

April 14th, 1962

Board of Commissioners  
Public Utility District No. 2 of Grant County  
Ephrata, Washington

Gentlemen:

In addition to the salvage archaeology being completed in the Wanapum Reservoir by the University of Washington under contract with the National Park Service and financed with funds provided by your organization, the Washington Archaeological Society has conducted an intensive excavation of a house pit located in section 12, R22E, T19N, at the north end of Osborn or Boothe Bar on the west side of the Columbia River (River M.P. 436) which is part of the site 45 KT 28. The work that the Society has been doing at this site is completely independent of the University's program. The University has been advised of our project. The artifacts and field data will be available to them as a result of this material being turned over to the Washington State Museum as provided for in our articles of incorporation. The information or interpretation that will be made of this material will eventually be a part of the total knowledge resulting from the research done in the Wanapum Reservoir.

Several years of intermittent field work at 45KT28 have yielded a vast amount of information and materials. Three house structures have been almost entirely excavated and four others tested. Much cultural debris in and around these structures has also been removed and many important features recorded. In addition to this, several layers of midden were found encased within the sterile sand below the main body of the site. Some of these layers may be very old, dating, it is certain, at least several centuries before the Christian Era. They have revealed a long and relatively unbroken sequence of cultures each of which is distinguished by the artifacts it left behind. In these excavations over 10,000 cubic feet of earth have been carefully removed, and over 4,000 artifacts recovered and duly catalogued. Our work there has also yielded many thousands of pieces of mammal, bird and fish bone, which will, when analyzed, reveal much invaluable information about the climate, flora and fauna of prehistoric times, as well as the habits of an extinct culture.



Above and beyond these excavations we have conducted a general survey of the Columbia between Lodge Pole and Quilomene Rapids. This survey has revealed many, previously unrecorded burial areas and rock shelters. From the latter we have salvaged about 100 perishable articles including matting, cordage and sandal fragments. The significance of KT 28 cannot be over emphasized. It is one of the few large village sites of great antiquity to have been excavated in this area. The many artifacts, features and associated materials which have been recovered from it will serve to enhance our knowledge of the pre-history of the Central Columbia perhaps more than any other single site. Yet to realize the full potential of these materials, exhaustive analysis and comparative research will have to be undertaken and a thorough, comprehensive report written and published.

The problem facing the Society is that of actually preparing and publishing this report in a period of time which would be current with the field work. Work performed by members of the Society is usually on a volunteer basis. As such this work is done at their own convenience. The person who would be best qualified to prepare the report is an university student. In order to have his services available on a full time basis for the three months required for the preparation of the manuscript, we should offer compensation which would at least equal his usual summer employment. The financial condition of the Society cannot support this type of expenditure. We are therefore soliciting funds from you to prepare and publish our report on this site.

The person referred to in the above paragraph who would prepare the report is Charles M. Nelson. He has been and is in charge of the excavations at 45 KT 28 and consequently has the greatest knowledge of the work. Six years of field experience (summer work) under professionals gives him a limited professional status. His competency to prepare such a report can be verified by Dr. Douglas Osborne, Dr. Richard Daugherty and others. There is no question in our minds regarding his ability to produce a report that will be acceptable at the professional level. He will be attending Washington State University as an anthropology major during the proposed period of publication.

A minimal budget for the preparation and publication of our report on 45 KT 28 is

Compensation to author	\$ 555.00
Preparation of plates & illustrations	250.00
Contribution for publication	900.00
- TOTAL	<u>\$1,700.00</u>



April 14, 1962

The compensation provided for the author is actually a token payment since there will be at least 1,000 man-hours of his time required. Other members of the Society will donate their services in equal proportion assisting in many of the research aspects of the work. We might add parenthetically that over 3,000 man-hours have been expended to date in the recovery of the artifacts, cataloging, survey and compilation of data. The item of \$250.00 for the preparation of plates and illustrations is required for the materials and services necessary to produce illustrations suitable for publication. The item of \$900.00 for the contribution to publication is based on the preliminary discussions we have had with Washington State University. They will publish the report in their Occasional Series published by the Department of Sociology and Anthropology of Washington State University. In order to guarantee publication upon receipt of the manuscript, a certain portion of the publication costs have to be provided. The exact amount of this cost cannot be determined until the final manuscript is available. Dr. Daugherty has assured us that \$900 would be a conservative estimate and there is a possibility that it could be less. One of the conditions of publishing in this series, is having a manuscript acceptable to the editorial board. This point was also discussed at some length and again we have been assured that on the basis of articles published in the Washington Archaeologist by the author, that there would be no problem. Prior to submitting the manuscript to WSU, we will submit the report to at least three professional archaeologists who have published on this area for their review and comments. The prestige implicit in having a report appear in an established scientific series precludes immediate consideration of privately publishing a report of this nature.

The schedule of work we have in mind is for the preparation of the manuscript to run from June 15, 1962 to September 15, 1962, with actual publication taking place during the first quarter of 1963.

