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June 21, 1993

Maureen Finnerty, Superintendent
USDI National Park Service
Olympic National Park
600 East Park Avenue
Port Angeles, WA 98362-6798

In reply to: N1621(OLYM-CRN)

Dear Ms. Finnerty,

I have read the two reports by Randall F. Schalk and Susan Schultz on the ethnographic and archaeological, and historical data, respectively, for native mountain goats in the Olympic Mountains. They are fine and informative reports, but both have their weaknesses, some more serious than others. I thus include with this letter copies of my detailed comments on each report. I presume that your office will forward copies of these to the respective authors. Here, I simply offer a few general comments.

First, I am pleased to see that some detailed research on the issue of "native goats" is being undertaken. I presumed, when I wrote my paper in 1988, that such research had already been carried out by the NPS. I was thus somewhat surprised in particular to find a May 1993 date on Schultz's draft report. Perhaps as a result, I find Schultz's report to be less than an objective treatment of the issue, as I note at length in my detailed comments. Schalk's report is more even-handed, but does not critically evaluate the available samples for their weaknesses to the extent that it perhaps should. Thus, there are some unsupported assertions in it, as well as some factual errors.

Second, both reports are rather convincing despite the lack of objectivity and the errors of comission and omission they contain. I find the Press expedition and Gilman reports to be tantalizing, but not convincing evidence for pre-1925 or native mountain goats in the Olympics. Thus, I agree in part with Schalk's contention that new and original archaeological (or paleontological) work may be the only way to resolve the issue.

And third, throughout my detailed comments on the two reports I offer some ideas I hold. In those remarks, I am not trying to weave a complex web in order to refute Schalk's and Schultz's data or arguments. I am only trying to resolve the Gilman and Press expedition reports of goat presence with other late 19th and early 20th century reports of goat absence, given my understanding of mountain-top historical biogeography and the apparent history of mountain goat biogeography in the Cascades of Washington, Oregon (and Wallowa Mtns.), and northern California. That I can build such a model or scenario (as I did in my 1988 article), of course, does not mean that goats were present in the Olympics prior to 1925. Rather, it serves as a warrant, in my mind at least, for the new archaeological work mentioned above.

The issue of native goats, and the catalyst for my 1988 paper, began for me as an interesting problem in historic biogeography. That is also happened to have significant implications for modern wildlife management made it all the more intriguing. I still find it interesting, and note that I am presently preparing another article on the issue for publication. I thus thank you for giving me the opportunity to see what the NPS is doing regarding this important issue.

Dr. R. Lee Lyman
R. Lee Lyman

Comments on: Schultz, Susan, 1993, DRAFT report "A Review of the Historical Evidence Relating to Mountain Goats in the Olympic Mountains Prior to 1925"

This is a very good and rather convincing report that has much interesting information. I believe, however, that its value might be slightly compromised by several things and in several ways. I elaborate on many of these, and on ways to improve the document, below. But first, I must speak to Schultz's (pg. 55) report that I (Lyman 1988) did not consider much of the historical data on pre-1925 exploration of the Olympic Mountains. While that is correct, it was never my intent to do so. I presumed that Moorehead and Stevens (1982) had already done precisely the detailed kind of research that Schultz does in this report. Apparently they did not, else one must ask why Schultz is writing this report in May 1993. Given that this report has been written "in part" (Finnerty letter of June 15, 1993) as a response to my article, the report seems a little late to me; the barn door is being closed after the horse has already escaped. The NPS made up its mind as early as 1981 and acted on that decision by removing goats from the Park, and this report gives the impression that the NPS is now seeking justification for its actions. From my perspective that results in (a) Schultz's report appearing to be less than objective, and (b) uneven evaluation of relevant data by Schultz. The lateness of this report does not, however, detract from its value. There are, however, some errors, omissions, and implicit assumptions that do detract from its value. I note these and other issues in the remainder of these comments.

The report is incomplete and contains errors of commission and errors of omission:

Schultz cites a book/article by "Catherine H. U" published in 1989 on pages 48-49, but there is no listing in the references for such an author. Similarly, Schultz' Fig. 7 is "From Wood, Men, Mules, and Mountains", but there is no complete citation of this book in the references. Why wasn't Wood's (1967) book on the Press expedition cited? Both of these books contain details not covered in the references apparently consulted by Schultz, although they do not seem to mention mountain goats.

