels or diverted channels completely not natural channels. All right let's go over to the next channel."

Trench T-6

Dean explained "This whole section is all fine grain, which I think, you can still see but it also has lots of carbonate in it and it has a pretty much complete carbonate soil profile. That is it looks like this was an early Holocene fill that has been pretty much stable for the last many thousands of years and the one exception to that is in the first section here up to about where Bob is you can see a series of flags along here. There is a little bit different deposit inset into the top of this, that thins in that direction towards Bob. As we move up this trench we see the upper deposits thinning and we see the soil profile that we would normally expect to see on this thing become more and more intact. So what we think has happened here is that this straitgraphic evidence of erosion and redeposition up to a point about at a level just a little ways past Bob and radiocarbon ages from this unit indicate that was probably again our 500 year flood. So there was enough flow and erosion here to strip off the upper horizons and in the waning stages re-deposit it, this thin unit here which seems to thicken again right at the edge. So from this section we have some age control in this upper unit and we also have radiocarbon ages in the lower part of this that suggest about 6,000 years or older. One of the real problematic issues was this whole deposit in here. It's fairly heavily burrowed and there are these little things about the size of my thumb that are called cicadas that are burrowing insects and they are pervasive throughout this section.

Dean commented this is really hard to see. As you move that way, it becomes a little more bedded. I think I showed you photographs yesterday of a couple of the sections just up there that have now collapsed that was what I was talking about as the on lap section where this stuff inter-fingered with the Pleistocene gravels. This is the section that I showed of the basic straitgraphy of the Big Lost River is really this trench with Pleistocene gravel and fine grained overburden….

Bob said formation and then by early Holocene, late Pleistocene the channel is actually developing its current geometry. This doesn't look so good any more.

Dean agreed yeah, well this is what happens over time. It's not dangerous. Here through this section that has collapsed and off to the right what we could see here was the bedding there were channel-like margin deposits and sort of the successive infill of this deposits as the system inside and then in the trench log we actually can trace a lot of units through this section here. We did find in one these, a hole to the left, some basaltic ash that seems to have come from the Craters of the Moon but we couldn't get a positive identification on which one it was, but that is most likely, or almost certainly an early Holocene. It was inconclusive.

The soils in this and the soils in the channels up here are all very similar. There are actually two or three channels through this section. Now one of the things that is interesting here as Bob said earlier is the saddle. The saddle is right back there. If flow over tops the saddle it would come shooting out in this direction. These have well developed soils in them and really we don't see any real evidence in here that anything would have happened. Once you start putting too much water over there, there is enough elevation drop that there gets to be some velocity. The other thing that is interesting is the geomorphic evidence suggests that if there ever was any flow on the surface here it actually would have gone this way; not coming from that way but it had to come this way. All those little terrace escarpments and subtle channel morphology on the late Pleistocene surfaces in this area run in the northeast direction as opposed to the southwest directions. I think these channels are associated with the waning stages of flow on the braid plain and the beginnings of the incision along the main Big Lost River channel because the sedimentology in these and the soils in these are very similar to what we see in the inflow and the offlap sequence there along the main channel. This section here there is slightly thinner loess along much of this as compared to Trench 1 and Trench 3 were we first stopped this morning. The carbonate is not quite as well developed in this trench although it is pretty darn close. One of the things that seems to characterize this is because the loess is a little thinner the carbonate went down a little further so its all in the gravel through most of this trench but there is big silt trans locations in the upper parts of the gravel all through this trench. Essentially that is a signature of long-term stability as sort of the upper unit on top of this gravel. It's a proxy for the movement of the carbonate as well.

Bob commented this is a pretty good carbonate horizon it's up there pretty high."

Dean continues in the fine grain stuff but that's the thing when you look at the logs and map this all out the carbonate in here matches up pretty much with the top of the gravel and what happens is the carbonate is a few centimeters higher in the fine grain than it is in the gravel. Although you can find it, barely, in there and it starts to fizz so that is the moisture front at the main focus of the carbonate deposition in this stuff is deeper in the gravels than it is in the fine grain and that's the permeability.

Dr. Lall-(makes a statement that is undistinguishable)

Dean explained the carbonate gets stranded on whatever collects on the bottom of the pebbles and what we have found is that when you can find well developed carbonates in the gravel unit and then there will be a nice, clean sand bed of uniform 2 millimeter sand or medium sand. It will barely fizz, there will be no visible carbonate, and then there will be massive carbonate in the gravel floor. My supposition is there is enough capillary connection in the sand so the moisture is going through the sand and then when you get into the gravels there is more void space in there and than it stops and evaporates and precipitates in that but the sands don't pick it up. Now if the sands had fines in them then it stays moist and then it evaporates out of there and the carbonate stays there so there is this, well, complex trade off between the real small variations in the grain size and gradation in the sediments in how the carbonates in the soil and that is a soils problem but I think our take away conclusion from all that is despite lots of variability in the soils there on a foot by foot basis through the trench the overall interpretation in the soils is that they are all the same. If you just had one tiny little observation of this you'd be hard pressed to figure it out with lots of views then you can fully appreciate how much variability there is and put it together. With a single panel exposure and everything; you think, oh, this is different, this is different, when, no it's really the same.

Dr. Lall stated so you tell that you show evidence of....(the rest is indistinguishable)

Dean added that slightly scalped section at the beginning of that trench. That is one place but it doesn't show up today at all, I apologize to Dr. Lall, and then we will be seeing that again at BLR-8 if those are going to be still visible. Here we had evidence; what our interpretation from this is that there is evidence of erosion with the 400-year flood up on this Holocene terrace. The majority of that terrace is underlain by deposits that are early Holocene 6000 to 10,000 so we had a big flood by historical standards which we think was probably 130 to 150 cubic meters something like that. That is our positive evidence of flood characterization. Now when the floods get much bigger than that we get more power on that surface so that surface becomes a bound but it's a bound over much longer timeframe now because that whole terrace in under-lain by deposits that are early Holocene. So at that site we would say okay there is a bound there for discharges of 150 to175, I think is the range that we find, to over a period of about 6,000 years because there is no channel cut into that other than that small little erosion on top there and that deposit extends all of the way out and so even over that long timeframe we have a limited channel geometry there."

Dean explained "Here again you can see we have more of these channels. As I mentioned when we were down at Trench 1, this is very similar to what we saw at T-3. There is a step up here 50 meters down the trench and I think you can see, well hopefully, it looks like the loess is actually a little thicker when we go up there. From there on there are fewer channels and we are up on the main P-2 surface and generally we have stronger carbonate so there is some indication of a timeframe there.

Bob stated one of the things that is important here is that we know based on its lateral position with the Big Lost River and all of these other things this is P-3 Pleistocene stuff and yet you don't see the heavy duty carbonate stuff in this. So even though the story is systematic it does require some care and attention on how you relate the carbonate to dates and figures. That is one of the things that will hurt us when these are filled in is that it is much easier to explain the story when you are standing here and once the trenches are filled in it is going to be problematic.

Chris said so the backside of the saddle where everybody says oh, that's a washout is down this way where you've got a real thick developed soil of Pleistocene age.

Dean replied no, the low spot is the saddle is actually right over here now.

Chris answered okay.

Dean continued and if anything came over the top of that it would come right through this area in Trench 6 back here.

Chris asked but you not are seeing any indication of early Holocene channeling or anything there.

Dean replied no, we don't think so and in fact when you look at the surface morphology here there are scarp-like features and small channels on the P-3 that go this direction. That goes right across the trend of anything that would have come out of the saddle."

Chris stated then playing devil advocate too the other argument I've heard made as well is the old channel is all full of eolian sands but you don't see any indication of that either in these.

Dean replied no, not until you get actually further back up in that direction then there is lots of eolian sand up there but again there is good soils evidence to the fact that it is fairly old there were well developed carbonate through that."

Bob said astute observers like Glen pointed out that carbonate issue. Where the saddle channel sediments ought to be, there is not as much carbonate there as you might expect if it was really old so you have to invoke these systematic but intricate lines of reasoning for how and where the carbonate does and doesn't go and under what conditions. I think that was your argument at one point, wasn't it?

Glen answered yeah, a couple of years ago; I remember discussing it then.

Bob stated, it is a valid point but it also points to the need for care, attention, and detail on how you get the data and interpret. Like Dean said at one point you can't just auger in one place and make an interpretation on stuff like this. Being morphologists and geologists, we all know that, but the hydrologists who are playing at geomorphology don't. That causes problems. A feature of the fine grained lithology, where we've actually documented it, that would be recognizable, observable, detectable straitgraphy.

Dean explained the fact that we don't see straitgraphic breaks is positive evidence of a limit on what could have happened.

Dr. Lall commented so this trench, I don't understand why it was in this orientation because this was presumably a direction that things could flow in?

Dean explained no, there is a road right here and what we wanted to do was kind of loop it around. It was mostly a constraint by the road but see the saddle has this kind of geometry. So it started over here going this way but we had to change directions at some point and then there is a road that runs right on other side fence there. The other constraint is the old bombing target. I wanted to stay out of that because I didn't know what's there. See the circle, that's the target range and this is after it burned. So before it burned it was even more visible because the vegetation is really different in here. In 1940 something, they dumped something all over this.

Chris added that did a real number on the vegetation.

Dean said I'm sure it is completely harmless to me. I just decided that I believe that its probably not too toxic but I'm going to and sniff the dirt for a few weeks. I think I am just going to stay out of that section. We are right to the edge of it and it was pretty clear on the old photographs that whatever it was are in here.

Dr. Jorde commented there hasn't been too much dispersion with time since it shows up quite well. (more but can not distinguish what is said)

Dean replied "For many reasons we had to stay on the correct side."

Dr. Lall suggested "We could bring students out here and have them look at all of the casings and have them decide if there was unbiased or minimum variance possibility."

Trench T-9

Glen said, these burrows are now weathered out.

Dean agreed, yeah, at one time I was seriously considering doing some thermoluminescence dating here what you do is bulk samples. You drive tubes into the wall and bulk sample it and then you try to keep it dark. Then you can measure the optical stimulation that the grains have received and it's a way of getting ages for these types of deposits. I did all of the sampling in places I thought were un-burrowed then subsequently as I can, come back to these sites and have seen how they are how they are weathering out. You see all of these little pockmarks here in the middle of sections and at one time I thought looked pretty good. Those are all ancient burrows. You ramp that observation back over 10,000 years the whole thing is getting churned over with some kind of frequent metabolic, who knows what kind of frequency, but it is significant. So I backed out of that because I decided that we were probably going to get spurious results. This is Trench T-9 and we are down there at the lower Pleistocene and what we hope to do here because of the geometry of this site are two things. There is a thumb shape to it and it protrudes out into the flow and as the flow goes up things will cut across this point here. We have a trench that we call T-8 and we'll look at that in a minute and we have this other trench. I did some hand auger holes here back along the axis of the trench and found that there were thick sequence of fine grain deposits here and thought this might be interesting to see because at that time I didn't realize there were all of these channels here all over the site here and we thought we would get a good strong straitgraphic record. we kind of did; we got what seems to be a big channel fill on top of the Pleistocene gravels that has a Stage 2 soil developed in it here and again this seems to be evidence of a P-3 late stage channel fill on top of the Pleistocene gravel. The real objective of this trench was to try and drop off the edge down onto a couple of small terraces down at the lower end there where we thought that were slack water sites where we would actually have a real continuous deposition and record of areal floods would be preserved because there is a constriction just \down stream of this and most of this area has real low stream power and pretty much stagnates under most of the flood conditions. We got a pretty limited record out of these things in the end. We have the 400 year flood here.

Dean continued, that is not a contact that is the level to which the water filled in the hole down here. That didn't look anything like this the last time I was here. What it should look like is that it all looks like this, okay, a lot of the white stuff. We have here a wedge shaped deposits, we have this bench right here which is a meter or so above (the channel) and what you could see before a lot grass grew up here was that this was like a stairway there were a series of steps going up this slope and what we were hoping was that each one of steps corresponded to a deposit that would represent a higher flow and it didn't. What we do have here though is we have this sort of tabular wedges of deposits that come down here and each of these say that this is the 400-year flood. These are 500 years old more or less within the window of the 500-year flood. Anything younger than that is up here in 10 to 20 centimeters of height and there really wasn't anything recognizable below that but the problem with this is that stage wise at this level we are at 60 cubic meters. So a very ordinary historical flow would have inundated this site and potentially could even deposit any of this and the last trace of these deposits here that we thought we could say yes this is tied straitgraphically to what we thing is the 400 year flood is about here at about the stage or way below the stage of what we thought that flood really should be. So these deposits are all ending up in very minimum stage locations were they really don't provide any upper bound information for us on larger flood. We didn't get enough straitgraphy here to give us a record that goes back very far at all. So that is why I was disappointed in this one, we did get datable straitgraphy but we didn't get it in a position where we can really do much with it in terms of extending and confirming this story.

Dr. Lall asked But this stuff is old right?

Dean answered, yeah; this is an erosion of cut into the Pleistocene gravel. This is not a stable; it has been an eroding so we can't really say a lot about it. Then we have another one of these channels. This is were Bob was making a joke about one previous guy who said this is an active fault because there was some really funny lineations and color contrasts in that deposit.

Dr. Lall inquired, so how old is that material in that channel.

Dean explained, we think that is Pleistocene. That is the same as the stuff back there, it is part of the channel sequence that's formed before the river incised. If you look at the margins of this, it is actually coming out this way southeast-northwest.

Chris asked, so this is one of your old braid streams.

Dean replied, yeah, it is the last braid stream on the P-3 surface. It is shooting out this way. So it was coming here, trying to get out through that rock where that tree is.

Trench T-8

Bob added, we can speculate a lot but it looks like this fine grain stuff in the channel cut between these two channel beds. Over here is some of that strange coloring. Did we determine that this was not manganese or is it?

Dr. Lall commented, again, it seems we are in a section where the flow….is this way or that way. So judging by the change is elevation the larger flows would be going in that direction."

Dr. Jorde added, when you fill that back in it doesn't look like an alluvial deposit any more."

Dean explained it's depending upon what people do and what you think you're looking for. If it is not real bedded like the deposit you were looking at, say it's more like the channel fill with a little bit of gravel in it, then it doesn't look a whole lot different from back there. What we were trying to do here is this is the site, BLR-8; get some kind of fix on this. Our original intent was we were going to dig a trench across most of the length in this so we would have a nice continuous record through here because what we expect to see here is a record of stripping, erosion and re-deposition as well, associated with larger floods; but due to the archeological concerns here we ended up limiting our excavation to these three short pits and as I talked about yesterday we were not allowed to go way out to the nose where it would have actually been most diagnostic it terms of modeling. It gives a disconnected and inherently somewhat confusing record when you factor in some erosion here but I think in the end it all makes sense but there is going to be some residual geologic uncertainty here because of the disconnected nature of this."

Bob added, it really was sort of dumb to do it like this but it is one of those things.

Dean replied, sometimes we have to be happy and dumb, you know.

Bob agreed you have to make concessions.

Dean explained what we have here to start with and actually this is still pretty clear here and you can see the gravel at the bottom and the fine units embedded into it. We have the carbonate pipes, the well-developed carbonate soil, and a thick fine grain sequence on top. It is a little whiter in the lower part of it, which is carbonate accumulation in the overlying loess and the eolian deposit. One thing that is a little different here than in the some of the other sites is that we have more eolian sand on top of the loess that is typical late Pleistocene, early Holocene loess and there is a subtle grain size difference between the lower part of this unit which is a little finer and the upper part which is a little bit courser. If you walk around back behind us here, you will see that there is actually a lot of fairly fresh and recently active sand that has moved around and that is real typical of any place where there is basalt outcropping. In the vicinity of the basalt outcrops in the down wind side of those there tends to be a lot of recently active sand because there is nothing to hold it in place. We have a little bit of eolian stuff kind of moving through the system here. That's where we start out is on the left and you can see it is pretty thick. As we move to the right here, we move once again into one of these channels. The straitgraphic context of that is pretty clear it is inter-bedded with the gravels and soil continues right through it. In this trench everything looks fine; we have an intact soil profile in the channel. It changes a little bit as you move into the gravels but it looks very normal, stable and in place. This is long-term stability since the late Pleistocene and that's it. Let's move to the next one.

Dean said, I disagree with you; the sand and silt filled channels are in the abandoned meander in the Big Loop area in the Holocene fine grain section. That is exactly what is going on here; exactly the same process. There is an analog. Okay, here no gravel except a little tiny bit of sand right down at the bottom. We got teased at the bottom corner thinking we could link the gravel section together and the soil is kind of the same but it seems like here we are missing the part of the upper most darker horizon. Overall the carbonate here is too shallow and that is it is only 20 or 15 centimeters below the ground surface instead of the normal 30 or 40 for these fine grain deposits. So starting very subtly in the middle of our trench interruption here is the beginning of some very minor stripping of the soil that has been here a long time but we have removed a small amount of material from that corner and slowly increasing as we move to the right here (toward the channel.) We think the basic channel fill sequence that is over there on the bottom is still intact and that's a reasonable correlation but the soil is slightly stripped.

The archeology pits that were dug before we did the trenches and that one we thought was the same that trench but again just a little more stripping of the upper horizon and the same thing here. Here most of the upper "A" horizon is gone so we infer here that we are missing about 20 centimeters of material and then we get back here. Again, we are missing at 20 centimeters of material and in parts of this it seems like the upper one-half meter of this was eroded and possibly replaced with mixed materials. This is a record that is similar to the very thin deposits we had in T-6 and it seems to be the upper deposit composed of a material that had been eroded from this terrace and re-deposited. On the other hand it is pretty minor. At some point here the feeling is if we got out there we would see a channel fill but we never got to see that. Our interpretation here is that we had a flood circa 400 to 600 years ago that the highest stages of which got back to the vicinity of the upper pit up there and which had sufficient power to move materials from these areas and start to produce some pretty significant erosion. The modeling kind of suggests that is probably a discharge that would define our erosion criteria.

Bob asked how do you bracket that age wise.

Dean replied age wise we have zip for age control on this site. We couldn't find anything in here to do radiocarbon so it is relative. It is similar to all of the soils that we have 500-year radiocarbon ages from.

Dr. Lall inquired is it the same as the radiocarbon there.

Dan added it finally overtops the channel over there. It takes the shortcut through there.

Dean said it goes through the rock outcrop over there and trying to shortcut across this and get out through the low spot there.

Dan added it takes such a hard turn that it backwaters behind that turn.

Bob asked do we need to go down the channel to see what it's doing there.

Dean replied when we walk out maybe we'll walk out down the channel and loop around then you can see the rock. It is all that mixed rock control.

Bob commented there are quite a few Native American artifacts over there. I don't know if they have any ages associated with them or not.

Dean said I need to loop back to Brenda and see if she has done anything more with this. There was an archeological survey through here and one of the kind of interesting things that they did turn up is in the areas that are H-1 and H-2 surfaces generally the only artifacts that they have described are things that are consistent with ages that are younger than a few hundred years. When you go onto the slightly older surfaces it is a mixed population of all ages all throughout the Holocene. So there is a spatial restriction there and of course their artifact age boundaries don't coincide with my flood boundaries exactly. When you go below the area that seem like that they inundated probably by the 400 year flood there is no older stuff on the surface there is only younger stuff. I take that as a signal.

Dr. Jorde inquired so this is the material, which you used to determine the required stream power, right?

Dean answered yeah.

Dr. Jorde added and then how much of that material might have been removed.

Dean said it increases as we go in this direction (toward the channel) and it goes from back in these trenches a few centimeters from 0 to 10.

Dr. Jorde interjected would increase the flow but not dramatically, if you changed the geometry of it. Is that the only indicator that you have for the 400 year flood or are there others which you have flows smaller. This is not the only place that you have evidence.

Dean explained we think we see it at Trench 6 and then we have actually about a half a dozen bank exposures, 4 bank exposures in this reach just stream banks. Like that bank over there, where there are deposits in the upper half of the bank that are 400 years old, just as a round number. What happens here is interestingly enough is that at this site hydraulically my suspicion is that, all though not confirmed by the trenching over there, that there should be deposition and that channel is going to fill so we are going to cut something here but all of the existing channels become more or less stagnate. So those are going to be filling…

Dan said not exactly stagnant, they move to the inside of this turn and then over this bank. They cut across that basalt there and it stalls there but as soon as the flows are receding the river returns and cleans all of the soft stuff that was deposited into the channel right out of there.

Dean added so unless you fill it to the brim,

Dan interjected I think if you fill it to the brim the bank will slough in.

Dean explained I mean you have to fill it to the brim in order to make a permanent change in the channel and there is rock all underneath this point here, so the channel is just going to slide back into low spot there every time. That's what Kyle House has called progressive self-censuring. Floods leave a record and then come back and wipe it out. It becomes hit or miss as to whether you are lucky enough to find the few little remnants that are left alone.

Dr. Jorde asked at some point with these flows, if you make them big enough and strong enough we change the channel geometry. What we are saying is that the numbers we are invoking for erosion, what I'm saying is that for this upper stuff it is really just the loose silty A-horizons, that moves under very low power and then when we get down into the road into these carbonated horizons like one-half a meter then we would say that we would have to have much higher power to do that.

Dr. Jorde then asked another question but the wind drowned out the question.

Dean replied well I think that is probably conservative. I don't know how to quantify that or what arguments I should be making because it becomes a magnitude of erosion questions.

Dan replied we haven't come out with our stuff yet either, everything takes time.

Dean said 10 centimeters higher than it is today and extend it quite a ways further out so the one section is in one sense the beginning of the flood. We probably had a smaller cross section potentially this argument goes both ways we might have had a smaller cross section than we do today. So it might have actually taken less discharge to initially over top it and initiate the erosion. There are elements of the cross section arguments on these kinds of sites some ways I think you could construct them both ways.

Dr. Jorde commented you know you can construct them in both ways and analysis them and see what the differences are.

Dean replied that might be a good idea if I made like a section across the top of this that projected original ground surface. At least we could do it out to the riverbank edge and say well this is probably the inferred Pleistocene surface so I have a lot of speculation to produce that. Dan is laughing at me all ready.

Dan added with all of the other uncertainties you have to decide if that is the way you would do it.

Dean said yeah; I'll play with that because I think what we could is then dot one through the trenches and say well this is sort of the maximum erosion and we could compare the maximum at least for part of here we could say well here is how much as been eroded below the present topography and then re-deposited by the 400 year flood. But then at the beginning here is how much has to added back in. My guess is that there is more material missing from this thing before the flood starts than is absent and then we would take, due the later deposition because it is much more aerially expensive that the longer sections. The big unknown in that is still out at the end in the area where the models say at these flows we get really high power and we never got the trench out there and we don't know what is out there. That uncertainty is a factor of 2 greater than anything that I could actually document through the trenches.

Big Lost River Flood Hazard

October 7, 2004

Stoller Corporation

Idaho Falls, Idaho

Attendees:	Dan O'Connell	Bureau of Reclamation
	Robert Creed	Department of Energy-Idaho
	Dr. Klaus Jorde	University of Idaho
	Dr Paul Link	Idaho State University
	Dr. Manu Lall (Dr. Lall)	Columbia University
	Chris Martin	S.M. Stoller Corporation
	Kathy Morgan	S.M. Stoller Corporation
	Dean Ostenaa	Bureau of Reclamation

Chris Martin welcomed the group and said the day would be spent answering questions Dr. Lall and Dr. Jorde had after the field trip.

Dean stated that the group also needed to cover the topics not talked about on Tuesday, October 5. He mentioned that the group needed to review the flood frequency components, inundation maps, stage probability, and then have a wrap up discussion.

Bob then reviewed the key products he needed from the group.

Uncertainties adequately captured.

Does the process data analysis properly support the probabilistic model and its conclusions?

Bob strongly urged Dr Lall and Dr Jorde to remember the pragmatic and utilitarian issue involved. That is that this review has to be completed and to remember this is not the first review. The quality and magnitude of the data indicates that most if not all of the major issues have been addressed. He reminded them that if an issue was a problem he wanted to know that. He asked that the final report included the following:

1. What is good about the report and makes it defensible.
2. What is weak in the report, if you see anything, and
3. Advice or recommendations for research and additional work

Bob interjected foremost have we captured the uncertainties and is the probabilistic model adequate and appropriate for what we want to do.

Dean asked if there was anything that anyone wanted to touch on from the field trip yesterday before going into the discussion on field frequency, inundation, mapping and stage probability.

Dr. Jorde asked about the question he had already raised and how that would be addressed.

Dean stated that it has not been written in the report, however it has been talked about in the past and it does need to be mentioned. He suggested coming back to that. The uncertainties in the stage probability estimates are another factor that needs to be brought in yet.

Bob asked Dean to review the issue so the group would be clear on it.

Dean said they had been talking about the stability of the channels of the Big Lost River on the site, downstream of the highway and the impacts of it. Dr. Jorde had asked if there were shear stress and power maps for the inundation reach. Dean stated the answer to that was no, those had not been produced. It was a question of how much would all that effect the study.

Bob said that it would but maybe there were other things that were incorporated in terms of conservatism that would bound those in that.

Dr. Jorde mentioned that after reviewing the terrain yesterday, it would have but people will ask that question. He brought up the idea of additional modeling.

Bob stated that in a broader context, his project managers will say is this typically what is done in 2-D routing studies. We have done the sediment transport channel modification studies for the flood hazards. It is an issue

Dr. Jorde said that is why he was hoping for the shear stress and power maps. The channel is only slightly deeper than all of the terrain. So the stream power and shear stress in the channel will not be of different magnitude, although it will be slightly higher on the surface. Erodability of that surface is very low so there is a potential that the channel will start moving all over and dig new channels. The way to find out if this would happen would be the shear stress or stream power map. It would create a whole new pattern of inundation.

Dean said he did agree and that had been thought about but in terms of limiting the volume of work they stopped at the inundation depth maps. That producing a set of maps of stream power and shear stress maps and discussing was doable and appropriate.

Bob stated that some assertion had to be made. For instance, what is the stream power now we do a sediment drain size distribution and etc.

Dan observed we are at the point it would like doing a linear analysis in the earthquake thing. You are looking at ….some other thing.

Bob added it would be useful for the litigation proposes also.

Dan said really these maps are an initial early inundation scenario and we are not...it becomes more of an integrated process.

Dean stated he would come back to this discuss as he had planned to touch on it.

Dr. Lall stated that he thought that laws would have caused more studies of this sort. What he thought was missing in the summary section was a way to bring this stuff together in a systematic discussion of uncertainties versus assertivities versus possibilities. He suggested a table of summary of the upper section where there is high confidence because of stream controls and so on. At the lower section, where things are more open and there should be a list of itemized reasons. Dr. Lall stated his main comments would focus on presentation rather than analysis.

Dean said that admittedly there is more work to do and there are missing parts in the presentation.

Dean asked about Dick Baker. Bob liked idea of some of the group going to Tucson and showing him the information.

The group began discussion on the flood frequency.

Dan began basically there was a historical and paleoflood about 500 years ago and there was a later, shorter duration Holocene bound on discharge. That information was taken and uncertainties incorporated, and then combined them with the gauge information, and the gauge information is not direct estimates from the diversion gauge. It is from the Howe Ranch, operated for 100 years, and Arco gauge, operated for 50 years. It is a reconstructed gauge we are trying to estimate at the diversion dam.

Dean stated that when they did in the1999 study as a gauge record they used the Arco gauge data. The Arco gauge is a heavily regulated record in terms of irrigation diversion, etc. The flows from that are much smaller than any of the flows in the upstream basin that is unregulated. Based on the work done by USGS, a proxy unregulated record was developed. So using the Howe Ranch record, almost 99 years old, and attenuated it according to the suggestions of the USGS guys' estimate of the 100-year flood.

Dr. Lall asked how the attenuation was done. Dean replied USGS developed a series of regressions based on the gauges down river using how much the flow attenuates between gauges. In the end, it boiled down to whatever was started with is 25 percent at the diversion dam.

Dr. Lall then asked how Mackay Dam figured in.

Dean responded that the impacts of Mackay Dam on the gauge record are very minimal because for the annual peaks that are of any consequence there is no attenuation through the Mackay Dam system. The attenuation comes from the channel loss and infiltration in the alluvial reaches upstream and downstream of Mackay Dam and the Lost River Valley upstream of Arco as well as channel losses and infiltration in Box Canyon. Mackay Dam is a buffer in terms of the time sequencing of the flows.

The USGS is a more formal analysis compared to what was we presented in the 1999 report which was a graphical presentation. USGS did regressions on that and formalized it.

Dan stated that there are quite a few flows in the reconstructed 100 year record in the 70-80-cms ranges. He said they have and he can actually accommodate any arbitrary shape of uncertainty but it is easier to parameterize it in terms of small numbers of parameters related to the uncertainties of the verdict. This is the limit on peak discharge and he used a preferred value of 250 cms and it is extended out to 175 and there is a bound that is basically positive evidence that something for at least the last....

Dean explained the map and said it is describing the flow across the Pleistocene surfaces in the paleoflood study reach. It is showing the discharges that would modify the big surfaces at the Big Loop and over flow the saddle, the P-2, P-3 surfaces. The field evidence for that is erosion into the soils in the terraces H-1 and H-2 at T-6. It is the principal bound where that is really constrained at BLR8. The 110 – 150 bound for the last 500 years is the occurrence of a flood larger than the 400-year flood in the last 400 to 500 years. It is a recent occurrence or reoccurrence of the 400-year paleoflood. There is another one of these deposits in the "white flood" in trench T-4.

It is a uniform probability somewhere below 90 and 105. There is also historical bound.

Dean stated that the 400-year paleoflood is the largest flood that has occurred since time. The last time we had a big paleoflood was 400 years ago. Dan added that exceeded discharges in that range.

Dean said that when they lay out the bounds those are based on the stream power criteria, integrated all the sites, and came to the conclusions.

Dan said these are probable estimates of the discharges associated. Used the process of relating stream power, shear stress, and the other parameters.

Bob asked if the results were guestimates or computed.

Dan replied that these become the measurement of uncertainties.

Bob then asked if this was a traceable and auditable process?

Dan commented that yes it should be. He understood that there is a professional judgment or some degree of subjectivity about how you have to weight the information from different sites. It would never be completely objective.

Dean also added that in the report we should, and I know its not all quite there yet. The intent is when we close out the discussion of paleo-bounds criteria in section 2.3; we have a table of values that corresponds to the bounding values on the plots. His conclusions in those sections are that the best discharge estimates--based on criteria and integration of all the sites--of the 400-year paleoflood were between 130 – 175 cubic meters. This is how those numbers transform into formal probability estimates for the flood frequency. That transformation is documented in Dan's discussion and likewise for the bounds.

Bob asked that is this a kind of arbitrary assessment and Dean replied no. Then Dan explained that it could have been done in relative likelihood, the factor would have been one and it would have a cumulative frequency. It also depends on how many different points in the interval.

Dr. Lall said one way to put this into context is you chose different faults for these distributions and you chose parameters for them. If one had chosen the uncertainties what would have been the implication of that. The easiest thing to do is just writing that and be done with it.

Dan explained that you could come up with some form or process of sampling and re-sampling and create intermediate PDFs and integrate through all those and end up with an objective of the probability density for these discharges estimates coming from some weighting but it would never be completely objective because you have to assign weights to the way you find weight the value of the different sites and the different estimates of shear stress and stream power.

Dean said that you would use the six steps he had outlined earlier. Dan stated you could take those and go through a logic tree process and quantify almost to the end.

Dr. Lall commented the only thing that shows up negative in this particular picture is that somebody who is not very familiar with things might comment about is having a triangular distribution and suddenly you don't have a triangular distribution so why do you chose to change that one because basically your natural argument would be; hey, I have a most likely this by an assignment of that and I've got some scattered around here and I'm using on the triangular because I know there are limits to this.

Dan observed that he thought you could explain that physically because of the difference of the stream power and shear stress between 130 and 150 even above 150 there is a substantially change in state. That would have explained the near form weighting.

Dan stated it might not stand out as much if we had the plots of all the inputs here because also it's the near uniform weighting between discharges.

Dr. Lall stated that a natural choice for such uncertainty distributions is a triangular or trapezoidal distribution.

Dean stated that there is very little of that discussion in section 2.3 where we discuss how we estimate. The table in there has a column for the type of distribution that is used for the bounds and paleofloods. The beginnings of that are traceable in the report.

Dean said the transformation from the geologic through the bounds discussion has always been subject to lots of discussion. While we are talking about this, one of the criticisms we have received in the past is one the use of the triangular distributions is that we are to deterministic. That when we take these triangles down to zero like this as one guy put it one day—you guys have just decided what the PMF is. I didn't quite agree with him but that is what his portrayal of this. You have just said nothing can happen beyond this. Well that isn't really what we are saying.

Dan added we are saying within this time period we are confident at some level we haven't seen a flood bigger than this. That was really a suspicious comment about PMF. There are a lot of things that are in the context at the time of optimization.

That was Bob Jarrett. He was saying that you guys are slamming the door on anything bigger than this. Dean thought he was obliviously trying to over state something to make a larger point. Dean also pointed out that there are other people who can participate in this with less exposure and have appreciation for the value of getting the geologic information and have the same views.

Dan said we basically have the fundamental in the time period where we observed the floods have exceeded. The role of the flood frequency maps and what we are trying to get…all of these things have some uncertainty estimation and we would like to know probabilistically what scenarios are consistent with those measurements.

This analysis is something called a non-parametric flood frequency analysis. When you use these parametric functions they basically assume that the data has a primary mode of density. The parameters looked at are a central tendency for the dispersion and the skew, depending on which one you use the parameters has a slightly different meaning.

Ultimately what they mean is there is a single state of primary load of multiple densities. What we find when we do this is that there are rates to the discharge where floods will repeat. That means that these floods that are nearly the same magnitude and that means that locally you have a secondary mode of density. Consequently when you use these parametric frequency functions they use up most of the degrees of freedom and they really can't tell you of the range of behaviors that are consistent with the data at some other part of the distribution. These phenomenona are just so common with data sets that I had to come up with different way of estimating flood velocities. So what I've done here is basically the Monte Carlo approach and generate flood frequency models. The flood frequency model operates under a simple constraint, which is that as you go to increasing probabilities. There are several ways to generate frequency models and just basically have added a scoring function or a likelihood function that can rate how well a particular generated model agrees with the data that is available, hence a model of flood frequency and a rate that gives a score. To reduce the number of degrees of frequency I am doing the Monte Carlo and I'm going to say that there is enough data to constrain the very frequent floods. These show models that almost fit the data equally well and really gives you an idea of the classes of models that are well constrained by the data within the models. There are not that many variations in the models that fit that data. We have a lot of data within a relatively narrow discharge range so the uncertainties are relatively small. We have this one positive evidence of flooding at 150 cms paleoflood. There isn't a lot of density out here that we could say a lot of probability it could as easily be an infrequent or a more frequent flood. What constraints these models to the left are non-exceedance bounds for stabile durations. So if we have 500 years were we haven't exceeded discharges, which have uncertainty in duration and similarly we have this mid-Holocene bound and then a bound that persists through most of the Holocene. So they limit how frequent floods could have occurred and are consist with these data. Conversely if you have positive evidence of floods they now curve to the right because they require that floods at least occur that frequently. Really the range of flood frequency is bounded to the left by non-exceedance bounds and to the right by positive evidence of flooding. What we don't have here is a lot of positive evidence of flooding as we go out to the longer duration records of the Holocene. Consequently there is only required that you be consistent with this data within a factor of two of its plotting position left or right. But once you get out here you only have the bounds and the requirement that the floods at least increase a little bit as you go out to longer return periods. The model that almost fits the data equally well spread all over these ranges that is really not a requirement for floods except that they need to be somewhat larger bounded from below by the largest observed flood. So as you go out here there is not a strong central tendency. That means that the mean would be to reflect a central tendency. Well you are bounded to the left by how frequently these things occur and, frankly, out to the right you can say that there is no requirement that they ever occur once they get above the largest you have ever observed. There is the fact that there is a strong central tendency plus this tells you that eventually with 10,000 years of data you really can't say much out past that 1 in 20,000. Except bounding to the left because you don't have a lot of constraint where you don't have a lot of data. The big difference between this result and a parametric result is the parametric result is constrained by factors that are not related to these bounds about what shapes they could assume so they tended to project a narrow range of possibilities as it went out to the longer term periods. This tells you eventually that when you run out of constraints over a certain duration of time you run out of observational constraints on the limits of flooding.

Dr. Lall suggested running on a log scale so you'd end up with a slope that is relatively constant and it could be aesthetically better. This is another distribution that is going to suggest that morphographically increasing floods with no upper bound and yet the analysis you guys present essentially suggests that there is a logging process so if it were a log scale you can avoid that perception.

Dan stated what he is trying to emphasize is that there is just as much of a possibility that floods do not increase above this values and that is consistent with the data also. What we do have is that up to 1 in 10,000 there is a strong limit on what discharges are allowable and that is just what reflects what information we have on the field. For instance between the one and couple hundred year flood we have to be consistent with the positive evidence of flooding but there is quite a range of possibilities and when I've done data sets when you add more positive evidence of flooding this range becomes narrower because the positive evidence of flooding basically pulls the right hand confidence limits to the left and up against the data. This is what we call the simple scenario. There might be suggestions of some flooding somewhere between floods we observed here and floods we observed here. We don't quite know how to quantify those so certainly in a sensitivity test you can run cases where you add floods in between and see how that might change the perception of the probabilistic.

Dean commented that they had done a lot of sensitivity testing of the parametric models in the 1999 report with those kinds of scenarios and basically the conclusion from that was when you go way off the deep end with your.....beyond your intuitively credible limits for how many floods could occur everything blows up.

Dan said that what happens is the scoring function is....when you only have bounds that will score higher if you move things to the right and the scoring function when you have floods will score high if you move to the left. It is that combination of information and a trade off between the two that constrains the flood frequency from both sides.

Bob asked why the red line doesn't run through the tops of these. The answer was that the historical bounds are not on it.

Dan stated that is putting a hard limit here and its operating....we have in the historical record that says we haven't exceeded 105 and that is sort of bounding this one here. That is one of the things that is driving this to the right. If they did some more areas with more possibilities of intermediate floods that would require the whole field to move to the left, that wouldn't act as strong scoring functions and in the absence of much positive evidence of flooding being used in the flood frequency analysis they both dominate the scoring of any data points plotted what its says about the actual uncertainty is it can plot out to here and because of the historical bound it doesn't want to allow us to move that far to the left. Part of the problem is the plotting position for the 500 years duration is actually over here if there was to be a flood. It's the issue of how do I choose to represent something that is really just a duration on a flood frequency plot. I just plot it as its duration equals return period, which actually works fairly well. If there were more floods in here it would tend to force the curves up to the left.

Bob added these features that we tend to have certain ages on and if their plotting position is determined by the ages we assign to them versus....creating all that data so the real position of these in respect to the red line ought to be slightly different. In other words, the 400-year flood in the larger context may actually be here.

Dean commented that is the way the floods plot. See the one that says "P" that is the 400-year flood but it is not plotting at 400 years of return period. Where as at the bounds we plot just strictly at age.

Bob inquired so these are strictly age and this is probabilistic issue not its age.

Dan said it is because it's the only one in the interval and is plotted at two times the age.

Bob remarked the model shows that you shifted all this stuff this way and it really ought to be......

Dan replied there is not enough information as these are probabilistic estimates.

Bob said that is not in the 1999 report though

Dan explained I did that parametrically and as far as is this stable results with a given play, I ran at 8,000; 40,000; and, 320,000 models Monte Carlo and got the same results. There is 4 percent accuracy. For instance, the one in a hundred flood this black curves sort of PDF's and we are getting out of this analysis.

We've got a range of about 20 cms between the credible limits. Within the PDF, we bracket the USGS number. We get closer to the USGS number at 1 in 50. What is going on here is again it's bounded very strictly from below because of a lot of data from that reconstructed gauge record says that these things occur. On this bigger than 80 but as you can see the tail is starting to look more log normal so its allowing for a broader range of flows. So while you're not really accounting for that well you know yeah at this credible limit or not because there is so much area in this curve but the tail allows for it. The tail is spreading way out, so in fact we're saying that there is a slightly non zero possibility of the paleoflood being the 200-year flood. It's very small, like 99.9 as you look at the full range of possibilities that are probabilistically compatible to the data. There is a much broader tail out to the larger discharges so by the time you get to 150 or 1 in 500 it is spreads out and is getting out toward the upper limit of what we would allow for the paleoflood discharge. The requirement of a lot of floods is the local density if these floods occur two or three times like we have seen on some of the other rivers. When these have positive evidence of these floods reoccurring then these can concentrate more around those positive observations of flooding. When you just have these bounds in a couple of floods then you get the broad distributions that allow for a wide range of behavior. They obviously get more weight around the range of floods but now for instance in one in two thousand we are reaching the lower limits of the Paleocene bound.

Bob asked what this curve here is.

Dan answered that is was the estimated sampling density of the range of the charts generated in Monte Carlo of flood frequency models.

We wanted to show that they extend beyond the limits of the non-zero density of that score. They have an adequate sampling breadth. This is an important sampling function. Most of the models I am generating are in here but I do extend out with less frequency in the models that I generate out to these larger and smaller discharges and they do have to be broader than the posterior you are trying to estimate so that you haven't artificially truncated the possible range of discharge. The red is just the accumulative function and these are 5 percent credible limits. As you extend out to longer return periods basically things get very broad. On one hand with just that positive evidence of that paleoflood basically that is the only thing forcing you to putting a constraint on the lower limits of the flood and the only thing constraining the upper limit are the bounds. These become very broad functions. If we do these with more paleofloods included they tend to narrow more.

At one in 10,000, which is the limit of the time period, it is becoming very lognormal. There is a small possibility that 10,000 years observation is not representative of a full 10,000 years and the constraint gets weaker….and get very broad. So there is less meaning for a central tendency measure like a mean and now you are at a range for possible models that are nearly equally consistent but you are losing your constraints statistically.

Dean commented you mean the plot that shows the 100,000 years per stage? You don't need a flood frequency plot that shows 100,000 years.

Dan agreed, then stated the other thing you can look at is the range of annual exceedance probabilities for target discharges. That is what some of these plots are trying to do. So we also did this and you can see the blue is the sampling functions again. They always bound very strongly how frequent these things can become. It is possible that they never occur and again it is real hard to come up with….not really a normalizable function and so I can't really say a credible limit.

When you get out to these small annual exceedance probabilities, you can calculate a mean but it doesn't solve looking at the density function. There is a large range that is equally compatible and if it turns out that we find a way to use the stuff out to 1 in 10,000 and meet all of your criteria then we are in a much better position to make a statistically significant statements.

Dan stated it is a limit from the data on how large they can become and how frequently. It is not necessarily a limit except that they have to be larger than the largest observed flood.