This communication is exploratory in nature. There are many points which have not been covered. We suggest that before any decision is reached on your part, that a meeting be arranged between a representative of the Society and a member of your staff so that we could make a comprehensive presentation of the proposed work and possibly arrive at some agreement. Please advise the time and place for a meeting and we will arrange to be there.

Sincerely,

Jack Thomson, President



# Public Utility District of Grant County

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E. L. DOUGLASS, AUDITOR

EPHRATA, WASHINGTON

P. O. BOX 876  
PHONE: SK 4-3541

June 12, 1962

*Jack Parkes*

C  
O  
P  
Y

Washington Archaeological Society  
4037 - 15th Avenue NE  
Seattle 5, Washington

Attention: Mr. Jack Thomson

Gentlemen:

The Commission of this District, by Resolution No. 1092, approved financial participation by this District with your Society in the publication of a written report concerning archaeological findings from excavations and archaeological work in the vicinity of Site 45KT28 as per the request set forth in your letter of April 14, 1962.

The maximum amount of the financial assistance to be provided by the District is to be \$1,700.00. This money will be advanced as expenditures are incurred upon presentation of proper voucher signed by a responsible officer of your Society.

We will be most interested in receiving copies of the report when completed.

If the terms of this letter are agreeable to you, please signify in the space below, returning one copy for the District's files. We include voucher forms for future presentation.

Very truly yours,



E. B. Gibbons  
Manager

EBG:m  
Enc.

Accepted this \_\_\_\_ day of June, 1962 for  
Washington Archaeological Society

By \_\_\_\_\_



RESOLUTION NO. 1092

A RESOLUTION AUTHORIZING THE DISTRICT'S  
PARTICIPATION IN THE PUBLISHING OF AN ARCHAE-  
OLOGICAL REPORT WITH THE WASHINGTON  
ARCHAEOLOGICAL SOCIETY

Recitals:

1. Under the provisions of the license for Project 2114 the District is required to assist in the preservation of archaeological information in the Project area;
2. Personnel of the Washington Archaeological Society have conducted extensive investigations of certain archaeological sites in Section 12, Township 19 North, Range 22 E.W.M., in Kittitas County, being Site 45KT28;
3. It is deemed desirable that the findings and results of the investigation be catalogued and published to preserve such information;
4. The Washington Archaeological Society requires the District's financial participation to the extent of \$1,700.00 for the publication of this information; and
5. It is in the public interest and in the interest of the District that such report be published.

NOW, THEREFORE, BE IT RESOLVED that the Manager of the District is authorized to enter into an agreement with the Washington Archaeological Society wherein the District shall be obligated to provide funds up to but not exceeding \$1,700.00 for the preparation and publication of a thorough comprehensive report of the archaeological investigations and studies relating to Site 45KT28 on the west bank of the Columbia River in Section 12, Township 19 North, Range 22 E.W.M., in Kittitas County.

PASSED AND APPROVED by the Commission of Public Utility District No. 2 of Grant County this 11th day of June, 1962.

ATTEST:

/s/ Paul Neihart  
**Secretary**  
/s/ Eric D. Peterson  
**Commissioner**

/s/ Wm. Schempp  
**President**  
/s/ D. T. Martin  
**Commissioner**  
/s/ Geo. Schuster  
**Commissioner**



David G. Rice  
207 Lincoln Ave.  
Pullman, Washington  
October 4, 1965

Mr. Charles M. Nelson  
1340 Berkeley Way  
Berkeley, California

Dear Monty,

By now you should have received the edited copy of the Kt28 report. Fryx said he sent it early last week. I asked him about the appendix to the report, but really don't know when it will be ready.

I spoke to Doc about sending the Asotin Report to Butler and Swanson for any editorial comments they might have. He said not to do so. Doc thought that the rest of the plan for the Asotin Report was fine. I will need tabular information from your section of the report in order to complete the material culture, so I await your section.

This semester I am taking four courses:

541 Prehist. of W. North America - Daugherty  
501 Human Ecology - Littlewood  
506 Economic Systems in Primitive and Peasant Soc.- Cole  
430 Paleoenvironments - Fryxell

These all look pretty good, except that Littlewood never seems to be prepared, nor does he seem to know where he is going. Fryx's class is very good. We are starting out with soil morphology. He has several required field trips planned. Last weekend was the first of these. We examined tills in the Wenatchee - Leavenworth region, and drew soil profiles. Next weekend I think we will be in the vicinity of Ft. Spokane. Daugherty's course will probably be similar to the one you are taking from Heizer.

I am Walker's T.A., and as things are working out, I am functioning more like an R.A. His course is Introductory Anthropology, so it is little more than a review for me. John Ross from Montana is an additional T.A. for the course. Presently I am spending my time reworking one of Walker's papers on the Nez Perce sweat bath. I gather that he has more such work planned ahead, so it looks interesting.

Enclosed is a check which came in the mail. I received one too, so I suppose that they are from this summer.

Doc hasn't said anything about the Tucannon material yet although I mention it now and then. I don't believe that he anticipates any trouble getting it down to Berkeley, however.

I didn't have a chance to tell you that J.C. was headed for Berkeley, but I understand that he was going to look you up. I will talk with him later this week.

Hope to hear from you soon.