On page 22, reference is made to "(Majors 1981)" but I find no such reference in the references; rather, he is listed as editor under the reference for Barnes and Christie. Note that there is a typographical error in Schultz's Fig. 6 indicating the Press expedition trail was laid in "1189-1890" rather than the correct 1889-1890.

The account of Belmore Brown's 1907(?) ascent of Mount Olympus is not cited or mentioned (see references at the end of these comments). This ascent is mentioned in the Geographical Journal 37(4):565 [1911], and again in that journal in vol. 38(3):319[1911]. In the latter, the editors of the journal indicate that they have "a cutting from the Boston Evening Transcript of June 3, in which the question of the first ascent of Mount Olympus is discussed by Mr. J. Ritchie"; Ritchie apparently goes on to discuss the 1907 ascent by the Mountaineers.

Schultz does not cite Grant's (1905:236) report that goats are "absent...from the Olympic Mountains." This might strengthen Schultz's case, but Grant's paper can be considered a secondary source that, unfortunately, does not cite any references. Similarly, Bailey's (1931) report and map of mountain goat distribution is not cited, and might be considered a secondary source that cites no references, but Bailey's by-line is "Chief Field Naturalist of the Biological Survey" so certainly he had access to the Survey's records.

That the Puget Lowlands acted as a biogeographic "barrier" to mammals is repeated by Schultz at least twice (pages 37 and 40-41). As I implied in my paper (Lyman 1988:24) this is probably the wrong use of the term; the Puget Lowlands were more a biogeographic "filter" route (e.g., Simpson 1953:21).

The report is not as exhaustive as it might be:

The editors of Wickersham's (1961) article note that "an almost identical manuscript" to the published one was found in the National Archives at the Smithsonian; this manuscript included maps. Was this manuscript examined by Schultz? And, was it determined if the National Archives contained any other papers by Wickersham, and was there an attempt to contact "Ruth Allman of Juneau, Alaska, Judge Wickersham's niece" or other descendants for copies of the Judge's papers? Similarly, were the National Archives consulted for papers by S. C. Gilman? If so, then this should be noted. In this respect, it is good that Schultz notes on page 21 that an attempt was made to locate the original manuscript for S. C. Gilman's 1896 report.

Similarly, was the USGS or the National Archives explored for notes from Dodwell and Rixon's survey? The point here is, sometimes it is obvious that intensive and extensive efforts were made to locate particular relevant documents, but it is not clear that such efforts were made in all cases.

The report is not even-handed in its evaluations of data:

On page 21 Schultz suggests that Gilman's 1896 report "may have been considerably edited and altered." This evaluation of published accounts is limited to this one report. All published accounts, including those that do not mention mountain goats, could have been "considerably edited and altered" prior to being printed.

While the taxonomic identity of the "cinnamon bear" is questioned for most who report it, the taxonomic expertise of all quoted authors is not. In one significant place, it is questioned. Charles Barnes and other members of the Press expedition may have seen "a free-ranging goat belonging to a settler" according to Schultz (pg. 23). This would be the only report of any domestic animal any of the early explorers made, wouldn't it? Why would these explorers bother to mention a domestic goat? Christie had spent three years in Alaska exploring just prior to the Press expedition (Wood 1967:17), so I doubt that he would have mistaken a feral domestic goat (or a mountain sheep or domestic sheep) for Oreamnos americanus. Thus perhaps this record cannot be dispensed with so easily.

In another place, the uneven treatment of different observers is clear. Private H. Fisher "made extensive excursions [and] 'kept a vigilant look out for goat and sheep'" (Schultz pg. 27). His taxonomic expertise and his observational abilities are not, however, questioned by Schultz. Rather, the fact that Fisher observed no mountain goats is underscored. Fisher, like many other early explorers, was not a trained naturalist, but that fact is obscured by noting that he was apparently learning natural history from Bretherton (a "biologist") and Henderson (a "botanist") at the same time that he was looking for goats (Schultz pg. 24, 26). Given his alias and the reasons for it, he might not be the most trust-worthy observer or reporter. These points are ignored, but Schultz emphasizes that "none of the members of the Press Expedition were naturalists" (pg. 51). Similarly, while Schultz notes that Gilman's paper may have been edited to include mountain goats, she simply suggests that "it is unusual that the sighting of such a game animal in the Olympics would not have been noted in the detailed daily journal entries" of the Press expedtion; maybe the latter sighting was edited out of the daily journal entries and left for inclusion in Barnes' "Found in the Olympics".