Dr. Lall commented it's similar to the one before. I was going to revisit that when I got this job. The other thing here is the….model I'm trying to work through is the way you guys have presented now rather than of the normal approach, which is start with this and then go to that other thing. So the thing is here is the site, here are the areas of concern; here are the flows of which the concern was developed. Now we are giving you a range of estimates of the probabilities of those flows. I'm quite comfortable with that approach. If we were to start here and then you went to that other thing. That it isn't really a full uncertainty analysis in this or any other application there's uncertainty analyses in some pieces of it and there is sensitivity analysis in other pieces of it. If this part comes through as uncertainty analysis—fine. The hydraulic part is coming through as sensitivity analysis. What he has to present…the challenges…no video, regular Bulletin 17B procedure or whatever and then represent a full uncertainty analysis because Bulletin 17B allows you to put confidence limits on everything. That's what he needs protection from is the naive position that that is actually better. We could go through the details of what you have done and keep doing it. I don't know that that is very productive. So I have to think through how I suggest that's done and it could be that we discuss it a little bit and then write it.

Bob stated that what is going to happen is that certain engineers and NPH design folks are going to pull out these curves and say oh yeah there is a need to design or mitigate for this and so I need clear a framework.

Dr. Lall replied that we start from the hydraulic site issue and say this all that we are really trying to push. Then this is pushed in the background.

Dan said the flood frequency is used to drive the site inundation.

Bob said the stage versus frequency period. That is where we have to get. This is the background stuff. We are going try to avoid the position of having to defend. You know it has to be there so it is really smart if we can look at it.

Dan then stated that this is stuff relates to the regulated flow. This is strictly a scenario for how we…unregulated flow.

Bob commented that is the other issue that is going to keep driving us to the point that this is just one part of the flood characterization logic tree.

Dean agreed.

Break

Dean said he had five or six slides to try to organize the discussion of the last couple of days. He stated that what this is, is a conceptual tree that was put together in the early stages of the project. As to how we were going to go through the process and what were the factors that we really need to consider along the way for making probabilistic stage/discharge curves. We started with a flood frequency estimate of some type which we incorporated the paleoflood data and centered on the diversion dam area. We needed to have suitable ranges of probability. The hydrograph issue is one that we are kind of dodging here but it is clearly an important factor contributing to the stage probability. We move down the road here towards considering alternative scenarios like dam breaks versus natural flows the flow duration is going to have an important impact on the ultimate stage at the site. These inundation maps are kind of a proxy for the dam break flow across the site. They are a very transient picture of what could happen. We have run these for 40-hour flow durations and clearly for some of the bigger flows on some of the small channels off to the side, the flow just gets stranded even after 40 hours. It never fully propagates through the pathway that it could take and if you have a peak flow that has a short duration peak discharge then it is not going to propagate through the system. So what we are trying to lay out here is a broader framework where we could incorporate lots of different kinds of scenarios and operational issue and issues of the diversion dam. What are the factors that we are going to have to work into that? They start with some kind of frequency depiction, we have hydrograph issues, and we were at that time trying to figure the effects of infiltration, effects of culverts and then we can compute all the late stage discharge AEP for the sites of interest.

Bob asked why AEP sums of 1…

Dan stated that you would have to choose some confidence limits; you would have to pick how many points you are going to use.

Dr. Lall said this is for example for the 100-year flood the AEP is .01 but there is a….estimate of value

Dan added so it is a probabilistic estimate of what the AEP actually is.

Dean continued so in terms of this and trying to summarize where we sort of think we are at this point. What we would like to say is we have a pretty good handle on the flood frequency up to a certain point for unregulated flow up to the diversion dam that is probably adequate for making 100 to 500 year estimates; with a notation that those flows are within the diversion dam capacity. The inundation maps provide some kind of conservative proxies for any peak flow scenario. The RICOM results are probably more representative than the TRIMR2D for the site inundation due to the better resolution of the topography in those models. The stage probability curves that we put in here that could be modified to incorporate scenarios for diversion dam flow as they are described. There is some conservatism incorporated in this that the modeling was done for steady state durations of about 40 hours and maximum instantaneous stage. In the framework of possible flow hydrographs, these are going to boost the stage relative to what will probably happen. TRIMR2D are based on 20-foot cells. RICOM results resolve topography better. We think there is some remaining conservatism in that, but really don't know how much. The other side of that is the RICOM and TRIM results start to converge as we get to larger flows. So that topographic resolution is relatively less important for the really big floods. What do we see in this framework that is not contributing very much to the uncertainty and to the over all results. That is for most scenarios the culverts across the site really don't change things. We have investigated that uncertainty and we look at some of the stage probability curves and some of the maps but there is not a huge impact. Likewise within the bounds of the infiltration models we put up so far it doesn't seem like that is having a huge impact either. So unless someone wants to say we should be using very much bigger infiltration numbers the difference of no infiltration versus some infiltration isn't going to have a huge impact on the stage.

Dan added if you use Fritz's estimates, most of the time you have no flow.

Dean commented we can make it all go away if we go too far.

Bob said it should be included somewhere though so that we can get an idea.

Dean said that statement is kind of in there. In one sense, we can make a case of within the data that is available to calibrate to our high-end infiltration numbers sort of match up with the gauge data. In fact it is discussed in there; if you use the values that Fritz uses, you cannot match the gauge data for the channel infiltration.

Bob added but on the other hand as we talk a little bit about it when you get outside the channels the infiltration was maybe more valid.

Dan said Just to mention one aspect of that. We still have flow in channel by INTEC that is really spillage out of the right or south side of the channel. That is a place where whatever is in the channel has some impact on INTEC. So unless you can lay claim to substantially different infiltration in the channel.

Dean explained where you are going to get much higher infiltration, are the areas where there is basalt under the gravel. That would be anywhere where the flow paths cross the Pleistocene surfaces around INTEC and so on. You might potentially lose a lot of the pathway that goes north because north of the cannel, up into the basalt terrain, there is a lot of basalt and likewise when you start putting water in the spreading areas.

Bob said that near INTEC is the big pit in the ground next to gauge station where the water goes straight down there.

Dean said we are assuming alluvial channels infiltration values all through that area.

Bob wondered if the gauge related infiltration data include that gauge. Dean replied that constraint comes from the differencing between the gauges downstream of the diversion dam versus that gauge. That is what we are matching; observations from flows of 5 – 12 cubic meters.

Bob stated that he thought it was worth pointing out that there is no conservatism is a legitimate the statement from Fritz.

Dan replied that Fritz also said we should calibrate this. Dean said that they did that and got some buy in back from Fritz. Fritz said go ahead and calibrate it; then we talked back and forth. Dean reported that Fritz said he thought that seemed reasonable.

Dan explained an area that is dry should have whatever infiltration capacity it can. We calibrated in the channel area, admittedly the highest infiltration was in the early part of the flow but it wasn't completely dry and didn't get inundated. In that sense it is conservative because we've never run tests of opening the gates at the diversion and immediately looking at a stage of 10 cms coming down and looking at the loss. It was always some days of high flow.

Dean said that is where the whole element of conservatism of long duration steady state flows.

Bob reiterated that was one of the examples of conservatism that bounds a lot of the other uncertainties that we talk about.

Dan added ideally that work like Fritz's would have given us something we could have directly put into the framework.

Dean said the other side of that staying in practice. In running these kinds of inundation models including issues of infiltration is well beyond sort of a normal state of what people are used to seeing. The standard analysis if you decide that you want to rely on very high infiltration parameter values as a basis for a scenario we are going to have to go to greater lengths to justify those numbers.

Dean stated those were some of the INTEC issues. I think we talked about this yesterday. Even for some of the very large flows, most of the flows through there are going to be fairly distributed and shallow. Flow through the facility is going to be channeled by the interior roads and ditches within the facility. Those characteristics within the boundaries of the facility; we have some detail in the present modeling but we have to be very cautious of about the fidelity of details that are shown in our modeling in terms of going to far with those results. The distribution of flow around the site is controlled by small-scale topography features much of which is transient. For example, Lincoln Avenue elevation and characteristics. Our model basically is the elevation and profile shown on the aerial photographs from 1993. It has not been updated for the repaving that has occurred or any kind of changes for the roadbed. That small-scale 2-3-4-5-6 inch increments are turning out to be fairly significant in where things go and how they get from A to B.

Dan added that right now water flows near Lincoln Ave bridge is going south across Lincoln then going north across Lincoln indicating that the channel was bulldozed and tends to point a little bit toward the south bank so there might be a little super elevation there. The thing that is really sensitive to where the roadbed is in our model. If the roadbed were six inches higher on the south bank then the flow might go to the north bank.

Dr. Lall asked a process question. As one of the modes of failure is it possible to speculate on or find potential locations of breaching that would have an impact. You are not saying that there is a breach there but if there were a breach at such and such location then that would potentially have a big impact.

Dean stated then we have details of the ditches at the facilities and discussed yesterday about assumption of blockage at Lincoln and INEEL railroad those are all going to have strong impact on how you characterize stage discharge.

For TRA, the most extreme scenarios are influenced by the Pioneer diversion canals. The big canal on the north side of the river. The control for flow in and around that is located upstream of Highway 26 in paleoflood study reach. There are several intermediate sites where mitigation is pretty straightforward and we can identify those. The ponds at TRA are more susceptible to more frequent events. Most of the buildings at the facility are up above the flow levels that are resulting from flows of the Big Lost River Flood Plain channels.

In the big scheme of things, going back to our conceptual framework what's really going to contribute to the stage probability estimates, obviously, the input flood frequency upstream of the diversion dam. Then it boils down to how you deal with scenarios around the diversion dam.

The present diversion capacity is based on the flood frequency is potentially capable of full control flows well beyond the 100 and 500 year frequency analysis. In one sense if we develop a probability scenario that is contingent upon the diversion dam and up to these levels the stage probability is depicted by the inundation associated with the diversion dam capacity flows downstream of the diversion. Even with a Mackay dam failure or upstream dam failure, there is no doubt going to be much flow continuing into the spreading area and the fraction that continues downstream to the facilities through a breach of the diversion dam is clearly much less than 100 percent of what you input as flow of the Big Lost River. These are scenarios that need to be developed down the road in terms of stage probability contributions. Finally, site specific details at INTEC and TRA with regards to how some of this might be managed and mitigated.

Bob stated that this is where we needed to go and hopefully this meeting will…he wants a final product at this stage.

Dan commented that he wanted to walk through the two facilities. For INTEC, you are targeting to get 1 in 10,000 exposure metric Bob agreed. Dan continued the safe capacity at the diversion is basically…if you did not divert anything…you could accommodate….you are approaching 190 to 200 capacity at INTEC. You have a hazard of what you allow through basically for the situation.

Bob stated that the load to the diversion dam could be 7300 cfs and 1200 cfs would get shuffled for to the spreading areas so what you would get is 6200 makes it to INTEC.

Dan replied he was going to start with what he all ready had thought through so 7300. We looked at 1 in 500….what's our estimate of the max….that's we are very confident of our estimate of the 1 in 500 flow. It is going to be 150 or less. So we are saying at 1 in 500 the diversion structure….you should have high confidence that it will perform. That means the flow to INTEC is whatever you allow through the diversion structure. There is a 90 percent probability of performance for the diversion structure. You can go out at a high degree of confidence and have a flow less than capacity of the diversion.

Bob commented that is at 1 in 10,000. He continued that the latest estimate of what the culverts would pass is 1200 cfs.

Dan said we are assuming that you will operate the diversion structure and that would be not to allow the flow to exceed….or not to divert more that a certain flow.

Dean added that this is where the 90 percent reliability issue comes in. The statement of reliability of the diversion dam is such that you have a 10 percent chance that the culverts plug or something goes haywire. Dan and I were talking about this in the car this morning as we were driving over here and reflecting on it. When we think about how do the geotech guys at Reclaimation normally assess, for better or worse, the performance of dams and everything and hydraulic structures. When they reach a judgment in the performance stuff that reliability of something is 90 percent. That reflects their opinion that something is really haywire with that structure. Their are feeling not to good about the way that thing behaves.

Dan commented in your judgment you have to assign a probability of .1 something will fail. That generally is what is going to happen because those are static loads. A failure probability of .1 is very significant for an engineering structure

Dean said if you were to have Clint and everybody do a risk analysis of the diversion dam. He said it is good for 6000 but to have them come out of that with a 10 percent probability of failure after making that statement that they think it is good; that would be astounding...is my view.

Bob stated there is a factor of safety of 1.9. Dean replied that is what I mean. It means that they are saying that means they have really high confidence that it's...they are comfortable with things at that level. That's inconsistent with a 90 percent...or 10 percent failure probability.

Bob asked how you convert that factor of safety to a failure probability. Dean responded that it becomes subjective judgment. Dean continued in our dealing with these guys, high factors of safety approaching 1.5 or 2 never translate into failure probabilities of 10 percent.

Dr. Jorde commented it depends what they mean what factor of safety.

Bob said they computed a dynamic...a total load on the face of the diversion dam based on shear stress and water velocity as well as potential for infiltration and demand over capacity.

Dr. Lall added average distance divided by average load. So

Dr. Jorde added...of the downstream phase. This would be an item thing; looking at this thing and how steep the slopes are by the time it is completely...by the time there is seepage flow through the dam; you will have seepage flow leaving the downstream side. The phreatic surface will touch the downstream side. So I would never believe that this dam has a factor of safety of 1.9. That is why I'm asking what they calculated.

Bob commented that the other caveat there is that is based on two standard penetration tests near the culverts. They did install a tensiometer so they do know the infiltration rates. They did have large flows in 1997.

Dr. Jorde asked how they got that because they can't .....if this is a homogeneous dam, which I would grant from looking at it, looking at the length of it, and it has to be built really cheap. I would assume this is a homogeneous dam that doesn't have a core or anything; I wouldn't think that this was a factor. I am not going to question it. I just don't think this thing has a factor of safety. Maybe for a very short-term flood or seepage flow before the thing is soaked.

Bob replied that we all recognized that. That is one of the functions of this report is to give a set of loads to assess the diversion dam. Presumably my management will concur in a rigorous geotechnical assessment.

Dean added he thought that was one of the major things they highlighted that really the stage probability estimates at the facilities are entirely dependant upon how you characterize the performance of the diversion dam.

Bob said management likewise could say well look at how shallow all these flows are; we don't even care.

Dean agreed.

Bob continued that if he was honest with them he would have to say yeah look at how shallow these flows are. But it just looks bad when you are planning for an infrastructure potential for a new operating reactor, for example, you really don't want to have one of these big blue swaths in a place where you might build the reactor.

Dan said you have the same kind of thing at TRA. TRA's main facilities don't get inundated until you reach…if you want to get to 100,000 at TRA you need to look at the 1 in 10,000 load and see how that would partition. The 1 in 10,000 load exceeds your…how much of that flow partition into flow into the spreading areas and how much partition into flow onto the site. It's less than say 250 cms partitions onto the site then you don't have a flow path to exercise that diversion bypass and dump flow to TRA. So if you can accommodate, some how, through the 1 in 10,000 loads you have a confidence that the flows will partition such that flows less than 250 you don't really have an inundation hazard for the primary TRA facility. That's not a classic….

Dean said that there are two big differences you have to consider for the diversion dam. One is when we talk about the flood frequency and tie of the diversion dam being there or not being there what we are talking about is a breach of the diversion dam at the location of the culverts. It could happen anywhere from the head of the diversion channel and the culverts to the north. But regardless of where it happens in there, you are never going to get 100 percent of the flow going down the Big Lost River. So there is some partitioning that goes on there and that part of it you can tie into the frequency. You could have a probability of a failure associated with the flood frequency. For failures of the diversion dam associated with water in the spreading area which would take a whole different set of flow paths; the probability that you have a certain stage in the spreading area, which is a couple of steps removed from the flood frequency and likewise the flows be occurring from that are starting from differences places than the Big Lost River channel. Those things do not relate to these flow maps at all.

Bob stated he wanted to give a flavor of the things he will have to worry about eventually.

Dean replied that he would need to be real clear with people in terms of distinguishing those two types of scenarios and the probabilities associated with them.

Bob commented that what we have seen and what we know about the amount and persistence of flow required to do that is almost impossible. I think it was Kerrigan's 1973 flood analysis that there was 35, 800 feet backed up behind the diversion dam.

Dean answered that was a scenario that could be evaluated.

Bob then commented that for our purposes it's a distraction. But your arguments about the water and where it's going that's worth noting.

Dan explained another logic tree branch that you have to consider for INTEC are the 1 in 10,000 flow will partition and some part of the flow will go in the Big Lost River and spreading area.

Dean stated it would be hard to end up at RWMC.

Dan said flows would not end up at RWMC through the Big Lost River channels.

Dean said that would be a failure…the only way….if you have a failure of the training embankment along the diversion dam channel just south of the culverts. Then you are putting the water in a position that it could flow down the gradient of the fan and with enough duration and enough time, you could eventually overtop the sill. Our modeling says....that doesn't really get going until 700 cubic meters. You got to get way out there and that's partly a consequence of the spreading thing and the way we start the flows.

Dan added with your breach scenario in there you try to take care of your worst-case scenarios you will route a bunch water onto the…. That is one of the avenues that becomes exploited here only with the larger flows. Of course if you set some kind of breaching area where you force water to route on the structure where part of it gets diverted by the railroad embankment, which is a flow path to the south.

Bob commented that brought up an interesting question. We are assuming that diversion dam doesn't exist, do we also assume that railroad embankment doesn't exist. Dean replied yes.

Dan responded that it does exists and in fact the water downstream. In fact water in the very large flood they backwatered onto that.

Bob clarified so you are putting water downstream of the railroad and it backs up onto it.

Dr. Lall stated that the very last thing they saw yesterday which was a funny little channel that seems to be making a beeline for TRA.

Dean clarified the canal, the original canal.

Dr. Lall then went on to ask if there is any mechanism by which the water just cuts into that from the Big Lost faster than through a breach scenario.

Dean said no because to get into that you have to get water…the pathway to that is through the high flows south of the diversion dam or north of the diversion.

Bob said if we block Highway 20/26 maybe we can use that channel because its lower it could go north…

Dean agreed that it maybe there is a variance. Bob and Dean agreed that it could be run north of the highway.

Dr. Lall commented that his reaction yesterday was we should get the water all going out onto the plain. Where infiltration loses that occur through it is not accounted for…perhaps the big leader could be…in any case, you would have a swallow depth of flow every where and low stream power every where so that is not to exciting but if the diversion channel were to fill and keep breaching. That could be..

Dan said it doesn't even have to breach because it becomes….the north bank and becomes the channel.

Dean said the best scenario is if you get water into it and they have actually breached the south side of that thing in many places and the water will leave that. But in our scenario that has come out of this is the water gets on the north side of it and they haven't breached both sides. They didn't breach the other than….

Dan said they did in a couple of places but it still the water still has to flow over a hump to get through those breaches and continue down. As we were driving out, there is that last little diversion that went northeast of TRA and perhaps relieves some of the flow that is going along the north bank and directs it toward the northwest corner of TRA.

Dean said that it captures local runoff diverted away from facility and would in effect be doing that up to a point. So it would be a volume dependent issue.

Dr. Jorde asked what is the purpose of the discussion right now. It sounds like basically we all agree that this whole situation is fairly safe. You would have to do something very strange.

Bob replied that we have to look at performance of the key structures.

Dr. Lall said I'm at the same spot; this looks like boring. We could get some excitement we could look at some sort of cascade type of failure. The other thing yesterday was the ice-jamming scenario. Then another possibility here would be, it is completely outside of the domain of your analysis, was you get a cloud burst flood that focuses just right on the site. So why are we wasting time on the Big Lost.

Dr. Jorde asked what are we trying to figure out now because I feel if we do not present that the diversion dam does not exist and if we look at the situation as it is now...it fails and if not you will not ever get that entire flow towards the INEEL even if you remove the diversion dam, which will not happen. So the situation is very safe, so what are we trying to achieve with this discussion.

Bob said what he needs from the reviewers is an assessment that we have captured the uncertainty for the Big Lost River Flood Hazard Study. We are assuming the diversion dam does not exist.

Dr. Jorde replied which is a very funny way of capturing the uncertainties. If you impose a big error and then after you impose this big error you start talking about the uncertainties, the uncertainties are many dimensions bigger.

Bob said let discuss a couple of issues here. One is because the facilities safety. To mitigate the natural hazards for these facilities is a much more dimensional problem than just the flow of the Big Lost River. However a fundamental part of that is what do the flood loads do to the Big Lost River itself. Our primary focus is to determine have we captured in the probabilistic sense the flow magnitudes and their uncertainties for the natural Big Lost River system on the INEEL..unregulated. That should be our primary focus. Now a bit unfortunately we sort of diverted into the discussion of the diversion dam but for regulatory purposes we have to assume the diversion dam does not exist because it is not certified as a flood control structure. The bad news is we know its there and is going to have an impact. However as a result of this study we will have the loads we can impose on the Big Lost River to make an assessment whether it serves as a flood control structure. Once we have that in this rigorous and defensible framework...

remember my slide about risks and defensibility and how they have to be justified in the total risk framework? We do all this work so if I go to my management or the taxpayer, or the public I can say we know what the loads are on this diversion dam and now we know it is or is not worth spending x amount of dollars to fix it. That is my job to go to management and say we have a certainty that procedurally and technically we have a process and product that supports these decisions. That lets me assure my management and the public that yes we have followed a rigorous process and yes the numbers that have come out of this with their associated uncertainties are defensible and supportable given the process and data that we followed to this point. Don't worry about facilities risk and mitigation, the diversion dam and all that; that is not our job. Explicitly not our job but implicitly there are things we have to worry about. If I were you guys, I would remember that we are talking about risks but in reality there is a diversion dam. If we have made some horrendous mistake that nobody can think of right now, its probably not as critical as how the Apollo 13 mission made a tiny mistake and everyone could die. That's not the case here. It's a very awkward situation and as I said early in my first talk in a more rational world we would have accepted the results of the 1999 report, and said this is good, we are done, let's move on. However, because of some politics and agendas and egos, people blew up some of these uncertainties that were actually were very insignificant and that won't have much impact in the long run and we are forced to do this. It's the same thing happening again, I am calling on you and Dr. Lall to assert that we have captured all of the significant uncertainties.

Dean stated that the irony of all that is that we did find a big problem. The topography! We have the same geologic framework now that we had in 1999 but we have more detail as a result of the trenching. The fundamental conclusions there i.e., is we have this 400-year-old deposit that we can use as a bound and we have the Pleistocene deposits that we can use as a bound, that is unchanged. But what we have is very different discharge numbers that are directly attributable to the change and the flaws we have uncovered in the original topographic data. We didn't recognize in 1999.

Bob said that it is not all bad. The flaws that were discovered were not due to the insignificant uncertainties people were harping on. Only indirectly. Those flaws would have been there regardless of the aleatory uncertainty. That term meaning uncertainty of your universal model.

Dean added the other one was we had not closed the quality assurance loop on that.

Dr. Lall asked if he could try something with everyone and see how they would react. Yes, you are making up a PMF, there is not doubt about that. Everybody does it just depends on how they look at it. The key thing that you are bringing to the table is that you have a procedure by which you say that in terms chronology we can put some bounds on stage and you try to integrate those stages with a flow model, so that there is a pseudo flow number, one could argue that this is crap because who knows if your hydraulics are right or not. There is no verification of those things and I understand this. The information does go together because that stage corresponds to this fake flow model that we have manufactured. Since that has been marched forward you are maintaining consistency with that particular assumption all the way through and that gives you the inundation maps. So I think that part is reasonably well documented, now it is still somewhat of a speculation as to have you captured the uncertainty across the flow to the stage frequency part at the points of impact—I don't think so. Could one do it, is it worth the investment in doing it. The answer to the first part is I don't think you could. Answer to the second part is the amount of resources you have to spend to prove that you could not do it. That is where I am with this.

Bob stated that is what we need to know and again that is my job and my management's job to take your assessment of that and say we do or don't deal with it. My recommendation to my management is we have been through enough review, we know enough of this stuff regardless the remaining uncertainty…we have done enough. Given the standard practice and going beyond that, which we have done here, we just can't do any more.

Dr. Lall said lets try one more, which is flow frequency. They are trying to quantify the uncertainties. The depth flow response relationship, it would be very difficult to say whether or not uncertainty could be quantified at all because the number of parameters you could play with…

Dan interjected giving how you scale. These models are millions of cells.

Dr. Lall said so what are you going to gain by coming back and saying what is the bound of that. So you look at it qualitatively and….once your range this is just not going to support depth and if its not going to support depth you have got tremendous….so you have no depth and you have no power. The place I am stuck is there are places there were you could build up depth. Just because there are obstructions everywhere, then you have breaches, which is the point he was trying to make. If it breaches that is not represented in the model at all. So you could speculate all you want on where it would breach but that's what I am saying you could come back and say which breaches would likely bother you and maybe some documentation could be done on that.

Dan explained that is what you are recommending to identify potential key points. We can identify points of value and run blocking Highway 20/26. We could identify based on those at this point there might be some sensitivity of inundation at TRA. We could identify some potential branch points but would have to keep them very small to execute the calculations.

Dan then reviewed inundation maps and said for the most part you only see two (colors) because, except for very small flows, there was no effect of the culverts versus....once the flow reached a certain discharge there is no effect to the culverts. Right here shows the effects again we either had full culverts or partial culverts. In the case of infiltration all of the culverts operating or no infiltration and partial culverts once again there is not much effect of the culverts. So now we are isolating the effects of infiltration in flow models. Not flow models first order, first order we are looking at the difference of the grid resolution on the estimated inundation. So these thick curves that have a lower estimated inundation of these flow models, which uses the five-foot cells in the channels. So basically we've used a resolution on the test grid here to establish six feet would be adequate to produce.... We used 20 foot cells in TRIM...we don't really resolve some of the channels well enough and so we break out of channels at smaller discharges so we have more inundation. This is one of the most striking places. This is where we have really strong sensitivity and having infiltration produced the lower stage here. At 63 there is just water spilled all over and the infiltration isn't having a large an effect. This is the kind of thing that TRA is sensitive to the grid resolution...this just shows that no matter what you do using RICOM with or without infiltration.... At INTEC, again, you see the effect of the grid resolution where the thin lines at higher estimates of inundation at smaller discharges effected with 20 foot cells we don't have the volume fully represented in the channels and so we spill out of the channels at smaller flows. But once its inundated, it's inundated...completely blocked. That sensitivity is larger than the grid resolution sensitivity and comparable in effect. So in one sense once I allow for all the volume that is available in those channels, there is a lot more conveyance in those channels and when it spills out it finds more channels. It doesn't inundate the facilities quite as fast as if I do the same thing but block the culverts. But it does show that until you saturate the system, which you do with larger flows, you will get some substantially differences in the estimated depth. Once you do go to the very large flows the whole system gets inundated.

Some interesting things here is that it found more resolution in routing more water into this part of INTEC with the higher resolution so there is something different about the details of how routing onto the site between these two resolutions of the grid. And it may not always be intuitive, oh, I have higher resolution so there is more channel available so it will never get high. Well in this case it is just the opposite at higher flows.

Dean interjected 'cause we found a channel.

Dan stated we found a channel....the difference between 20 foot flood grid is these secondary channels at 5 foot we've resolved the channels all the way down. At 20 feet it might hit a hump or something and they're blocked. There is still impact going from 20-foot to 10-foot cells. Then using the high-resolution grids and blocking Lincoln versus Lincoln being open allowing the full conveyance. At 25 there is no difference but what it does it very quickly spills to what you would say is a flow of 106. Now with Lincoln blocked the 63 inundations about like 106. They do converge to similar behaviors at large flows.

Dr. Lall asked a question he had from yesterday. If you look at the flow of the culverts basically what you end up with is flows greater than 106 plotted.

Dan said the flows were greater than actually 50. In essence this is more realistic even at 25. Even at its fullest throttle it doesn't have that much impact on the inundation.

Dean commented that in one sense the actual pathway…is this correct if we had more data on this spot the most probable pathway would be following the black curve for a while and then it would jump and we would find the transition point. If we had it blocked and the inundation would be probable and all of a sudden…

Dan added that it has diminished conveyance and then it would shoot over here.

Dan said at 25 – 63 and you maintain a flow below a certain value the behavior of the Lincoln culvert is not really relevant to the site

Bob observed the issue he would like the reviewers to think about is if you were to meet or exceed FEMA requirement that is going to be the legal issue that is going to pop up. We are supposed to have 100- and 500-year flood analysis. In RCRA space that is equivalent to FEMA. You can see that we have exceeded FEMA requirements by order of magnitude in my opinion are not weighted very heavily in these discussions.

Dr. Lall said that because of the analysis FEMA requires. Back to your running question has HEC-2 dash 46M or something been used because it was certified….I don't know the answer, I don't know how to answer that.

A discussion about FEMA codes then took place with the group actively participating.

Dan said their position is that the code tested probably beyond the scope of what FEMA….

Bob said the equivalence between the codes that are accepted versus….Dean interjected we would in comparable situation would use….Dan said 21 was recent.

Bob stated that a long time ago in general FEMA discouraged the use of 2-D. You had to prove the 1-D code did not work.

Chris said that this is completely out of the realm of using a 1-D code.

Dean added that has been written into many reports not just by us. All of the USGS reports going back about 10 years have said the site inundation is not a 1-D problem. That goes back to Berenbrook's original 100-year flood plain analysis. The final sentence of the conclusion was there needs to be a 2-D analysis of this because 1-D doesn't capture it.

Dr. Lall said he was uncomfortable with writing a sentence saying that what models have been used here are equivalent or superior to. I just couldn't do that it is too bizarre.

Dan said there might be people who could do that but

Chris added that is part of the problem as Dean was saying they do say this site is completely irrelevant to a 1-D code. Now having said that, here is our 1-D code results. Yeah, they did it but why is kind of beyond anybody because they said right up front this is totally irrelevant.

Dean said they did look into it quite a bit because of the regulatory issue. What it would take to get this stuff certified as a FEMA approved code. What they found is that they have a process and essentially the Secretary of Energy and the Secretary of Interior would have to sign off on it. Dean said he didn't think that was going to happen any time soon.

Dan added that it is remarkable that there was no technical specifications.

Dean informed the group that they could go to the FEMA web site and check it out if they wanted to. He said they could look into FEMA 37 and flood plain mapping. Eventually the web site would get into discussing requirements for certification, of modeling codes and so on. There are lists of their approved codes because it's a big issue with regards to their whole flood plain mapping process which is all done under federal contracts by subcontractors. They have to lay all that out.

Bob said so what you are telling me is that if a lawyer asks me the question I just have to say no we are not legally...

Dan interjected that is what Appendix C Part 2 Section 1 is all about. That is a real life demonstration for capabilities.

Dean said that is what we gather with some of the stuff from Nic. All of the testing he asked us to do that is what all of that is aimed at documenting. You know, meet or exceed some high standards on the technical side.

Dr. Jorde added that in general the FEMA codes...it's a FEMA thing and a lot of people who have nothing to do with FEMA are bound by these things. Models accepted by FEMA are crude and very simple.

Bob stated to give you a flavor of what I'm going to face when the lawyer asks me...

Dan said they have been pursuing the verification for a year. Now we have FEMA approved and now we need to stop. He has a proposal in to actually....sedimentology program in Europe to do additional research...in using 2-D codes.

Dr. Lall asked "and you said Appendix C had some comparisons." He suggested having Nic write a note saying based on the comparisons in Appendix C...he feels there has been a demonstration of equivalence.

Dan said that is why Nic provided those comments.

Bob asked if they had ever done a formal comment response.

Dan responded yes, that's Appendix C Part B Section 2

Dr. Jorde said that we might ask if the FEMA criteria are really very much below what this is all about. FEMA is not applicable to the level we are discussing here.

Bob added that a lot of this depended on his management. If they stand behind this it will standup to legal scrutiny.

Dean added Bob that's a really important issue. It would be worth your while to go back and look at the FEMA certification requirements, because the process we are doing right now is essentially what FEMA says you need to do to certify a code. The whole QA process with Nic and everything. The final step in the process of certification is an agency saying we have done this and here it is. Then it gets certified. That is it.

Paul stated that maybe they should take that tact first instead of this is defensible and once that is done.

Dean stated yeah, we conducted all the reviews that are necessary for certification.

Bob said the challenge to him is to avoid the "yeah but" thing…He stated he is perfectly comfortable with what has been done.

Bob then asked the reviewers at this point in the process what else do we need to provide you?

Dr. Lall said this is better education than he has had before. It would be the reading that will make him ask questions.

Dr. Jorde stated you will look at the stream power thing that we discussed and make an argument why from this point on you don't want to go any further.

Bob asked Paul to get a summary of observations on this process or both; then Glenn's as an independent observer. Bob said liked to take advantage of the fact Paul and Glenn could put their two cents in about what they saw that was valuable or useless—important, not important—probably an independent assessment that the reviewers were well briefed. That nothing was covered up and everything was laid out on the table. Any technical insights and comments Glenn would have would include that he had been to the site several times and that he and Dean had hashed and rehashed field issues. Conformation that the field review was thoroughly addressed pertinent issues.

Bob reviewed timing and products. As I stated yesterday, the "bins" in terms of review comments should fall into:

- ✓ The primary issue is: have we captured the uncertainty well enough to march ahead with these numbers?
- ✓ Are the realistic estimates and flow bounded reasonableness? Yes, they are bounded well but given what we've exposed you to in terms of future needs what can be improved.
- ✓ Other things as a scientist/researcher. We really want to pursue some research that has a chance of paying off and what would that be.

The uncertainties are sort of narrowed down and we know that the model framework and we know that approaches we are using are reasonable it is just a matter of the parameterization and bounds have been captured as thoroughly and rigorously as they could have. There are probably other uncertainties and conservatism that we have looked at but that bound the universe of other issues.

Dr. Jorde asked what kind of research should be done. Are you talking about scientific type of research or site related?

Bob answered he would leave that to Dr. Jorde and Dr. Lall. He said as scientist we can always have a little fun tinkering around with what should or could be done, especially having been exposed to this amount of data. He thought as scientist the group was creative and interested enough that it's an opportunity to identify some areas. With the understanding it doesn't in the near term effect uncertainties and the results of the outcome we have so far but if DOE wanted to; these are some of the things we could look at. One of his personal interests is it possible to use some kind of remote sensing scanning to determine grain size distribution for flows.

Chris said that research on the site to come up with real infiltration numbers beyond an academic thing.

Dr. Jorde said the effect of everything seems to be completely disappearing if you just take the diversion dam out. That would be my immediate recommendations forget about all of these little things and take into account the diversion dam.

Bob stated we all know that but the regulatory purposes we can't assume the diversion dam exists and even if we do, we need do know the loads on it.

Paul asked if the previous peer reviewer comments were included in any of the documentation. Chris will make copies for Glenn and Paul. There are "along the way so and so said this" and do you have the response or is the response the new report.

Bob said there are several layers of comments and responses.

Dean stated that there are sets of comment and response documents that go back to the 1999 report. Then we have sets of comments we received as a result of the review meeting we have had along the way over the last three years. Kyle's stuff, Kevin Coppersmith, and Nic's stuff. There is formal comment response stuff in Appendix C for Nic's stuff. At this point, we haven't written comment response stuff for everybody else's comments because most of those comments were directed along the lines of things we have to do in the process building the report and my response to almost everything there is it has been incorporated into the report as part of the participatory process. Nic's comments were for quality assurance and very specific.

Bob asked if the information had been circulated to those guys.

Dr. Lall inquired about one thing he still was unsure of. He said you have maps of surface exposure, then you generate flows and you compute wetness or whether or not that thing touches. Do you do any evaluation of whether the things you have mapped there because it will be for surfaces that are stable for certain age versus not correspond to the flow that has been generated with that?

Dean stated yes and reviewed the map.

Dr. Lall asked do you have estimates like a score over the whole domain..that could be categorized as such and such.

Dean replied yes, that is part of the summation chart.

Dean said that in section 2.6.5 he sort of says here is the applicability of the various areas to these judgments and is trying to summarize in the tables. It is in the draft summary documents.

Dr. Lall said that he was mentally thinking of an approach, which these uncertainties are associated with those flows. There is a deterministic stage associated with each of those flows along the whole section. So now if I was playing the game described the first day, which is, he can independently tell you that this is a good place to go and check whether of not something has been cut. If I had a probabilistic scoring function associated with this and I mapped that, I could get a cross value and sort of check. I could say you go out there and now you dig there and you say yes. The probability at which you predicted something would have happened at that flow there and that showed up. Then I looked at that location and yes indeed it matched up. Something like that is what I had in my mind set.

Dean stated this is much the same as what we were talking about with ranking of the various sites. With the site ranking chart or plot that we had what we are really trying to do, the only points we plot out there are the ones that apply to a specific bound.

Dr. Lall said he had put that down, as a research needs question for more work. This would be a great application of the depth of flow and exposure relationships for mutually consistent in locations which would sample at the moment. Then you get closer to the sites we are questioning where the landscape doesn't have control. I don't know if anything could be said about that.

Dean replied no, because you don't have the detailed geomorphic mapping to support that. I think we have to be careful with supporting the paleoflood conclusions and the consistency there. Within the study reach, we have chosen that study reach because it has certain geologic, geomorphic characteristics—i.e., stable channel. When we move downstream, we move into alluvial system and the characteristics in that system are very different.

Dr. Lall observed that this is where he could say he has questioned every aspect of the characteristic modeling in that section.

Dean said absolutely. But we are not using that hydraulic modeling to establish the flood frequency. We are now in the....hydraulic modeling in those alluvial reaches is being done strictly in a context of inundation. The question we have to ask is are we doing that in the standard of normal practice and I think the answer is yes. Regardless of the flow modeling we would use I don't know anybody who is doing inundation mapping that is not using fixed bed models.

He continued we are in full compliance with every kind of engineering standards that has been written for that stuff. Dr. Jorde, in answer to...thinking back as to why we didn't do the stream power maps that is why we kind of stayed away from doing that for the downstream stuff. Although we have thought about it a lot, we decided that we were just portraying inundation here and we don't want to have anybody pulling any other significance beyond that, but at the same time I think you are absolutely right we need to portray that as here are the sites that are most sensitive to channel instability. We need to highlight those.

Dr. Jorde said one thing is and I haven't read the USGS final report and I do need to look at that and oh they didn't even address that. Stream power would be one of those things that...yes we have thought about that and looked at it and decided it is not relevant or we feel...

Bob injected or it is another branch on the model tree like run on/runoff is, ice jamming, and dam failures. Do you still feel strongly about the climate change? How long did we string out this study – about 10 years?

Paul stated all of these studies assume the climate was the same and flood frequency....

Dr. Lall said we will have a Lake Bonneville here with a climate change scenario.

Dean said you must be careful with climate variance. If you go the wrong way you have the return of Lake Terreton.

Bob said he was thinking about Yucca Mountain where those guys arbitrarily increased rainfall by factor of three without any evidence for it and when you look at the evidence they said oh! That is silly the last three million years since the Sierra Nevadas popped up there has been absolutely no reason to believe there have been increases.

Bob reviewed QA logistics and said sooner or later we should get meeting minutes and distribute those and let people read them and say this is what I said and this is what has been left out and it could be important.

He then asked Dr. Jorde and Dr. Lall if they could have their comments in 10 working days. Dr. Jorde replied that they were moving into a new building but he wanted to get it done quickly but there is a little bit of uncertainty.

Bob asked Dean and Dan if they could have their assessment in 20 working days and respond to Dr. Jorde and Dr. Lall. He stated that very soon after that they would say yes or no. Things have been accommodated or tell him what is required to deal. Either way you'll communicate with these guys.

Dean said in the context of producing finals what it is going to be is we agree that yes we will address this within this framework. The stream power maps and stuff that, we are not going to get that done in the 10-day period.

Bob said there are no constraints in regard to communication. If there is anything that is going to impact schedule or cost, he needs to know. I have committed to my management that by the middle of November we will have this thing wrapped up.

Dean said that wrapping up the peer review would be possible by November. The other thing is the issue with Vick and he would like to get some input from him. Pending discussion with Vick, that arrangements be made to make a trip down there. He also said that Vick had not seen the trenches. Vick was last at INEEL in 1999.

Bob said that he wouldn't see the trenches and Dr. Lall interjected that the pictures were quite good and someone who was a geologist would be able to tell a lot from them.

Dean said it is important to see the real thing but it is unfortunate that Vick didn't get to come but it was just one of those things.

Bob stated he is under pressure to fill in the trenches. On the other hand, if you talk to Vick and he says he absolutely has to see those trenches then I won't do it. He wouldn't be able to see the trenches right now it would be a safety issue.

Dean replied that Vick is probably not walking much as it is an Achilles surgery.

Bob stated they would have to wait until spring to see the trenches.

Paul asked about Bob Jarrett's comments from the earlier stage.

Dean said Bob has never written any formal comments but he did review the 353 paper. The paper is a summary of the 1999 report and he has been on field trips. Dean informed the group his comments of the morning were one of those standing out in the field kind of comments. We have the USGS comments and a lot of those things have come in under the umbrella of the NPH concerns. NPH is Natural Phenomenon Hazards.

Bob said those are all aleatory type of discussions like why are you using geomorphology when you have 60 years of stream data or nobody has ever done this before you can't do this.

Bob stated that Bob Jarrett stood out in the middle of the saddle and said the flow obviously came through here you just don't see it. Even though you see all of the micro mounds right in the path of where he thinks the flow is. There is a small but significant chance that those micro mounds don't have any thing to do with para-glacial conditions 8000 or 9000 years ago. This is an example of the very small uncertainties that we have had to treat as significant.

Dean said the way they had to treat the uncertainty with the mounds. I think we can make a good case have originated and been in place as features for over a long timeframe. They have a long history as individual sites. Now as to whether they could be....the surface expressions of those things could be wiped out and then they rebuild in a more dynamic way. That's kind of another question and that's where the real uncertainty in those mounds lie. Let's say after a big fire they just blow away. With all of the vegetation gone, the sand and top blows away in a certain sense. Then as the place re-vegetates they reform, become big again, and become sand traps.

Bob asked if there was evidence of that in the trenches and Dean stated he didn't think so.

Dean said a lot of what they have been through on the paleo-bound side is disproving assertions of how do you know that something can't happen.

Bob commented that it gets back to the whole discussion of resource allocation. Sometimes you have to and this is one of those occasions where you have to nail things down.

Bob said it is pretty rare that you have a multi disciplinary project like this where each of the disciplines and sub disciplines have the rigor attached we have put on these things. The problem is condensing it all for publication. As reviewers you are dumped with an incredible amount of information.