The apparent double standard—people who report goats [the Press party; the Gilmans] didn't really see Oreamnos americanus and are not to be trusted, whereas people [Fisher and others] who did not see goats are to be trusted, even though people in both categories are perhaps equally trained or knowledgeable as zoologists—seriously compromises and weakens Schult's otherwise excellent report. The trustworthiness of people who saw no goats (or at least did not report seeing any) is seldom questioned. For example, Schultz's point that Wickersham (pg. 28) "did not mention observing mountain goats" may be irrelevant. Wickersham wrote the paper Schultz cites in an attempt to get a national park established, not to provide a complete bestiary or natural history of the area. It would perhaps be relevant to note that he was concerned with the preservation of a portion of the Olympic Mountains as a park to "serve the twofold purpose of a great pleasure ground for the Nation and be a means of securing and protecting the finest forests in America" (Wickersham 1961:13). He noted that the area was a "veritable hunter's and fisherman's paradise," and he wanted to keep it that way (Wickersham 1961:13). Thus, he might have strengthened his argument for establishing a park had he included mountain goat in his list of potential prey for big-game hunters. The fact that this (or any) kind of evaluation of Wickersham's paper is not given by Schultz leads me to seriously question Schultz's objectivity.

In some cases Schultz notes when an explorer or early naturalist was in prime goat habitat (e.g., Fisher [Schultz pg. 27]), in others she does not. Reagan (Schultz pg. 33) apparently was not, but that is not indicated by Schultz, rather she simply says "[Reagan] does not mention mountain goats." Further, Schultz's (pg. 46) statement that "several biologists writing about Washington mammals, after the 1920s, state that mountain goats were not native to the Olympic Mountains" is not evaluated nor is the basis for these biologists' statements indicated; could these "several biologists" have been stating a belief based on previous research and reports? Again, we seem to have a double standard. The result is sometimes rather subtle innuendo that effectively increases the weight of the evidence towards there not having been goats in the Olympics prior to 1925. I find such treatment of the data unacceptable; a more even-handed treatement would still, I believe, convince the reader, and be more acceptable.

The report contains a hidden assumption:

I perceive the point on pg. 53 that 19th century observers reported that goats "could be seen 'from a great distance feeding in large droves' where they were present. Yet numerous accounts...do not mention mountain goats [in the Olympics]" to entail a hidden assumption. That is, that goats in the Olympics would have been abundant and thus easily seen in the late 19th century, had they been present. Again, there is a rather subtle innuendo here that goats were not in fact present prior to 1925. The fact that the Press party seems to have seen just one goat, plus the absence of goats in virtually all other historic documents Schultz reviews (with the exception of Gilman's paper), could be interpreted to indicate that goats were quite rare in the late 19th century. Perhaps the Press party saw the last living native goat there. As J. H. Brown (e.g., 1971, 1978) has shown (as well as others in the southwestern U.S., e.g., Findley 1969; Patterson 1980), isolated mammal populations on mountain-top island-like habitats very often go extinct. To go extinct, the population typically must be reduced to below some critical size. The fact that this kind of possibility is not mentioned or entertained by Schultz seriously detracts from her arguments.

Other good things:

Pg. 44—the evaluation of W. P. Taylor's research
Pg. 49—the evaluation of Eliza and Pantoja's descriptions

Summary:

Schultz has produced a very good draft report. However, my reading of it leads me to suspect she did not want to find historic evidence of goats and in fact wanted to conclude goats were not present in the Olympics prior to 1925, despite her caveat that "a failure to find reported evidence of mountain goats in the historical record cannot alone be used as proof that mountain goats were not present: a failure to report is not proof of absence" (pg. 3). The reasons for this suspicion are outlined in the preceding, and include in particular (a) her uneven critical evaluation of the documents she has examined and (b) the hidden assumption that goats should have been abundant. Overall, I am convinced that there is minimal (note that I did not say no) historical evidence for the 19th century presence of goats in the Olympics, but I cannot help but be intrigued by the Press expedition and Gilman reports that goats were present there prior to 1925. These tantalizing data fit nicely with my understanding of and ideas regarding the late Quaternary mammalian biogeography on mountaintops in the west. As Grayson (e.g., 1987) has shown regarding Brown's (1971, 1978) model in Nevada, prehistoric evidence is necessary to confirm my (Lyman 1988) ideas, something I will be arguing for in a paper I am presently writing for publication.

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Comments on: Randall Schalk's 1993 "A Review of the Ethnographic and Archaeological Evidence Relating to Mountain Goats in the Olympic Mountains"

This is a truly excellent, and largely even-handed and mostly complete account of the available evidence. Most of my comments concern errors of omission or differences of opinion regarding interpretation, although a few do concern errors of comission.

On the ethnographic data:

On page 6 Schalk says goats are "limited to the subalpine and alpine zones of the higher mountains" but on page 40 he indicates "mountain goats may occasionally travel to sea level." This is surely a contradiction when cast in the light of his discussion of the ethnographic data regarding human access to goats only in the mountains.

On page 7, and elsewhere, Schalk argues that the "ethnographic data on the distribution of wool dogs suggest a mutually exclusive relationship to the distribution of mountain goats [and therefore] the archaeological distribution of wool dogs...may provide an indirect means of mapping the former distribution of mountain goats." This is clever. But, no such pair of covarying distributions has been demonstrated. Rather, it has been indicated with the data at hand, and must be tested with other data, as Schalk correctly concedes later in his report.

On page 9, Schalk argues that a 90+ year old informant whose words were recorded in 1925 and/or 1927 provided data that "can be extended back to about 1840." Splitting the difference, and subtracting 90 years from 1926, gives a date of 1836. So, this 90 year old informant was remembering what he saw (or was told) when he was 4 or 5 years old? I find that difficult to swallow. The other Quinault informants were 30 years or so younger (1870). Swan's research is thought to be extendable to the "early nineteenth century" (Schalk pg. 11), but it is unclear if this is 1810 or 1830. Virtually all other ethnographic data seems to extend back only to the middle of the 19th century or to the last half or last third of that century. The <u>weight</u> of the ethnographic evidence, in my professional <u>opinion</u> (and that surely is what we must believe, whether the opinion is Schalks' or mine) pertains, then, to probably not before about 1850, and much of it to a decade or more later. This is particularly troublesome when it is realized that there were probably some major cultural upheavals subsequent to the decimation of the indigenous population beginning in the early 19th century (prior to 1840; see Boyd 1990 [references are listed at the end]).

Elemendorf's early <u>20th</u> century statement that "the mountain goat did not occur locally [in the Olympics]" is perceived by Schalk (pg. 15) as contradictory to my 1988 statement that ethnographic data provides negative evidence. In fact, Schalk (pg. 15) indicates I "[<u>asserted</u>] that inferences regarding mountain goats from ethnography are based on negative evidence" (emphasis added). That is not at all what I said; what I said was "ethnographic data <u>may</u> be poor indicators of the presence or absence of native <u>Oreamnos</u> <u>americanus</u> in the Olympic Mountains" (Lyman 1988:14; emphasis added). I still think that to be the case. I concede that the ethnographic data provide no evidence for the presence of goats but with the qualification that I trust it no further back in time than about 1850, and even for the last half of the 19th century it is debateable, given, as noted in the previous paragraphy, the major upheavals in cultural systems caused by significant decimation of the human population. Further, should goats have been present during the early or middle 19th century in low abundances (on its way to extinction; see below), then it is not surprising that Elemendorf was told what he was by his informants. Thus, I cannot agree with Schalk's assertion on page 42 that "the ethnographic data indicate local extinction must have occurred before the late eighteenth century."

Schalk's statement on page 16 that the Songish of Vancouver Island may have been "an intermediary" in the trade of mountain goat materials between peoples of the Olympics and mainland British Columbia is intriguing. What would happen to this argument if mountain goat remains indicating they were at one time in the past present on the Island do to this argumen? Just curious....

On page 17, Schalk suggests "explaining why Peninsula groups valued mountain goat wool and horns and obtained them from their trading partners outside the Peninsula is difficult." This is a two part statement. The first, why the folks valued the materials provided by goats, is evident in the data Schalk presents; wool, for example, was great clothing material. The second, why they obtained those materials from non-local sources, might be explained if goats were rare in the Olympics in the 18th and 19th centuries, much as I suppose. Rare resources might have more expeditiously been obtained via trade from those with easier and/or more frequent access to them. Various theories of foraging and resource acquisition seem to bear this out for non-human and human organisms (see Bettinger 1991; Stephens and Krebs 1986; and references cited therein).