Dean added that the tact they've taken on or are attempting to take is...our intent here is to attempt the make this document, sort of a summary, but you know it is going to be the main stand alone document that people pick up and look at for the results. All of these appendixes with all the plates and everything are where the people will put the details. I am anticipating for file distribution we will print a few of these but everything else will be on CD.

Bob ended by thanking Dean and Dan for their presentation of the multi disciplinary project.

Big Lost River Flood Hazard

November 8, 2004

Conference Call

Idaho Falls, Idaho

Attendees:	Robert Creed	Department of Energy-Idaho
	Chris Martin	S.M. Stoller Corporation
	Dan O' Connell	Bureau of Reclamation
	Dean Ostenaa	Bureau of Reclamation
	Dr. Victor Baker	University of Arizona

Dean stated they, Dr. Baker, Dan, and Dean, were sitting there reviewing a little bit of what we went through in early October. We had started and just gone briefly through the overview presentation and objectives that you guys did and now we are talking about the paleobounds criteria issues a little bit and kind of stepping through that a little bit.

Bob asked if he needed to remind Dr. Baker what the nature of the review is? Did you guys go over that?

Dean replied yeah, we stepped through the slides....

Dr. Baker added it might be good to hear it from you; if we went over the slides but fairly quickly just as a background before we start talking about some of the details.

Bob responded yeah, the first assumption is that we have been doing this for enough years with enough review by qualified individuals such as yourself that....and there is a high probability that there are no serious, outstanding scientific and technical issues that we have been on the path to addressing those and based on participatory review and some other reviews, there is a high probability that we have addressed those. The purpose your participation and Dr. Lall and Dr. Jorde is to validate, if that is the right word, that we have captured the uncertainties adequately such that we can present this as a final flood, Big Lost River flood hazard characterization document to my management. Now, having said that we are all scientists and we are naturally inquisitive and based on our particular perspective we can always find things that need more work and I have encouraged the reviewers as an aside to maybe document some of that stuff but that is a separate question from...given those uncertainties and interests, have the uncertainties that we have presented in this report bounded those issues such that we can certify that yes, this is an adequate characterization of the Big Lost River flood hazard at the INEEL. Is that clear?

Dr. Baker replied oh yeah, I think we have been talking in that framework.

Bob stated good. As I was telling Dean at this point in my career there is no such thing bad news.

Dr. Baker laughed you've seen it all.

Bob responded I've seen it all and some how it will all work out one way or the other. If I have to retire doing Big Lost River flood hazard characterization work, there are worse things.

Dr. Baker said I got updated on the notion that the Mackay Dam failure is a 100 year flood.

Bob replied so you know where we are at here with our status of flood characterization at the INEEL.

Dr. Baker stated yes.

Bob said it is pretty bleak.

Dean added I realize that.....I hadn't really been planning to touch on that this morning but I realized that was probably one issue that was no where stated in our report anywhere that is actually a pretty important issue in the background here and we are doing all this at this point....

Bob interjected for Vic's edification and comfort level we realize and I'm constantly communicating to my management that the Big Lost River flood hazard is just one component of the total flood hazard which includes Mackay Dam, the INEEL Diversion Dam, and some other things. The primary purpose of Dean's report is to help establish the Big Lost River flood load so that we can then make some decisions about engineering evaluation and work on things like the INEEL Diversion Dam, the bridge at Monroe Blvd at INTEC....

Dr. Baker said yeah; we had a helpful discussion of the INEEL Diversion issue, because that was somewhat of a mystery to me in reviewing the report. These seem to be natural flows but we are not to talk about where the INEEL diversion is because....but natural flows would have gone through the point if there was no diversion structure so...but I now understand the issues related to that.

Bob stated yeah it is just a regulatory quirk that.....

Dr. Baker interjected sure, yeah; I totally understand these regulatory quirks and flood related problems.

Bob added and hopefully with the loads that I get out of this report I can now go my management and say here are the loads, let's assess the diversion dam and see where we go from here.

Dr. Baker said okay, this has all been very helpful so far and we talked about trenches to and that sort of thing and I think I can see where that aspect is. That was one of my questions when I was initially looking at the material.

Bob inquired and have you gotten enough information? Do you think you can satisfy yourself that you do or do not need to go see those trenches yourself?

Dr. Baker replied oh, I am satisfied that I don't need to go see them. I think the information that has come recently I can look at it and get what I need from that.

Bob said and I would expect that given your familiarity with the area....

Dr. Baker interjected I have concluded that it is not necessary for me to go up there to see the trenches. Which probably don't exist any more anyway.

Bob responded I've stalled them a little longer you know. I take....Dean might have told you....I take various blows about the head and ears for keeping these things opened but your involvement is important enough that I was willing to do that as long as might seem necessary.

Dr. Baker added we've done more talking about the extrapolation from the trenches to other locations and I think that's more an issue of communicating to people that aren't familiar with how geomorphic mapping works. It's not a technical problem.

Bob said right.

Dean stated where we were just before we called we were....I had brought up part of the presentation that I did for the INEEL about the paleobounds criteria it was one of the Day 1 presentations and we were talking about the area where we compiled the stream power information and kind of the relationships in those areas to the geomorphic maps and how it extrapolates from sort of a point observations on geomorphic maps into larger scale areas.

Bob said right, that is sort of similar to Dr. Jorde's observation.

Dean stated help me with that, I don't recall....

Bob went on well that sort of lead to the discussion about how we relate the 2-D power and shear stress modeling to sediment transport and some other things.

Dean replied oh yeah, on a larger scale for the inundation....

Dr. Baker added yeah, I haven't come to that aspect yet but I was more into what is a kind of a innovative component of this which is the various expressions of resistance; grains sizes, vegetation, materials, etc versus levels of stream power and shear stress that are able to move that and then we were just talking about how that's given a subjective probability of achievement. We were going over all of that which was a kind of a new part of the study that I hadn't seen in the pervious studies and it looks reasonable to me. It looks like the best kind of an approach one could do then.

Bob added right I agree I think obviously and your input is very valuable there because

We recognize that...I think Dean has done a first of a kind acquisition of all the data and at this point as far as we know a unique correlation between as you observe stream power modeling and geomorphic features that you might see out there.

Dr. Baker added and that is one aspect of that it is, is not these criteria are not things that you could translate just exactly the way they are here to another location; they depend on the nature of soils here and certain.....a whole lot of circumstances that are defensible in this region but you wouldn't take them to New England for example.

Bob said I think that is a very important point. I think, Vic, as you know this sort of a key element in the integration of geomorphology in engineering. It is important that I think that engineers understand this and it is kind of interesting you take an engineering class and usually within the first week the professor tells you only use these empirical relations where they were derived and can be applied reasonably....

Dr. Baker agreed it is exactly the same thing

Bob continued and then a few weeks later you find you are using those relations however, wherever you want to.

Dr. Baker said what happens is people just remember or they just look up the equations, they don't realize those are empirical and they came from some place and that they are only as good as the data that went into them.

Bob stated perhaps and you would be more able to comment on this more than I could probably what we have established here is a defensible methodology with defensible uncertainties that perhaps people could take away from this particular site and use elsewhere but as you alluded to the specific….

Dr. Baker interjected methodology you can take away and certainly some…many aspects of the relationships you can take away but you have to think very carefully about how the whole picture is coming together at those other sites and there may be other materials that may be behaving in different ways that you have to understand in those other sites on the resistance side of the equation. If you could quantify the driving force, the stream powers and the shear stresses pretty much can mechanically transfer that but the resistance side is very dependent of the sites just like in any study you are going to find different materials and drill bores in one places versus another place, that is why you have geologists.

Bob said much to the chagrin of the hydraulic engineers.

Dean interjected from that point of view this site is such that the values here are as low as you are probably ever going to sort of see them in those areas certainly the surfacial parts of those are you know variable bands of resistance.

Dr. Baker added yeah, and from that point of view it's a great place to do this first because you can defend these more easily than you could in….a lot of flood plains have clays and all kinds of variations in resistances related to vegetation and complex history of scroll bar development and interbedded clay that those would be much more difficult places to justify this.

Dean said at least this area has relatively uniform stratigraphic you know conditions beneath the terraces from everything we have seen they're all pretty much. So anyway…. we have up here a slide with the sort of the percent area versus discharge right now. The best ????? in terms of the area and stuff…..

Dr. Baker said that is relevant to the point I raised. We were talking about how you sort of express and justify the extrapolation from the trench where you can show in detail the time period to the surface where the model shows you what the stresses are but you still....to generate the bound you have to express the time period over which that hasn't been exceeded.

Dean stated and in that there really is sort of two components and that is another issue that has always plagued us in terms of that transformation going from typically when we have age information it really comes from the B or C horizon of the soil profile, sort of deeper in the stack versus the upper most or the A horizon or the upper most horizons as it is the question of how old are those horizons potentially coupled from the underlying deposits in terms of their age, the amount of time and stability that they might represent and that is….partly to address that is one of the reasons that I have developed this sort of dual criteria of this sort of soil erosion criteria to represent the idea that we can remove the upper most A horizon and perhaps replace it and that takes a very small threshold value versus you know a full scale erosion of say terrace deposits into the B and C horizons were the bulk of the dating of the soil stratigraphic information demonstrates stability and comfort. That requires a much higher level of power, lower force basically, than we expected there.

Dr. Baker added it is the second one, I think, that is more relevant to the flows because it envision scenarios for removing A horizons that don't have to do with the water flows on the surface that you could have a drought, you desiccate it, and blows away so that is not a flood but you can't do that with the deeper layers.

Dean said that actually has certainly been happening here in that right before we started this study in 2000 there was a big range fire that burned over two-thirds of the study area. In many areas you can see the upper part of the A horizons are being stripped or there is redeposition of sand and the A horizon is clearly being influenced.

Dr. Baker interjected over grazing and all kinds of things can affect the A horizon but they don't effect the deeper layers. Except locally if there is gullying or something like that, but you can recognize that.

Dean agreed. Let's move on through the rest of this. So okay this is kind of the issue we are really talking about is detectable geomorphic change and you know to achieve virtual certainty do we have to have 100 percent of the sample area be inundated and our kind of basic precept here is that no as long as we have large sample areas we can resolve geomorphic features within those and they have to be of a scale that we would expect to detect it geomorphically. So, we have talked about that. Maybe....it looks like we are having a computer problem here. Are we up now? We have been having technology problems this morning. Are we okay? So then.....anyway what I had up there was the slide which we were combining the results from the sample areas. We have all....so all along the lower access we have FP-1 through FP-21 and basically not every area is created equal. Some areas have....will have more resolving power because they're....of the sort of local geomorphic conditions that we would expect really for any given estimate that we might try and make with surfaces that are all about the same age and have a really broad range in terms of their resolvability as a paleohydrologic bound because first and foremost the bound is a flow that has never occurred so there is nothing intrinsically there related to discharge for the sites necessarily associated with their development, but and rather what we are talking about is that discharges have removed them and some of them have in a sense they have....within events they are in a position where the stage that would be required to inundate them to reach high power is higher or lower than the hydraulic condition. So in that way when we start to sum all the....it's okay I guess conceptually to talk about...to go out of bounds and have your conclusions of what you think the range of discharge for the bound might be. A basic requirement is that you know you can't go below. So that....this is you know a test to sort of portray how well....we sum all the data in and in the meeting we had some discussion with Dr. Lall. Dr. Lall would say well couldn't you take these and specifically divide them and weight them. That is an intriguing idea but I don't think we are quite there yet. We are still at the, you know......this is going to be another subjective probability basically where we say based on our judgment we think that the lower bound is to the best of you know here and the high bound here and that is the number actually that goes into the flood frequencies analysis.

Dr. Baker asked this figure wasn't in the materials you sent. It was in the disk but not in the hard copy.

Dean responded I don't remember Vic.

Dr. Baker replied I don't remember it in the hardcopy.

Dean said then it probably wasn't

Bob and Chris both stated it wasn't in the hard copy.

Chris added it is not in the report; it is just in the presentation you made.

Dean responded okay, okay, right.

Bob added that is one of the key data analyses I guess that we need some assurance on that even though it is somewhat subjective those things are, in my opinion, good threshold indicators at the least. The question is given that are the uncertainties in that captured in the results. Obviously, in our opinion is yeah they are.

Dr. Baker said yeah this is an aspect I am trying to get my mind around which was made a little problematic by not seeing this and not being at the meeting because it was not in the written part of the report but this diagram makes it more conceptually clear what's going on with that.

Dean added and I think the key thing is this that, which I obviously haven't finished writing up here, is that like I said that all sites are not created equal and some have high resolving power and some have low resolving power and we have to treat them accordingly framing a kind of probabilistic subjective probability description of what a bound would be that we all can equate to flood frequency. If you have only one site and you have to take what you get from that one site, but if you have multiply sites you can combine them at least subjectively in some way to help constrain your ends.

Dr. Baker said I agree completely conceptually and in fact it is totally parallel to issues in flood water deposit paleoflood studies but it needs to be exclusively stated, I guess, in the report how this is being done but I have no problem with it. I see that its going to work out fine and basically what you did you just didn't write about it in the report. This is the timing issue that I learned about more, in terms of bringing this all together and laying it on your reviewer's heads.

Dean agreed right….Another element in this is that there were sort of two ways that we can reach conclusions, in particular from the Pleistocene. One is on a site to site basis which is really the kind of the traditional use of the shear stress and stream power data and then the element or one frame of reference for changing….geomorphic change that comes out of mostly the geomorphic phases is this idea that there is some structural value associated with catastrophic change and Frank Macgilligan kind of criteria that he has proposed. So in some of our prior discussions people have said well we should use both numbers you know on a site basis okay that if we want to have erosion at a trench we have Macgilligan's case where he has 300 watts per square meter for average channel power for the beginning of change, well 300 watts is what should be applied on a geomorphic surface for change and I would say on the basis of the compilation that, no that is not the way we should be using that particular completion (?) rather we should look at channel values basically as an indication of what the channel is going to start doing now but it really isn't, those aren't directly related to specific geomorphic sites that we can associate with a specific geomorphic paleohydrologic bound. Rather there are general indications that at some certain level we are going to see major (channel) change. So anyway that is the catastrophic channel change issue. Let's go on here, I think we have…… this is what I just said. Now we have a plot of the stream power versus kind of channel length showing the power derived only from a channel transect. Basically I had a box defined that went down the channel and then we could….at different discharges here we reach very high values here which would provide backup support for the idea that when we get up into those discharges the 250 to 400 cubic meter discharge ranges we are probably greatly changing the Big Lost River system and many areas of that are also involving Pleistocene channels that is complimentary to the idea that we would also be seeing some erosion.

Dr. Baker asked are many of these areas where the power levels goes up narrow, deep cross sections?

Dean said flanked by Pleistocene banks

Dr. Baker replied yes, so they are sort of resistant components of the….

Dean interjected yes they are more resistant. For example, one of these high….one of these areas are….this is the Juniper Bends area where we have essentially a bedrock channel but I would also say that the way a geomorphologist would go out and measure these values those are the sites that we would go to, to measure a cross section and make an estimate of what the power was for change just downstream were it not part of the river, because people would say well I can't make that estimate downstream because the channel is all changed and modified and obviously the alluvial, but here the power was very high. When I looked at the background and where the numbers came from are embedded in the estimates, they come from resistant sites, so you know when the resistant sites go into these higher levels then that is an indication that the system is kind of over all under stress. So this is being used strictly in a really kind of complimentary way let's say I don't think we can necessarily use it directly from a public relations stand. But I think the jest of that is when we reach conclusions about this area specific basis that we should look over here and say well does this indicate that major change has been going on and if it were to be no, you would have to question your other conclusions. But if the answer is yes it is consistent. So these are our revised bounds…the numbers that we used….I think compared to the 1999 study really the geomorphic basis and the geologic basis is really unchanged. We have refined somewhat, which isn't well written up at this point, the aging basis but there is really minimal changes in those aging behaviors. The discharge numbers have changed in absolute terms but the first order cause of that is the difference in the topography and then we have refined the ranges here within the framework of this kind of paleohydrologic bound material. (Technical problems on Dean's end) We had broken this up into several sort of subdivision covering various topics. It's going dead? We started this morning out, Bob and Chris, with Dan's laptop and the projector that we had up there and we put it on the table and we couldn't get the projector to come up. We spent about 10 or 15 minutes fiddling with that and now the laptop is in the process of having a melt down. So….

Bob stated did Dan take off. Is that him leaving?

Dean replied yeah. This is one of those "we never dreamed this was going to happen." Light bulb for the projector, yeah; we have one a bulb in the pack but having the laptop melt down this morning we just did not see that.

Dr. Baker said when we were talking before you said there were two main issues from your point of view one was the issue we have been talking about which is the processes of the paleoflood information and the issues with the shear stress and power and relating that to bounds that sort of thing. You said there is a second issue did we….did we capture that or did we…

Dean replied that the second issue was the fact that we didn't have any trench logs and trench exposures documented in the report.

Dr. Baker interjected oh yeah, well we talked about that.

Dean agreed yeah and those are I think the two main issues that I want to make sure you….that we can walk through with you and you feel okay with now.

Dr. Baker continued we could talk more about the trench logs but you don't have the slides for that. Now those were sent to me in the materials I received just before coming to the meeting. Is that right? The trench is that in the stuff that came or I got the slides from the meeting. Is there a slide of the trench or is that what you were going to show me here this morning, because you presented that in Idaho.

Dean responded right there are a series of slides of the trench stratigraphy…..

Dr. Baker interjected so I have the disk with that on it.

Dean asked is that correct, I haven't looked at the disk that you guys have?

Dr. Baker said Chris made the disk..

Chris responded yeah, that was the presentation you made here in October. So it has all of the slides with the little flags in it and all of those things.

Dr. Baker stated so even if we don't see it here this morning; I have got it so I could at it. We could talk about it; you may have some hard copy of it.

Dean replied I am trying to decide what is quicker…..I think what I am going to do is I have kind of hard copies of all of the trench logs and I am going to bring those up right now rather than running back and trying to set up another laptop for the next half hour.

Dr. Baker added at least you can alert me to it and I can look in detail on those disks.

Dean said I think in the time we have….

Bob stated and that was one of the key issues that came up in the previous reviews was that Dean and his gang can make all of these assertions about geomorphic interpretations and people would say what are you talking about where is the data. So the trench logging, the detailed photographs, and the interpretations are sort of a key element in the defensibility of this business.

Dean said what we are going to now are the real hard copies of….sort of the field logs of some of the key trenches that I'll lay out for Vic. This will kind of cover a….both a methodological and a result side of this, of how we did this. I am definitely adlibbing right now because I have got to shuffle through papers and figure out what should go first here. So, bear with me guys.

Dr. Baker asked in the report appendix that has the trench log, are you going to have all this in it or are you going have the figures? Okay

Dean explained the basic construction of the report in the end what we did….man, that is probably where I should start. A lot of these trenches, the basic idea was like we put in a grid in a straight line. They were typically five foot deep trenches and I grided them in one meter squares then I took a digital photograph of each one meter section and the mosaic goes together into a rectangular grid so we had a true scale trench log and then with the photographic base we logged directly on this. So what we are going to do with the final report is….these will all be drafted up and we will have a book basically kind of like we did with the flow model but it will cover all the trench logs. It will have original photos plus our overlays….interpretative overlays….objective…in there somewhere….because it has elements of both, being what people call objective and subjective logs objective interpretative logs and I think in the context of the real regulatory side of things that….it should fit very well with those long term document aspects of…and this one is going to be one of those long trenches. In the context of all the trenches here this was from T-1 so trenches T-1, T-2, and T-3 were basically were on the Pleistocene surfaces and also caught several of the earth mounds. I don't know if I want to go through the earth mound thing in a lot of detail right now but we transected several earth mounds and in general they are pretty interesting creatures. Did you ever see Julie Tullis' trenches through the earth mounds?

Dr. Baker replied no.

Dean continued okay well the earth mounds had....what we found in the subsurface is that they had a kind of convexity, so they have a surface shape which was a convex upward and then in the subsurface they were convex downward and completely jumbled and mixed.

Dr. Baker asked they are what size, silt, sand?

Dean replied they were silt, sand, and they were....so they not only had a surface expression with mostly silt and sand but then they had a subsurface expression that was filled with that same material and there was some gravel in most of them but they were generally deficient in the gravel fraction.

Dr. Baker asked so they were initiated on gravel surfaces but in at depressions on the gravel surfaces.

Dean agreed yeah either at depressions or by virtue of whatever formed them, originally they created a depression on the gravel surface. So you know what they have clearly....they have subsequently or contemporaneously through the Holocene been extensively burrowed...and but they are burrowed.

Dr. Baker stated because if you are an animal that wants to burrow that is where you live instead of in gravel.

Dean agreed "A target of opportunity." He then added and that goes right to the heart of the dueling ideas for the origin of these things that they appear to have been there for a long time in that what we found was that the soil development under the fine grain parts of those was....there was an enhanced carbonate accumulation under the mounds in the gravels right below the mounds at most of those sites....close to these things. So they have clearly been in existence through most all of the Holocene.

Dr. Baker added well stuff was there but the structure of that stuff has probably been mixed continuously through the whole time period when they were there so...

Dean interjected yes and in the fine grain part there are remnants of poorly developed soils that document partial periods of stability within parts of the mounds that are then subsequently remixed. So I think what our conclusion regarding those from the trenches and surfaces as a whole is the mounds actually have been persistent, spatially stable in their location, because every mound we saw had enhanced carbonate accumulation which indicated long term existence at specific spatial location relative....there is more carbonate under the mound than in the profile than there was in the adjacent gravels. So they have been there at least as long as the gravels but they are not popping up intermittently through the Holocene. Likewise there are no sort of relic and abandoned craters, if you will, that were encountered in any of the trenches and we had several hundred meters of trenches. It is like these things are unique....where we see a depression in the top of the gravel you know there is a mound on the surface and so where we see them on the geomorphic map is apparently where they have existed mainly on these surfaces through time. So in that way the mounds.....because one of the issues that we were trying to get to was, are the mounds potentially indicators of stability on Pleistocene surfaces and I think the conclusion to that is yeah they are because they exist in these channels and if we can recognize them in the flood surface stratigraphy of these kind of channel-like features on the Pleistocene surfaces then they are indeed indicators of long term stability of the Pleistocene surfaces and absence of a lot of fluvial modification of those surfaces.

Dr. Baker asked but then that makes a presumption on the nature of the origin of the mounds in that if they are continually sort of being reworked could you imagine a scenario that, because they are very fine grained, that you could remove them and they would just reform in the same stuff from silt. This presumes that the formation of the mound...I haven't read the literature on it....but a possible model is that you are trapping silts and sands maybe by the vegetation, roughness as vegetation will concentrate where those sites are but those are also sites of intense bioturbation because that is where the rodents live in association with the vegetation, so they are continually being churned and of course you can say maybe that this part was not churned because you said there was some preserved paleosols so those would be things that could be indication of whether there was modification but I'm just saying is there a scenario where a flow could remove some of the mound material or all of the mound material and it would just reform in the same spot from subsequent wind and...

Dean answered I think the answer to that is yes and that is in part happening presently due to the eolian erosion of some of the mounds due to the removal of all the vegetation by the range fires and we see certain areas on these surfaces where the mounds are somewhat muted relative to other areas and I.....whether that is the result of human impact at INEEL or whether it is the result of eolian action on those sites there is...they do have an element of transitory surface expression due to things but the....if they had been removed by flow and one would expect to see a stratigraphic record through those areas as well and if that is what has happened in the trenches on the Pleistocene surfaces even though it crossed all of these channels is there is sort of no stratigraphic evidence of younger flow in the upper parts of gravel horizons.

Bob interjected he would also add in the case of the muted mounds I don't think it tell whether it is a case of the mounds being removed or eolian sediment being piled up in the areas so that you can't see the relief any more.

Dean said right, right

Bob continued and then regarding the origin I think Julie Tullis discussed this a little bit. My favorite mechanism is permafrost conditions coming and going and sort of having points of stress related to hexagonal features that you might get in a permafrost area where the points of the hexagons are interacting and forming this locus of stress that is results in one of these mounds.

Dr. Baker replied now that would explain maybe that the sequence of depressions that the silts later formed in so that would be something that would be late Pleistocene and that is actually related to a question I was going to come to because we have these mounds....these silt mounds in Washington state too and I've looked at them quite a lot there. But a difference there is that they form not only on the Pleistocene gravel surfaces but they also form on the bedrocks surfaces and that would relate to your point about a paraglacial origin. You wouldn't expect them necessarily on the rock surfaces if they were just paraglacial.

Dean added right and they are present in Julie's studies you know she documented the existence of these mounds in some areas underlain by basalt but you need a certain thickness of loess covering the basalt to support the growth of the mound and actually in our kind of digital renderings here of the study reach there the mounds are limited in their extent to the Pleistocene alluvium and they really don't occur in any area on basalt to any degree and likewise they don't occur on the Holocene deposits the Holocene fine grain deposits. So they only occur here on sites that have....that are, were in existence in the Pleistocene and have gravelly substrates.

Dr. Baker added I think that Hal Multbee described somewhere on the Snake River Plains relic sort of polygonal ground on these gravel surfaces that is just a recollection of an old paper.

Dean added right, you know the origin of these in the literature is widely disputed, I mean there are those….

Dr. Baker stated oh yes it is….

Dean continued who are struggling in the biogenic origin or strong…..I don't think that really matters here….

Dr. Baker interjected oh yeah it is a classic problem certainly there is biogenic association and some people immediately want to make association into causation so….

Dean stated and I think here the key point for us is that they are spatially stable and that is what….we looked carefully at all of these things there is about 7 or 8 of them that we actually crossed with the trenches and we looked for frost wedges, you know, to see if there was relic frost features associated with them and, I think we found one, out of all the mounds we crossed. I think part of the reason there is that they probably expand through time because they are being grown out to the edges. One of the things that we did in the descriptions was to try to characterize the percent of gravel associated with each and in general the amount of gravel contained within the mound volumetrically appears to be somewhat less than would be required to fill the depression represented by the mound and there is no surface covering around them so it appears that whatever was involved with the origin of the mound has involved, initially at least, a depression in the gravel surface and that doesn't seem like it could have been a biogenic process. That is most reasonably some kind of patterned ground/frost wedging process.

Dr. Baker said yeah, no that sounds okay.

Dean added the key point for the paleohydrologic side is spatial stability and preserved stratigraphy in the subsurface, you know, kind of consistent with its origin and that they are not modified. Because one of the suggestions I guess from prior views from some quarters was that all of these channels on the Pleistocene surfaces are somehow related to Holocene modification of those Pleistocene surfaces and I think, that was the why we did the series of trenches in the Big Loop Area was to demonstrate that indeed even though those channels are dimpled as the Pleistocene surfaces….

Dr. Baker interjected they are relic Pleistocene channel and you're documentation of that is important and it is a reasonable interpretation because they're subdued and they look like relics of sort of braided stream process that have plagued the surface and maybe some Pleistocene modification of the original braided stream form.

Dean continued and on that note, not so much in the Big Loop trenches but what we found is we went to areas upstream of the saddle was that some of those channels actually there are channel deposits that were infilled with fine grain deposits and had well developed Stage 2 carbonate soil in them….the fine grain deposits…

Dr. Baker inquired do you have radiocarbon dates on….

Dean replied no, of course not. We hauled bags and bags of stuff out of there and sifted it and couldn't find anything.

Dr. Baker asked if they had tried OSL.

Dean responded you know I sampled a lot of that and every time I went back to look at that I got more scared of it because all I saw were burrows. All the carbonate horizons are extensively cicada burrowed and that is you know I finally at the end of the day said all I'm going to do with the OSL age here is get a real minimum. We are going to get a series of numbers we are going to have to explain away and so I finally decided to back out of the OSL. Although I have sampled this thing extensively and the samples are all sitting in storage right now; we could go down that track still, but at this point I am hesitant to do so for....because....well we are going to get conflicted results.

Dr. Baker said the best you would do with OSL is if you knew you had a horizon that had been reworked by wind and buried but people have been getting results with fluvial sediments with it and it just always....because of the technique, particularly this one where they zap 100 grains with a laser and they can average those together they can then tell the bad grains and get much better numbers.

Bob asked what does OSL stand for?

Dr. Baker answered Optical Stimulated Luminescence (OSL). It is a major technological advance over what used to be called TL and they....basically it is an result of laser technology and optical technology that has just really advance quite a lot from people working on I guess microchips and stuff like that. They know much more about the mineral physics of quartz from that than they did before and the instruments for analyzing this have really gone way up so if you could get the people who want to devote their life to the laboratory what this takes it is a fantastic new dating tool for late Quaternary sediment.

Dean said and so at least in terms of these channel fills right now we have no numerical ages. The soils are completely consistent though with the soil development that we see on the Pleistocene and in fact the soil laterally merge right into the Pleistocene soil....the soil on the gravels and the gravels interfinger on the margins with the channel fill so they have to be contemporaneous. One of the things in the saddle area that we noticed once we got this mapped was that the....which doesn't....oh up here....is that the subtle surface scarps on the topography are actually circling around the saddle and this is were these channel deposits are is in this trench T-6 and T-7 and are apparently associated with the last stages of.....late stage modification of the Pleistocene surfaces. As opposed to being geomorphic features that indicate any kind of flow over the saddle or some.....you know, some other flow direction there. So the ?? of the geomorphic surface evidence as well as the trenching evidence is sort of saying well there is....even though these are the youngest parts of the surface they are not associated with flow that would have been coming through the saddle, rather it had to be flow that came through the constriction and around the backside. So that was a major stratigraphic finding on the Pleistocene surfaces and then sort of bringing into that from the way....okay....this is trench T-6 now which started on the Pleistocene.....excuse me....H-1, H-2 Holocene surface and then continued up on to the Pleistocene surface....here, I'll just arrange these....but....in the slides I have a cartoon of this, which is why I'm trying to lay it out on the table here. Basically we have the fine grained infill of the Big Lost River incision into the Pleistocene surface which is underlying these H-1 and H-2 surfaces and is a monotonous silt, sandy silt with a Stage 6, Stage 2 carbonate soil developed in it and then this is practically on the stream bank and I'll come back and talk about the late Holocene part of that. But this sort of package of soil and sand is what typifies these H-1 and H-2 surfaces and into multiple decay horizons. Then at the back edge of this, there is a sort of on lap gravel sequence that ties back to the Pleistocene gravel surface.

Anyway in about the next sheet they would tie right into kind of the typical plain bedded gravel sequence that we see underlying all the Pleistocene surfaces as we go up the terrace risers there and so these soils look very much like the soils in the channel fill and this is the soil at the site that we call BLR-6 that looks identical which was just down stream of these trenches and from the lower part of that horizon in the first study we got an age of about 10,000 years.

Dr. Baker asked how was that?

Dean replied we got a 10,000 year radiocarbon age and this trench we got about a….I believe it was….yeah, we got a 6,000 radiocarbon age out of that but there is a bunch of burrowing stuff in here so….as you can see we have major burrows….in the region of burrows this was the one we could actually find some charcoal in that we could date; a real small sample but….you know….so 6,000 has become a sort of defacto minimum age for these late Holocene bounds. Plus we got a 7,000 year age from another site at the saddle in the Stage 2 carbonate rich fine grain stuff. So the periods of….this represents sort of the latest Pleistocene to early Holocene aggradations of a previously incised Big Lost River channel system….incised to the Pleistocene. Then in the latest Holocene that was partially eroded from the stream bank and a series of silty sand aggraded in that and radiocarbon ages from this are 500 to 800 years at all our sites and this site as well and that is expressed in the soils on this as sort of a scalloping of this underlying carbonate soil that sort of feathers out as we move back to the terrace and by the time we get to the middle or back part of the terrace the whole soil profile is intact on this H-1 surface and it is only partially eroded and you can see that these units are sort of pinching out as we move toward the stream bank here and it is partially eroded here. So this is the evidence for what we had previously called or this is kind of stratigraphic evidence for what we had previously called the 400-year flood in terms of stripping of the….partial stripping of this surface and where….versus long term stability of them. And this is so when we talk about setting the bounds and going back to these areas what I'm using for the area right in here…..is a long skinny box that incorporates most of the this low terrace….the full width of this terrace. What we see as we look at the flow modeling is that the initial kind of higher power shear stress values invariably comes from the outside edge and then the discharge increases as it moves inward and occupies most of the terrace surface so the stratigraphy here should be reflecting well kind of partial inundation and insipient stripping of this surface and we should be able to relate that in terms of the hydraulic modeling to a specific discharge.

Dr. Baker said remind me how does that 400 flow level relate to the overspill on the south…what was that level at…it figured prominently in the 1999 report.

Dean replied in the 1999 report we said that discharge was 100 cubic meters, no 150 cubic meters…100 didn't go over and 150 did and one of the things that we found when we resurveyed, specifically for the saddle, was the first thing we did when the modeling results started to diverge is that we went out there with the GPS and planted the GPS at a low spot in the saddle and what we found is that it was two feet higher than was depicted on the topographic map in the 1999 report. Now it takes…depending on what exactly the topographic set we are using 250….225 cubic meters. TAPE ENDS

There was approximately five to ten more minutes of discussion. By completion of the conference call, Dr. Baker stated he felt confident that between this meeting and the material he received last week he would be able to complete his review of the report.

This Page Intentionally Left Blank.

APPENDIX B

QUALITY ASSURANCE OF TOPOGRAPHIC DATA

APPENDIX B

QUALITY ASSURANCE OF TOPOGRAPHIC DATA

This Appendix contains the report only. Full text and cited appendices can be found in Appendix A of the final flood hazard report.

RECLAMATION

Quality Assurance of Topographic Data

for the

Big Lost River Flood Hazard Study, Idaho National Engineering and Environmental Laboratory, Idaho

U.S. Department of the Interior
Bureau of Reclamation
Technical Service Center
Seismotectonics and Geophysics Group
Denver, Colorado

March 2005

MISSION STATEMENTS

The mission of the Department of the Interior is to protect and provide access to our Nation's natural and cultural heritage and honor our trust responsibilities to Indian tribes and our commitments to island communities.

The mission of the Bureau of Reclamation is to manage, develop, and protect water and related resources in an environmentally and economically sound manner in the interest of the American public.

Quality Assurance of Topographic Data for the Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho

Prepared by
Dean A. Ostenaa
Matt Jones

Report 2003-4

Bureau of Reclamation
Technical Service Center
Seismotectonics and Geophysics Group
Denver, Colorado

March 2005

Prepared by:

Dean A. Ostenaa
Digitally signed by Dean A. Ostenaa
DN: CN = Dean A. Ostenaa, C = US, O = Bureau of Reclamation, OU = [illegible]
Reason: I am the author of this document
Date: [illegible]

Dean A. Ostenaa **Date**

Digitally signed by Matt Jones
DN: CN = Matt Jones, C = US, O = USBR, OU = D-8330
Reason: I am the author of this document.
Date: [illegible]

Matt Jones **Date**

TSC Peer Review by:

Digitally signed by Jerry Wright
DN: CN = Jerry Wright, C = US, O = Seismotectonics and Geophysics, OU = BOR D-8330
Reason: I have reviewed this document
Date: 2005.03.17 13:07:07 -0700'

Jerry Wright **Date**

SUMMARY

Accurate depiction of topography is a basic requirement for flood-inundation modeling estimates. This document describes the process and Quality Control efforts associated with obtaining accurate topographic data for use in flood hazard modeling and paleoflood studies of the Big Lost River at the Idaho National Environmental and Engineering Laboratory (INEEL). Topographic data was required for three areas along the Big Lost River near the INEEL including 1) a detailed paleoflood study reach downstream of the INEEL Diversion Dam for which 1:4000 scale aerial photographs were flown in August 2000; 2) the main Big Lost River corridor at the INEEL extending from the INEEL Diversion Dam to about 2 miles downstream of INTEC and TRA; and 3) the Big Lost River - Box Canyon areas upstream of the INEEL Diversion Dam.

The accuracy of topographic data used in the flood hazard studies was called into question when initial hydraulic modeling results based on newly acquired topographic data from the paleoflood study reach downstream of the INEEL Diversion Dam were found to be significantly different than modeling results from previous studies which used topographic data from the INEEL 2-ft contour map produced by Aerographics from aerial photography obtained in 1993. Extensive analyses of the topographic data used in these modeling studies has been undertaken in an effort to characterize the accuracy of each to the topographic data sets used in the flood hazard analyses. Each group of topographic data used in these analyses has been compared to an independent, more accurate, set of GPS field survey data.

Aerial photography flown in September 2000 was used to generate 3-ft grid data for detailed hydraulic modeling of a paleoflood study reach downstream of the INEEL Diversion Dam. More than 800 points were surveyed with GPS in this reach to evaluate the accuracy of the topographic grid. National Map Accuracy Standards (NMAS) and American Society for Photogrammetry and Remote Sensing (ASPRS) accuracy measures indicate that the grid data is sufficient for mapping with contour intervals of approximately 1.3 to 1.5 ft or larger. For the same reach, grids derived from the 1993 INEEL 2-ft contour map do not meet standards for 4-ft contour mapping and appear to be significantly warped in some areas.

For the main Big Lost River corridor between the INEEL Diversion Dam and areas downstream of INTEC and TRA, comparisons indicate that the original INEEL 2-ft contour map was close but generally does not meet ASPRS standards for class 1 mapping as a 4-ft contour map, the standard cited in FEMA 37 for flood hazard mapping. The original 2-ft map also does not meet NMAS standards for a 4-ft contour map based on the present comparison data. The 2003 BOR 5-ft grid developed from the 1993 photography and original Aerographics control data meets both ASPRS and NMAS standards for approximately 3-ft contour mapping. Measured accuracy values on the 2003 BOR 5-ft grid are generally 25 to 50% better than values measured on surfaces derived from the 1993 INEEL 2-ft contour map. The level of accuracy achieved by the 2003 BOR 5-ft grid appears to be limited by the underlying accuracy of the original control network used for the 1993 photography.

Topographic data generated for the Box Canyon area of the Big Lost River has been generated as part of the 2003 BOR 5-ft grid. More than 8000 GPS data points were surveyed along the floor of Box Canyon and included in the photogrammetric processing as breakline data to improve the resolution of the resultant topographic grid through the canyon reach. No formal quality checking of the grid upstream of the INEEL Diversion Dam has been conducted, but accuracy is assumed to be similar to areas downstream because of uniformity of the processing and use of the same 1993 control and photography data.

TABLE OF CONTENTS

LIST OF FIGURES

LIST OF TABLES

APPENDICES

Appendices are included only in electronic format as either PDF or HTML files.

Appendix	Title
A	Camera calibration reports, survey and coordinate data for the 2000 project control panels
B	July and October 2002 GPS check point survey data and point listings
C	Camera calibration reports and original coordinate data for the AG 1993 topography
D	February 2003 survey and coordinate data for 1993 control check
E	Survey data and point listing for supplemental Box Canyon points

1.0 INTRODUCTION

Accurate depiction of topography is a basic requirement for flood-inundation modeling estimates. This document describes the quality assurance and quality control efforts and process associated with obtaining accurate topographic data for use in flood hazard modeling and paleoflood studies of the Big Lost River at the INEEL (Idaho National Environmental and Engineering Laboratory).

Topographic data was required for three areas along the Big Lost River near the INEEL (**Figure 1-1** and **Figure 1-2**). These were 1) a detailed paleoflood study reach downstream of the INEEL Diversion Dam for which 1:4000 scale aerial photographs were flown in August 2000; 2) the main Big Lost River corridor at the INEEL extending from the INEEL Diversion Dam to about 2 miles downstream of INTEC and TRA; and 3) the Big Lost River - Box Canyon areas upstream of the INEEL Diversion Dam.

1.1 Data Requirements

Guidance on flood hazards is set forth in DOE-STD-1020, -1022, and -1023. Design and evaluation criteria set forth in these standards control the level of conservatism introduced in the design/evaluation process such that flood hazards are treated on a consistent basis. These criteria also employ a graded approach to ensure that the level of conservatism and rigor in design evaluation is appropriate for facility characteristics such as importance, hazards to people on and off site, and threat to the environment. Facilities at the INEEL for which flood hazard information is required span the full range of performance categories (PC-0 to PC-4) and probability levels (10^{-1} to 10^{-5}) outlined in the DOE standards. Thus, there is a need for both high levels of rigor and defensibility in the evaluation process as well as an internally consistent approach to flood hazards across the full range of performance categories and probability levels that are required on a site-wide basis.

In 1993, Aerographics Inc. flew 1:10,000 scale black and white aerial photography over a significant portion of the INEEL site centered on the Big Lost River. This photography was used to generate 2-ft contour interval topography (1993 AG 2-ft data) along the Big Lost River that would provide a consistent basis for flood modeling at the INEEL site. Among other uses, these

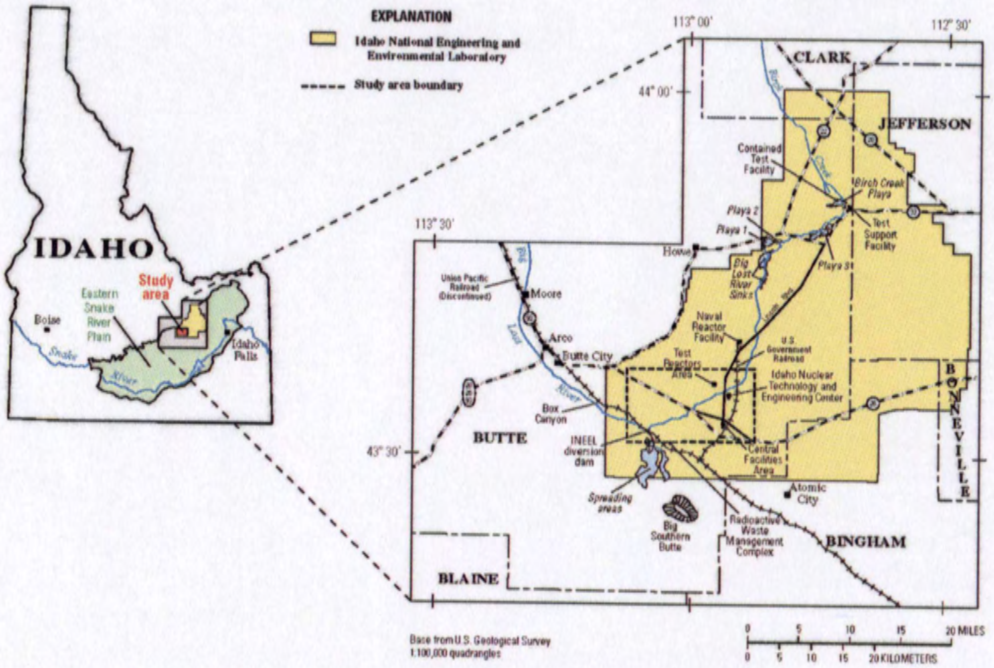


Figure 1-1 Location map of the INEEL and Big Lost River areas. Figure from Berenbrock and Doyle (2002).

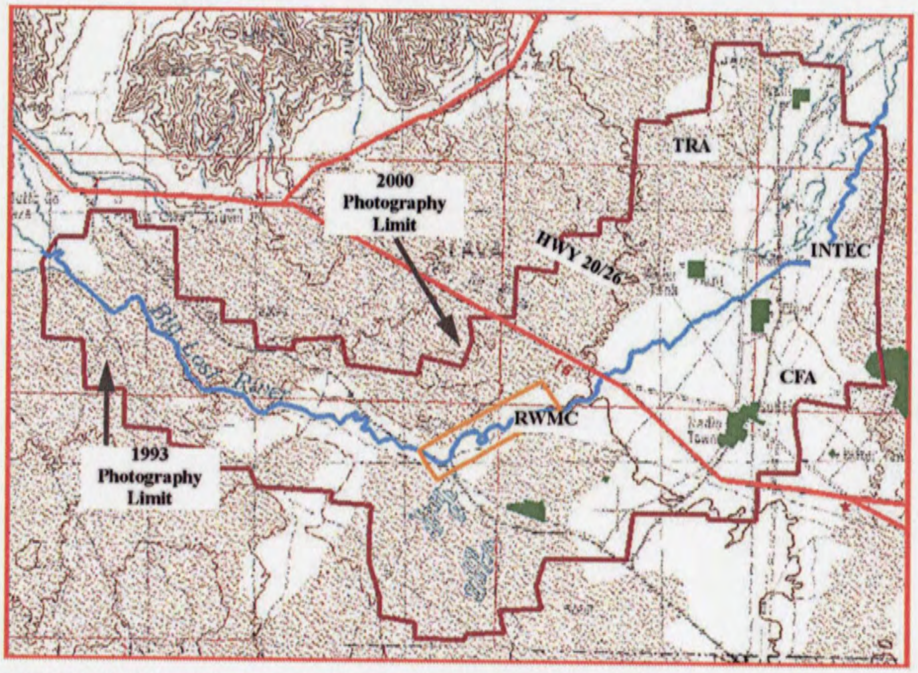


Figure 1-2 Limits of aerial photography coverage for 1993 and 2000 project areas along the Big Lost River. Dark red line is limit of 1993 photography; orange line is limit of 2000 photography; green blocks are major INEEL facility areas. Base map from USGS 1:250,000 scale series.

topographic data were input to hydraulic models used by USGS to estimate 100-yr flood inundation along the Big Lost River downstream of the INEEL Diversion Dam (Berenbrock and Kjelstrom, 1998) and by BOR to estimate the magnitude of paleohydrologic bounds for a ~3-km-long study reach on the Big Lost River just downstream of the INEEL Diversion Dam (Ostenaa and others, 1999).

To reduce uncertainties associated with existing flood hazard estimates, further studies are being conducted by both the USGS and BOR at the INEEL. Both sets of studies have required additional, more detailed topographic data as input to hydraulic models. Because the USGS studies are using one-dimensional models which require topographic data to be input along cross sections, detailed field surveys have been conducted to collect data for these models. BOR is using two-dimensional hydraulic models which require topographic data to be input as gridded data from Digital Elevation Models (DEM). For the 3-km-long paleoflood study reach downstream of the INEEL Diversion Dam, new aerial photography was flown in September 2000 to provide more detailed topographic data for input to these models.

The scope of current two-dimensional modeling studies requires modeling of a wide range of discharge scenarios along the Big Lost River from the INEEL Diversion Dam to downstream of INTEC and TRA. Initial renderings of the gridded outputs derived from the 1993 AG 2-ft contour map contained numerous artifacts related to interpolation between contours. Because the input topography has a major impact on calculated water surfaces in hydraulic models, it was determined in November 2001, following consultation with the Participatory Peer review group providing oversight to the BOR for the INEEL flood hazard studies, that substantial improvement in the topographic inputs to the two-dimensional models was warranted. Reprocessing of the 1993 aerial photography to directly output a DEM grid would provide substantial reduction in the uncertainty associated with the topographic inputs.

DOE standards for hazard evaluations, DOE-STD-1020, 1022, and 1023, do not contain explicit guidance regarding the evaluation of map data used in these evaluations. Implicit standards are given through application of sound professional practice, guidelines for peer review, and incorporation of other regulatory standards for some portions of the DOE standards. The National Map Accuracy Standard (NMAS) (US Bureau of the Budget, 1947) has been a widely used measure of map accuracy. FEMA 37 is one non-DOE regulatory guideline incorporated within the DOE standards which contains explicit guidance on mapping standards. Appendix 4 of FEMA 37 outlines standards for map data used in preparation of Flood Insurance Rate Maps (FIRM). The FEMA standards are derived from several sources including American Society for Photogrammetry and Remote Sensing (ASPRS) guidelines for large-scale mapping (ASPRS, 1990) and US Army Corps of Engineers Engineer Manuals (USACE, 1994). FEMA 37 guidance anticipated the completion of a new National Standard for Spatial Data Accuracy (NSSDA) which was completed in 1998 (Federal Geographic Data Committee, 1998). NSSDA was developed as an outgrowth of efforts to update NMAS. The ASPRS Accuracy Standards for Large-Scale Maps formed the basis for the initial update. The NSSDA was developed to provide a common reporting mechanism so that users can directly compare datasets for their applications. Map-dependent measures of accuracy, such as publication scale and contour interval, are no longer fully applicable when digital data can be readily manipulated and output to any scale and format. Accordingly, accuracy as defined in NSSDA is used in this report as the primary basis for evaluating the topographic data used in the flood hazard modeling. NSSDA also contains provisions for reporting of data accuracy in terms of earlier standards.

The NSSDA uses root-mean-square error (RMSE) to estimate positional accuracy. RMSE is derived from comparisons of dataset coordinate values and coordinate values from an independent source of higher accuracy for identical points. Thus, for horizontal accuracy where RMSE in the x- and y- coordinate directions are not equal

$$\text{Accuracy}_r = 2.4477*0.5*(\text{RMSE}_x + \text{RMSE}_y) \quad (1\text{-}1)$$

and for vertical accuracy

$$\text{Accuracy}_z = 1.9600*\text{RMSE}_z \quad (1\text{-}2)$$

where

$$RMSEz = \sqrt{\frac{\sum (z_i - z_t)^2}{n}} \quad \textbf{(1-3)}$$

and z_i = interpolated DEM elevation of a test point

z_t = true elevation of a test point

n = number of test points.

Accuracy assessments in this report focus on the vertical component because most of the data available for checking accuracy is best suited for vertical accuracy checks and because initial comparisons indicate that the largest uncertainties are present in the vertical accuracy.

1.1.1 Field Survey Measurement Accuracy. To assess the vertical accuracy of the contour map and DEM data that has been generated for use in the various hydraulic modeling efforts, spot elevation data were collected through GPS field surveys. The spot elevation data are compared to surfaces generated from either 1993 AG 2-ft data or photogrammetric DEM data produced by BOR. Accuracy of the photogrammetric processing is also limited by the accuracy of the initial control panel network. A limited number of control points used for the 1993 photography were resurveyed to assess the accuracy of the original control network. Accuracy of the individual GPS observations is dependent on several factors including the distance from the base station, satellite configurations, and number of measurements for each point. For each observed GPS point, the RMS value is the statistical measure of the GPS measurements from their mean once the carrier signal indicates initialization has been achieved. Additional statistics, e.g., mean, median, and standard deviation, have been calculated on these RMS values to describe the overall accuracy of the groups of points used for spot elevation checks.

1.1.2 Relationship Between NSSDA and Other Map Accuracy Standards. The NSSDA (Federal Geographic Data Committee, 1998) provide several formulas to relate horizontal and vertical accuracy reported according to NSSDA to previous standards including NMAS (US Bureau of the Budget, 1947) and ASPRS (1990). Relationships for vertical accuracy are listed below; generally similar relationships for horizontal accuracy are found in Federal Geographic Data Committee (1998).

NMAS standards for vertical accuracy (VMAS) call for "not more than 10 percent of the elevations tested shall be in error more than one-half the contour interval" (Bureau of the Budget, 1947). Under NSSDA

$$VMAS = 1.6449 * RMSE_z \quad (1\text{-}1)$$

$$Accuracy_z = 1.1916 * VMAS \quad (1\text{-}2)$$

Therefore, vertical accuracy reported according to the NSSDA is (1.1916/2)*CI = 0.5958 *CI, where CI is the contour interval.

For Class 1 maps according to the ASPRS Accuracy Standards, the limiting RMSE is set at one-third the contour interval. Although FEMA 37 calls for maps with 4-ft contour intervals, ASPRS class 1 standards are implied for FEMA products within floodplains unless special exceptions have been approved (FEMA 37, Appendix 4, p. A4-4). This implies limiting horizontal and vertical RMSE values of 5- and 1.33-ft, respectively (FEMA 37, Appendix 4, Table A4-1).

1.2 Acknowledgements

This report has been produced as part of Interagency Agreement DE-AI-07-00ID13972 between the Bureau of Reclamation and the Department of Energy. Project Manager R.J. Creed has provided overall guidance for these studies.

Ken Beard, INTEC Engineering at the INEEL, provided invaluable assistance with the field surveys and issues regarding control at the INEEL site. Dan Mahnami, INEEL Spatial Analyses Laboratory, provided data for the INTEC area which was used to update changes to the facilities since 1993.

Matt Norake, Vectors Inc., provided support for Trimble GPS equipment and control network issues.

Kris Myer, ZI Imaging provided consulting and support for ZI Imaging photogrammetric softcopy applications.

Chris Martin of S.M. Stoller Corporation and R.J. Creed reviewed preliminary drafts and provided many helpful comments.

2.0 PALEOFLOOD STUDY REACH - 2000 PROJECT

A paleoflood and flood frequency analyses for the Big Lost River at the INEEL was completed in 1999 (Ostenaa and others, 1999). As a follow-up to this study focused on reducing uncertainties associated with the initial estimates, more detailed geomorphic and hydraulic modeling studies of a ~3-km-long study reach downstream of the INEEL Diversion Dam were initiated in July 2000. Subtasks within these studies included detailed geomorphic mapping along the Big Lost River and additional two-dimensional hydraulic modeling through the study reach. To support these subtasks, new aerial photography at a scale of 1:4000 was obtained in August 2000 so that very detailed topographic grid data could be produced with photogrammetric methods. These grids could be rendered to produce high-resolution shaded relief and contour (~1-ft CI) maps for the geomorphic studies and provide a 3-ft grid as the topographic input for hydraulic models.

2.1 Photogrammetry

Photogrammetric analyses for the 2000 project was done using a ZI Imaging ImageStation softcopy system. This system runs under a Windows-based operating system on a dual Intel processor workstation. The ZI applications consist of 5 modules, 1) ImageStation Project Manager, 2) ImageStation Stereo Display, 3) ImageStation Feature Collection, 4) ImageStation AeroTriangulation, and 5) ImageStation Raster Utilities. These modules run with Bentley MicroStation Graphics for the user interface and for outputting results. ZI Terrain Analyst Software (currently Intergraph MGE Terrain Analyst) was used to generate the grid surfaces, perform edge matching, and to analyze the photogrammetric outputs for quality control.

2.1.1 Control. Prior to flying new aerial photography a network of 37 control panels was placed throughout the study reach (**Figure 2-1**). Coordinates for these panels were obtained by a GPS survey using the NGS monument, BIG LOST as a base station on August 17-19, 2000. This monument was well within the GPS range for the study area. The base control station was set up on the BIG LOST monument and the panels subsequently located with a Trimble 4800 GPS receiver. Centerpoints of the control panels were measured using RTK (Real Time Kinemetic) surveying with an occupation time of 10 minutes per panel. Mean and median values of RMS for these observations were both 0.010 m (**Figure 2-2** and **Figure 2-3**).

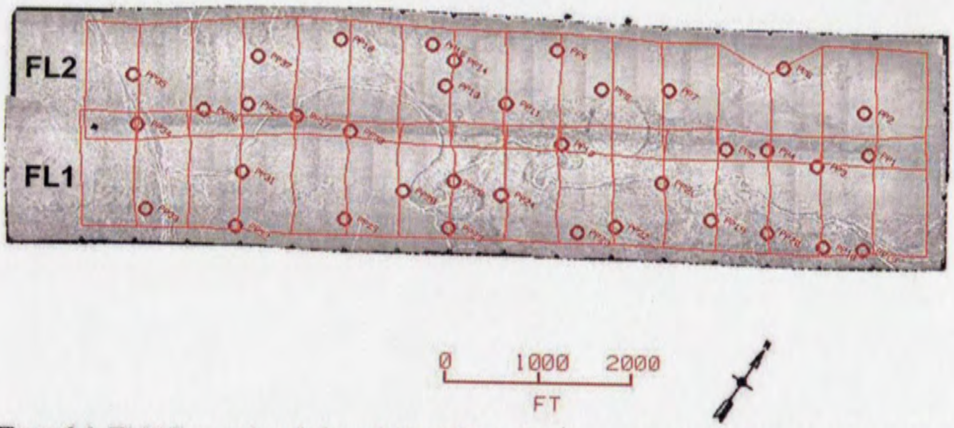


Figure 2-1 Flight lines and control panels for 2000 project. Boxes show limits of stereo models; circles with labels are photo panel points.Base map is orthophoto from 2000 photography. The INEEL Diversion Dam is located along left group of control points.

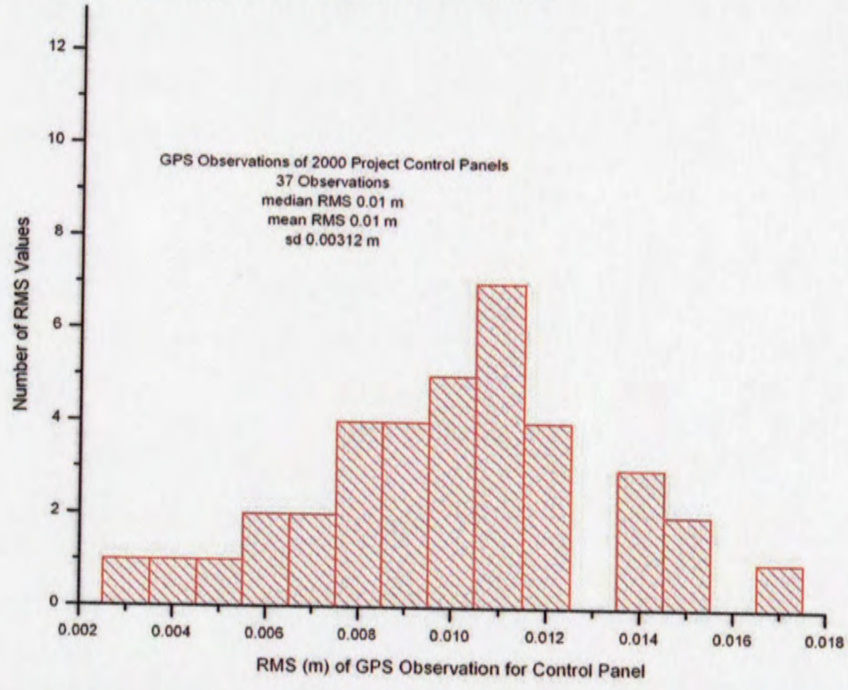


Figure 2-2 Histogram of RMS values for GPS field observations of 2000 project control panels. All points were observed from BIG LOST.

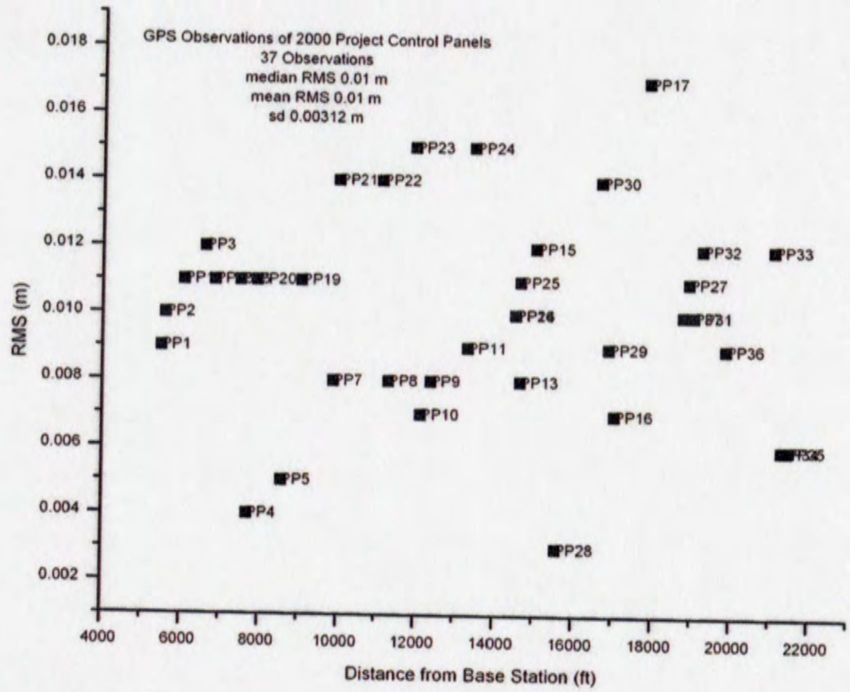


Figure 2-3 Distance from base station at BIG LOST vs. RMS for GPS observations of 2000 project control panels.

Field surveys and initial analyses of the study reach used a local rectangular grid system based on the average local vertical datum. This eliminated projection errors associated with projection to a State Plane system. However, as modeling studies progressed, it was recognized that the final data would need to be converted to State Plane coordinates in order to be fully compatible with other INEEL site studies. Thus, the control network was converted to NAD 27 State Plane, Idaho East Zone horizontal datum and NAVD 29 vertical. Final contours and grids were generated in this system to be compatible with other site studies. Survey data and coordinates for the 2000 project are included in Appendix A of this report.

2.1.2 Aerial Photography. Aerial photography for the 2000 project was flown on August 22, 2000 by Horizons Inc., Rapid City, South Dakota. Horizons provided the camera calibration file (Appendix A of this report), prints and negatives of the project. Mean flight height for the project was 2000 ft which produced a 1:4000 scale photograph. The flight consisted of 2 flight lines along the Big Lost River with a total of 34 images (**Figure 2-1**). Image Scans in Denver, Colorado performed the scanning of the images. The images were scanned at 14 microns in Intergraph JPG compressed format with a Q factor of 15. These parameters were established based on the resolution needed by the ZI softcopy software to generate surfaces at the specified accuracy.

In late July 2000, a wildfire burned over most of the paleoflood study reach area. Thus, much of the landscape recorded by the photography was completely devoid of vegetation and charred. In some areas, especially those underlain by basalt bedrock with very thin soil cover, generally thin (<0.1 to 0.5 ft) sheets or patches of windblown sand were mobilized and covered the charred vegetation. On the flatter alluvial surface flanking the Big Lost River, the thickest accumulations were generally in wheel tracks or as small dunes downwind of small outcrops or other surface obstructions. This resulted in a relatively uneven tonal contrast on the photography that limited its utility as orthophotos for base maps, but did not impact its use for photogrammetric analyses. In fact, the absence of vegetation throughout much of the study reach eliminated any questions as to whether or not the photogrammetric points were actually seeing the ground surface rather than on the tops of thick sagebrush accumulations. Of specific importance for the flood hazard modeling, there was little or no accumulation of windblown deposits from the July 2000 wildfire in the study reach channel of the Big Lost River at the time the aerial photographs were taken in September 2000.

2.1.3 Photogrammetric Processing. The photogrammetric processing was comprised of the following steps:

1) Establish and enter control and coordinate information for the project. This includes projection, coordinate and camera parameters. Input scanned images of photography.
2) Perform Interior Orientation. There is an automated feature in the ZI software that orients each photo by fiducial marks (**Figure 2-4**).
3) Perform Relative Orientation and Absolute Orientations and run bundle adjustment for the project. Relative and Absolute Orientation rectifies the photos relative to each

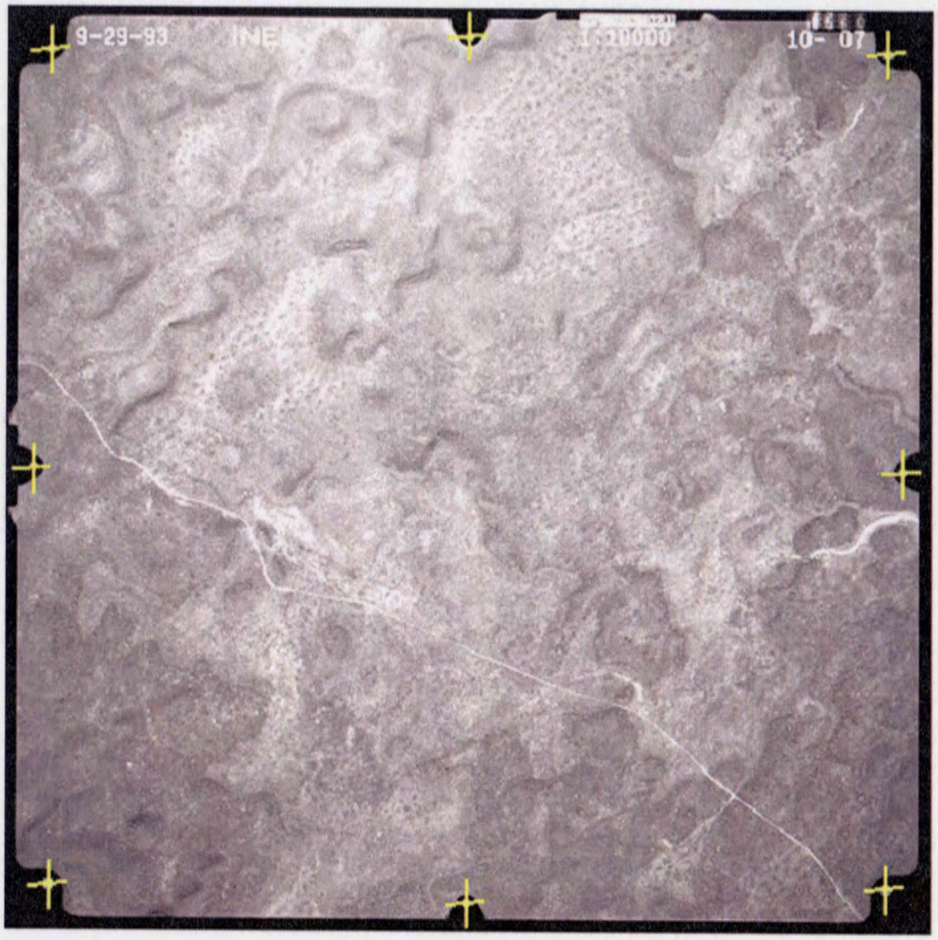


Figure 2-4 Example aerial photograph with fiducial marks (yellow crosses) used for interior orientation. Photo 10-7 from the 1993 Aerographics flight.

other and then spatially applies the control network to rectify them to the ground surface (**Figure 2-5**).

4) Digitize breaklines of identified features. Important features are digitized in three dimensional space (stereo-viewing) to ensure accuracy with respect to the grid surface. Examples of digitized features to which breaklines are applied are the top and bottom of the stream channel banks or other sharp topographic features, buildings, bridge abutments,

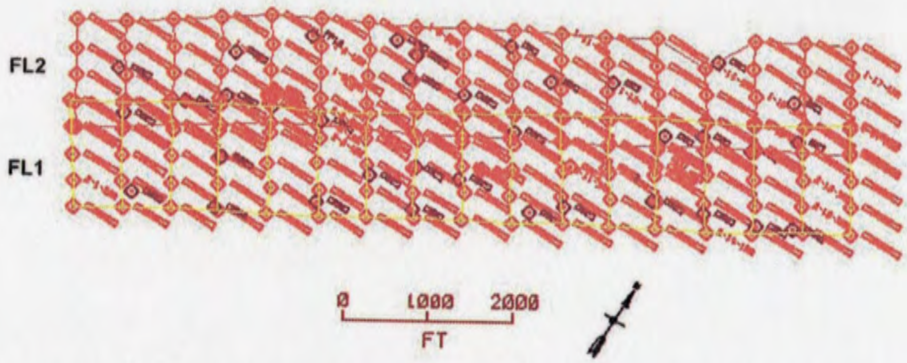


Figure 2-5 RO/AO layout for the 2000 project. Red diamonds are user points defined to compute analysis results. See **Figure 2-1** for base map.

road surfaces, levees and dams (**Figure 2-5**).

5) Run automatic elevation grid software at three foot intervals on all stereo models. After all features were digitized within the project, the automated grid elevation software was run. This software produced grid surfaces of all of the stereo models (**Figure 2-7**). Smoothing factors, breakline features, edge definition and model parameters were established with a five foot grid definition. The grids were output into Bentley Microstation CAD software.

6) Perform edge matching of grids between flight lines. Once the grids were generated for each stereo model, edge matching was performed to generate seamless surfaces across the models. The grids were then merged together by flight line (**Figure 2-8**). Final grids for the 2000 project have 3-ft postings.

Grid data that are input into BOR two-dimensional flood models are first rotated to minimize the number of generated points relative to the area that will be modeled and then output in ASCII file format along with projection and unit information. For other analyses, the gridded data are rendered as needed using Bentley Microstation or output in formats compatible with other software as needed.

Figure 2-6 Example of breaklines used to aid in definition of Big Lost River channel in 2000 project photography. Dot-dashed lines are breaklines added in stereo models to improve definition of channel features and banks. Smooth-textured areas adjacent to river channel were burned. Area in lower left part of figure with variable texture was unburned; similar areas in upper right were partially burned. Channel reach located in NE1/4, Section 6, T2N, R29E, about 2 miles downstream of the INEEL Diversion Dam.

2.2 Quality Assessment of the 2000 Photogrammetry Data

No follow-up surveys were originally planned to independently assess the quality of the topographic grids produced from the August 2000 photographs because 1) modeling efforts in the study reach were viewed as relatively independent from studies in other areas, thus small discrepancies at the boundaries of the detailed study reach and existing INEEL 2-ft contour mapping would be acceptable; 2) by normal photogrammetric standards the control network was extremely dense, thus high accuracy was expected from the photogrammetry processes used to generate the mapping, and 3) there were no INEEL facility sites within the detailed study reach. RMSE calculated on the control panel residuals from the photogrammetric processing (**Figure 2-9**) were low, indicating that the final photogrammetric models closely fit the input coordinate data. Initial inspections of the shaded relief renderings and topographic maps revealed a high level of detail, appearing to confirm these assumptions as did initial inspections of two-dimensional

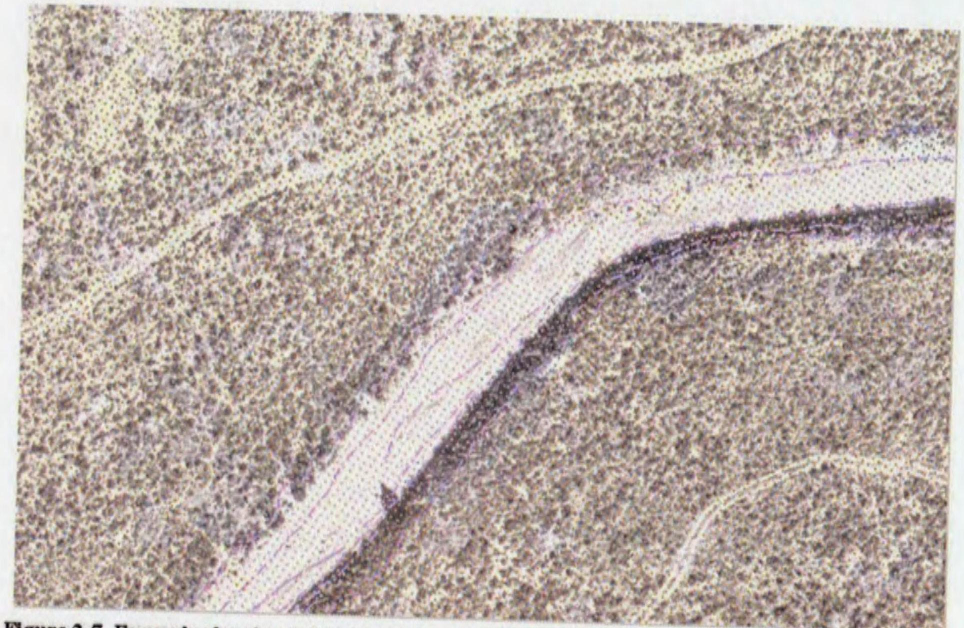


Figure 2-7 Example showing 3-ft grid points and breaklines on Big Lost River channel for 2000 project. Red dots are grid points with 3-ft spacing; dashed lines are breaklines. Unburned area in SE1/4, Section 2, T2N, R28E, about 1/2-mile downstream of the INEEL Diversion Dam.

hydraulic modeling results. Further confirmation of the relative accuracy of these data was provided by comparisons to cross sections along the Big Lost River surveyed in 2001 by the U.S. Geological Survey. Comparisons of grid-derived data to field surveyed data show a high level of correlation (C. Berenbrock, personal communication, 2002). Following a shift in the vertical datum of approximately 1.9 ft for the data initially provided to USGS, grid-derived data were used in lieu of additional field surveying to extend cross sections needed for USGS modeling inputs.

However, as two-dimensional hydraulic modeling efforts proceeded and results based on the 2000 photography were compared to modeling results from Ostenaa and others (1999), it became apparent that there was a significant discrepancy in water surfaces modeled on the existing INEEL 2-ft contour map (AG 1993) and the grid derived from the 2000 photography and control (BOR 2000 3-ft). The same hydraulic model was used, thus topography was the only difference. Modeled water surfaces on the 2000 topography appeared to be systematically higher in a

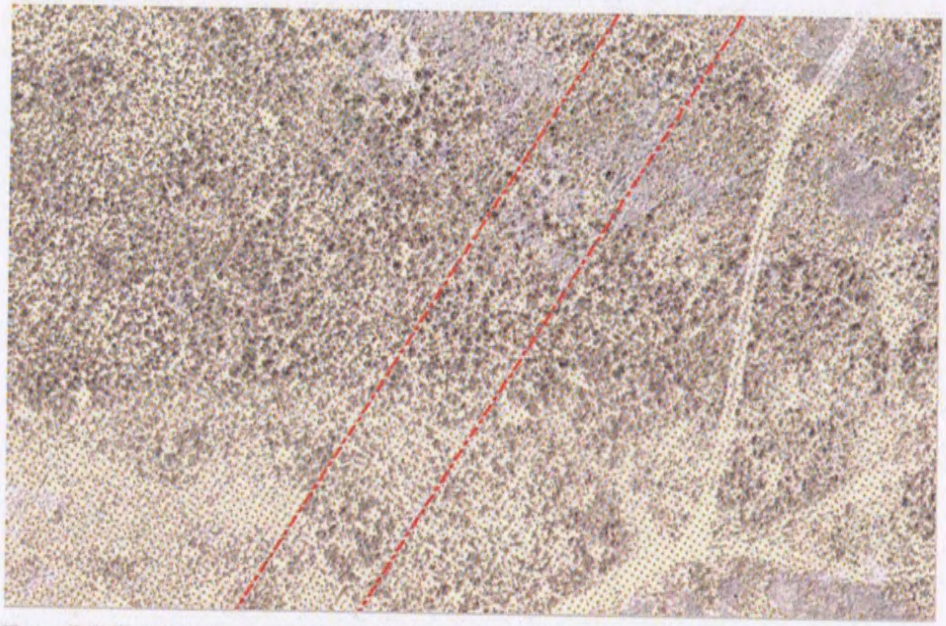


Figure 2-8 Overlap region and grid points for area of 2000 project photography. Grid points (red dots) within the area between the dot-dashed lines are merged and trimmed to assure smooth matching of adjacent photogrammetric models.

downstream direction, suggesting the possibility that one or both of the topographic surfaces used as inputs to the hydraulic models had a systematic shift or error. No discrepancies were evident in the horizontal (x, y) positions of features common to the two datasets. Sinuous channels, roads, ditches, and ridges present in both surfaces appear to match with minimal discrepancies in planimetric location. Any errors appeared to be primarily a result of differences in elevation (z).

2.2.1 Field Checking of the 2000 Photogrammetry Study Area. To assess the vertical accuracy of topographic grids within the paleoflood study reach, GPS field surveys were conducted July 15-16, 2002 that resulted in the collection of 827 elevation points throughout the study reach. These spot elevations were collected at intervals along a series of traverses spaced throughout the reach (**Figure 2-10**). An additional 63 points, including 37 points on the photopanels used to control the 2000 photography, had been collected in August 17-19, 2000 when photopanels for the 2000 photography were surveyed. The photopanel points could not be

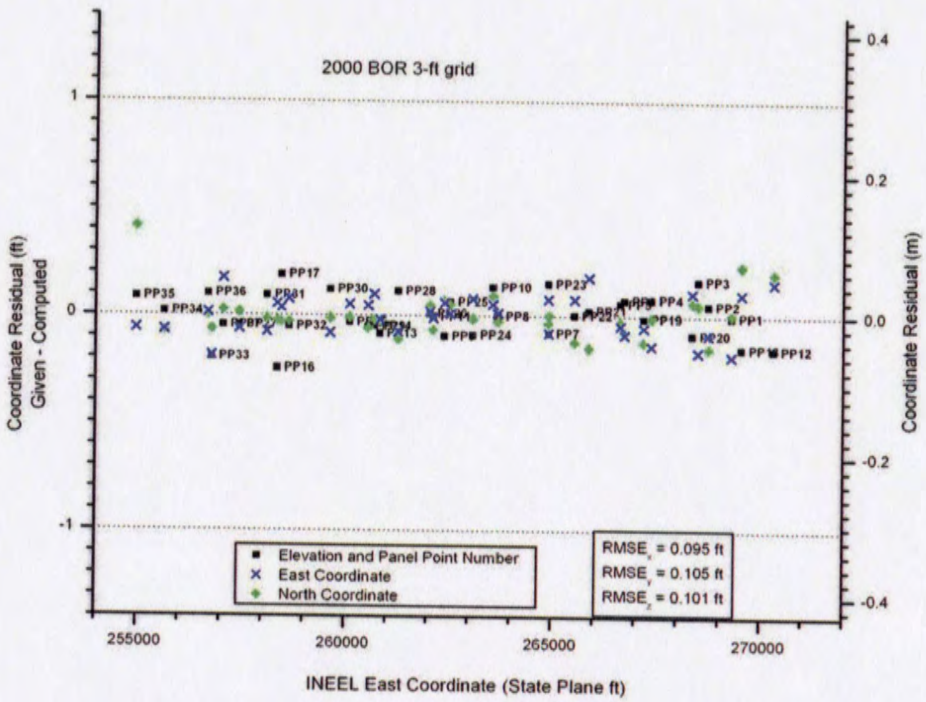


Figure 2-9 Computed residuals and RMSE on control panels for 2000 BOR 3-ft grid. The mean RMS of the original GPS observations was 0.033 ft (0.01m) (see **Figure 2-2 and 2-3**).

used to check the 2000 photogrammetry grids (BOR 2000 3-ft), but could be used to check the surfaces generated from the 1993 photography (AG 1993 and BOR 2003 5-ft). The emphasis of these surveys was to check elevations of the generated surfaces; no attempt was made to identify features in different sets of mapping that could be used to evaluate the horizontal accuracy.

The July 2002 spot elevation data were collected with a Trimble 4800 GPS receiver using the BIG LOST NGS monument for the base control station. NAD 27 State Plane, Idaho East Zone horizontal datum and NAVD 29 vertical coordinates for BIG LOST provided by K. Beard were used and field data recorded in these systems. RTK survey methods were used with mean RMS of

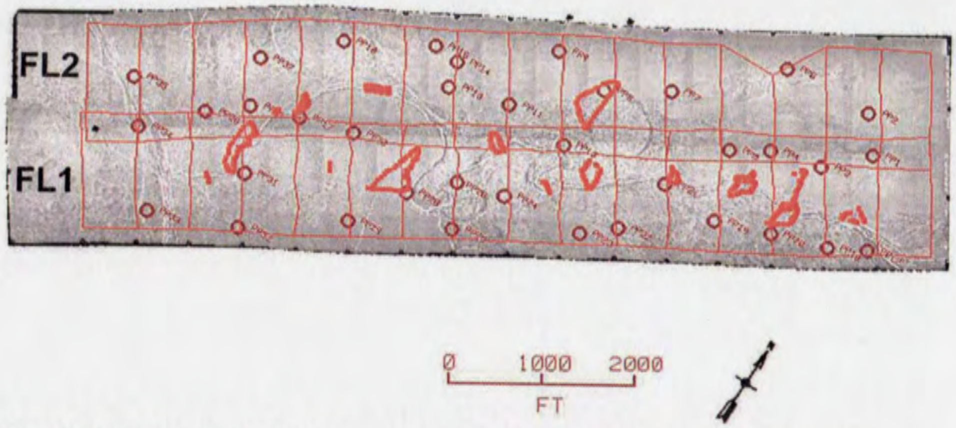


Figure 2-10 Distribution of check points along 2000 project flight lines and control. Base map shows sterero models and photo panel points as in **Figure 2-1**. Bands of red dots are traverses where GPS check points were collected.

the observations was about 0.01 ft (0.003 m) (**Figure 2-11a and 2-11b**). A listing of the points collected during the July 2002 survey is contained in Appendix B of this report. Points collected during the August 2000 survey are in Appendix A of this report.

2.2.2 Quality Assessment of the 2000 Photogrammetry Data. Calculated $RMSE_z$ and vertical accuracy measures for the generated surfaces in the detailed study reach are tabulated in **Table 2-1**.These measures indicate that the vertical accuracy of the surface derived from the 1993 AG 2-ft contours mostly did not meet the required values for a 4-ft contour interval map, and was well short of the standards required for a 2-ft contour interval map. Tests of the surface derived from the 2000 BOR 3-ft grid indicate that this surface met the values required for a 2-ft contour interval map, but fell short of values required for a 1-ft contour interval map. Similarly, the portion of the 2003 BOR 5-ft grid that lies within the detailed study reach area met standards for a 4-ft contour interval map, but fell short of values required for a 2-ft contour interval map.

Histograms (**Figure 2-12a and 2-12b**) and scatter plots(**Figure 2-13a and 2-13b**) of the check data show the reduction in scatter achieved by the BOR analyses of the 1993 aerial photography and through use of more detailed photography flown in 2000. Histograms of the elevation differences for the check points are strongly peaked between values of -1 to 1 ft for both the 2000

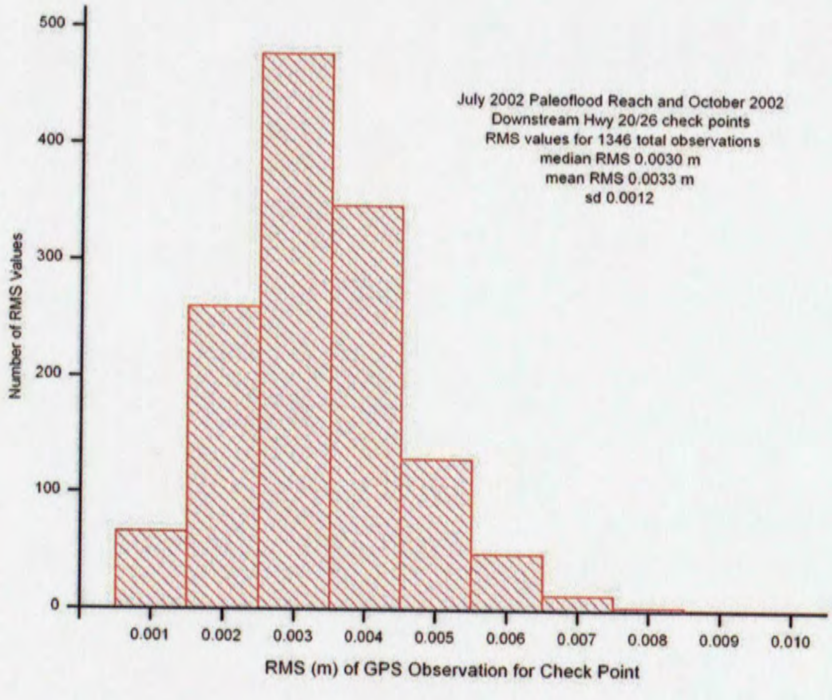


Figure 2-11a Histogram of RMS values from GPS check points in paleoflood study reach and downstream of Hwy 20/26.

BOR 3-ft and 2003 BOR 5-ft surfaces. The 2000 BOR 3-ft surface histogram shows a significant reduction in the number of observations outside this band compared to either surface derived from the 1993 photography.