On the archaeological data:

Schalk reviews most of the relevant archaeological research that has been done in the area. There are, however, some omissions. These include no mention or lack of citation of the following by Schalk:

- excavations at Waatch Village (45CA1) reported in American Antiquity vol. 57, pg. 352-353
- research at Tongue Point (45CA16) reported by Wessen 1981
- research at Van Os (45CA253) reported by Bergland 1983
- research at Quilcene (45JE14) reported by Bergland 1983
- research at Deer Park (45CA-?) reported by Bergland 1983
- research at Lake Cushman (45MS100) reported by Wessen and Welch 1988; Wessen 1990
- is Reagan's 1928 report on Queets relevant?
- what about Gustafson's 1985 report on Manis?

I note as well that Schalk cites a reference to Kennedy 1979, but no such reference is listed in the bibliography.

Schalk (pg. 19) asserts that quantitative data for prehistoric faunal resources are unimportant to the documentation of a species' presence or absence. I disagree to a degree because it is clear that for a naturally rare taxon to be found in prehistoric contexts, truly large samples of fossils must be examined, which Schalk in fact implies later. This generally means large volumes of sediment must be examined for fossils (Wolff 1975) and that large numbers of bones and teeth from those sediments must be identified if that rare taxon is to be identified (Grayson 1984). While Schalk does present various data on sample size (both volume excavated, and NISP), he does not evaluate those data in any way that might indicate what a required sample size might be (the statements on the bottom of page 19 are somewhat misleading because they imply that the quality and quantity of the available samples will be evaluated). His statement on pg. 20 that "an excavation of more than $200m^3$ of midden probably contained at least a few thousand mammal bones" thus has absolutely no basis what so ever. In this regard I note that for the eight sites with sufficient data to allow calculation of the density of taxonomically identifiable bones, the mammalian NISP per cubic meter of excavated sediment is 6.6 ± 1.8. Presuming that a similar density of NISP pertains to the Minard site (which it may not given that this site is the only one located on a large estuary), then an NISP of 1346 ± 367 [= $204\ m^3 \times 6.6 \pm 1.8$] for Minard is expected, not the "few thousand" indicated by Schalk.

On page 23 Schalk indicates that "squid have no osseus or non-perishable anatomical parts." This would probably come as a surprise to biologists who examine pinniped stomachs to determine diet; one of the things they often find are "squid/cephalopod beaks" (e.g., Jones 1981:412). While it might be true that squid parts, including beaks, will not preserve in archaeological deposits, that has never been tested or demonstrated.

On page 25, Schalk reports that marmot remains have not been found in any archaeological deposits on the Olympic Peninsula. That is apparently true, but what is disconcerting is the implication that Marmota olympus is an alpine obligate and thus montane habitats were not exploited by the prehistoric occupants of Ozette and other sites on the coast. Marmots have been reported "on a ridge between the Soleduk River and East Fort" by Reagan (1909:193), and reports summarized by Scheffer (1946:79) include Lake Crescent (579' elev.), Hoh River (300-400'), and near Sequim Bay (100'). That is, marmots can be found at relatively low elevations and thus there absence from archaeological deposits seem a poor indicator of a lack of exploitation of alpine habitats.

On page 35, Schalk indicates that, from Judd Peak Rockshelter, "a single element [of unspecified type] was identified as either mountain goat or mountain sheep." Schalk does not reference the final report on that site (Daugherty et al. 1987:202), in which it is stated that

> "a single, proximal first phalanx...is the only specimen referable to the mountain goat. The designation of 'probable' given in [the Table] reflects the fact that this particular fragment closely resembles ones from the bighorn sheep, but it matches <u>perfectly</u> the mountain goat specimens in the [Washington State University] Department of Zoology comparative collection." (emphasis added)

In my opinion, this sounds like a mountain goat toe. Why the 'probable' designation is assigned to it is never made clear by the investigators. But the point here is, Schalk is simply wrong regarding the artiodactyl remains recovered from this site.