All three distributions have long tails of negative values. Negative residual values indicate that the modeled surface is above the GPS measured value. At least two aspects of the field survey methodology contribute to increased negative residuals. 1) During the field measurements, the GPS receiver is mounted on a range pole with a sharp tip. This tip typically sinks into the sandy surface soils by as much as 0.2 ft. 2) Several of the check points were deliberately located in

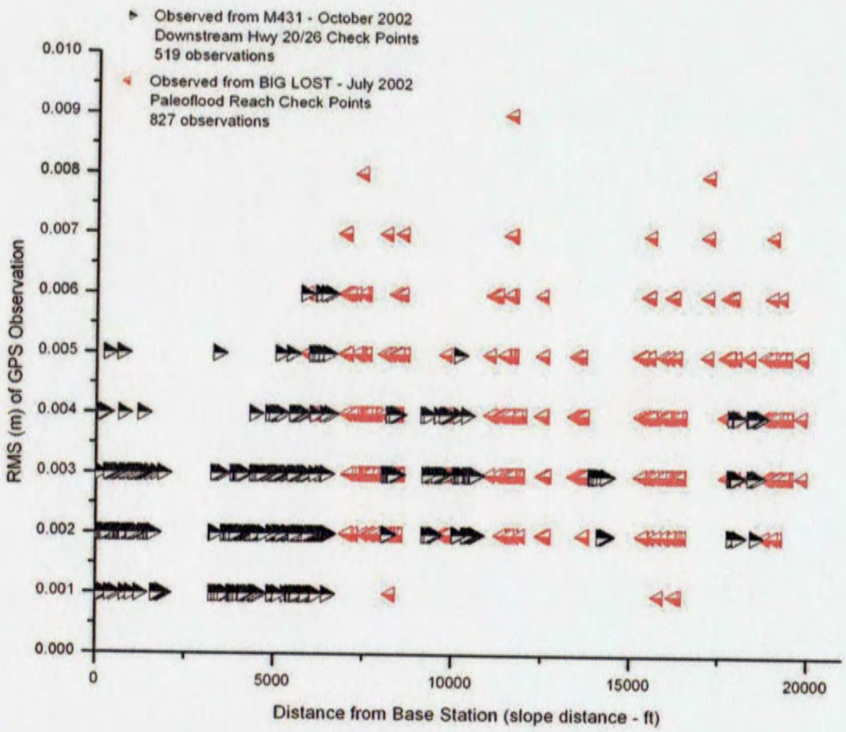


Figure 2-11b Slope distance of GPS observation from base station vs RMS value for GPS check points in paleoflood study reach and check points downstream of Hwy 20/26.

dense stands of sage brush to test whether the automated photogrammetry software was adequately picking ground surface points rather than points on top of brush and grass. This is illustrated by 11 points located at the upstream end of the study reach, INEEL east coordinates of ~258,000 to ~260,000, that have negative residuals between -1 to -3 ft from the 2000 BOR 3-ft grid (**Figure 2-13a**). Median residuals that are slightly negative, -0.1 to -0.2 ft, appear to indicate that in most cases the mapping software was resolving ground surface rather than vegetation. The figures and statistical values show that the distribution for 2000 BOR 3-ft grid is significantly tighter than either distribution based on the 1993 photography. This result is expected due to the

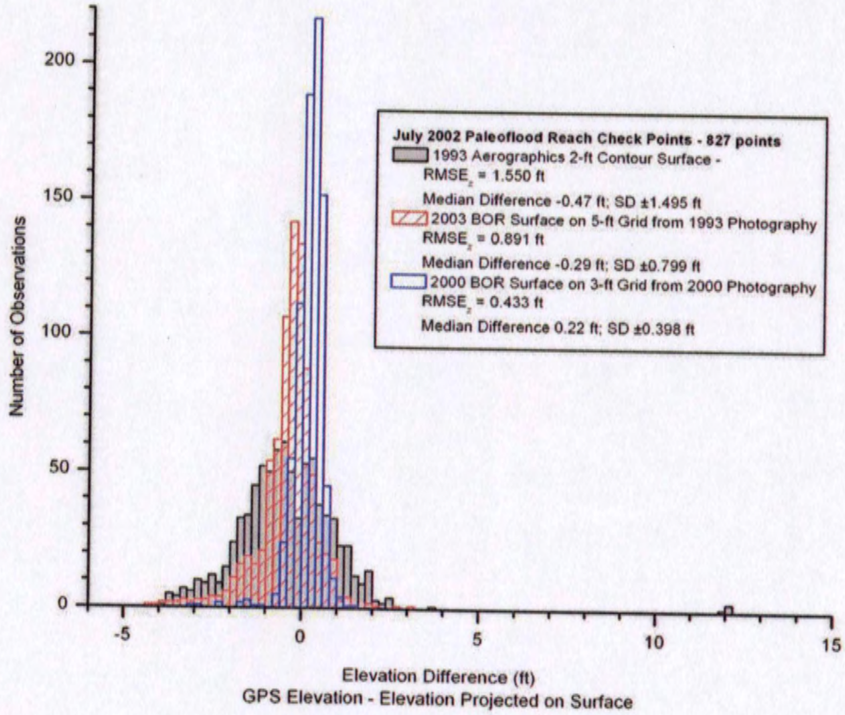


Figure 2-12a Histograms of elevation differences between July 2002 GPS check point observations and projected elevations on generated surfaces in the paleoflood study reach. Note that the mean of the RMS values for the GPS observations is 0.003 m (0.0098 ft) (**Figure 2-11a**).

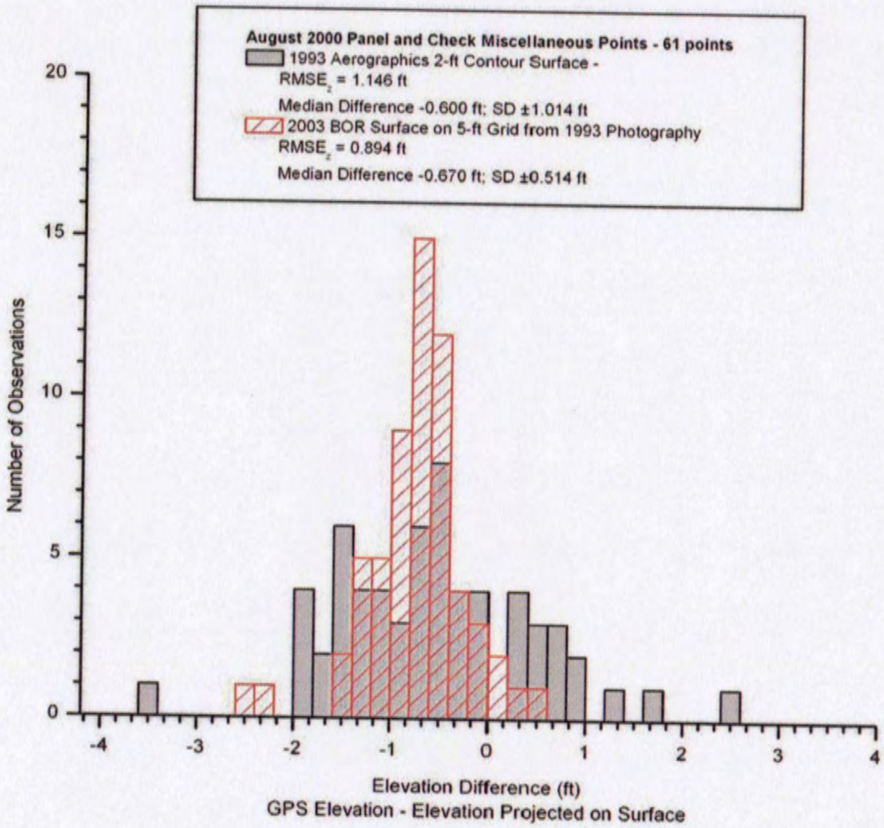


Figure 2-12b Histograms of elevation differences between GPS observations of 2000 project control panel points and projected elevation on generated surfaces. GPS observations of the 2000 project control panel points are not projected to 2000 BOR 3-ft grid surface because these points were used in the generation of that surface. Note difference in scale and number of observations compared to **Figure 2-12a**.

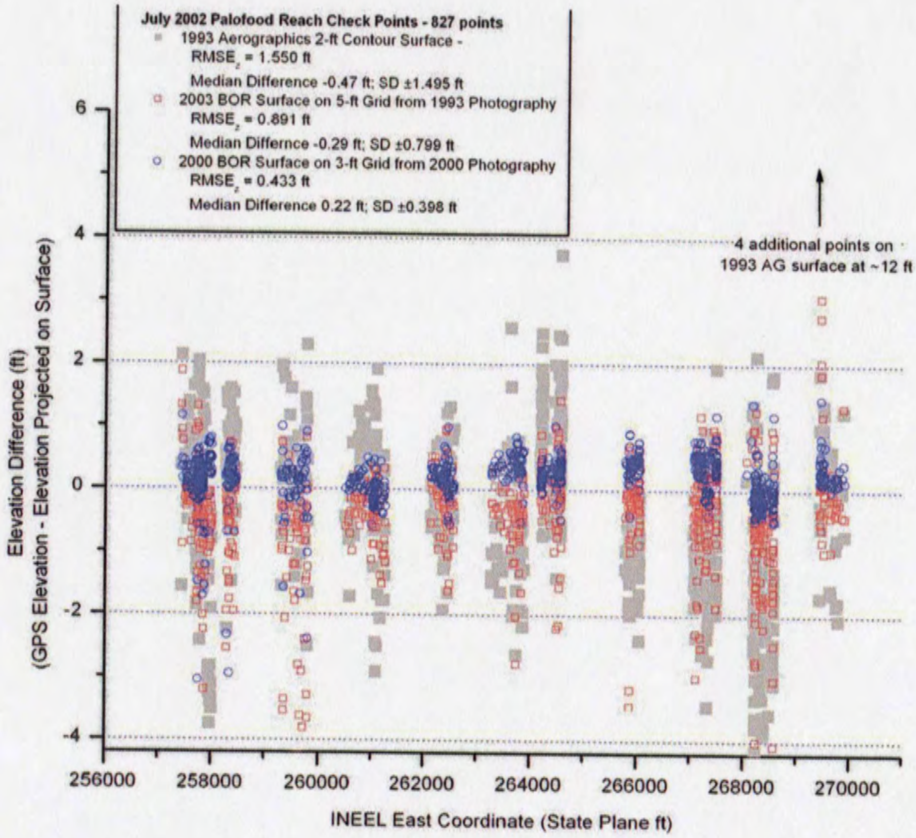


Figure 2-13a Scatter plot of July 2002 check point data for paleoflood reach. State Plane coordinate values increase in a downstream direction. Note the apparent tilt or buckling of the 1993 AG surface in the downstream portion of the study reach. Histogram of same data shown in **Figure 2-12a**.

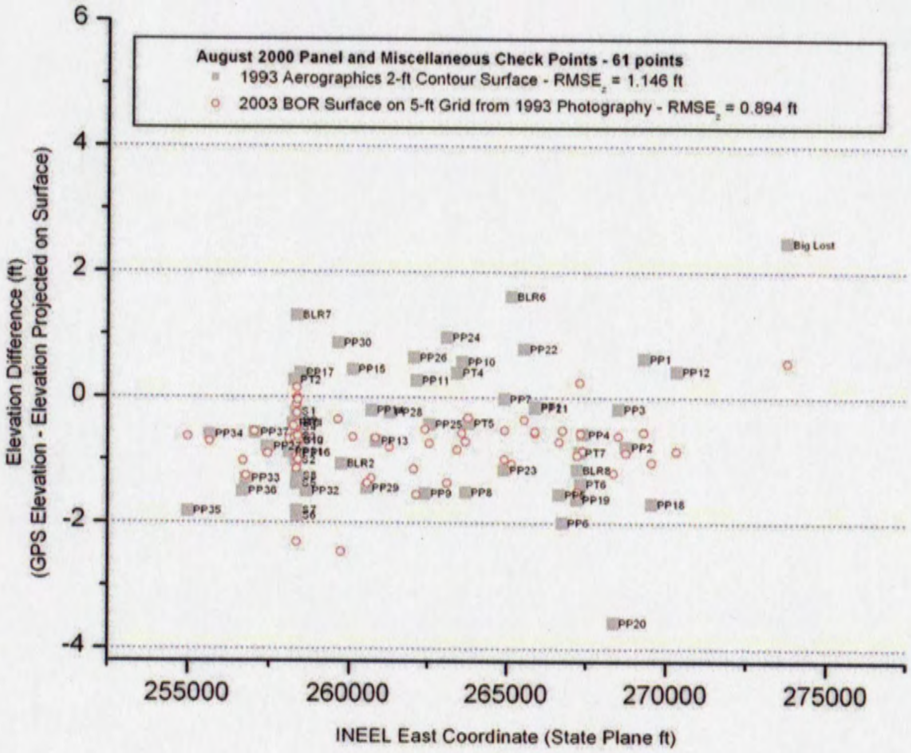


Figure 2-13b Scatter plot of 2000 project control panel points for paleoflood reach. Histogram of same data shown in **Figure 2-12b**.

Table 2-1 Comparison of Measured and Required Accuracy Values for Paleoflood Study Reach Area

Map/Data Source	Measured Values			Required Values				
	NSSDA			NMAS (Accuracy)		ASPR class 1 ($RMSE_Z$)		
	n=	Accuracy	$RMSE_z$	1-ft CI	2-ft CI	1-ft CI	2-ft CI	4-ft CI
1993 AG 2-ft Contours								
Aug. 2000 panel points	61	2.246	1.146					
July 2002 check survey	827	3.038	1.550					
2000 BOR 3-ft Grid				0.596	1.192	0.333	0.677	1.333
July 2002 check survey	827	0.849	0.433					
2003 BOR 5-ft Grid from 1993 Photography								
Aug. 2000 panel points	61	1.752	0.894					
July 2002 check survey	827	1.746	0.891					

Shaded values do not meet NMAS standard or ASPRS class 1 standards for 2-ft contour mapping.

Shaded values do not meet NMAS standard or ASPRS class 1 standards for 4-ft contour mapping.

larger scale of the 2000 photography. However, the median value for the 2000 BOR 3-ft grid is slightly positive, indicating than on average this surface is 0.4 to 0.5 ft lower than the 1993 BOR 5-ft grid surface and the 1993 AG 2-ft contour surface, both of which have small negative residuals. This result is probably an artifact of the process used to transform the 2000 data into State Plane coordinates and adjust the photogrammetry model to the INEEL elevation datum (**Section 2.2.1**). Based on the small dispersion of the data for the 2000 BOR 3-ft grid points shown in **Figure 2-12a and 2-13a** it appears that if the datum adjustment were modified slightly, the 2000 BOR 3-ft grid would meet accuracy standards for 1-ft contour interval maps.

Inspection of **Figure 2-13a** indicates that differences between GPS-measured elevations and the 1993 AG 2-ft contour surface are not uniformly distributed throughout the study reach. Overall, differences in the downstream portion of the study reach appear to be more negative, suggesting a possible local tilt or warping of this surface within the study reach.

3.0 TOPOGRAPHIC DATA FOR FLOOD-INUNDATION MODELING DOWNSTREAM OF THE INEEL DIVERSION DAM

A major objective of the current studies is to estimate probabilistic stage-discharge relationships for Big Lost River floods at critical INEEL facilities at TRA and INTEC. Both experience gained from previous hydraulic modeling studies at the INEEL and DOE standards indicates that this task requires both detailed topographic data and two-dimensional hydraulic modeling. For regulatory applications that are intended to meet FEMA 37 guidelines, topographic data must meet APSRS class 1 standards for 4-ft contour mapping.

In the current BOR modeling effort, topographic data is required for input to two-dimensional modeling along the Big Lost River that extends from the INEEL Diversion Dam to downstream of INTEC and TRA (**Figure 1-1 and 1-2**). Previous studies have noted that due to the complex topography along the Big Lost River at the INEEL, that two-dimensional models would be required to adequately address the routing of flood flows.

In a previous study, Koslow and Van Haaften (1986) used the one-dimensional model DAMBRK to model hypothetical failure scenarios for Mackay Dam, located about 45 miles upstream of the INEEL. Their model extended a total of 82 miles along the Big Lost River and used topography based on 45 cross sections input from USGS topographic quadrangle maps. Contour intervals on these maps were either 10- or 20-ft. They noted (p. 29) that: "The complex Big Lost River flood-plain geometry causes the physical system to depart from the one-dimensional assumptions made in DAMBRK."

The previous USGS study by Berenbrock and Kjelstrom (1998) also used a one-dimensional model to map the limits of the estimated 100-yr peak flow on the Big Lost River at the INEEL. That study used 28 field surveyed cross sections between the INEEL Diversion Dam and the Big Lost River Sinks, some of which were extended based on the 1993 AG 2-ft contour map. They also recommended application of a two-dimensional model due to the considerable uncertainty associated with how flowpaths are distributed laterally during a flood on the Big Lost River.

3.1 1993 INEEL 2-ft Contour Map

The initial BOR plan of study for hydraulic modeling on this reach was based on the assumption that the existing 1993 AG 2-ft contour map had adequate quality control and that satisfactory grids could be developed from the 2-ft contours. As discussed below, initial renderings of grids derived from these contours were not satisfactory and it was decided to reprocess the aerial photography data to obtain a gridded output.

Data that were obtained from INEEL on the 1993 aerial survey package consisted of the original black and white film, the camera calibration report, several sheets of panel control coordinates in various map projection systems and the flight plan map with panel locations. No documents describing the process used to generate the contour maps or quality assurance efforts associated with those maps were recovered. Assumptions required to proceed included: 1) control surveys were accurate, 2) flight paths adhered to standard practice, and 3) film quality would be acceptable for scanning. One immediate area of concern was the existence of several different sets of control panel coordinates with no clear indication of which set had been used in the final map production. Discussions with several INEEL staff with knowledge of the 1993 flights and map production indicated that problems with control had been recognized at the time the maps were produced, but no formal documentation of the process was recovered. However, based on these discussions, it appeared that surveying and control issues had been resolved to a sufficient accuracy that the data were still useful and could be reprocessed to produce the desired output.

3.1.1 Initial Topographic Grids from the INEEL 2-ft Contour Map. The 2-ft contour data was obtained from the INEEL Spatial Analysis Laboratory and assembled to produce a coverage of the 2000 study reach area. A three-ft gridded surface was developed from the contours using Intergraph Terrain Analyst and rendered in shaded relief. Inspection of these grids showed that the resultant grids were strongly faceted and stepped, apparently as a result of the need to interpolate data between contours in areas of relatively low slope (**Figure 3-1**). These initial grids were presented and discussed in early November 2001 with DOE and INEEL staff, and the Participatory Peer Review group engaged to provide oversight on the BOR flood hazard studies. It was the consensus of the group that the numerous facets and linear features that developed from interpolation of the 2-ft contour data into gridded data resulted in unsatisfactory

Figure 3-1 Big Lost River channel area in paleoflood reach rendered as 5-ft grid from 1993 AG 2-ft contour map. Note lack of detail on surface in lower right corner and faceted surfaces that extend across channel in bottom center and near double bend near photo center.

inputs for the planned hydraulic modeling efforts. Following this meeting, it was determined that if control information, film and other required information could be recovered, that the 1993 aerial photography could be reprocessed with a modern photogrammetric system and satisfactory topographic grids developed as input to the hydraulic models.

3.2 Updated Photogrammetry for the 1993 Aerial Photography

Photogrammetric analyses of the 1993 aerial photography was done using a ZI Imaging Imagestation softcopy system. This system runs under a Windows-based operating system on a dual Intel processor workstation. The ZI applications consist of 5 modules, 1) ImageStation Project Manager, 2) ImageStation Stereo Display, 3) ImageStation Feature Collection, 4) ImageStation AeroTriangulation, and 5) ImageStation Raster Utilities. These modules run with Bentley MicroStation Graphics for the user interface and for outputting results. ZI Terrain Analyst

Software (currently Intergraph MGE Terrain Analyst) was used to generate the grid surfaces, perform edge matching, and to analyze the photogrammetric outputs for quality control.

3.2.1 Control. Three sets of control points were obtained from INEEL along with the 1993 aerial photography. The first was in State Plane NAD83 Idaho Central Zone system with a handwritten note of "new 4/8/94". The second was State Plane NAD27 Idaho Central Zone with "Central" crossed out and "East" handwritten underneath the zone designation. The third was titled "Control For INEL Flood Study" and was labeled as State Plane NAD27, NGVD29, Idaho East Zone. After discussion with INEEL personnel, the third set was taken to be the final coordinates generated for the project. Copies of these coordinate data are included in Appendix C of this report.

A remaining issue was whether these coordinates were pre-aerotriangulation coordinates or converted coordinates. Because there was little documentation of the 1993 aerial survey, the assumption was made that these coordinates could be used in the aerotriangulation process. This is not the preferred method because the State Plane system is based on a sea level definition and the INEEL site is located at about 5000 feet elevation. When using the State Plane coordinates, there is an error introduced of approximately 1 part in 10,000 across the site since the coordinates are being projected from sea level. Across the entire study area, this creates a horizontal error of approximately 10 feet. Over a distance of 20 miles, this is not significant when working with topography for studies of this magnitude. However, this error will impact the aerotriangulation results. The coordinates were weighted and adjusted and a solution was obtained that was satisfactory for the study.

Figure 3-2a to **Figure 3-2d**, show the layout of flight lines and control panels for the portion of the 1993 photography that was reanalyzed. **Table 3-1** lists the photograph numbers that were included in the updated photogrammetry analyses from each flight line. Flight lines 1 through 8 comprise the Box Canyon study area and are discussed separately in **Section 4.0.**

3.2.1.1 Field Checking of 1993 Control Information. The initial aerotriangulation solutions for the 1993 photography indicated that there were several stress points in the panel control network despite repeated reanalyses. These problems suggested that the accuracy of the

Figure 3-2a Flight line and control panel layout for 1993 flight lines 1-8. The Big Lost River flows from the upper left corner approximately through the center of each flight line. The USGS gaging station on the Big Lost River near Arco is located on FL1. Box Canyon extends across FL2 through FL5.

photopanel control coordinates was a potential issue. Field checking of these control points was done in February 2003 to verify the accuracy of the control information being used in the analyses. During this investigation, it was found that rebar had been placed at the center of the 1993 photopanels, and although the panels were no longer present, the rebar could generally be relocated in the field. The State Plane Coordinates were used to seek out the site, and at many sites the rebars were found either flush with the ground surface or protruding by a small amount. Most often, rebar was hidden by vegetation or small amounts of soil and was located with a metal detector. Fifty-five of the 144 photopanel points in the study area were reoccupied during this survey and checked using RTK GPS. Mean RMS of these observations was about 0.01 ft (0.003 m) (**Figure 3-3 and 3-4**). At 3 other sites, the approximate coordinates were field verified by

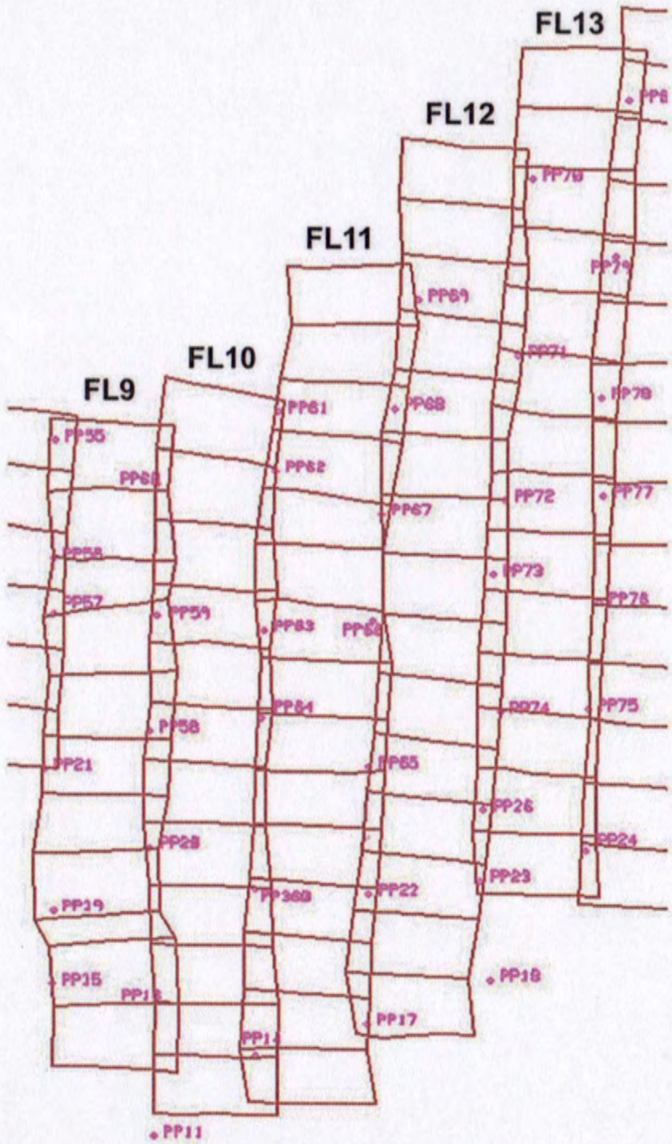


Figure 3-2b Flight line and control panel layout for 1993 flight lines 9-13. The Big Lost River flows from the left center edge on a diagonal to the right edge. The INEEL Diversion Dam is located along FL10. Paleoflood study reach is located on FL10 and FL11. RWMC is located on FL12 to FL13.

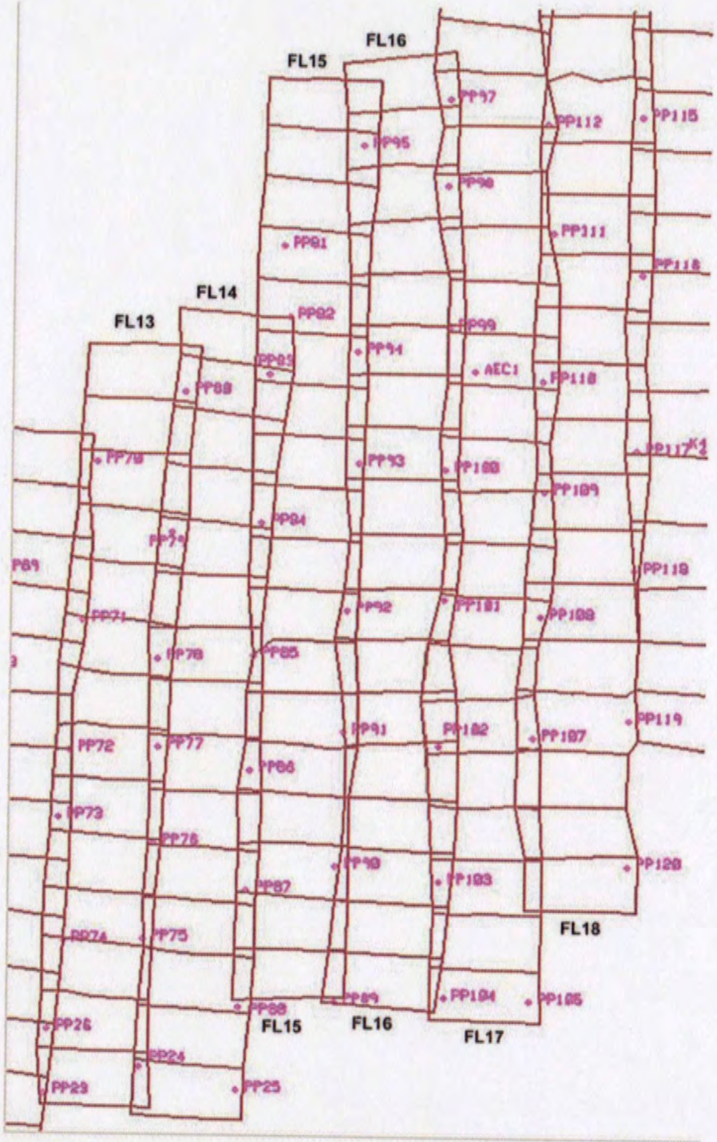


Figure 3-2c Flight line and control panel layout for 1993 flight lines 13-18. The Big Lost River flows from left to right from the lower left center to the upper right center edge. The Big Lost River Rest Area is near the center of FL13. TRA is on FL16. CFA is on the lower part of FL17. INTEC is on FL18.

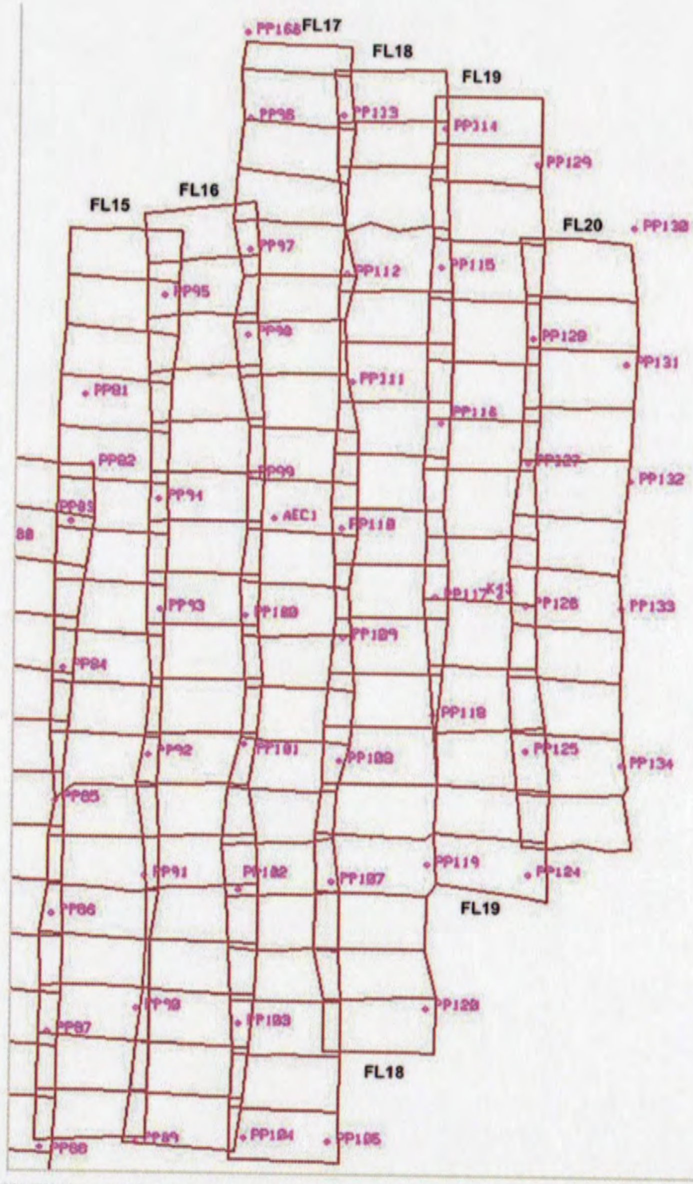


Figure 3-2d Flight line and control panel layout for 1993 flight lines 15-20. The Big Lost River flows left to right from the left center diagonally to the upper right corner. Lincoln Boulevard extends from the bottom of FL16 to near the center, then diagonally across to the top of FL19. TRA is on the upper part of FL16. INTEC is on the lower part of FL18 just up from the lower end of FL19.

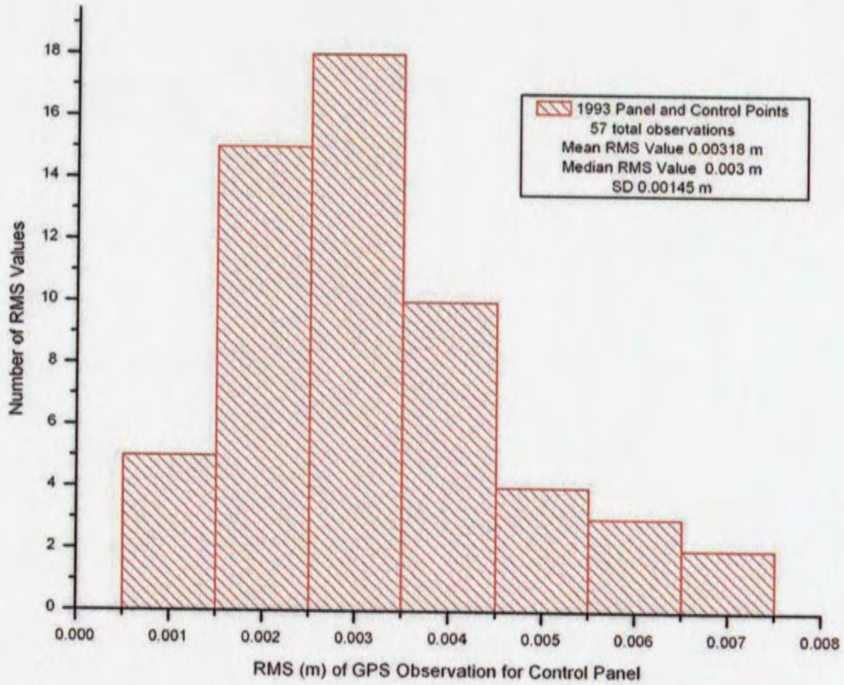


Figure 3-3 Histogram of RMS values for February 2003 survey of 1993 AG control panels.

inspection of the features visible on the aerial photography, but rebar at these sites had either been disturbed by grading or the rebar could not be relocated. February 2003 survey data for the 55 resurveyed points are tabulated in Appendix D of this report. Original coordinates for these points are included in Appendix C of this report. **Figure 3-5** is a histogram of the differences between elevations associated with 1993 AG mapping and GPS reoccupations of the rebars in February 2003. NSSDA horizontal and vertical accuracy (equations 1-1 and 1-2) of the 55 points that were resurveyed was found to be 0.3608 and 0.48000 ft, respectively. These values are substantially larger than the ~0.01 ft total RMS (x, y, z combined) of the GPS observations used to check the control points (**Figure 3-3 and 3-4**). Control points are directly incorporated into the photogrammetric models used to generate contours and grids and as such cannot be directly used as independent check points. However, if these points are considered analogous to spot elevations on the maps, the maps would meet class 1 ASPRS standards for map scales of 1:480 or smaller

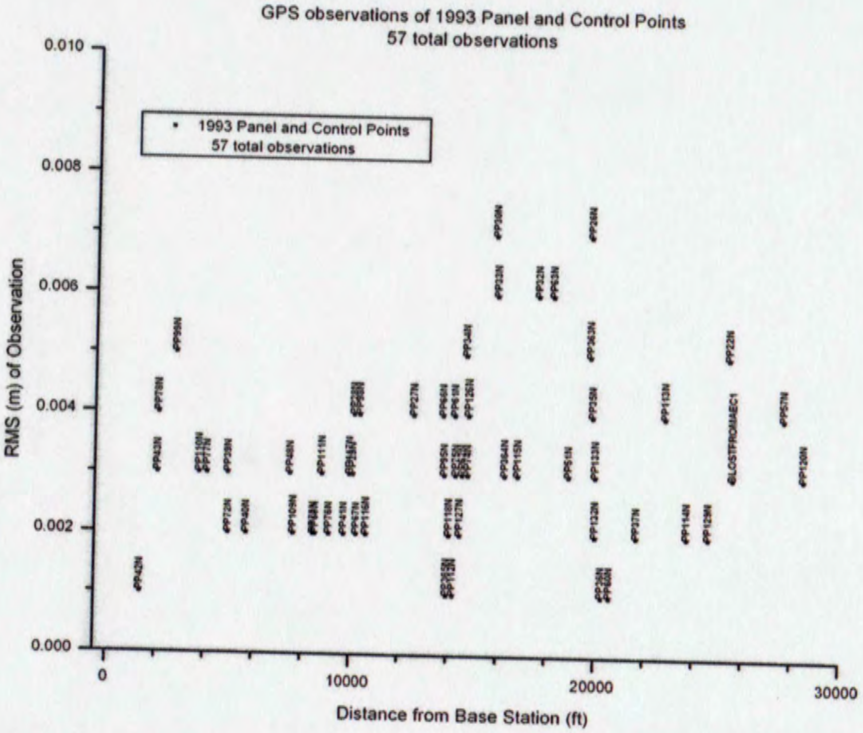


Figure 3-4 Distance from base station vs. RMS on observation for 1993 panel points checked in February 2003.

and a 3-ft contour interval. As noted in **Section 3.1**, the underlying accuracy of the control network is a primary limiting factor on the accuracy of the photogrammetric analyses that follows. Accuracy of the resultant maps and grids can not exceed the initial accuracy of the control network.

3.2.2 Aerial Photography. The 1993 aerial survey was flown by Aerographics. The flying height was 5000 feet above mean ground surface producing a 1:10,000 photo scale. The camera calibration report was obtained from INEEL and is included in Appendix C of this report. The original black and white film roll was obtained from INEEL and scanned by Genysis International in Denver. The scanning specifications were to produce 14 micron resolution images

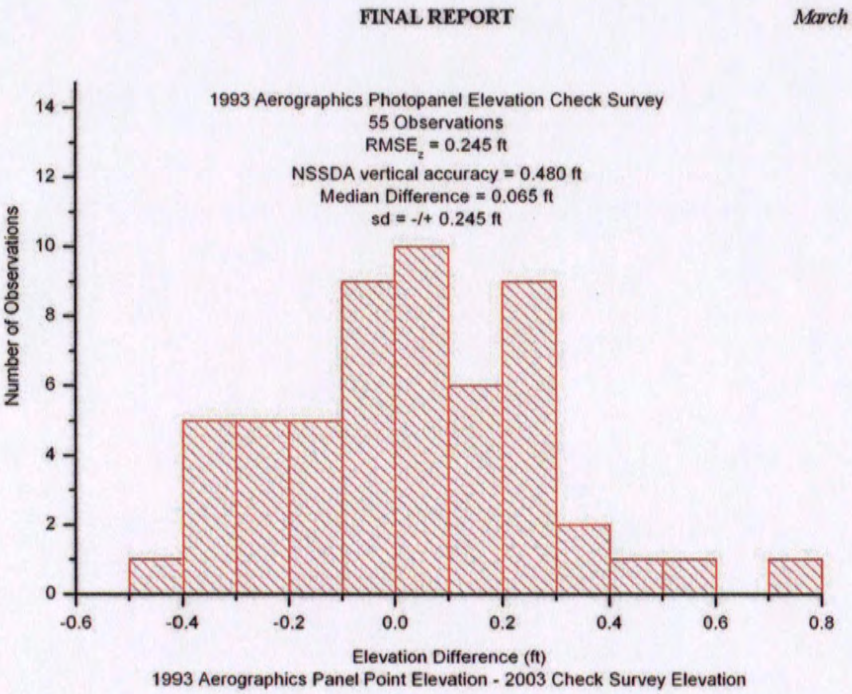


Figure 3-5 Histogram of observed elevation differences for control points.

in Intergraph JPG compressed file format with a Q factor of 15. For this phase of the project, a total of 252 scanned images from flight lines 1-20 of the 1993 flight were input (**Figure 3-2a** to **Figure 3-2d**). As tabulated in **Table 3-1**, additional images in these flight lines, but outside the area of interest for the flood modeling were not included.

3.2.3 Photogrammetric Processing. The processing was comprised of the following steps and was similar to the process used for the 2000 project described in **Section 2.1.3** and illustrated in **Figure 2-4** through **Figure 2-8**:

1) Establish and enter control and coordinate information for the project. This includes projection, coordinate and camera parameters. Input scanned images of photography and control panel points.
2) Perform Interior Orientation. There is an automated feature in the software that

Table 3-1 1993 photographs used in BOR photogrammetric analyses.

Flight line number	Starting photograph number	Ending photograph number	Total number of photographs included in reanalyses	Photographs not used in reanalyses
1	1	5	5	6.
2	1	9	9	n.a.
3	2	9	8	1, 10
4	1	7	7	n.a.
5	1	8	8	n.a.
6	1	7	7	n.a.
7	1	7	7	8
8	1	7	7	n.a.
9	5	17	13	1-4
10	5	17	13	1-4
11	5	20	16	1-4
12	7	22	16	1-6
13	1	15	15	16
14	1	16	16	n.a.
15	1	19	19	20-21
16	2	20	19	1, 21-39
17	1	23	23	24-37
18	3	21	19	1-2, 22-36
19	6	18	13	1-5, 19-39
20	7	18	12	1-6, 19-41
Total Number of Photographs Used			252	Total Not Used - 138

NOTES: The 1993 flight included 12 additional flight lines north and east of the area included in the BOR analyses.
n.a. indicates that all photographs in a flight line were included in the BOR analyses.

orients each photo by fiducial marks (see **Figure 2-4** for example).

3) Perform Relative Orientation and Absolute Orientations and run bundle adjustment for the project. Automated aerotriangulation software was used to generate the relative and absolute orientation solution and then to perform a bundle adjustment for the project (see **Figure 2-5** for example). This solution indicates problem areas and inaccuracies in

control points within the models. These problems are fixed and the iteration repeated until an adequate solution is obtained. During this process it was found that small gaps existed between some flight lines, several lines were not straight, and that many stress points existed within the control network. Several iterations were required to achieve an adequate solution at this stage.

4) Digitize breaklines of identified features. Important features are digitized in three dimensional space (stereo-viewing) to ensure accuracy with respect to the grid surface. Examples of digitized features to which breaklines are applied are the top and bottom of the stream channel banks or other sharp topographic features, buildings, bridge abutments, road surfaces, levees and dams (see example in **Figure 2-6**).

5) Model buildings on site. Buildings on the site were modeled by digitizing the roofs of each structure. The roof was then projected straight down onto the grid surface to form the ground pad. The ground pad was defined as an obscure area to generate an edge at the intersection of the building with the ground. The roof was then added as a graphic feature and a model of the building was then generated within the grid surface.

6) Run automatic elevation grid software at five foot intervals on all stereo models. After all features were digitized within the project, the automated grid elevation software was run. This software produced grid surfaces of all of the stereo models (see **Figure 2-7** for example). Smoothing factors, breakline features, edge definition and model parameters were established with a five foot grid definition. The grids were output into Bentley Microstation CAD software.

7) Perform edge matching of grids between flight lines. Once the grids were generated for each stereo model, edge matching was performed to generate seamless surfaces across the models (example in **Figure 2-8**). The grids were then merged together by flight line. Final grids from the 1993 photography have 5-ft postings.

Grid data that are input into BOR two-dimensional flood models are first rotated to minimize the number of generated points relative to the area that will be modeled and then output in ASCII file format along with projection and unit information. For other analyses, the gridded data are rendered as needed using Bentley Microstation or output in formats compatible with other software as needed.

3.3 Quality Issues With the Updated 1993 Photogrammetry Data

Numerous problems emerged during the photogrammetric analyses using the 1993 aerial photography and control data which required multiple attempts at reanalyses with revised inputs and parameters. Resolution of these problems proved complex and time-consuming, but the final grids appear to be of satisfactory quality for the hydraulic modeling needs. The primary causes of these problems appear to reside with several factors. 1) The number and spacing of the control panels placed for the 1993 flight was sufficient, but left little margin for errors. Thus, one or two survey errors associated with the photopanels could result in significant problems in photogrammetric processing. Field checks of 55 control panels indicated horizontal and vertical accuracies of 0.3608 and 0.4800 ft, respectively (**Figure 3-5**). Accuracy of the control network ultimately limits the accuracy of both 1993 AG and BOR photogrammetric analyses from the 1993 photography to map scales of about 1:480 or smaller and contour intervals of about 3 ft (**Section 3.2.1.1**). Accuracy of the control network appears to be the limiting factor. 2) Locally, there were small gaps and irregularities in the flight lines which created additional stress for the control network. 3) The use of a state plane coordinate system and the 1929/1927 datum over a 20-mile extent of the project introduced distortion due to projection issues. 4) Problems that appeared to be related to scanning may be partly attributed to a scanner error that necessitated that the entire job be redone.

Computed residuals and RMSE for the 2003 BOR 5-ft grid from the 1993 photography are relatively low (**Figure 3-6**), with all residuals (Given - Computed) within a range of ±1 ft. RMSE values are about three times larger than for the 2000 BOR 3-ft grid in the paleoflood study reach (compare **Figure 2-9** and **Figure 3-5**).

Comparisons of rendered images derived from the 1993 BOR 5-ft grid and of 5-ft grids from 1993 AG 2-ft contour map show that the updated photogrammetric analyses yielded images with significant improvement in the resolution of fine topographic detail (**Figure 3-7a and 3-7b**)., The numerous facets and planes that appeared on the renderings of grids derived from the 1993 AG 2-ft contour map are not present in the updated results.

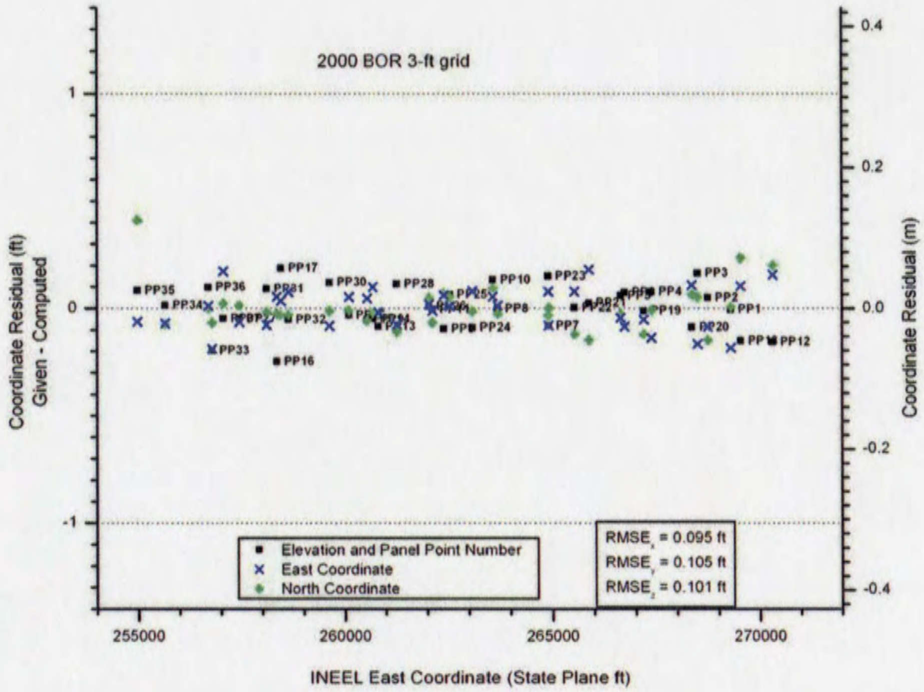


Figure 3-6 Computed residuals and RMSE on control panels for 2003 BOR 5-ft grid from 1993 photography.

3.3.1 Field Checking of the Updated 1993 Photogrammetry Study Area. Accuracy of the topographic grids produced from the 1993 aerial photography was spot checked through comparison with spot elevation data collected through GPS field surveys in July and October 2002. There are 888 points from the paleoflood study reach discussed in **Section 2.0**. An additional 519 points were collected October 3, 2002 from near the Big Lost River Rest Area along Hwy 20/26 downstream to the INEEL Railroad Bridge. Because the intended use of the updated topographic data is for use in flood hazard modeling along the Big Lost River, the

1993 BOR 5-ft grid data

5-ft grid from 1993 AG 2-ft contours

Figure 3-7a Shaded relief image of portion of 1993 BOR 5-ft grid and 5-ft grid derived from 1993 AG 2-ft contour map for area southwest of TRA.

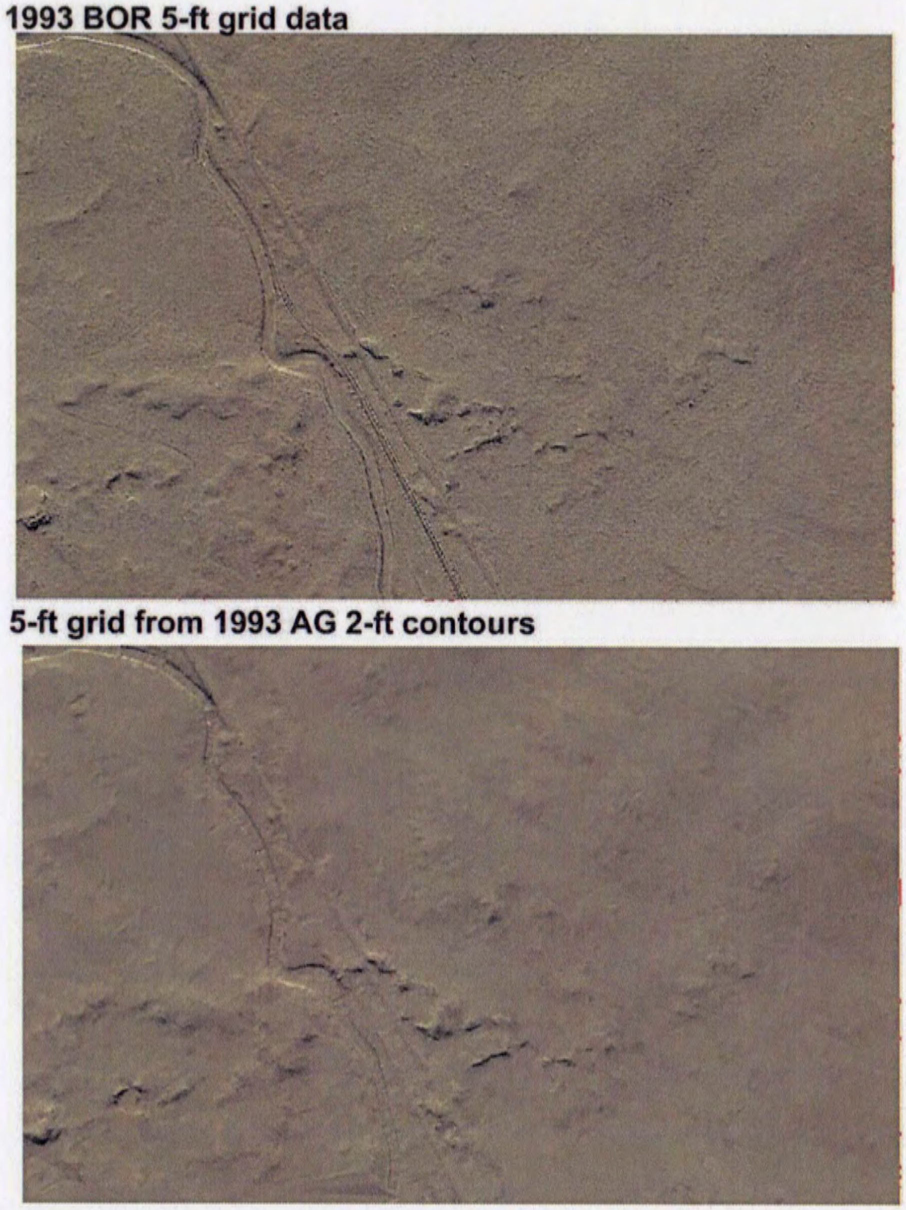


Figure 3-7b Rendered images of 1993 BOR 5-ft grid and 5-ft grid derived from 1993 AG 2-ft contour map for an area just upstream of the INEEL Diversion Dam.

primary focus for acquisition of check data was a ~1-mi-wide corridor along the Big Lost River. The distribution of points through the study reach is shown in **Figure 3-8.**

GPS points were surveyed using the RTK method, similar to those collected for the paleoflood study reach in July 2002 (**Section 2.2.1**). Mean RMS values, x, y, z combined, of these observations were 0.003 m (0.01 ft) (**Figure 2-11a**) and tended to increase slightly with distance from the base station (**Figure 2-11b**). Points downstream of Hwy 20/26 were collected with the base station setup on NGS monument M431, located near TRA. Points upstream of Hwy 20/26 were collected with the base station setup on NGS monument BIG LOST, located north of the Big Lost River near Hwy 20/26. Survey data and a point listing are included in Appendix B of this report.

3.3.2 Quality Assessment of Topographic Mapping from the 1993 Aerial Photography. Field surveys along the Big Lost River corridor between the INEEL Diversion Dam and the INEEL Railroad, a distance of about 10 miles, provide a basis for assessing the vertical accuracy of contour maps and topographic grid data used in flood hazard studies at the INEEL (**Table 3-2**). For the areas checked, these surveys appear to indicate that the 1993 AG 2-ft contour map was close, but generally did not meet ASPRS standards for class 1 mapping as a 4-ft contour map, the standard cited by FEMA 37 for flood hazard mapping. The 1993 AG 2-ft contour map met APSRS, but not NMAS standards for a 4-ft contour interval in the area checked downstream of Hwy 20/26 and near INTEC and TRA. In the areas checked between the INEEL Diversion Dam and Hwy 20/26, most of the data indicates that the 1993 AG 2-ft map does not meet either NMAS or ASPRS class 1 standards. The 2003 BOR 5-ft grid from the 1993 photography meets both NMAS and ASPRS class 1 standards for 4-ft contour maps as required under FEMA 37. Accuracy of the 2003 BOR 5-ft grid appears to meet both NMAS and ASPRS class 1 standards for about a 3-ft contour interval. Measured accuracy values on the 2003 BOR 5-ft grid are generally 25 to 50% better than values measured on surfaces from the 1993 AG 2-ft contour map. The level of accuracy achieved by the 2003 BOR 5-ft grid is approximately that indicated by limited checking of the 1993 control points which were used to create both datasets (**Section 3.2.1.1**). The underlying factor that appears to limit accuracy of topographic data generated from the 1993 photography is the accuracy of the photopanel control network survey

Figure 3-8 Layout of GPS check points along Big Lost River downstream of the INEEL Diversion Dam. A total of 519 individual check points are represented by the red symbols. Points generally follow the trend of the Big Lost River. See **Figure 3-2a** through **Figure 3-2d** for explanation of base map.

Table 3-2 Comparison of Measured and Required Accuracy Values for Areas Downstream of the INEEL Diversion Dam

<table>
<tr><th rowspan="3">Map/Data Source</th><th colspan="3">Measured Values</th><th colspan="4">Required Values</th></tr>
<tr><th colspan="3">NSSDA</th><th colspan="2">NMAS (Accuracy)</th><th colspan="2">ASPRS class 1 ($RMSE_z$)</th></tr>
<tr><th>n=</th><th>Accuracy</th><th>$RMSE_z$</th><th>2-ft CI</th><th>4-ft CI</th><th>2-ft CI</th><th>4-ft CI</th></tr>
<tr><td colspan="4">1993 AG 2-ft Contours
(Paleoflood Study Reach)</td><td rowspan="10">1.192</td><td rowspan="10">2.383</td><td rowspan="10">0.677</td><td rowspan="10">1.333</td></tr>
<tr><td>Aug. 2000 panel points</td><td>61</td><td>2.246</td><td>1.146</td></tr>
<tr><td>July 2002 check survey</td><td>827</td><td>3.038</td><td>1.550</td></tr>
<tr><td colspan="4">(Downstream Hwy 20/26)</td></tr>
<tr><td>Oct. 2002 check survey</td><td>519</td><td>2.446</td><td>1.248</td></tr>
<tr><td colspan="4">2003 BOR 5-ft Grid
(Paleoflood Study Reach)</td></tr>
<tr><td>Aug. 2000 panel points</td><td>61</td><td>1.752</td><td>0.894</td></tr>
<tr><td>July 2002 check survey</td><td>827</td><td>1.746</td><td>0.891</td></tr>
<tr><td colspan="4">(Downstream Hwy 20/26)</td></tr>
<tr><td>Oct. 2002 check survey</td><td>519</td><td>1.586</td><td>0.809</td></tr>
<tr><td></td><td colspan="7">Shaded values do not meet NMAS standards or ASPRS class 1 standards for 4-ft contour mapping</td></tr>
</table>

put in place at the time of the photography. Detailed notes and records on these surveys do not appear to be available.

Histograms and scatter plots for data from the paleoflood study reach collected in August 2000 and July 2002 are shown on **Figure 2-11a, 2-12b, 2-13a and 2-13b**. Data from the October 2002 survey, which covered the area downstream of Hwy 20/26 to the INEEL railroad bridge on the Big Lost River are shown on **Figure 3-9a and 3-9b**.These data demonstrate a significant improvement in the accuracy of the 2003 BOR 5-ft grid from the 1993 photography as compared to grid surfaces derived from the 1993 AG 2-ft contour map.

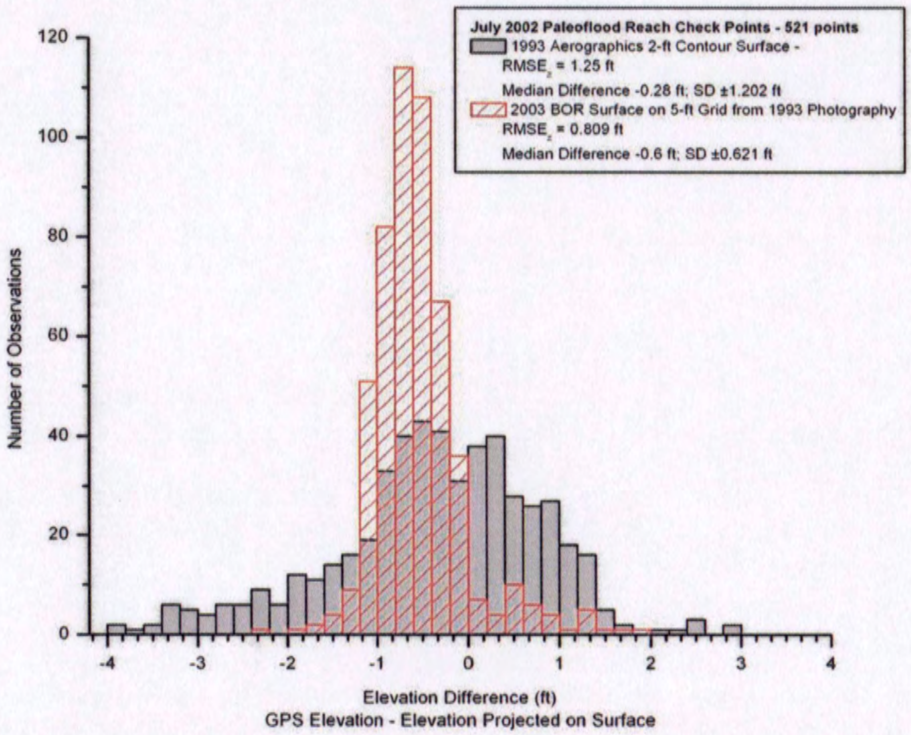


Figure 3-9a Histogram of GPS check data collected along the Big Lost River corridor between Hwy 20/26 and the INEEL Railroad bridge.

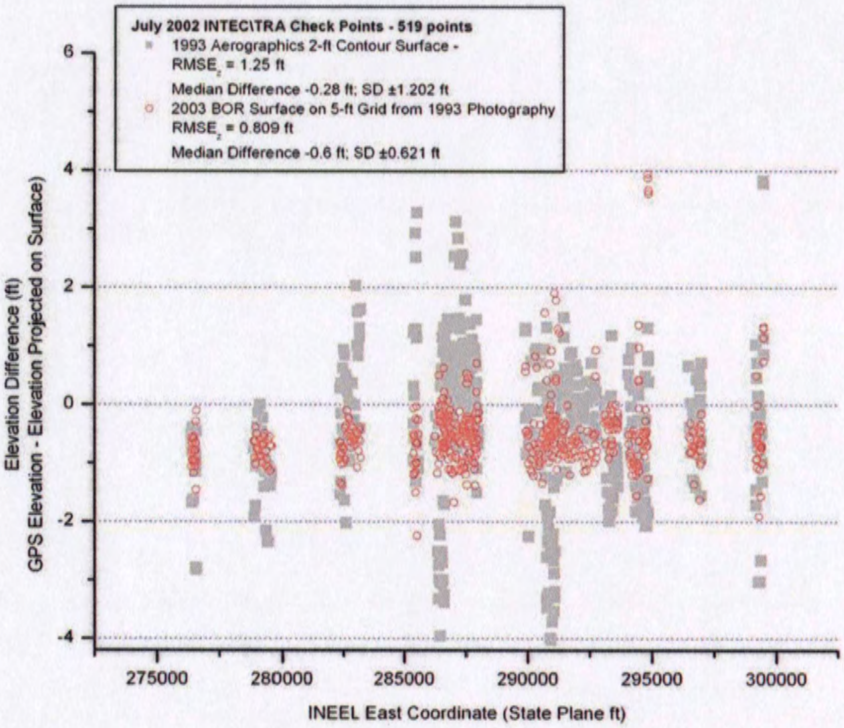


Figure 3-9b Scatter plot of GPS check data collected along the Big Lost River corridor between Hwy 20/26 and the INEEL Railroad bridge.

4.0 TOPOGRAPHIC DATA FOR THE BIG LOST RIVER - BOX CANYON AREA

Flight lines 1-9 of the 1993 aerial photography extend upstream of the INEEL Diversion Dam along the Big Lost River nearly to Arco. Two foot contours were developed by Aerographics from these photographs to the INEEL boundary, but data available from the INEEL Spatial Analyses Laboratory outside the INEEL boundary is derived from USGS 7-1/2 minute quadrangles. Because future flood hazard analyses at the INEEL will likely require additional hydraulic modeling through this area, it was decided to include these flight lines upstream of the INEEL Diversion Dam in the photogrammetry reanalyses of the 1993 data. By including this area with the initial processing, it was felt that a relatively uniform quality product could be extended through this area as well. However, it was recognized that the steep canyon walls of Box Canyon presented several problems and that special treatment of this area would be required. To partially mitigate these problems, field surveys of breaklines through the canyon reach were incorporated into the initial work plan. This was facilitated by the lack of streamflow in 2002 which resulted in a dry streambed through the canyon and relatively easy access. The streambed was also dry at the time of the 1993 aerial photographs.

4.1 Updated Photogrammetry for the 1993 Aerial Photography

Background on the aerial photography, control, and photogrammetric processing of the data for the Box Canyon area are outlined in **Section 3.1 and 3.2. Figure 3-2a** shows the layout of flight lines and control used for the processing in the Box Canyon area. Photogrammetric analyses for the Box Canyon area were done in conjunction with analyses of the main INEEL study area.

4.1.1 Supplemental Breakline and Ground Point Data within Box Canyon. As noted above, to increase the accuracy of the photogrammetric results through the steep and narrow reaches of Box Canyon, additional ground points were surveyed through the canyon that could be incorporated into the photogrammetric model. In order to GPS survey points on the floor of Box Canyon, it was necessary to locate base control stations along the rim in order to obtain and maintain radio link between the rover and the base station. In conjunction with K. Beard, INEEL surveyor, a network of rim stations was established by using two known INEEL monuments and doing a static survey of the new points. Once these new points were in place, a

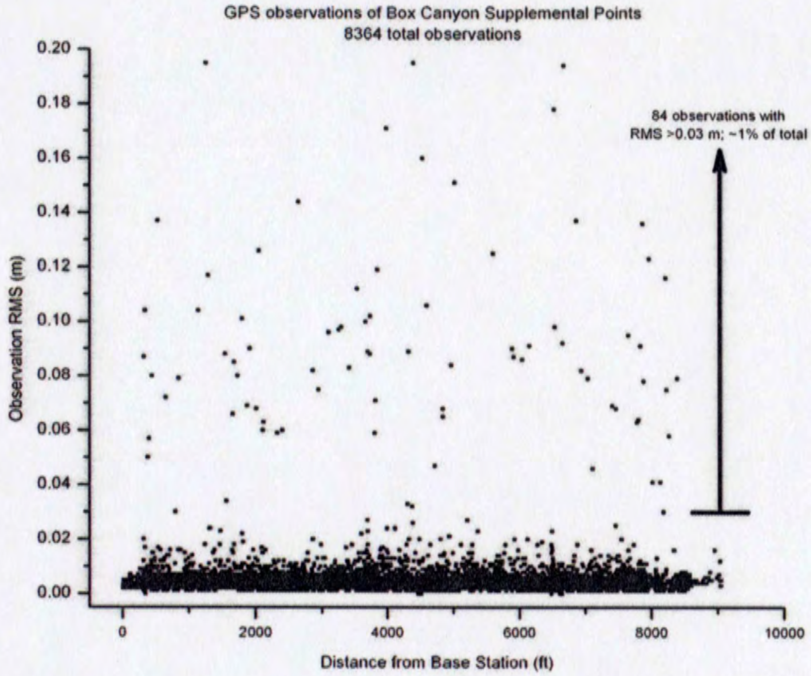


Figure 4-3 Distance from base station vs. RMS of observation for Box Canyon supplemental points. See **Figure 4-1 and 4-2** for histogram of these data.

The photogrammetric processing of the Box Canyon area was done in conjunction with the main INEEL study area. Accuracy measures for the photogrammetric processing were discussed in **Section 3.3.2**.

4.2 Quality Assessment Conclusions for the 2003 BOR 5-ft Grid Derived from the 1993 Aerial Photography in the Box Canyon Area

No formal assessment of the topographic grids generated through the Box Canyon area has been conducted. Overall, it is likely that the general accuracy of the final grids is similar to those produced downstream of the INEEL Diversion Dam and discussed in **Section 3.0**. The resultant grids clearly depict considerably greater detail and finer topographic features than are evident on

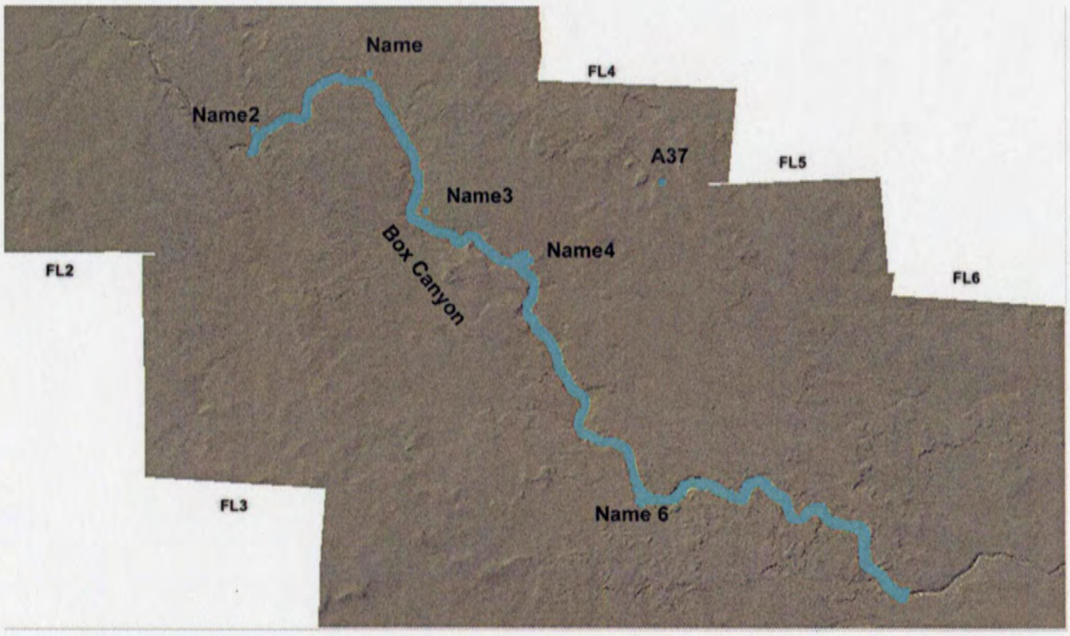


Figure 4-4 Supplemental ground points and control network in the Box Canyon area. Base map is rendered image of BOR 5-ft grid. Blue points are supplemental ground points and control network points (labeled)

Figure 4-5 Oblique view of segment of Box Canyon with supplemental points. See **Figure 4-4** for explanation

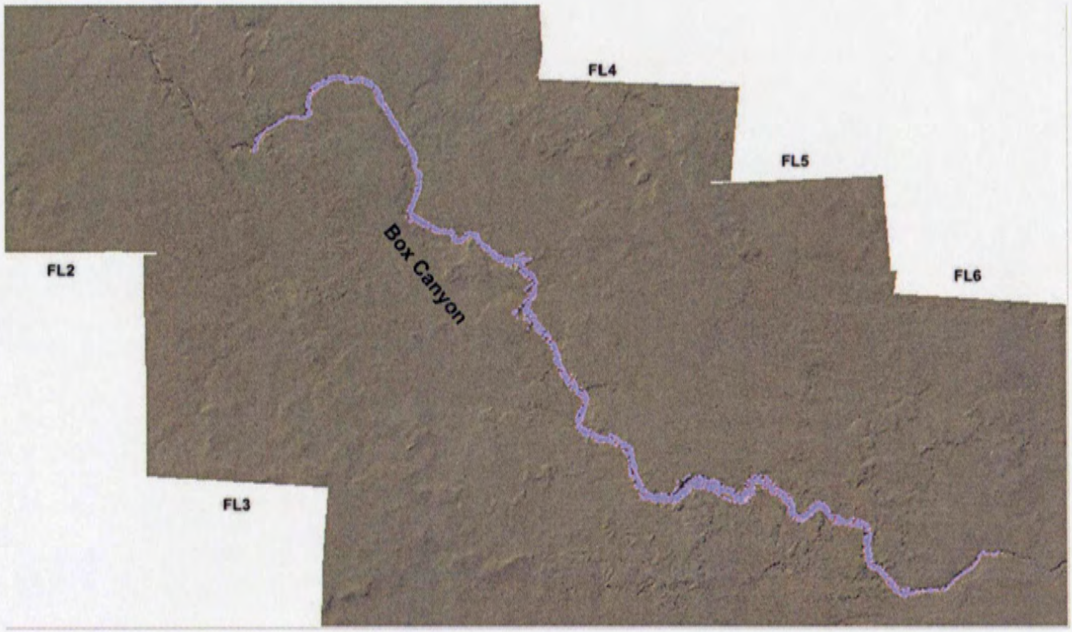


Figure 4-6 Breaklines added for photogrammetric processing of the Box Canyon area.

Figure 4-7 Oblique view of breaklines on rendered image of segment of Box Canyon.

either the limited areas covered by the 1993 AG 2-ft contour map (**Figure 4-5**) or the USGS quadrangle maps which have 20-ft contours. Areas that are subject to the largest potential inaccuracies are those associated with the local areas of near-vertical slopes and large boulder fields in Box Canyon. A few of these areas are cast in shadow in the 1993 photography and the gridding process is dependent on interpolation and the supplemental grid points surveyed through these areas. In other areas, the canyon floor, river channel, or small areas of side slopes consist of 3- to 15-ft boulders and rock blocks separated by gaps and spaces of variable sizes. Both of these effects were mitigated to some extent through incorporation of GPS-measured breakline points into the gridding process. Depiction of an accurate ground surface through these areas will inevitably require some judgement and averaging and there is uncertainty in such areas in the photogrammetric processing results.

Horizontal accuracy appears to be high because the sinuous and complex topography of the canyon overlays all existing mapping without adjustment. Both control and processing of the Box Canyon area were done in conjunction with the remainder of the INEEL study area and horizontal errors appear to be similar (see **Section 3.3** and **Figure 3-6**.

1993 BOR 5-ft grid data

5-ft grid from 1993 AG 2-ft contours

Figure 4-8 Oblique shaded relief images of downstream portion of Box Canyon in area covered by 1993 AG 2-ft contour map.

5.0 REFERENCES

American Society for Photogrammetry and Remote Sensing (ASPRS) Specifications and Standards Committee, 1990, ASPRS accuracy standards for large-scale maps: Photogrammetric Engineering and Remote Sensing, v. 56, no. 7, p. 1068-1070.

Berenbrock, Charles, and Kjelstrom, L.C., 1998, Preliminary Water-surface elevations and boundary of the 100-year peak flow in the Big Lost River at the Idaho National Engineering and Environmental Laboratory, Idaho: U.S. Geological Survey Water Resources Investigations Report 98-4065, Boise, ID, 13 p. and 1 plate.

Federal Geographic Data Committee, 1998, Geospatial Positioning Accuracy Standards, FGSC-STD-0007.3-1998: Washington, D.C., Federal Geographic Data Committee, 25 p.

Koslow, K.N., and Van Haaften, D.H., 1986, Flood routing analysis for a failure of Mackay Dam: EG&G Idaho, Inc., report prepared for the U.S. Department of Energy, contract no. DE-AC07-76IDO1570, 33 p.

Ostenaa, D.A., Levish, D.R., Klinger, R.E., and O'Connell, D.R.H., 1999, Phase 2 paleohydrologic and geomorphic studies for the assessment of flood risk for the Idaho National Engineering and Environmental Laboratory, Idaho: Bureau of Reclamation, Geophysics, Paleohydrology and Seismotectonics Group, report 99-7, 112 p., 4 appendices.

US Army Corps of Engineers (USACE), 1994, Topographic Surveying, Engineer Manual, Chapter 2, Topographic Accuracy Standards: EM 1110-1-1005, 31Aug 94, US Army Corps of Engineers, Washington, D.C., 12 p.

US Bureau of the Budget, 1947, United States National Map Accuracy Standards, rev. June 17, 1947, 1 p.

APPENDIX C

PEER REVIEWER COMMENTS AND RESOLUTIONS

Review comments are presented in alphabetical order of the reviewer.

1) Dr. Vic Baker
2) Dr. Klaus Jorde
3) Dr. Manu Lall
4) Mr. Chris Martin
5) Dr. Glenn Thackray

DOCUMENT REVIEW RECORD

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Victor R. Baker Org: University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
	See text below table	Overall comments are embedded in the text below table - Specific recommendations in this text are flagged with red numbers and discussed below.	The general comments do not require specific changes to the draft report except as noted for those listed below. The draft report is being finalized with inclusion of other specific comments. Addressing the research issues are beyond the scope of the current report.	*
1	See text below table	OSL and cosmogenic dating issues	When the trenching was initiated, it was thought that luminescence dating could provide useful for dating of stratigraphic units exposed in the trenches, and in fact many samples were collected, and these are noted on final trench logs and discussed in association with trench/dating discussion. However, as logging and analyses of the trenches proceeded, several factors led us to not proceed. The fine-grained, loess and eolian deposits that overlie the Pleistocene gravel units are <~1 m thick, well within the zone of active soil processes, frost action, carbonate accumulation, roots, and animal/insect burrows. These are all factors that are considered undesirable for this type of dating. Similar issues are present for dating of the limited sequence of Holocene fluvial deposits. Trench exposures in suitable materials were all impacted by similar soil process related issues. Further discussion of these issues is included in Appendix B and Summary Report. Cosmogenic dating of boulders and scoured basalt surfaces would only be applicable to dating of possible outburst flood deposits, not to the Holocene flood history of the BLR in the INEEL study reaches. An additional complication for the BLR floods is that most of the boulders are relatively	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
			small, <1m, and may record a complex history of repeated movement.	
2	See text below table	Outburst flood issues	The trenches on the Pleistocene braid plain (T1, T2, T3, T6, & T7) at the site did not provide illumination regarding the possible Pleistocene outburst flood beyond the basic observations noted in the previous studies (Ostenaa et. al., 1999, 2002; Knudsen et. al., 2002). These deposits all appear to post-date any such flood. Bedforms and stratigraphy of the Pleistocene gravels exposed in these trenches do not appear to indicate high-energy deposition or subsequent erosion, but are rather consistent with braided flow on a distal outwash fan system. The sites downstream of Box Canyon may be more useful in this regard.	*
3	See text below table	Mound origins	Discussions of the trench observations of these features is now included in the summary report and Appendix B. Soils within the mounds reflect the ongoing biological activity associated with the mounds; profiles are highly variable in age, mostly young, and are disrupted in irregular patterns. The topographic forms may be dynamic, but no evidence of a fluvial role, such as channel features crosscutting the mounds or fluvial deposits embedded in the mounds, in the dynamic process was observed.	*
4	See text below table	Hydraulic motion criteria	We generally agree and have tried to move beyond the limitations of a single motion criterion by developing a probabilistic envelope across many differing types of data. Admittedly, protocols differ between the criteria, but the resultant probabilistic range is broader than the uncertainty range indicated by any single criteria. As noted in the report, application of this type of approach needs to be carefully evaluated for the specific settings to which it might be applied.	*

BIG LOST RIVER FLOOD HAZARD PEER REVIEW

Preliminary Comments by V. R. Baker

These are my preliminary comments on the draft report *Big Lost River Flood Hazard Study, Idaho National Engineering and Environmental Laboratory (INL), Idaho*, prepared by Ostenaa et al., U.S. Bureau of Reclamation (BOR), Denver, Colorado, for INL, U.S. Department of Energy (September, 2004). These following comments are based on my review of the subject report, including Appendices A through F, supplemented by (1) the draft meeting minutes from the Big Lost River Flood Hazard Study Peer Review Meeting held at the S.M. Stoller offices in Idaho Falls on October 5-7, 2004, (2) a meeting with the BOR report authors at their offices in Denver (plus a related conference call with DOE and Stoller staff) on November 8, 2004, and (3) PowerPoint presentation materials from the October 5-7 meetings.

Detailed comments on the written report, sent to me in draft form, are provided on separate Document Review Forms, keyed to section and page numbers. The comments that follow deal with more general issues, the PowerPoint materials, and related communications. In my review I have been especially concerned with the geological and geomorphological aspects of the study, including the trench logging and interpretations, and the coupling of paleoflood information with the hydraulic modeling to determine paleohydrological bounds for the flood probability assessments.

1. GENERAL CONCLUSIONS

My general conclusion is that the probabilistic models for flood hazards (particularly for the 100-year and 500-year return period floods) are fully supported by the data and analyses, that is, by the technical approach in this report, as it will be finalized with the addition of materials shown to me at the Nov. 8 meeting and provided in the PowerPoints. The relevant flood hazards are appropriately delineated by site-wide inundation maps (Appendix E of the report) and stage/probability plots for the INL area (Appendix F). The process used for generating these products is defensible and appropriate, with acceptable weights and uncertainties applied to their accuracy.

In particular, this report integrates the paleohydrologic bound approach with state-of the-art hydraulic flow modeling into a robust predictive framework, appropriately incorporating natural variability (aleatory uncertainty) and knowledge limitations (epistemic uncertainty), into the prediction of flood probabilities at INL. I am especially impressed with the innovative quantification of resistance to erosion, in terms of stream power per unit area and boundary shear stress.

The draft report dealt only peripherally with more extreme floods, notably the 100,000-year flood. While data from this study can reasonably be extrapolated to this extreme (e.g., Fig. 2-12), it would be better to have an independent paleoflood evaluation in order to fully defend this approach. While the "catastrophic 20,000-year glacial outburst flood" can defensibly be "eliminated" from consideration in the context of the present study, this issue remains significant for (1) its intrinsic scientific interest, and (2) potential challenges to extrapolation to the 100,000-year regulatory level. Some suggestions along this line are made in the Future Research section.

These comments are preliminary. Aspects of the report have yet to be presented in final form, notably Appendix B, which was of considerable interest to me. Also, there will be further discussion of responses to the detailed comments.

2. FUTURE RESEARCH

1 Several future research topics are specific to the INL region. One innovation would be to use some of the new dating tools to deal with geochronological issues in regard to paleohydrologic bounds. Examples of new tools that can be applied are Optically Stimulated Luminescence (OSL) and cosmogenic radionuclides. OSL has recently proven to be very useful for fine-grained fluvial sediments and associated eolian silts and sands. While subject to limitations, very recent advances in OSL technology have surprisingly increased its applications for fluvial geochronology. Similarly, there are several excellent possible applications for cosmogenic radionuclide studies. Relict late Pleistocene soils can be dated by using the ratio of two isotopes, sampled in vertical profiles. Cosmogenic radiocarbon has a short enough half-life to be applicable to Holocene geochronology. Paleoflood erosion of bedrock, and paleoflood transport of large boulders afford excellent possibilities for cosmogenic geochronology.

2 Related to the new dating tools, would be a more extensive study of the Pleistocene paleoflood record. At the study site this would include the Pleistocene braid plain (Surfaces P2/P1). Just off the site, downstream of Box Canyon, is a scabland complex, with flood-transported boulders. There are also possible ice-rafted boulders. Though the age of the responsible "cataclysmic flood" is probably pre-Pinedale, and its magnitude seems to be considerably smaller than what was calculated by Rathburn (1993), it is not scientifically reasonable to leave this anomaly unresolved. The age and magnitude of this "cataclysmic flood" may also have practical importance, in that regulatory requirements to estimate a 100,000-year flood could lead some critics to ask for a more complete analysis of the relationships in the Box Canyon region. If such a "cataclysmic flood" can indeed be documented, its magnitude and effects would be of interest in regard to hypothetical scenarios involving the failure of Mackay Dam.

3 A final INL geomorphic research issue is the origin and history of the silt mounds. Are these indeed relict features that persist from the latest Pleistocene? Alternatively, might they be equilibrium forms that evolve rather quickly at optimal sites (perhaps related to late Pleistocene periglacial micromorphology) from an incipient form to a climax mound morphology. When destroyed, by wind or water erosion, these features subsequently reform in the same locations. Resolution of this question relates directly to the use of these features in delineating paleohydrologic bounds.

4 A general research topic, inspired by this study, would be to study many sites, in different climatic and geomorphic settings, where one can quantify the flow hydraulics associated with surface erosion. Settings would include bedrock channels, terrace sequences, channel margins, etc. Data presented in Fig. D3-1 and D3-2 are important, but these data were not collected according to uniform protocol. Moreover, seemingly precise relationships, such as incipient motion criteria, are notoriously uncertain, and subject to numerous complex influences.

Part 1: Background Information

Document Information
Title: Summary Document: Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho
Draft/Rev. No.: Pre-Decisional Draft
Date: September 30, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Victor R. Baker Org: University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Sec. 1.0, First paragraph on page 1.	It might be useful to explain the bases of the two previous studies cited in Table 1-1 (Kjelstrom and Behembroch, 1996; Hortness and Rousseau, 2003). The second of these is not listed in the (admittedly incomplete) reference list (page 95). The great differences in the 100-yr peak flow estimates call for some up-front explanation.	Paragraph discussing these prior studies is added. Missing reference added.	*
2	Sec. 1.0, Table 1-2 on page 5	The reference to Ostenaa and Jones (2003) is not provided on on page 97.	Changed to Appendix A.	*
3	Sec. 1.2, first paragraph on page 7	It is not necessary to list M. Jones in the Acknowledgments since he is an author of the report.	Text is modified.	*
4	Sec. 2.1, first paragraph on page 10	The reference to Ostenaa et al. (2002) is not included on page 98.	Reference added.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
5	Sec. 2.1.2, Third paragraph at bottom of page 13	Reference is made to the various excavated trenches, logs of which I presumed to be in Appendix B. However, I could not find the logs in that appendix, which only contained the unit descriptions for Plate 2	As noted on page 10, these sections of the draft were not complete and were extensively revised. Missing materials in Appendix B have been added.	*
6	Sec. 2,1.2, First full paragraph on page 14	Reference is made to "trench exposures." Are these specific trenches that have been numbered and logged in the report? Again, I did not find the trench logs in my copy of Appendix B.	Same as #5	*
7	Sec. 2.1.2, second full paragraph on page 14	Trenches T6 and T8, and the radiocarbon date locations in the trench profiles/logs could not be checked because I could not find these logs in Appendix B.	Same as #5	*
8	Sec. 2.1.2, last paragraph at bottom of page14	I do not understand the reference to "(Fig. 6)" This does not follow the numbering scheme for figures in the report. Perhaps this is meant to be Fig. 2.5.	Same as #5	*
9	Sec. 2.1.2, last paragraph on page 15	There should be a reference to the actual trench descriptions (Appendix B?), and I was unable to check the latter because they were not included in the copy of the report that I received.	Same as #5	*
10	Page 16	A figure (2-2?) is given without a caption and without a reference in the text.	Same as #5	*

Part 1: Background Information

Document Information **Title: Summary Document: Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho** **Draft/Rev. No.: Pre-Decisional Draft** **Date: September 30, 2004**	
Document Originator Name: Dean Ostenaa et al. Org: BOR	Document Reviewer Name: Victor R. Baker Org: University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
11	Sec. 2.1.2, page 17	This figure (2-3) does not seem to be referenced anywhere in the text.	As noted on p. 10; section 2.1 was not complete and would be extensively revised. Reference to this figure and those noted in comments 2 and 3 below are incorporated into revised and expanded discussion with added subsections and revised section numbering.	*
2	Sec. 2.1.2,first full paragraph on page 15	Reference is made to "Fig. 4" but there is no such figure.	Same as #11	*
3	Sec. 2.1.2, pages 18-19	No references are made in the text to these figures (2-4 and 2-5).	Same as #11	*
4	Sec. 2.3 Page 25	Be more specific on what is meant by "stability" of a geomorphic surface in regard to paleohydrologic bounds. I presume that this means some preserved, observable amount of erosion for that surface	Discussion is revised and expanded to clarify this point. In the context of bounds, stability primarily refers to a <u>lack</u> of evidence for erosion or deposition and the observation of "normal" pedogenesis and stratigraphy for that site over the given length of time.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
5	Sec. 2,6 Pages 45-56	This section is not complete. Tables 2.6-5 and 2.6-6, and Figures 2.6-1 to 2.5-10 have no accompanying text, and are not referenced in the report.	Agree - Revision and expansion of discussion is added.	*
6	Sec. 2.3, Pages 27-30	The terms "shear stress" and "stream power" are used somewhat imprecisely here and in Figures 2.3-1 and 2.3-2. The actual terms should be "boundary shear stress" or "bed shear stress", and "stream power per unit area". These more precise terms should be developed in Appendix D.	Revised terminology is implemented and discussed in Appendix D along with similar comments made on Appendix D.	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix B, Geologic Data**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Victor R. Baker Org: University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
11	Sec. 2.1.2, page 17	This figure (2-3) does not seem to be referenced anywhere in the text.	As noted on p. 10; section 2.1 was not complete and would be extensively revised. Reference to this figure and those noted in comments 2 and 3 below are incorporated into revised and expanded discussion with added subsections and revised section numbering.	*
2	Sec. 2.1.2,first full paragraph on page 15	Reference is made to "Fig. 4" but there is no such figure.	Same as #11	*
3	Sec. 2.1.2, pages 18-19	No references are made in the text to these figures (2-4 and 2-5).	Same as #11	*
4	Sec. 2.3 Page 25	Be more specific on what is meant by "stability" of a geomorphic surface in regard to paleohydrologic bounds. I presume that this means some preserved, observable amount of erosion for that surface	Discussion is revised and expanded to clarify this point. In the context of bounds, stability primarily refers to a lack of evidence for erosion or deposition and the observation of "normal" pedogenesis and stratigraphy for that site over the given length of time.	*
5	Sec. 2,6 Pages 45-56	This section is not complete. Tables 2.6-5 and 2.6-6, and Figures 2.6-1 to 2.5-10 have no accompanying text, and are not referenced in the report.	Agree - Revision and expansion of discussion is added.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
6	Sec. 2.3, Pages 27-30	The terms “shear stress” and “stream power” are used somewhat imprecisely here and in Figures 2.3-1 and 2.3-2. The actual terms should be “boundary shear stress” or “bed shear stress”, and “stream power per unit area”. These more precise terms should be developed in Appendix D.	Revised terminology is implemented and discussed in Appendix D along with similar comments made on Appendix D.	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix D, Evaluating Hydraulic Modeling Results for Paleoflood Analyses**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 28, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Victor R. Baker
Org: BOR	Org: The University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Sec. D.1.1, Page D-3	This should be flood power per unit area of bed (not "flood power") and boundary or bed shear stress (not "shear stress').	Terminology revised	*
2	Sec. D.1.1.1, Page D-5	SS and Sp are ….estimated by hydraulic models…(not "hydrolytic models")	Revised.	*
3	Sec. D.3.3, Pages D-54 to D-64	No reference is made in the text to Figures D3-27 through D3-37 (pages DE-54 to D-64). In particular, Figures D3-27 and D3-28 illustrate an issue (stream channel power variation longitudinally along the study reach) that is not discussed in the text.	Discussion added in App. D and to summary report.	*
4	Pages D-7 to end of report	The same issue involving stream power and shear stress applies to the whole Appendix and its many figures.	Revised as per comment 1.	*

Part 1: Background Information

Document Information

Title: **Big Lost River Flood Hazard Study; Appendix D, Electronic Supplement – Output Plots for Discharge and Modeling Scenarios for the INL Diversion Dam**

Draft/Rev. No.: **Pre-Decisional Draft**

Date: **September 28, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Victor R. Baker
Org: BOR	Org: The University of Arizona

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	All	The plots are only given up to a maximum discharge of 250 cms. However, the uncertainty on the 10,000-year paleohydrological bound goes to 400 cms. Are plots needed for 400 cms?	Plots in draft report were included for discharge up to 300 cms. Additional plot for 400 cms is now added. Depth, stream power and shear stress values up to 400 cms were included on all summary plots for subareas (e.g. Figs D3-6 to D3-37 of App. D).	*
2	All	All these plots are labeled "shear stress" and "stream power". They should be more correctly labeled "boundary shear stress" or "bed shear stress", and "power per unit area."	Revised per other comments.	*

DOCUMENT REVIEW RECORD

For Dr. Klaus Jorde

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Klaus Jorde
Org: BOR	Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
	See text below table	Overall comments are embedded in the text below table - Specific recommendations in this text are flagged with red numbers and discussed below.	The general comments do not require specific changes to the draft report except as noted for those listed below.	*
1	See text below table	Elevations error uncertainty	We had not considered this; although it could be implemented it would appear to require another suite of model runs to implement and thus is well beyond the scope of what we could accomplish under the current study. The elevation errors only appear to be systematic on the original INEEL 2-ft contour map as noted in Appendix A. More detailed and reprocessed datasets do not appear to have systematic errors. Additional discussion of the results to small-scale topographic features will be added to the summary document.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
2	See text below table	Fixed bed modeling limitations on inundation patterns due to erosion at higher flows	Plots will be added to the Appendix D depicting power and shear stress for the inundation modeling. Discussion of these results and potential sites of channel instability will be added to the summary document	*
3	See text below table	Other flood mechanisms	Discussion of flood mechanisms will be added to summary document. This is primarily covered in other prior studies.	*
4	See text below table	Culvert blockage probabilities	Final conclusions will be revised to reflection this perspective.	*
5	See text below table	Modified topography	Discussion will be added to summary document emphasizing the sensitivity of results to small changes in the topography.	* see remarks below
6	See text below table	Additional studies and integration with other flood mechanisms	These topics are generally beyond the scope of the present report and are potential issues for follow-on studies. No specific changes made to the present document.	*
7	See text below table	Climate change	Comment echoed by other reviewers, but topic is beyond the scope of present study. Brief discussion will be added to summary discussion in concert with discussion of other flood mechanisms.	*

5: My question was not clear: I meant to say that based on the existing study you could explore the possibility of artificially creating new flow paths (by blocking some existing flow paths or opening additional ones) and thus reducing the flood hazard, obviously the first step would be to look at the diversion dam. That could possibly be much more efficient than other strategies to reduce flood risk and/or uncertainty. So, no changes requested, this is a suggestion for future work.