Overall, Schalk's evaluations of the archaeological data are good, with the exceptions noted above. At present there simply is no archaeological evidence that leads me to believe mountain goats were native to the Olympic Peninsula. Schalk has taken important steps towards indicating what might be involved in providing such evidence. However, Schalk's (pg. 43, emphasis added) statement that "a <u>single</u> subalpine deposit with conditions for faunal preservation <u>could</u> provide definitive evidence regarding the prehistoric <u>presence or absence</u> of mountain goats in the Olympics" can not be ignored. First, he contradicts this statement at the bottom of page 43-top page 44 when he says "if mountain goats <u>were not</u> formerly present, a conclusive demonstration of their absence may be more difficult to achieve." The problem of demonstrating the prehistoric absence of a taxon from an area has been clear to zooarchaeologists for some time (e.g., Grayson 1981). How to deal with negative evidence in the archaeological record has also long plagued archaeologists, and ways to deal with it are still under development (e.g., Schiffer 1987). I am presently developing one way to build a conclusive argument for the <u>absence</u> of prehistoric mountain goats from the Olympics on the basis of comparative data, somewhat along the lines Schalk describes.

The second thing about Schalk's "single subalpine deposit" statement that bothers me is the word "could." A complete skeleton of a mountain goat dated to, say 200 radiocarbon years ago and found in a subalpine cave <u>would demonstrate</u> their native presence, but the absence of such a skeleton would demonstrate neither that they were present nor that they were absent.

Finally, the qualification that a "single subalpine deposit" will be sufficient (changing the "could" to "will") is not necessarily true. I can demonstrate this for the southern Cascades of Washington and for northeastern Oregon. In both of those areas well over three dozen sites have been archaeologically sampled, about half of which have reported faunal remains. But in both areas only two sites have produced definite remains of goats. Thus, Schalk's single deposit will demonstrate little, unless it happens to be one of the between 15 and 35 sites that might contain faunal remains and just so happens to also contain remains of mountain goats. The probability that the correct "single" site or deposit will be selected on the first try, based on these other two areas, seems to be less than or equal to about 5%. I simply cannot agree that a single site, even one with good preservation and within modern mountain goat range, as Schalk correctly emphasizes, will be a sufficient sample.

Summary:

There are many good things in Schalk's report which I will not elaborate on. Suffice to say that if I did not comment specifically on something in the above, I liked and agreed with what Schalk has to say.

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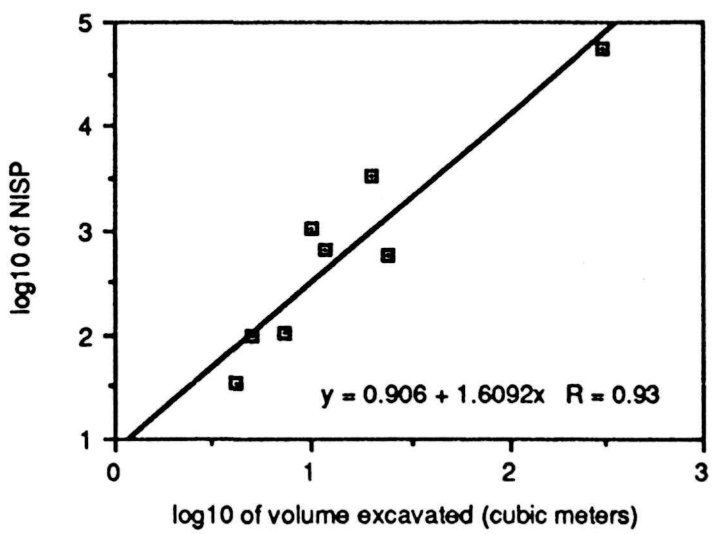
June 25, 1993

In reply to: N1621(OLYM-CRN)

Dear Ms. Finnerty,

I have been re-reading the report by Randall F. Schalk on the available ethnographic and archaeological data for native mountain goats in the Olympic Mountains. The reason for this letter is that I detected a significant error in my June 21 comments on that report, and I wish to bring that error to your attention. On the second page of those comments, I suggest that the average NISP per cubic meter of excavated sediment is 6.6 ± 1.8, and then on the basis of that average I suggest Schalk's estimate of "several thousand" identifiable bones should have been recovered from the Minard site is in error, and that "1346 ± 367" bones should have been found. My numbers and estimates are incorrect. The correct average NISP per cubic meter is 46.8 ± 60, and thus the correct estimate of identifiable bones from Minard would be about 9500. This estimate is much closer to Schalk's estimate. That the volume excavated and the number of identifiable mammal bones (NISP) from a site are closely related is shown in this graph:



I still believe that my comment regarding Schalk's suggestion that several thousand identifiable bones "has no basis" is correct, because he never explains how he came up with his estimate.

Again, I thank you for giving me the opportunity to see what the NPS is doing regarding this important issue.

R. Lee Lyman

Dr. R. Lee Lyman