Big Lost River Flood Hazard Study, INEEL, Idaho

This document is part of the review prepared by Klaus Jorde following the scientific review panel meeting and field visit in Idaho Falls, from Oct. 7 to Oct. 9, 2004.

General evaluation of the project:

First I would like to thank the project team to invite me to participate in this review. The work that has been done here is outstanding and cutting edge science in several ways and although I was hesitant in the beginning due to time constraints I am glad to be part of this procedure. The quality of the information that has been provided for review is excellent, based on the reports and the presentations in Idaho Falls. I could not find any critical gaps in the information and all questions I raised during the review process were answered profoundly and in a very open scientific atmosphere.

What makes this work so outstanding is the comprehensive integration of uncertainties into the final inundation probabilities. I feel very confident that this work is fully adequate to represent the situation as it is now as good as can be done with today's available knowledge and tools, and is by far exceeding normal regulatory standards. I recommend to move ahead based on the results of this study. My comments are therefore to be seen more as suggestions for future work based on the findings of this study than as required additions to it.

The approaches to the various components of the study that finally lead to PIMs have been carefully evaluated and chosen, and I want to comment on a few.

Hydraulic modeling in general:

Both models, RICOM and Trim2D, have been tested rigorously under different conditions relevant for this study and compared to measured data. The results show that the models produce results with sufficient accuracy regarding stage and flow velocities if recommendations regarding the time steps and Courant numbers are followed. The fact that they are not FEMA approved does, from a scientific point of view, play no role here. It can be assumed that the performance of these models exceed criteria that are required for FEMA approval. If I may use a somehow personal view for a comparison: You don't need a US driving license to qualify for testing software for a remote control robot vehicle that drives around on Mars. This is certainly a bit of an exaggeration but it is going in the right direction.

Paleofloods and bounds:

Excellent choice for the modeling reaches to establish paleo floods and bounds because of stable channel reaches and selection of trenches.

After the presentations, discussions and the field visit I agree with the project team that a fixed bed hydraulic model was the adequate tool to use to calculate discharges in the paleoflood study reach. Throughout the river bed there is a high number of basalt outcrops and bedrock channel sections that will not change during a flood event. It can be assumed that even with erosion taking place between these control sections this would only change local small scale flow patterns but not influence water level elevations at certain flows. The goal of the hydraulic modeling for the paleoflood analysis were stage-discharge relations over the paleoflood study reach. These results would remain largely unaffected if erosion would be allowed and it is therefore not considered necessary.

The approach that was chosen to describe criteria for erosion are more appropriate than traditional formulae based on threshold criteria. The non parametric probabilities chosen here include the imprecise character of today's understanding of sediment transport phenomena and erosion.

Flood frequency analysis:

excellent work, no comments

Inundation depths:

One of the most important components obviously are the hydraulic modeling approaches and models that have been used. From looking at the terrain topography and the modeling results it is quite obvious that a 1D model would be completely unable to identify the flow processes that occur at overbank flows in the study area. This applies most of all for the inundation area and depths at various INEEL sites but to a lesser degree also to the paleofloods and bounds. The flow patterns that cause certain water depths at INEEL sites are often completely detached from the water levels in the river itself because the flow paths leading to the inundation are detached from the river at the respective "flow coordinate" and have their origin in the topography or various channels (natural or man made) far upstream.

Also, the flow patterns within the INEEL facilities that have a significant impact on the inundation depths which could not be captured with a 1D model. The same applies to the obvious influence of the topographic terrain represented by the different grids used for the two models, RiCOM and Trim2D.

Inundation depths at INEEL sites for higher flows cannot adequately be solved by 1D models, the use of 2D models is an absolute requirement.

Questions and concerns:

1Given the remaining uncertainties in the elevation data for the inundation modeling and the differences that resulted from using two different models and input topographic data (e.g. fig. 3-2b of the summary), have you thought about actually integrating the standard deviation of elevation data into the models (by introducing a random error into the elevations) and find out if this would cause significant differences in the flow patterns and inundation depths. The question is if the errors are really random or if they follow larger patterns. It is somewhere mentioned that the errors seem to have an increasing tendency in one direction.

2The results for the inundation maps are based on fixed bed models. At the very high flows which were modeled the river is spreading across a vast plain with small topographic variation and very erodible material. As the river starts spreading over this plain, erosion will occur and small changes in the topography following erosion could possible cause new flow patterns that cause further erosion and finally influence the inundation depths at locations far away from where the erosion takes place. I therefore recommend to plot the stream power and shear stress maps and try to evaluate if erosion in areas of high erosive capacity could eventually lead to significantly altered flow patterns in completely different new channels, or if this would only cause locally changed flow patterns without any overall consequence. Basically, this is what alluvial rivers do during extreme flood events: They reshape the valley and create completely new patterns of channels.

2Question: are there potential breach points able to support a totally different flow pattern forcing avulsion and new channel formation?

3There should be a paragraph that discusses if there are any scenarios that might occur but have not been covered in this study and could lead to a significantly more severe event, e.g. rain on snow, rain on frozen ground, channel reformation further upstream.... I am not familiar enough with the local climatic conditions to evaluate if anything specific should have been addressed. If the paleo flood approach integrates every thinkable scenario, it should be specifically mentioned.

Suggestions for future procedure:

4Instead of trying to integrate uncertainties such as culvert blockage into the logic tree, I suggest that the significant structural uncertainties (such as culverts at Lincoln Ave.) are removed by 1. changing the structures or 2. assuming that they perform in the least favorable way (this is how the diversion dam is treated presently). I cannot see a way to estimate some quantitative uncertainty of culvert blockage for the logic tree.

5The results of the inundation modeling could be used to identify the most critical flows paths leading to the inundation of critical sites. In some cases it should be possible to change these flow paths by blocking existing or opening new flow paths, e.g. by opening breaches in existing low embankments (road embankments, canal embankments) or blocking preferred flow paths by low elevation embankments.

6In my opinion, the greatest potential for generating additional safety would be to consider the existing diversion dam and integrating it into the analysis. The dam and its hydraulic structures (culverts) probably will need to be evaluated and enhanced but it is quite striking that this study has integrated nearly every possible uncertainty into the inundation probabilities, but at the same time neglects the certainty of the physical existence of the diversion dam for regulatory reasons. Taking the diversion dam into account and reducing flows by diversion capacity would significantly reduce inundation risk.

6Integrate floods from upstream and combine with dam break scenarios and diversion dam into complete analysis. Run unsteady flow analysis.

Research questions:

6Fundamental research on large scale infiltration rates could make a significant difference in the evaluation of inundation risks. There are large infiltration ponds in the Silver Creek watershed, dimension of hundred meters in diameter. May be there could be a starting point to get some larger scale information.

7The phrase "climate change" seems not to occur in the entire report. Although I find it difficult to make any specific recommendation without spending more time on it, I believe this issue will come up.

Part 1: Background Information

Document Information **Title: Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho** **Draft/Rev. No.: Pre-Decisional Draft** **Date: September 30, 2004**	
Document Originator Name: Dean Ostenaa et al. Org: BOR	Document Reviewer Name: Klaus Jorde Org: University of Idaho

As Chapter 2.1 will be replaced and it was read but not reviewed.

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	22	2nd paragraph: …n of 0.030 and 0.038 *where* implemented…	Changed per suggestion (I misspelled it…)	*
2	22	3rd paragraph: Typically, *flow* times….	Changed per suggestion	*
3	26	1st paragraph, last sentence: The difficulties……*are* ….	Changed per suggestion	* *
4	31	Last paragraph, line 6: …has not *been* used.	Changed per suggestion	*
5	34	2nd p. line 1: … subareas *are* summarized.	Changed per suggestion	
6	46 and 59	The pdfs in Fig. 2.11 refer to the different floods and bounds in table 2.6-6, I don't quite understand which ones are included and which not	Changed table 2.6-6; and revised discussion around Figure 2.11 to clarify. Better legend in Fig. 2.6-6	*
7	chapter3.2, p. 77	If RiCOM calculations are more demanding, why do they seem to run shorter? 1/10 real time seems to me shorter	Explain	*
8	Chapter 3.2.5 ,p.80	Line 3: Typically, *flow* times…	Changed per suggestion	*

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix A, Topographic Data for Big Lost River Flood Hazard Studies
Draft/Rev. No.: Pre-Decisional Draft
Date: September 22, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Klaus Jorde
Org: BOR	Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	17	Bottom line: …RTK survey methods were used and mean….	Deleted extra word to clarify.	*
2	18	While physical scientists often use RMS as a synonym for standard deviation, this is not so familiar for engineers. Also, you are using the term "mean RMS" which actually doubles the "mean", so RMS here describes simply roots of square deviation for individual measurements.	Explain what RMS stands for and how you use it. (Added text with explanation of RMS for the GPS observations and to clarify these issues. What is plotted is the RMS values for individual GPS observations; mean and median are for the distribution of RMS values.	*

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix B, Geologic Data
Draft/Rev. No.: Pre-Decisional Draft
Date: September 30, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Klaus Jorde
Org: BOR	Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Page B-ii	Unidentified where note [2] applies in the table	Table is removed from this appendix. Repeated elsewhere.	*
2	Page B-4	Fill gaps I first paragraph.	Extensive revisions and additions to this appendix.	*
3				
4		No further comments		
5				

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix C, Hydraulic Modeling Methodology and Quality Assurance
Draft/Rev. No.: Pre-Decisional Draft
Date: September 29, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name:
Org: BOR	Org:

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	1.2, pages C19/20	Figures C1-2 and C1-3 are identical. I assume C1-2 is the wrong one because it does not show a plan view of the grid as I assume it should.	Fixed the cross reference to include the proper plots on each page.	*
2		References to other "sections" are unclear sometimes	Cross references to other sections and appendices were made more specific.	*
3				

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix D, Evaluating Hydraulic Modeling Results for Paleoflood Analyses
Draft/Rev. No.: Pre-Decisional Draft
Date: September 28, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Klaus Jorde Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1				
2		A calibration and validation of the sediment erosion threshold values is not realistic for this large scale study. The approach of using a probability distribution for critical shear stress and stream power based on a number of comparable studies is suitable as compared to using crisp numbers for thresholds for incipient motion and erosion rates. This type of modeling resembles fuzzy logic based modeling and produces robust results where small alterations and uncertainties in the input variables do not result in significant changes in the output.	No specific change required to report text.	*
3	Page D-65	Citation Brooks, 1990 is missing	Added to list	*
4				

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix D, Electronic Supplement – Output Plots for Discharge and Modeling Scenarios for the INL Diversion Dam
Draft/Rev. No.: Pre-Decisional Draft
Date: September 28, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Klaus Jorde
Org: BOR	Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No	Comment	Resolution	Acc./Rej.
1		In general, the comparison between the different outputs allows an assessment how significant the discrepancies between modeling approaches based on different data sets and input parameters are. The conclusion from the information presented is, that the way the data sets and models have been tested, compared, and finally used, is appropriate to generate information as reliable as possible on flood stages at various flows. The comparison between the 5ft (based on 1993 data) and 6ft (based on 2000 data) shows that while locally, the grid from 1993 reprocessed data leads to considerable differences, it represents the larger scale overall flow pattern reasonably well, so that it can be used for the inundation study as long as the water level elevations are compared to true terrain elevations to calculate water depths. This has been done so in Appendix E and F.	No changes required to report.	*

Part 1: Background Information

Document Information **Title: Big Lost River Flood Hazard Study; Appendix E, Hydraulic Modeling to Estimate Big Lost River Flood Inundation at INL Facility Sites** **Draft/Rev. No.: Pre-Decisional Draft** **Date: September 22, 2004**	
Document Originator Name: Dean Ostenaa et al. Org: BOR	Document Reviewer Name: Klaus Jorde Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1		There are 2 different aspects that were considered: 1. Culverts: They have not much of an effect, the few ones that have a considerable effect should either • be re"structured", e.g. removed and replaced by a roaddip or a bridge that cannot be blocked, or completely closed, to make sure that they perform in the desired way or they should • be assumed to be behaving in the most undesired way. Assigning probabilities is not making sense (what is the probability that one of the few dead cottonwoods ends up just in the inlet of the most important culvert) 2. Infiltration: It seems that there is also no adequate way of assigning any probability to infiltration rates because of lack of field observation data. Further research is required if it should be taken into account. The way it is taken into account in this study is conservative. In the Silver Creek catchment, near Picabo, Idaho, there	No change to report required based on this comment.	*

		are some large (appr. hundred meters long) infiltration ponds for groundwater aquifer recharge. As a conclusion, I agree with the approaches that were used in this part of the study.		
2				

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix E, Electronic Supplement – Plots of Modeled Flood Inundation for the Big Lost River Downstream of the INL Diversion Dam
Draft/Rev. No.: Pre-Decisional Draft
Date: September 18, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Klaus Jorde Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Page 1	Notes: RICOM instead of RICOH	Changed	*
2	All plots	The legend "difference in depth" in the TRIMR2D/RICOM comparison plots is not clear to me, blue seems to be between -0.25 and +0.25 but then it is unclear	Explain somewhere (Legends modified for clarity)	*
3	Plot wet_eta_trim_minus_ricom_ni_pc_400cms.dat_1 (Used as example only, applies to others as well)	If on the plot you look at #1 of the selected locations within INEEL you can see that the stage differences are yellow and purple in adjacent cells. That means that TRIMR2D goes from >7.5 cm underestimating to >7.5 cm overestimating within on cell. That makes a 15 cm difference between the 2 cells within 20 ft or 7 m. That is a 2% gradient which is a pretty steep river. Looking at the flatness of the terrain, I wonder how such a result emerges. Next, if you look at the stage plots in App. F for RICOM-TRIMR2D comparison at location #1 and 400 cms, there is a difference clearly more than 30 cm between the 2 models with TRIMR2D estimating higher stages. I assume that somewhere in the text it is described how these data were determined, but since time for this	The plots were constructed by differencing depth values of TRIMR2D and RICOM results. This produced artifacts, such as pointed out here, because it included the differences in the topographic grids. Thus, the large difference values were generated mostly in locations along channel edges where gridding resolved the bank heights/shapes differently. The plots now use differences in water-surface elevation instead of depth, which eliminates artifacts along channel boundaries, etc. These results show that in most areas WSEL differences are very minor <0.25 ft, except where differences in the grids resolve small-scale topography differently and allow/deny flow to adjacent cells. Other differences at the downstream limits of flow in small channels result from use of different flow durations in the models. Clear explanation of the difference process will be added to the introductory text.	*

		review is running out I can't keep searching. Here is my interpretation: I assume that for the AEP plots the elevation of point #1 has been measured accurately and that it is not taken from the DEM (which would then be different for each model) and that then the water level at that location from each of the models has been used and compared with the measured ground elevation to determine water depth. On the other hand, the inundation depth map (or the difference) is based on two different DEMs and does probably not at all include the accurate elevation of location #1, but only the mean elevation of the different cells from the RICOM or TRIMR2D DEM which could be different from the true elevation of point 1. The use of two different DEMs which are then compared based on the TRIMR2D mesh obviously introduced some errors which may be exaggerated because "difference in depth" may be larger than difference in "water level elevation" because difference in depth reflects differences in ground elevation plus differences in water level elevation. The difference between the inundation plots and the AEP plots may result from the fact that the true elevation of point 1 is not included in the of inundation depth maps. If this is explained somewhere and I simply did not find it, it's ok. If it is not explained somewhere, I think that giving an explanation would support the credibility of the analysis. See my comment on Part F		

Part 1: Background Information

Document Information
Title: Big Lost River Flood Hazard Study; Appendix F, Stage Probability Plots for Selected Locations at INL Facility Sites
Draft/Rev. No.: Pre-Decisional Draft
Date: September 27, 2004

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Klaus Jorde
Org: BOR	Org: University of Idaho

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	F-1	2nd paragr. Last line, first word "printer"	Changed to "printed"	*
2	Map Ref #1, RICOM vs. TRIMR2D (as example only)	See my comment in appendix E part 2 referring to the inundation depth at location #1, comparison RICOM vs. TRIMR2D.	Discussion added to summary document clarified and expands on these issues.	*
3				

DOCUMENT REVIEW RECORD

For Dr. Manu Lall

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Upmanu Lall
Org: BOR	Org: Columbia University

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
	General	See text below. More specific recommendations in this text are flagged with red numbers and discussed below.	Draft report is being finalized with the inclusion of specific comments from other reviewers. The review comments below largely focus on the various types of uncertainty that are present in the overall study but do not indicate specific revisions to the text or report except as noted below. We generally concur that these uncertainties are present and they are in general discussed within the context of the overall report and appendices. Some enhanced discussion of the uncertainties will be added to subsections in the report that summarize related points to accent these uncertainties. They will be especially noted in the report summary. Initiating and addressing research issues are beyond the scope of the current report. The latter portion of the comments discusses the potential impacts of climate change on the flood hazard estimates. Caveats on these issues will be added.	Acc.
1		Prior investigations	Comments discusses the clarity of the report presentation of prior investigations and this will be modified per more specific comments by Baker.	Acc.

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
2		Input discharge to models	A fixed value of input discharge was used for all hydraulic models.	Acc.
3		Complications to paleoflood estimates	This paragraph highlights some of the uncertainties associated with the paleoflood discharge estimates. Subpoints (a-f) are noted as complicating factors for these estimates. These are particular factors that need to highlighted in the discussions of derivation of the paleoflood bounds.	Acc.

Big Lost River Flood Hazard Study, Idaho National Engineering and Environmental Laboratory, Idaho, *by Dean A. Ostenaa, Daniel, R.H. O'Connell, and M. Jones*

Review comments by *U. Lall*

The authors present the results of an innovative study of the flood and inundation risk posed to the Idaho National Engineering and Environmental Laboratory (INL) facilities in the proximity of the Big Lost River. The study addresses (a) the apparent discrepancy in past estimates of flood risk through additional field investigations and the use of a novel approach to integrate different sources of historical and paleoflood information for defining flood flow return periods; (b) steady state, two dimensional numerical modeling of flow during flood events to identify potential inundation pathways, using high resolution, revised topographic data at the site; and (c) the integration of these investigations towards a characterization of inundation modes for selected locations of concern suitable for a formal risk based decision analysis.

The authors do an excellent job of documenting the detailed analyses, the data sources, model sensitivity analysis, and parameter and data uncertainties. The analyses and methods used are cutting edge and advance the state of knowledge in addition to providing a response to the task they were asked to address. The discussion of background information is adequate, and the field visit was effective in educating this reviewer about the key aspects of paleoflood reconstruction used in this study. The authors are to be congratulated for performing a thorough analysis for the quantification of risk and its uncertainty.

The site under investigation is unique in that it is in a closed basin, where flow typically decreases in the downstream direction due to losses to the subsurface. The basin has been modified over time, and modifications due to dams and diversions need to be accounted for. Almost all historical floods and floods prior to the period of record are presumed to be related to snowmelt rather than rainfall dynamics. It is assumed that given the geomorphic and geologic controls (e.g., unmodified Pleistocene deposits, rolling topography, highly porous subsurface), the spatial scale of rainfall events in the current and recent paleoclimatic periods is unlikely to be large enough to compete with snowmelt driven floods from the large catchments at the INL location. Given the nuclear waste storage facility and the nuclear power plant in the vicinity of the Big Lost River, there is an interest in flood risk management. Consistent with FEMA guidelines, inundation maps for the 100 yr and 500 yr flood risk were deemed of interest. The 10,000 yr to 100,000 year return period flood events are also of interest given the nature of the site. It is clear that existing Bulletin 17 guidelines for doing flood risk analysis are not likely to be applicable at this site.

Prior investigations documented by the authors, resulted in a wide variation in the estimate of the magnitude of the 100 year flood at the INL Diversion Dam, which is upstream of the INTEC and TRA sites at which flood risk is a primary concern. Indeed, some 1recent estimates of the 100 year flood flow appear to be higher than paleoflood bounds reported by the authors. The study was expected to (a) provide improved estimates of flood risk, and (b) a better characterization and quantification of the key uncertainties in the analysis. These would be used to guide risk mitigation strategies that INL may pursue. The primary innovation introduced by the authors is the use of paleoflood markers and dates as additional information into flood frequency analysis to achieve these two goals. The hope is that such information provides more direct information as to rare floods than can be provided by the relatively short historical record. Flood probabilities can change at a location as climate changes and as land use, land cover or channel characteristics change. Landscape and river basin changes are addressed by the authors at the local scale by considering the diversion structures and their potential capacity, and assuming that the upstream dams do not significantly alter the peak flood. The latter may be a reasonable assumption for extreme flood flows at this site. However, the demonstration by the authors that the inundation patterns at the site are highly sensitive to small scale differences in topography, *highlights* the nature of the *uncertainties* associated with mapping flows to inundation. The inability to accurately estimate capacities of drainage culverts under flood and debris flow conditions is an added source of uncertainty in this regard, that is unresolved.

As with the approach based on historical flood data, the paleoflood based approach assumes that in the long run climate and hence flood risk have not changed. Thus, *uncertainties associated with anthropogenic climate change*, e.g., due to increasing greenhouse gases are not addressed. However, the longer record of floods does presumably address interannual to century scale climate variations, such as those due to the shorter time scale fluctuations of the El Nino Southern Oscillation, and may dramatically improve estimates of the 100 and 500 year flood flows relative to analyses that do not use the methods pioneered by the authors. No reliable means of estimating potential flood risk under an anthropogenic climate change scenario are currently available. The ocean-atmosphere general circulation models used for climate change projections are known to have significant biases in precipitation statistics even for control runs using current climate conditions, particularly in orographically varied domains such as Idaho and the intermountain West. These biases are not significantly ameliorated by nesting Regional Climate Models into the boundaries of IPCC based projections. Consequently, it is not useful to pursue an end to end modeling project for INL flood risk mapping using such climate change projections. *We can only recognize that this is a new and as yet non-quantifiable source of uncertainty*. Qualitatively, we expect that winter precipitation dynamics could change dramatically. The models predict with relatively high confidence that the mid-latitudes will be warmer leading to warmer, moister air masses bringing precipitation and heat into the region. The precipitation is likely to be in the form of rain rather than snow. Consequently, the flood mechanism could shift from snowmelt dynamics to large frontal storms and rainfall-runoff. The frontal storms could have a spatial scale that is of the order of 50 to 100 km of simultaneous impact. At this point, it is unclear whether intermittent frontal storms (with what frequency?) could generate larger flows in the Big Lost River than the seasonal accumulation and melt of snow. This issue constitutes a significant source of uncertainty, but not one that can be resolved by either the authors of this report or by others. These uncertainties impact all estimates of flood risk, but may be particularly severe for the assessment of rare flood potential (the >500 year return period events).

I have reviewed the nonparametric Bayesian Monte Carlo flood frequency estimation approach pioneered by this group and applied to the Big Lost River data. The statistical methodology developed and reported on in the Journal of Hydrology paper is sound and innovative. The authors systematically consider a variety of sources of uncertainty in measuring flow, estimating the time and level of paleoflood bounds etc. While some subjective choices as to measurement error uncertainty distributions and their parameters are made, from the report and from the presentations it is clear that the assumptions made are reasonable and should not dramatically impact the final results presented. A dominant source of *uncertainty* here is associated with the manner in which *paleoflood stage is related to paleoflood flow.*

The authors use a hydraulic modeling approach. A 2-dimensional, steady state numerical model of spatially varied flow (? Is this correct **2** – at some points it seems like a fixed Q is used) over a certain section of the Big Lost River above the INL site is used to infer flows that correspond to specific stands of high water as inferred from debris and soil stratification in selected trenches in the area. A limited sensitivity analysis of the parameters of these models (e.g., 2 values of Manning's n) was made and "calibration" to known discharge conditions and stage in the region at a few locations was attempted. A semi-quantitative assessment of the reliability of these analyses is made by computing stream power and shear stress corresponding to the proposed discharge for a paleo flood and assessing whether these indicators would be consistent with the geometry and topography of the area in a spatially distributed manner. Specifically, do areas of potential deposition and erosion, and sheet flow during the proposed flood correspond between those inferred from the model runs for a particular flow rate and those observed from the existing topography. Existing data for stream power and shear stress measurements and their relation to erosion and sedimentation for different soils are used to guide this process. This is an innovative application of this technology. It translates into a subjective assessment of the "minimum" flow necessary to produce the observed features (related to channel stability) from the field observations for each subarea. These estimates are then used to specify paleoflood bounds for use in the frequency analysis. Complicating factors in this analysis include

a) **3** The difficulty in reliably estimating the discharge associated with a shallow sheet flow over a large area as the flow overtops the main plateau like feature in the study region. However, this may not be critical since much of this flow may not make it to the INL site.
b) The inability to robustly identify the rate of sedimentation/erosion as a function of stream power, and its implications for paleoflood bounds
c) The lack of consideration of changing fluid density as sediment is entrained or deposited may lead to some systematic biases. However, these may be subsumed into or overwhelmed by the variation in the effective parameters used for modeling.
d) The high sensitivity to detailed topographical data suggests that paleoflood estimates may be very sensitive to any landform changes that may have taken place in between paleo flood events. These changes may be masked by the current setting. However, it does seem that the authors have developed the "best" available topographic data set, and that their assertion that no significant changes in the topography have occurred over the last 10,000 years may be reasonable. As to the channel itself, there is an assumption that "stable channels" existed in the specific control sections used for assessing paleoflood bounds. The field visit was reassuring on this point. While this is still a critical assumption, the field evidence suggests that the sites chosen were reasonable with respect to the assumption
e) The performance of the many culverts in the region in actual flood conditions is a source of uncertainty for the projection of the flow and inundation field for existing conditions, but presumably this is not an issue for identifying the paleoflood bounds.
f) The loss rate of flow to infiltration has been calibrated to a particular estimate that may or may not be representative of conditions under high floods. Given that basalt underlies the gravels in much of the area, the loss rate could perhaps be sustained at the higher flows, but there is no direct evidence to support this assumption.

The authors seem to be cognizant of these concerns and have addressed them under the framework of engineering judgment and weight of evidence from the field investigations and hydraulic modeling conditional on the parameters specified. A striking conclusion offered by the authors is that the discharge of approximately 110 to 150 m^3/s has not been exceeded in the last 400 to 600 years, and a discharge of 130 to 175 m^3/s has not been exceeded for 800 to 1200 years. This is in contrast to the assessment of a 100 year flood of >200 m^3/s by other authors as recently as 1996. Clearly these paleoflood bounds will have a significant influence in the procedure used to combine paleo and historical flood frequency information, since they dramatically modify the tail of the probability distribution. It is important to note that these bounds are based on rather specific assumptions on the reliability of the computed stream power (dependent on channel geometry and roughness estimates) and soil erodability (dependent on stream power and soil/vegetation assumptions). Certainly the degree of field investigation and sampling performed by the authors provides an extensive high quality data set for the types of investigations performed. The assumptions as to the parameters are based on published literature, which actually does reflect a high degree of variability.

Consequently, the final estimates reflect a certain degree of subjectivity that then carries through to the rest of the analysis. Based on the complexity of the analysis and the information presented, it is not possible for this reviewer to assert whether the choices are conservative as indicated or not. A saving grace or a nonlinear uncertainty (is there a threshold beyond which the % loss is dramatically reduced?) that also emerges is the role played by the regional soils and the loss rate of the flood wave as it approaches the INL at the downstream end of the channel studied.

The authors present a considerably more sophisticated approach to flood risk estimation than is usually attempted. Many more sources of information are used to constrain the problem, and are integrated through quantitative tools. However, these tools and data introduce their own set of uncertainties since parameters have to be specified, and judgments need to be made as to the applicability of procedures, parameters and definitions en route. This is not a bad thing. The authors make a valiant effort to maintain transparency, and it is clear that many critical parameters are subjective and will impact the final results with weakly constrained uncertainties. However, the traditional approach to flood frequency analysis using only historical data and a prescription of probabilistic modeling tools reflects even more severe subjectivity and uncertainty since the viability of those assumptions is rarely assessed directly with at site data. Thus, through their model formulation, field investigations and applications the authors have significantly improved on flood risk characterization as practiced.

A bottom line question for a reader may be whether the 100 year flood estimated by the previous USGS study is a better or worse estimate than the number presented by the authors. Such a question can best be answered only in the context of the decision problem considered. First, the question of whether the USGS estimate is plausible under the framework of the authors work can be examined by seeing the probability assigned to that flow magnitude by the uncertainty distribution of the 100 year flood. Conversely, one could compare the uncertainty bands associated with 100 year flood from both procedures. We know that a traditional approach (USGS?) does not account for model uncertainty. The authors Bayesian approach does. Thus, consideration of an additional source of uncertainty would likely make the reported uncertainty bands from the authors approach be wider than those reported if model uncertainty were not considered. So, one would need to inflate the uncertainty bands from the USGS model further to make that aspect comparable. On the other hand, since the authors use information as to rarer events, they will potentially see a dramatic reduction in the uncertainty bands associated with flood events that are of return period longer than the historical record (e.g. 100 year) relative to procedures that do not use such data. The most important aspect of fitting flood frequency models is constraining the right tail of the distribution, and the paleoflood inclusion allows one to do this. Of course, both approaches are non-informative if we consider a non-stationary world with future climate changing.

Second, in terms of developing a flood risk mitigation plan, it is essential that the uncertainty bounds for flood frequency be used in a formal way to guide expected loss/benefit calculations for each failure mode and proposed solution. One notes readily that the range of flows associated with low risk (Probability of exceedance <0.01) events is progressively broader as rarer events are considered. Thus, consideration of uncertainty may allow a much wider range of possible outcomes to be considered appropriately weighted for risk. Since flood loss/avoidance cost are typically nonlinear as a function of flow, consideration of the uncertainty in the nominal risk values for a particular flow used as a control threshold would most likely translate into a different choice than when just the flood frequency curve is used. Thus, the better estimate of uncertainty bands for the flood frequency curve provided by the authors approach is likely to translate into better decisions provided that this uncertainty rather than the mean flood frequency curve is considered.

Third, we know that there are significant effects of Pacific Ocean conditions on moisture and temperature changes in the Idaho region in Winter. Specifically, the El Nino Southern Oscillation and the Pacific Decadal Oscillation impart a significant interannual to multi-decadal signature to snowmelt volume and peak flow. Thus, the historical record is unlikely to satisfy the assumptions of being an independent, identically distributed data set. This assumption is central to both the author's procedure and the standard (USGS) procedures. However, the impact of this assumption on the analysis is much greater on the analysis based on approximately 50 years of data (USGS) than the one provided by the authors where events well beyond the characteristic time scale of these climate oscillations have been sampled. Hence, I would expect the estimates from the author's procedures to have lower variance given the better constraint from exceedance bounds. However, since we have no information on the frequency with which those bounds may have been approached over that period, I would still expect that the true uncertainty associated with the paleoflood based flood frequency curve could be higher than reported here.

In the meeting in Idaho, there was a discussion of potential failure modes and their characterization, including the possibility of a cascade of multiple events such as dam failure and a flood. These have not been analyzed in the report, and I understand that they were beyond the charge given to the authors. I presume that the flood and inundation maps and probabilities developed by the authors could form the basis of such an analysis conditional on specific hazard mitigation strategies that are proposed. An unanswered question is whether there is potential for at site flooding at the two INL facilities from mechanisms other than the Big Lost River overflowing. This consideration may be a factor as very low risk events are considered in conjunction with climate change.

In summary, the report represents a significant improvement over prior work on characterizing the flood risk and its uncertainty for the Big Lost River. Both model and informational uncertainties are addressed. In many cases, the uncertainty assignments are subjective, and in many cases only a limited sensitivity analysis is attempted. However, the approach taken is generally self-consistent, reproducible (at least in consultation with the authors), and well documented. The subjective uncertainty assessment and testing procedures are consistent with those used by others for structural or hazard reliability assessment and mitigation and appear to be properly applied. The basic assumptions as to the geomorphic evidence introduced to constrain rare floods appear to be sound and the methods used for integration of different pieces of information are appropriate and seem to be adequately tested. I cannot critically evaluate the applicability of the critical argument or assumption of the stability of the landscape and the associated fluid dynamics for flow-inundation-erosion-sedimentation modeling and paleo-flood determination. However, I do find the scientific arguments presented to be convincing and reasonable. The caveat that anticipated climate changes and unanticipated landscape or structural changes have not been accounted, and a limited number (yet all obvious) of mechanisms of exposure have been investigated needs to accompany the document, particularly in the discussion of low probability events and their impact. It is important that future use of the results of this study use the uncertainty bands developed for the flood frequency curve, rather than just the curve.

DOCUMENT REVIEW RECORD

For Chris Martin

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Table 0-1/ p iii	Hortness and Rousseau – Values are reversed.	Changed per suggestion	*
2	Table 0-1/ p iii	"Rousseau" is misspelled as "Roseau".	Changed per suggestion	*
3	Table 0-1/ p iii	Hortness and Rousseau (2002) is missing from the References.	Changed per suggestion	*
4	1.0/¶ 1/p 1	Line 1 – Reference for Ostenaa et al. (2002) is missing from the References	Changed per suggestion	*
5	1.0/¶ 1/p 1	Line 2 – Reverse "INL" and "Idaho National Engineering and Environmental Laboratory" and insert "" first (…at the Idaho National Engineering and Environmental Laboratory (INL)…).	Changed per suggestion	*
6	1.0/¶ 1/p 1	Line 3 – Add parenthesis around "**Figure 1-1**" (… (**Figure 1-1**) might be…).	Changed per suggestion	*
7	1.0/¶ 3/p 1	Line 3 – Use Figure 1-2 as the reference since that show the location of both diversion structures.	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
8	1.0/¶ 3/p 1	Line 6 – Change "bounds" to "bound" (…paleohydrologic bound estimates…).	Changed per suggestion	*
9	1.0/¶ 1/p 4	Line 1 – Change "stages" to "stage" (…probabilistic stage estimates…).	Changed per suggestion	*
10	1.0/¶ 2/p 4	Line 8 – Cite Table 1-2 at the end of this statement.	Changed per suggestion	*
11	1.0/¶ 2/p 4	Line 13, 15 – Move the citation to Table 1-2 from after "corridor" on line 13 to after "(Appendix A)" callout.	Changed per suggestion	*
12	1.0/¶ 3/p 4	Line 2 – All the figures in the later appendices show stream power and shear stress. Should this say the models "were used to calculate inundation, stream power, and shear stress" instead of "inundation and flow velocities"?	Modified text. Power and SS are post-processing results of model calculations.	*
13	1.0/¶ 3/p 4	Line 3 – Change "as" to "that" (…approach that was used…).	Added text to clarify	*
14	1.0/¶ 2/p 5	Line 7 – Correct appendix callout is Appendix **F** for stage hazard curves.	Changed per suggestion	*
15	1.1/¶ 2/p 6	Appendix A line 2 – Add closing parenthesis.	Changed per suggestion	*
16	1.1/ GENERAL	Make sure Appendices Titles and Sections are correct (Appendix E – Hydraulic Modeling to Estimate Big Lost River Flood Inundation at INL Facility Sites, has four separate papers that should be listed as Sections).	Modified and checked	*
17	1.2/¶ 1/p 7	Line 1 – Use full name (Matt) instead of abbreviation (M) at the start of a sentence.	Modified per Baker comment	*
18	1.2/¶ 1/p 7	Lines 8 & 10 – Insert citation to Appendix C after both these sentences.	Changed per suggestion	*
19	1.2/¶ 2/p 7	Line 2 – Spell out first use of DOE.	Changed per suggestion	*
20	1.2/¶ 2/p 7	Lines 3, 4 – Use full names (Robert Creed and Chris Martin) at start of sentences	Changed per suggestion	*
21	1.2/¶ 2/p 7	Line 3 – Change the period following "Manager" to a comma.	Changed per suggestion	*
22	1.2/¶ 2/p 7	Line 5 – Change "Stoller and Asso." to "S.M. Stoller Corp."	Changed per suggestion	*
23	1.2/¶ 1/p 8	Line 1 – Spell out "University" (University of Idaho).	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
24	1.2/¶ 2/p 8	Line 5 – Insert "The" in front of "INL" (The INL GIS Laboratory…).	Changed per suggestion	*
25	1.2/¶ 2/p 8	Line 7 – Use full name at start of sentence.	Changed per suggestion	*
26	1.2/¶ 4/p 8	Lines 3 & 6 – Use full name at start of a sentence (D. Simpson…; L. Piety…; _ Piety and…; K. Duran and …).	Changed per suggestion	*
27	1.2/¶ 4/p 8	Last line – This would be a good place to cite Appendix B, especially if it will eventually contain the trench photomosaics.	Changed per suggestion	*
28	1.2/¶ 5/p 8	Line 2 – Correct "insight" to "insightful" (…and insightful comments…).	Changed per suggestion	*
29	1.2/¶ 1/p 9	Lines I, 2 – Use full name at the start of a sentence (K. Puseman…; M. Kuntz…).	Changed per suggestion	*
30	1.2/¶ 1/p 9	Line 3 – Does the star (*) refer to something that is missing?	Yes - additional text is added.	*
31	2.0/¶ 1/p 10	Line 2 – Reference for Ostenaa et al. 2002 is missing from the References section.	Changed per suggestion	*
32	2.0/¶ 1/p 10	Line 8 – Insert "and" between "relationships," and "4)" (…relationships, and 4) additional…).	Changed per suggestion	*
33	2.0/¶ 1/p 10	Last line – What happened to Sections 2.5 and 2.6?	Modified and corrected	*
34	2.1/¶ 1/p 10	Line 1 – Cite only Figure 1-1 as Figure 2-2 doesn't show the INL in relation to the Snake River Plain.	Changed per suggestion	*
35	2.1/¶ 1/p 10	Line 2 – Change "an" to "a" (…a large area…).	Changed per suggestion	*
36	2.1/¶ 1/p 11	Lines 13, 14 – Reference to Geslin et al. 2002 is missing from the References section	Changed per suggestion	*
37	2.1.1/¶ 1/p 13	Line 2 – Reference for Hortness and Rousseau (2003) is missing from the References Section.	Changed per suggestion	*
38	2.1.2/¶ 2/p 14	Line 1-2 – There seems to be a word missing. Either insert "that" (…surfaces show that a distinct…), or "with" (…these surfaces with a subdued…).	Modified - deleted "on these surfaces"	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
39	2.1.2/¶ 4/p 14	Last line – Correct reference to "(Fig. 6)". Figures are numbered as Chapter-x (i.e., 2-6); also there is no Fig. 6 that I could find. I think this should be Figure 2-5.	Changed per suggestion	*
40	2.1.2/¶ 2/p 15	Line 6 – Correct reference to "(Fig. 4)". Figure 2-4 isn't the BLR8 location.	Changed per suggestion	*
41	Figure /p 16	There is no caption or number (should 2-2) with this figure. Also it should be rotated like the following three figures. This is the figure to reference for comment 40.	Changed per suggestion	*
42	Figure 2-3/p 17	Caption has been cut off. Missing the "e" on "image" and "…and geology from Plate 2. Blue boxes are…"	Changed per suggestion	*
43	2.2/¶ 1/p 21	Line 5 – Appendix A does not discuss the angle of rotation. Appendix C discussed the angle of rotation, but only for the reprocessed 1993 grid (36.9° clockwise rotation). Suggest both adding a statement on the rotation angle to Appendix A or Appendix C and referencing that section.	The rotation is documented in Appendix C Part A-Section 2 subsection 1.3. Added an explicit reference to that location.	*
44	2.2/¶ 1/p 21	Line 15 – Table 2-1 doesn't have anything to do with impacts of the high edge walls. Should this reference a figure from Appendix E?	Table 2-1 documents the topographic grid used as a function of discharge and provides the necessary cross reference between discharge and specific grids.	*
45	2.2/¶ 2/p 21	To head off any negative comments, would it be beneficial to include a figure showing where the springs providing downstream flux were located?	The downstream limit of the wetting springs has been added to Figure C1-2 and a reference to Figure C1-2 has been added to the text.	*
46	2.2/¶ 2/p 22	Line 4 – Insert "was used" between "3 s" and "for the" (…step of 3 s was used for the…).	Changed per suggestion	*
47	2.2/¶ 2/p 22	Line 5-6 – Insert "were" between "0.038" and "implemented" (…and 0.038 were implemented…)	Changed per suggestion	*
48	2.2/¶ 3/p 22	Lines 2 &4 – What "study reaches" are being referred to? I thought there was only on study reach (the Diversion Dam section).	Changed to "study reach grids", since there are two grids (5-ft and a 6-ft).	*
49	2.2/¶ 5/p 22	Line 1 – Insert "in" between "contained" and "Appendix D" (maps contained in Appendix D.).	Changed per suggestion	*
50	2.2/¶ 5/p 22	Appendix D has maps of stream power and shear stress only. May want to reference Appendix E for maps of depth.	Plots of depth will be added to Appendix D.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
51	2.3/¶ 1/p 25	Line 3 – Reference for Levish 2002 is missing from the References section.	Reference added.	*
52	2.3/¶ 1/p 25	Lines 6, 9, 13 – Reference are missing from the References section (Magilligan, 1992; Carson and Griffiths, 1987; Jarrett and England, 2002).	Reference added.	*
53	2.3/¶ 2/p 25	Line 6 – First (and only) addition of English units. Suggest deleting "(1-3 ft)."	Unit usage modified to be consistent throughout report and appendices	*
54	2.3/¶ 1/p 26	Line 4 – Reference (SSHAC, 1995) is missing from the References section.	Changed per suggestion	*
55	2.3/¶ 3/p 26	Line 6 – Insert a "P" in front of "3" to be consistent with other map groups (P2-<u>P</u>3).	Changed for consistency throughout	*
56	2.3/¶ 3/p 26	Line 6 – Insert a citation for Appendix B at the end of the map groups (…H1-H2, H3-H4, and P1-P2 (Appendix B), are of…)	Changed per suggestion	*
57	2.3/¶ 1/p 28	Line 2 – Reverse abbreviation and full spelling to be "…as Probability Density Functions (PDF)…"	Changed per suggestion	*
58	Figure 2.3-1/ p 29	All previous figures were Chapter-x (2-1 through 2-5), now they have changed to Section-x (2.3-1). Need to make figure numbering consistent.	Changed per suggestion	*
59	Figure 2.3-3/ p 32	Figure is missing a caption.	Caption is located on right side of figure	*
60	2.3.1/¶ 1/p 31	Line 6 – Change "be" to "been" (…has not <u>been</u> used here…)	Changed per suggestion	*
61	2.3.1/¶ 3/p 34	Line 1 – Either insert "the" between "of" and "subareas" (…each of <u>the</u> subareas…), or delete "of" and change "subareas" to "subarea" (Within each subarea…)	Changed per suggestion	*
62	2.3.1/¶ 3/ p 34	Line 9 – Delete "be" (…are then estimated…).	Changed per suggestion	*
63	2.3.1/¶ 3/p 34 & Figure 2.3-5 & Table 2.3-4	Line 10, Y-axis, Column 2 – "Per cent" is one word, "Percent."	Changed per suggestion	*
64	Figure 2.3-5	Caption line 2 – Figure number cited should be 2.3-1. Line 3 – "per cent" is one word.	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
65	2.4/¶ 1/p 43	Line 4 – Reference (Levish, 2002) is missing from the Reference section.	Changed per suggestion	*
66	2.4/¶ 3/p 43	Line 2 – Should the phrase "…do not increase…" be "…do not decrease…"?	Statement correct as written	*
67	Figures 2.6-1 through 2.6-10	There is no call out for these figures. They need to be called out in the text or tied to Table 2.6-6.	Section is extensively revised and discussion of figures is added.	*
68	2.7.1/¶ 1/p 57	Line 12 – Change "The" to "To" (<u>To</u> minimize…). Line 17 – Change "not" to "no" (…is <u>no</u> longer…). Last line – Reference O'Connell et al. (2002) is missing from the Reference section.	Changed per suggestion	*
69	2.7.1/¶ 1/p 58	Line 1 – The term "pdf's" has been capitalized in the past (PDFs). Figure numbering is back to what was used earlier (Part-value). Be consistent, stick with 2-x or go down to subsection 2.7-x.	Capitalization of pdfs removed. Numbering simplified.	*
70	2.7.1/¶ 2/p 58	Line 5 – Table numbering has changed. Be consistent (see comment 69).	Numbering simplified.	*
71	2.7.1/¶ 3/p 58	Line 4 – The term "(*cf*)" is capitalized in Table 2-7 (*Cf*).	Made consistent	*
72	2.7.1/¶ 3/p 58	Line 2 – Changed "approached" to "approach" (…on the <u>approach</u> used in…)	Changed per suggestion	*
73	2.7.1/¶ 1/p 60	Line 6 – See comments 69 and 71 above.	Capitalization standardized	*
74	Figures 2-13 through 2-19	Caption – Change "discharges" to "discharge" (Peak <u>discharge</u> distributions…).	Changed per suggestion	*
75	2.7.1/¶ 2/p 62	Line 6 – Missing word, "…from slightly _ the lower limit…"	Missing words added	*
76	2.7.1/¶ 2/p 70	Line 10, also figures 2-20 through 2-23 – Need to provide some justification or discussion of why the credible limits went from 5% and 95% to 10% and 90%. At this reading it appears to be arbitrary.	Credible limits were selected for illustration. Specific credible limits must be selected based on risk objectives.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
77	Table 2-1/p 75	The last table presented was 2-7, need to renumber. Also, strongly suggest changing the symbol for multiplication from a star (*) to and x, to avoid confusion with the star used as a flag for AEPs < 0.0001.	Changed per suggestion	*
78	3.1/¶ 1/p 77	Line 4 – Insert "data" between "topographic" and "to" (…rotated topographic data to produce…).	Changed per suggestion	*
79	3.1/¶ 1/p 77	Line 6 – Change "spatially" to "spatial" (…increased spatial sampling…).	Changed per suggestion	*
80	3.2/¶ 1/p 77	Line 1 – Change the tilde "~" to "approximately" (…of approximately 33 million… and of approximately 7.2 million…).	Changed per suggestion	*
81	3.2.1/¶ 1/p 77	Line 3 – Reference Feidler (2002) is missing from the References section.	Changed per suggestion	*
82	Table 3-1	Note 2 – Change "RICOH" to "RICOM" (…R – RiCOM with…).	Changed per suggestion	*
83	3.2.4/¶ 1/p 80	Line 3 – Are there particular sections associated with Parts A and B of Appendix C? If so, they should be called out.	Changed per suggestion	*
84	3.2.5/¶ 1/p 80	Line 3 – Change "flows" to "flow" (Typically, flow times…).	Changed per suggestion	*
85	3.2.5/¶ 1/p 80	Would it be beneficial to discuss and show examples of the hydrographs produced during the flow simulations?	Salient characteristics are discussed in the text in a couple sentences which takes less space than figures that show the same thing.	*
86	3.2.6/p 80	In Ostenaa, et al., 1999 figures showing the velocity vectors were displayed. Would it be beneficial to discuss and show some of those here or later in this section (3.0)?	In the prior report velocity was considered in the context of erosion. In this report shear stress and stream power are used. Consequently, velocity vectors are not shown which makes the plots less busy.	*
87	Table 3-2/p 81	Previous table with AEP values use x $10^{-exponent}$ instead of $e^{-exponent}$. Be consistent. Suggest using $e^{-exponent}$ in Table 2-1.	Changed per suggestion	*
88	3.3.1/¶ 1/p 81	Line 5 – Use "AEP "as "annual exceedance probability (AEP)" has already been defined.	Changed per suggestion	*
89	3.3.1/¶ 1/p 81 & p 83	Lines 8 & 10 p 81; Line 1 p 83 – Change "than" to "that" (…probability that the AEP…).	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
90	3.3.3/¶ 1/p 84	See comment 85 above.	Salient characteristics are discussed in the text in a couple sentences which takes less space than figures that show the same thing.	*
91	3.3.5/¶ 1/p 85	Whole paragraph needs work. Line 4 – Delete "and either no conveyance". Line 5 – Move "("partial")" from end of sentence to after callout (…of the culverts ("partial") where…). Line 6 – Change "" to "" (The third scenario…).	Paragraph was completely rewritten.	*
92	3.4.1/¶ 1/p 86	Line 5 – Change "entrained" to "entrapped" (…and are entrapped on the…).	Entrain: To pull or draw along after itself. Entrap: To catch in or as if in a trap. The flow is entrained on the north side of the diversion channel, but it's not trapped because it continues to flow off the grid. So, the flow draws its self along the north side of the diversion channel.	*
93	3.4.1/¶ 1/p 86	Line 6 – Insert "see inundation maps in" before "Appendix E" (…of the TRA (see inundation maps in Appendix E).	Changed per suggestion	*
94	3.4.1/¶ 1/p 86	Line 7 – Change "flow" to "flows" (…dominated by flows that escape…).	Changed per suggestion	*
95	3.4.1/¶ 1/p 86	Line 11 – Insert "to" between "insensitive" and "topographic" (…insensitive to topographic…). Insert a period "" at the end of the sentence (…scenarios (**Figure 3-3**).).	Changed per suggestion	*
96	Figures 3-2 through 3-5	Change "Ricom" to "RiCOM" as used in text.	Changed per suggestion	*
97	Figure 3-2 through 3-5	Need to find a better way of separating and discussing subfigures (a) and (b).	Changed per suggestion	*
98	GENERAL	Need to work out how measurements are presented. The current report uses a mix of English and metric units. The INL Style Guide suggest metric units followed by the English equivalent in parenthesis [i.e., 300 m^3/s (10,059 ft^3/s) or 300cms (10,059 cfs)].	Agree: changed for consistency throughout report	*
99	GENERAL	Use consistent numbering for figures and tables. Either Chapter-value or Chapter section-value.	Changed per suggestion	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix A, Topographic Data for Big Lost River Flood Hazard Studies**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 22, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Abstract A-1	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
2	Summary/ ¶ 1/iii	Lines 4, 5, 7 – Insert "the" in front of "INL" on each line.	Changed per suggestion	*
3	Summary/ ¶ 2/iii	Line 2 – the word "acquired" is misspelled (acquired).	Changed per suggestion	*
4	Summary/ ¶ 3/iii	Line 4 – Define abbreviations "NMAS" and "ASPRS" when first used.	Changed per suggestion	*
5	1.0/¶ 1/p 1	Line 2 – "Quality Assurance and Quality Control" should be lower case.	Changed per suggestion	*
6	GENERAL	Insert "the" in front of "INL." As an example instead of "at INL" change to "at the INL."	Changed per suggestion	*
7	1.1/¶ 1/p 1	Insert "the" in front of "environment" on line 6 and in front of "DOE" on line 8.	Changed per suggestion	*
8	Figure 1-1	Call out of the Union Pacific Railroad above Arco (actually above the diversion dam). The proper term is "abandoned," not "discontinued."	Added attribution for figure to caption. Changed figure to be consistent with similar figure in summary document.	*
9	1.1/¶ 3/p 4	Line 7 – Reword section before comma as, "… to the BOR for this flood hazard study,… "	Changed per suggestion	*
10	1.1/¶ 2/p 5	2nd sent. – Is it worth including a statement as to what would constitute a "higher accuracy" for this study?	No change. This is general discussion of method- not specific to INL application.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
11	1.2/¶ 5/p 7	Line 1 – Chris Martin works for S.M. Stoller Corporation, not Stoller, Inc.	Changed per suggestion	*
12	2.1.1/¶ 1/p 8	Line 7 – Change "was" to "of" (…occupation time of 10 minutes…).	Changed per suggestion	*
13	2.1.1/¶ 1/p 8	Last line – Define "RMS" on first use.	Changed per suggestion	*
14	Figure 2-3	Y-axis label is cut off.	Changed per suggestion	*
15	2.1.1/¶ 1/p 10	Last line – Is survey data in Appendix A of this appendix (Appendix A) or elsewhere in this report? Suggest that it be in "Attachment A" of this appendix.	Added "of this report" throughout to Appendix references. Retained use of Appendix since this portion is an independent report which also happens to be included as an appendix in the larger report.	*
16	2.1.2/¶ 1/p 11	Line 1 – See comment 15.	Ibid.	*
17	2.1.2/bullet 3 & Figure 2-5	It is not clear from the text or figure caption what the marks on Figure 2-5 represent. Clarify in the text.	Added text in caption to explain.	*
18	Figure 2-6	Caption Line 4 - Change "was" to "were" (…upper right were partially burned).	Changed per suggestion	*
19	2.1.2/¶ 1/p 13	Line 2 – Change "… output as ASCII file format…" to "… output in ASCII file format…"	Changed per suggestion	*
20	2.2.1/¶ 1/p 18	Lines 2 & 3 – See comment 15. Also applies to "Appendix B."	See comment 15	*
21	3.2.1/¶ 1/p 29	Last line – Coordinate data is in Appendix C… of this appendix (Appendix A) or elsewhere in this report? Suggest that it be in "Attachment C" of this appendix.	See comment 15	*
22	3.2.1/¶ 2/p 29	Line 9 – Should "…topography or studies…" be "topography for studies…"?	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
23	3.2.1/¶ 2/p 29	Last sent. – Unclear what this means. Was the weighting and adjustment solution for correction of the introduced horizontal error?	This is standard part of photogrammetry process. Going further down the path of explanation here would require a lot more text than we really want to put into this section and would not be consistent with treatment of other similar subjects in this section. Yes, this takes care of introduced errors.	*
24	Figure 3-2a	Caption line 1 – Insert "from" between "flows" and "the" (…flows from the…).	Changed per suggestion	*
25	3.2.1/¶ 1/p 34	Line 2 – "…points are tabulated in Appendix D…" of this appendix (Appendix A) or elsewhere in this report? Suggest that it be in "Attachment D" of this appendix.	See comment 15	*
26	3.2.3/ Bullets	All – Align all the indents.	Changed per suggestion	*
27	3.2.3/Bullet 2	Line 1 – The word "automated" is misspelled as "atomated."	Changed per suggestion	*
28	3.2.3/Bullet 3	2nd sent. – Doesn't read correctly, aerotriangulation software was used to generate a process? Suggest rewording as "… software was used to generate relative and absolute orientations and generate…" or "…software was used for the relative and absolute orientation process and to generate …", whichever is correct.	Changed wording to clarify	*
29	3.2.3/¶ 1/p 38	Line 2 – Change "… output as ASCII file format…" to "… output <u>in</u> ASCII file format…"	Changed per suggestion	*
30	3.3/¶ 2/p 39	Line 3 – Insert either "are" or "were" after the parenthesis (…Given – Computed) are within …).	No change. This seems OK as is.	*
31	3.3.2/¶ 1/p 43	Line 1 – Correct line spacing.	Changed per suggestion	*
32	Figure 3-8	Is there any way to delete the panel numbers that appear on this figure as pink rectangles?	Not easily. No change	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
33	4.1.1/¶ 1/p 49	Last line – "…point listing are included in Appendix E." of this appendix (Appendix A) or elsewhere in this report? Suggest that it be in "Attachment E" of this appendix.	See comment 15.	*
34	4.2/¶ 1/p 51	Line 2 – The word "grids" is misspelled as "girds."	Changed per suggestion	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix B, Geologic Data**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Abstract/ B-i	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
2	Abstract/ B-i	Line 2 - Need a space at end of the first sentence.	Changed per suggestion	*
3	Abstract/ B-i	Other Appendices use regular numerals (i.e., B-1).	Changed per suggestion	*
4	Abstract/ B-ii	Footnote 1 – Delete the "X" after "of" and replace with "the".	Table deleted; not needed in this appendix	*
5	B.1.3/¶ 1/B-0	Line 4 – "Basalt" should be lower case (basalt).	Changed per suggestion	*
6	B.1.2. Unit Headings	Unit headings need to be made consistent. Either B.1.2.1 – B.1.2.4 or bold and underlined.	Changed per suggestion	*
7	B.1.3/ B-0	Present descriptions as in later parts (i.e., B.1.4.).	Changed per suggestion	*
8	B.1.4/Low-flow/B-1	Should this unit be H4?	Changed per suggestion	*
9	B.1.4/H3/B -1	Morphology, Line 2 – Delete tilde (~) in front "1.0 m."	Changed per suggestion	*
10	B.1.4/H2/B -2	Soils, Line 1 – Should stages be "I to II-" instead of "I to I-"?	Correct as written	*
11	P1/B-3	Morphology, Line 2 – Change "radiant" to "radiate" (…surfaces that radiate from low saddles…)	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
12	Qbb/B-3	Line 6 – Reword start of sentence as "P1 and P2 deposits have breached low areas of both flow tongues…"	Changed per suggestion	*
13	Qbb/B-4	Last 2 lines – Don't forget to replace the "***" place holders.	Changed per suggestion	*
14	GENERAL	Will this appendix also include photo mosaics of the trenches?	Yes- As well as other supplemental data related to trenches, soils, and dating	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix C, Hydraulic Modeling Methodology and Quality Assurance**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 29, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al. Org: BOR	Name: Chris Martin Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Summary/¶ 1/C-1	Line 1 – Misspelled "two" as "to."	Changed per suggestion	*
2	Summary/¶ 2/C-1	Line 7 – Change "…and Appendix E - Infiltration." to "both in Appendix E of this report."	Changed per suggestion	*
3	Summary/¶ 3/C-1	Sent 1 states "Part B contains three …," then only discusses two.	Corrected to two.	*
	PART A, Section 1			
5	¶ 5/C-3	Line 3 – Define other terms in equation (1), x', y', z, t and dx', dy' if they mean something other than standard derivatives.	A derivative is denoted by a dot centered over a variable. These denote primes which denote the integration variables in standard notation.	*
6	¶ 5/C-3	Line 4 – Sentence states "…the equations become," what equations does this refer to?	The word "conservation" was added.	*
7	C-4	Equation numbering restarts at bottom of page with (4).	Equations renumbered	*
8	C-5	Equations (7) and (8) – Define term "z."	Defined on the top of page C-4.	*
9	Model Formulation	General – How is roughness crest height found? Only reference in this section (last ¶ on p C-4) suggest the top of the roughness crest is land surface, but earlier statement (¶ 4 p C-3) says "…closure submodels or direct simulation can be used for the region below the roughness crests."	Roughness height is specified using Manning's n.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
10	¶ 1/C-6	Equation 9 – Define the term *z* (as *dz*) in the equation.	Defined on the top of page C-4.	*
11	¶ 2/C-6	Equation 10 – Define	Reference to previous equation added.	*
12	¶ 3/C-6	Equation 11 – Define the term **f** in the equation.	Defined on the top of page C-5.	*
13	¶ 4/C-8	Equation 15 – Earlier statement (Last sent. p C-7) says u_n^{m+1} is equation (13) solved for this value. Equation (15) doesn't look like a rearranged equation (13).	That's because the equation has been factored into the form of a wave equation, as discussed a on C-7 and C-8.	*
14	GENERAL	Unless this is reproduced from another report, suggest numbering the headings as in the other appendices.	It's reproduced from another report.	*
	PART A, Section 2			
15	1.0/top ¶/C-13	Line 1(end) – Change "is" to "in" (…area <u>in</u> great…).	Changed per suggestion	*
16	1.1/¶ 1/C-13	Line 1– Is there a better term than "decimated" for describing expanding the 5 ft grid to a 10 ft grid?	It's literally a process of decimation, not expansion.	*
17	1.1/¶ 1/C-13	Line 2 – In simple terms the river was rotated from running southwest - northeast to running west - east?	The entire mesh was rotated to minimize width.	*
18	1.1/¶ 1/C-13	Line 6 – Change "elevation" to "elevations" (…subtracted from all elevation<u>s</u> on the grid.).	Changed per suggestion	*
19	1.1/¶ 2/C-14	Line 2 – Why use "eta," which normally means "estimated time of arrival" as an abbreviation for water surface elevation? Suggest just using "water surface elevation" or "water elevation" for graph axis headings.	Eta is used in Appendix C, Part A, Section 1 to denote water surface elevation in all the equations and is used in the output file format variable name to ensure a clear association between water surface elevation and eta.	*
20	1.2/¶ 1/C-14	Line 17 – Correct "…elements sides…" to "…element sides…"	Changed per suggestion	*
21	1.2/¶ 2/C-15	Line 9 – Correct "…elements angles…" to "…element angles…"	Changed per suggestion	*
22	Figure C-1	Last line of caption - Change "…completed remove…" to "" (…to complete remov<u>al of</u> the…).	Changed per suggestion	*
23	1.2/¶ 2/C-16	Delete duplicate second paragraph.	Changed per suggestion	*
24	1.2/¶ 1/C-17	Line 7 – The word "shortest" is misspelled as "shorted."	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
25	1.2/¶ 1/C-17	Line 8 – Change "elements" to "element" (…maximum element coordinate dimensions…).	Changed per suggestion	*
26	1.2/¶ 2/C-17	Last line – Change "numbering ordering" to either "numbering order" or "number ordering" whichever is appropriate.	Changed per suggestion	*
27	1.2/¶ 3/C-17	Line 2 – Correct "m3/s" to "m^3/s."	Changed per suggestion	*
28	1.2/¶ 2/C-17	Line 6 – is it "…40-120 s(econds)" or "40-120 s(teps)"?, spell out first use to make this clear.	There are never steps except of the word steps is used. Using standard AIP convention for unit abbreviations; second=s. Changed 40-120 s to 40 to 120 s.	*
29	1.2/¶ 1/C-18	Line 3 – Delete " for the diversion grid." or change the period after "…flood plain" to a comma.	Changed per suggestion	*
30	1.3/¶ 1/C-18	Last line – Add closing parenthesis and period to end of sentence (…or larger).)	Changed per suggestion	*
31	Figures C1-2 and C1-3	Captions are different but the figures don't appear to be.	There was an incorrect reference to an imported file which has been corrected to produce the proper graphic in each figure.	*
	PART A, Section 3			
32	1.1/¶ 1/C-22	Line 2 – Change "of" to "for."	Changed per suggestion	
33	1.1/¶ 1/C-22	Line 3 – Insert "infiltration" in front of both occurrences of "depth" (…of the resulting infiltration depth or the current infiltration depth…).	It's just (water) depth, not infiltration depth.	*
34	1.1/¶ 1/C-22	Line 4 – Insert "the" between "into" and "cell" (…flux into the cell.).	Changed per suggestion	*
35	1.1/¶ 2/C-22	Line 3 – Depths on plots in Appendix D and E are in meters, suggest changing "10 cm" to "0.1 m". Also, should this be > 0.1 m?	Changed per suggestion	*
36	1.2/¶ 3/C-23	Line 2 – Insert "(Appendix E of this report)" at end of sentence.	Changed per suggestion	*
37	1.2/¶ 4/C-23	Last line – Sentence suggests there should be a figure, but none is provided. Add figure to illustrate discussion.	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
38	1.2/¶ 3/C-24	Line 4 – Delete the period (.) after "1."	Capitalized the following sentence.	*
39	1.2/¶ 4/C-24	Line 1 – Reword and missing word(s). "To accompany the discrete natural (what?) of the computational grids, …"	Multiple typos fixed. "To accommodate the discrete nature…"	*
40	1.2/¶ 5/C-24	Line 1 – Section 3.4 of what? This appendix or the summary report.	Clarified	*
41	1.2/¶ 5/C-24	Line 4 – Insert "are" between "culverts" and "on" (…culverts are on nearly …).	Changed per suggestion	*
42	1.2/¶ 1/C-25	Line 3 – Correct "inlet sand" to "inlets and"	Changed per suggestion	*
43	1.2/¶ 1/C-25	Line 4 – Does flow cease, or does it become constant?	Nearly ceases (no head to drive the flow either way).	*
	PART B, Section 1			
44	GENERAL	Unless this is reproduced from another report, suggest numbering the headings as in the other appendices.	Reproduced from another report.	*
45	¶ 1/C-26	Line 3 – Suggest citing Ostenaa et al., 1999, as referenced in other appendices.	Changed per suggestion	*
46	¶ 1/C-26	Lines 4 & 5 – Capitalize "model" and "dimensional."	Changed per suggestion	*
47	¶ 1/C-26	Lines 7-9 – Lower case "Tests" as in 4).	Changed per suggestion	*
48	¶ 2/C-26	Last line – Suggest using *v* instead of *u*, since *u* was defined elsewhere as a velocity perturbation and *v* is defined as a velocity.	Also used u for velocity in Part A, Section 1, so we retain the same convention here.	*
49	¶ 3/C-26	Line 1 – Is the bottom friction coefficient the same as (or related to) the roughness crest from Part A, Section 1? If so clarify, it sounds here like they are the same.	Clarified that it's Manning's n.	*
50	¶ 2/C-27	Line 2 – What are "these methods" referring to?	Made specific.	*
51	¶ 2/C-27	Line 10 – Delete the duplicate "by."	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
52	¶ 4/C-28	Line 4 – Something is missing from this part of the paragraph. The start of the sentence "The first 2 models…" suggest that Figure 2 is going to show a comparison of TrimR2D and RiCOM. However, Figure 2 shows a comparison of two time steps for RiCOM, and there is no discussion of this in the text.	Rewritten to correctly reference the figure.	*
53	¶ 2/C-29	Line 1 – Insert reference to "model description section " at end of sentence "(Part A, Section 1)."	Changed per suggestion	*
54	¶ 1/C-30	Line 1-2 – Is this a "field-scale <u>simulation</u>" or a "field-scale <u>test</u>" of the model?	test	*
55	¶ 2/C-31	Line 3 – Delete the period (.) after "4300" (…4300 cms…).	Changed per suggestion	*
56	¶ 3/C-31	Line 2/Figure 4 – Need to state where the "upstream" end is. Is flow from right to left or left to right?	Indicated by the color scheme.	*
57	¶ 2/C-32	Line 4 – Shouldn't model results be bounded by a horizontal line at the highest *observed/measured* water level, not the highest *modeled* water level?	The observed high-water marks are lower bounds.	*
58	¶ 3/C-33	Line 1 – Need to define transcritical flow as a Froude number greater than some value.	Changed per suggestion	*
59	¶ 3/C-33	Line 3 – Missing text at the start of the sentence, "Local regions of (what?) with Froude numbers…"	Deleted of	*
60	Figure 6/C-34	Delete duplicate figure.	Changed per suggestion	*
61	Figure 7/C-35	There is no discussion of this figure in the text.	Changed per suggestion	*
62	¶ 1/C-35	Line 8 – Suggest a rewording as "…edge of the site where the river turns to the northeast, that limits downstream discharge in the river."	Changed per suggestion	*
63	¶ 1/C-35	Line 9 – Suggested rewording as "…is bypassed to a series of interconnected spreading areas (playas). The reach used here for the paleoflood study extends…"	Changed per suggestion	*
64	¶ 1/C-36	Line 8 – Why isn't there a separate figure for Juniper Bends?	When Juniper Bends was lost as a potential paleoflood detailed investigations site, the focus was shifted downstream to other detailed investigation sites.	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
65	¶ 2/C-36	Line 7 – Change "Section ?" to "Appendix E of this report"	Changed per suggestion	*
66	¶ 2/C-36	Line 2 – Tense issue, change "…inundation are…" to "…inundation is…" or "…inundations are…" whichever is appropriate.	TrimR2D was used in the past to produce the results. The results showed and still show (present tense) that the inundations are underpredicted.	*
67	Figure 9/C-37	Caption – Correct capitalization of "Water" to "water."	Changed per suggestion	*
68	Figures 10-12	Captions – Correct spelling of "RiCom" to "RiCOM."	Changed per suggestion	*
69	¶ 1/C-37	Line 3 – Insert "With" at the start of the sentence (With the 1.82 m…).	Changed per suggestion	*
70	¶ 2/C-38	Line 1-2 – This description and reference for the Roe solver should also be included in the Introduction paragraph 1, line 5.	Changed per suggestion	*
71	¶ 1/C-39	Last line – Change "than" to "that" (…ensure that Courant…).	Changed per suggestion	*
72	¶ 1/C-40	Line 4 – Capitalize the "d" in "TrimR2d" (TrimR2D).	Changed per suggestion	*
73	¶ 1/C-41	Line 3 – Change the first "Section ?" to "Part A, Section 2."	Changed per suggestion	*
74	¶ I/C-42	Line 2 – Change "product" to "produce" (…started to produce transcritical…).	Changed per suggestion	*
75	¶ 1/C-42	Line 5 – Change "using" to "use" (…necessary to use θ=0.7…).	Changed per suggestion	*
76	¶ 1/C-44	Line 6 – Change "effect" to "effective" (An effective alternative…).	Changed per suggestion	*
77	¶ 2/C-44	Line 15 – Why is "theta" spelled out here? Use the symbol "θ" as done previously.	Changed per suggestion	*
78	¶ 2/C-44	Line 15 – Change "times" to "time" (…and time steps…).	Changed per suggestion	*
79	GENERAL	Needs a discussion either in this Section or in the Summary Report on the importance (or lack thereof) of the difference in the RiCOM and TrimR2D stage elevations. What affect, if any, does this have on the conclusions of the study?	Discussed in the Summary Report	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
80		Did not review PART B, Section 2. I assumed these were direct comments from Nik.	They are direct comments from Nik with replies.	*

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix D, Evaluating Hydraulic Modeling Results for Paleoflood Analyses**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 28, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Abstract/¶ 2/ D-i	Line 5 – insert "to" between parenthesis and "400" (…ft^3/s) to 400 m^3/s…). Delete "with" between "for" and "n-values."	Changed per suggestion	*
2	Abstract/D-i	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
3	Abstract Table	Footnote 1 – Delete the "X" after "of."	Changed per suggestion	*
4	General	Use a slash (/) for units as used in the other appendices, not an exponent (instead of Wm^{-2}, use W/m^2)	Unit notations checked and changed throughout Summary Doc and appendices for consistency	*
5	D.1.1/¶ 5/D-4	Equation D1-4 – Need to define "Q." Also include a statement like "… and γ and S are as defined above" at the end of the sentence.	Changed per suggestion	*
6	D.1.1/¶ 1/D-5	Line 1 – Need to define "u" in equation for V.	Changed per suggestion	*
7	D.1.1/¶ 3/D-5	Last line – Delete the tilde (~) and replace with "approximately."	Changed per suggestion	*
8	D.1.1.1/¶ 1/D-5	Line 2 – Is a hydrolytic model different than a hydraulic model. If not use the term hydraulic model for consistency.	Changed per suggestion	*
9.	D.1.1.1/¶ 1/D-5	Line 7 – The word "for" is misspelled as "tor."	Changed per suggestion	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
10	D.1.1.1/¶ 1/D-5	Line 8 – Tense issues, "factor" and "time" should be "factors" and "times," respectively.	Changed per suggestion	*
11	D.1.1.1/¶ 2/D-5	Line 4 – Insert "be" between "may" and "combined" (…may be combined…).	Changed per suggestion	*
12	D.2.1/¶ 1/D-7	Line 3 – Insert "be" between "considered to" and "conservative" (…considered to be conservative…)	Changed per suggestion	*
13	D.2.1/¶ 2/D-7	Line 3 – Insert "in" between "for use" and "evaluating" (…for use in evaluating…)	Changed per suggestion	*
14	D.2.1/¶ 2/D-7	Last line – Delete the parenthesis ")" at the end of this sent.	Changed per suggestion	*
15	D.2.1/¶ 3/D-7	Line 1 – Define "FHA" when first used.	Changed per suggestion	*
16	Table D2-1	Fourth column, second row – Only need to include the footnote on the first use.	Changed per suggestion	*
17	Table D2-1	Footnote 3 – Equation *** is Equation D1-1.	Changed per suggestion	*
18	D.2.3/¶ 1/D-13	Line 10 – To avoid confusion you may want to us d_A to denote average particle size, since d_i was previously defined as a particle size above which *i* percent of the particles occur.	No change - retained to be consistent with usage in original reference.	*
19	D.2.3/¶ 2/D-13	Line 4 – Insert "with" between "associated" and "deposition" (…power associated *with* deposition…).	Changed per suggestion	*
20	D.3/¶ 1/D-17	Line 3 – Insert the missing period after limits (…study limits. **Figure D3-1**…).	Changed per suggestion	*
21	D.3.1/¶ 1/D-17	Line 10 – Insert "of this report" to "(Appendix B) " so that it is clear this doesn't refer to an Appendix B of this paper.	No change - retained for consistent usage throughout. Will use other terminology in Appendices that have separate appendices (e.g. App. A)	*
22	Figures D3-1 and D-3-2	Caption says Black symbols point up, but figure shows black symbols pointing down.	Changed per suggestion	*
23	Figures D3-1 and D3-2	Need to include a discussion in the text of why the Big Lost River Distribution is and area and not a point or line. Include what each of the vertices represent, and what the area represents.	Added and expanded discussion of this topic	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
24	D.3.1/¶ 2/D-20	Line 5 – Insert "are" between "that" and "generally" (…sizes that are generally less …).	Changed per suggestion	*
25	D.3.2/¶ 2/D-22	Line 3- Insert "with" between "Gravels" and "these" (Gravels with these …).	Changed per suggestion	*
26	D.3.2/ ¶ 2/D-22	Line 5 – The word "scattered" is misspelled as "scatted."	Changed per suggestion	*
27	D.3.2/¶ 2/D-22	Line 7 – The word "both" is misspelled as "bother."	Changed per suggestion	*
28	TableD3-1/ D-26	Row 9 - "T2" should probably be "P2."	Should be H1-2; Changed and checked other entries for consistency with other appendices and Plate 2.	*
29	D.3.3/¶ 2/D-24	Line 6 – The word "be" should be "been" (…has not been used…).	Changed per suggestion	*
30	D.3.3/¶ 2/D-24	Line 12-13 – Insert "(21 floodplain and 9 channel)" between "subareas" and "were."	Changed per suggestion	*
31	D.3.3/¶ 2/D-32	Line 1- Either insert "the" between "of" and "subareas" or delete "of" and change "subareas" to "subarea."	Changed per suggestion	*
32	D.3.3/¶ 2/D-32	Lines 10, 13 – "Per cent" is one word.	Changed per suggestion	*
33	D.3.3/¶ 2/D-32	Need to discuss what criteria were applied for this study, 20%, 50%, or something else.	Added note. Discussion on this topic is part of summary doc.	*
34	D.3/GENERAL	Need to discuss what criteria for each of the broad categories listed (design, entrainment, and geomorphic) were used in this study.	Topic is mentioned at beginning of Section D.3; and discussed added in section D.3.1 per comments 23 and 33. Also discussed in Summary Doc.	*

Part 1: Background Information

Document Information

Title: **Big Lost River Flood Hazard Study; Appendix D, Electronic Supplement – Output Plots for Discharge and Modeling Scenarios for the INL Diversion Dam**

Draft/Rev. No.: **Pre-Decisional Draft**

Date: **September 28, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1				
2				

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix E, Hydraulic Modeling to Estimate Big Lost River Flood Inundation at INL Facility Sites**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 22, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	E-1	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
2	E-1	2nd line – Change "input" to "inputs."	Changed per suggestion	*
3	E.1/¶ 3/E-4	Line 8 – Change "that" to "than."	Changed per suggestion	*
4	E.1/¶ 3/E-4	2nd line from bottom - "per cent" is one word (percent).	Changed per suggestion	*
5	E.1/¶ 1/E-5	Line 3 – Decide on wording. Either "…show that a similar effect persists…" or "…show that similar effects persist…"	Changed per suggestion	*
6	E.1/¶ 2/E-5	Line 1 – Decide on wording. Either "…are mostly near…" or "…are most nearly…"	Changed to "mostly near"	*
7	E.1.2/¶ 3/ E-5	Line 4 – Change "analyses" to "analysis."	Changed per suggestion	*
8	E.1.2/¶ 3/ E-5	Line 4 – Insert "a" between "using" and "10-ft," "using **a** 10-ft."	Changed per suggestion	*
9	E.1.2/¶ 3/ E-5	Line 7 – There appears to be a word missing, "…run with a 10 m^3/s for a period..." A 10 m^3/s what?	Changed per suggestion	*
10	E.1.2/¶ 1/ E-6	Line 1 – There appears to be a word missing, "…distributed throughout the total wetted area calculated for the flow of 10 m^3/s, indicates that average..."	Yes - added missing text	*

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
	Infiltration Rates to Support High-Resolution Hydraulic Modeling at Idaho National Engineering and Environmental Laboratory			
11	1/ ¶ 1/ 1	Line 5 – Study reach goes beyond Lincoln Avenue to the INL railroad bridge.	No change - Report completed by F. Fiedler at Uof ID is already finalized. Included only as supplemental data.	*
12	2.1/ ¶ 4/ 1	Line 6 - Change "a" to "as" ("... such as loess ...").	See comment 11	*
13	2.2/¶ 2/2	Line 1 – Insert "of" between "compilation" and "soil," "A recent compilation of soil survey data,..."	See comment 11	*
14	2.3/¶ 3/2	Line 4 – Delete the parenthesis symbol "("and replace with "as."	See comment 11	*
15	2.3/¶ 3/2	Equation (1) – Also define "*x*" from the equation.	See comment 11	*
16	2.3/¶ 1/3	Line 1- Need more description for μ_x and σ_x. They are the mean and standard deviation of what?	See comment 11	*
17	3.3/¶ 1/6	Line 4 - Change "and" to "a" ("...to predict a saturated hydraulic...").	See comment 11	*
18	4.2/¶ 1/7	Line 8 – The word channel is misspelled (chanel).	See comment 11	*
19	4.2/¶ 1/7	Last line - Change "warrant" to "warranting" ("...thus warranting a separate...").	See comment 11	*
20	4.3/¶ 1/8	Line 1 - Change "present" to "presents" ("Shakofsky (1995) presents hydraulic conductivity...").	See comment 11	*
21	4.4/¶ 1/8	Line 1 – Shakofsky is misspelled (Shakofski).	See comment 11	*
22	6.0/¶ 1/12	Line 8 – Delete the second period in 4.7 (4..7).	See comment 11	*
23	GENERAL	Is it possible to include the GIS coverage maps in the final report?	Figure will be added	*

Part 1: Background Information

Document Information

Title: **Big Lost River Flood Hazard Study; Appendix E, Electronic Supplement – Plots of Modeled Flood Inundation for the Big Lost River Downstream of the INL Diversion Dam**

Draft/Rev. No.: **Pre-Decisional Draft**

Date: **September 18, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Abstract E-1	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
2	Abstract E-1 /¶ 1/1	Line 6 – Stage-AEP curves are discussed in Section 3.4 of the summary report and also in Appendix F.	Changed per suggestion	*
3	Table E-1/ footnote 2	Line 2 – Model name RiCOM is misspelled (RICOH).	Changed per suggestion	*
4	GENERAL	All the other appendices use RiCOM not RICOM.	Changed per suggestion	*
5	GENERAL	May want to note that overview and detail maps occur as pairs (overview followed by detail) for each discharge.	This portion of the appendix is intended as an electronic supplement, not as a printed document. The pairing is discussed in para 2 in the context of the electronic version.	*
6				

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study; Appendix F, Stage Probability Plots for Selected Locations at INL Facility Sites**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 27, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Chris Martin
Org: BOR	Org: S.M. Stoller (Project QA oversight)

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	Cover	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy	*
2	Table F-1	Don't print abstract from PDF file to eliminate "Return to Main Table of Contents" in lower right.	Applies to final printed paper copy.	*
3	Abstract/ ¶ 2/F-1	Line 4- The word "correspond" is misspelled (corrponds).	Changed per suggestion	*
4	Abstract/ ¶ 2/F-1	Line 5 – Insert "correspond" between "curves" and "to" ("… the solid curves correspond to the mean…").	Changed per suggestion	*
5	Abstract/ ¶ 2/F-1	Line 5 – Include what document the reference to "Section 2.5" refers to.	Changed to Summary Document	*
6	Abstract/ ¶ 2/F-1	Last line - The word "printed" is misspelled (printer).	Changed per suggestion	*
7	GENERAL	Add some text that discuss: What the sharp inflection point represents,	Text added for explanation	*
8		What a graph with only 0.0 depth represents, (i.e., Map Ref #5 – TRA-670)	Text added for explanation	*
9		Why some graphs appear not to have all four cases plotted (i.e., Map Ref #9 – TRA -715) (overlapping graphs	Text added for explanation	*

DOCUMENT REVIEW RECORD

Part 1: Background Information

Document Information
Title: **Big Lost River Flood Hazard Study Idaho National Engineering and Environmental Laboratory, Idaho**
Draft/Rev. No.: **Pre-Decisional Draft**
Date: **September 30, 2004**

Document Originator	Document Reviewer
Name: Dean Ostenaa et al.	Name: Dr. Paul Link and Dr. Glenn Thackray
Org: BOR	Org: Idaho State University

Part 2: Document Review Comment and Responses

No.	Sec./Para. /Page No.	Comment	Resolution	Acc./Rej.
1	iii	Hortness and Roseau should be value reversed, 106 (3750)	Changed per comment	*
2	5	Should say "bounds and to produce"	Changed per comment	*
3	25 para 1	HWM = High Water Mark?	Added notation as suggested	*
4	31 para 2	"very scattered", not scattered	Changed per comment	*
5	57 para 2	"frequency is no longer constrained", not "not longer"	Changed per comment	*
6	2.1.2 14 para 2	Specify degree of soil development on which age assessment is based.	Additional text is added to clarify this point.	*
7	p. 17, fig 2-3	Stream power is in legend but not on figure. Add stream power to figure or remove from legend.	Figures and text of this section are updated extensively and this figure is corrected.	*
8				
9				
10				