Dave



David G. Rice  
207 Lincoln Ave.  
Pullman, Washington  
October 26, 1965

Dear Monty,

Yesterday I received Table 7 of the Asotin Report as per your letter, so now I have most of the information that I need. Have you calculated the total number of cubic meters removed in the course of our excavations ?

I suggest that you go ahead with the map of site locations for the reservoir because the ethnographic data for the area is being incorporated into a thesis. In fact, at present there is great need for the aerial photos which show the site locations here in Pullman, so please send them as soon as possible to 207. I will glean onto any ethnographic data I can for our report.

[ Fryx's son Tommy has been in the hospital lately, so this and other problems beyond his control has prevented him from finishing up the report on Kt28 and sending it off to you. He said he would give the report his attention as soon as he could. ]

In the event that I get back to the Tucannon sometime this fall I will investigate the area around Test Pit 3. In preparation for this possibility I would like the profile drawings from that pit. Also, I have been thinking that it might be more economical if I write up that sector of the site, thus would you include the artifacts and form data that you have along with the profile drawings. If I have this material by Thanksgiving that will be soon enough.

Enclosed is a bit of mail for you which I forgot to include before. If it is what I think it is I really must laugh. Also included is a note to a "Mr. Dave Nelson" which is kind of interesting, if you are looking for a backhoe.

That is the status quo.

Dave



David G. Rice  
207 Lincoln Ave.  
Pullman, Washington  
December 1, 1965

*Doc 3  
Dec 3*

Dear Monty,

Just arrived back from Thanksgiving, so now will try to answer a few of your questions. For three weeks I have been trying to get down to Asotin to go over place names in the Asotin Res. with Elmer Earl, but there has been no time nor any means for getting down there as yet. In the second week in December I will probably have to go to Lewiston, so will try again then. Doc wanted the areal photos for some fellow who is interested in the Asotin area. I have not asked what he has in mind. The maps are being used to help Madge in her ecological studies of the Nez Perce for her thesis.

I am still interested in the "historic" article, so be sure to bring the level data when you come this Christmas.

Fryx is working on the text of the Kt28 report, which he told me he sent you earlier but did not. When he is finished with the conclusions I will try to appropriate it and bring it with me when I come to Seattle over the holidays. My guess is that unless the appendix is already written you had better forget about it. He also owes Greengo 3 or 4 appendicies which are long overdue. Part of the problem is that he is teaching this fall and studying for his PhD at the same time. Visiting international dignitaries have also interrupted his schedule. It looks like you will just have to wait for him and hope that by Christmas it is done.

The Little Goose report has been published but is on ice. Doc wanted me to tell you that he is not holding out on you, but it is just that no copies are to be distributed until the reservoir is flooded according to National Park Service policy. In essence, we are to forget that the report has been published



or even exists until that time. He will send you a pile of author's copies at that time. Meanwhile I managed to get a single copy which I will bring over to Seattle when I come, but according to agreement it is for our eyes only.

Your note concerning the rock counts at the Tucannon looks useful. I hope I will have a chance to try it out before it is too late.

Yesterday I spent in the field with Walker on the Nez Perce Reservation. We took an informant along to check out the locations of the ethnographically reported village sites - there are some discrepancies of course.

From this point it will be a real grind until the holidays; will see you then.

Dave



207 Lincoln Ave.,  
Pullman, Washington  
10 March 1966

Dear Monty,

By now you should have received the slides and B+W photos from the Tucannon. I have kept the personal shots and also two or three strips of B+W which show rodent activity.

Thanks for the information on microblades. The earlier blade industry, i.e. that from GA3, is definitely more well founded, but I did not recall whether or not we had more than the one "classic" example.

Oh yes, the NW Anthro Conference will be held April 7-9, I will probably go by state car from Pullman Wed., April 6, and return Sunday, April 10. This leaves little time to get to the Coast. If possible, and I won't know until the last minute, I will try to make it to Seattle between the 1<sup>st</sup> and the 4<sup>th</sup> of April. Because of these timing problems I should have these assemblage photos you are having taken by the 30<sup>th</sup> of March — about three weeks from now. I will write 706 and ask about the possibilities. Any particular comments you may have about the stratigraphy would of course be helpful. Also, anything that you find striking about the associated materials would work in. For instance, evidence for some emphasis on fishing in the Cold Springs horizon to supplement the several notched sinkers we have from that provenience.

This weekend I will send the Asotin Maps to Seattle. They will have the adjusted place names & archaeological site locations on them, except for the designation of the Oregon site. I am making up a table of the sites which fall within the ethnographically named settlement patterns. I think now that the ethnographic names should not appear on our maps — they have not been refined enough as yet.

News? Doc has given all his duties as a salvage archaeologist to Rick Sprague, so that he, Doc, may spend more time as an "archaeologist." My proposal for work in the Cascades has been



accepted, but will run on a shoestring. It looks as though the NSF grant for Lake Ozette will go through, at least in part, but Doc does not know what his operating funds for this summer will be yet. Sprague will have a field school on the Snake River - all he has said for sure is that he will survey Lower Granite Reservoir. (Granite point is now being blasted away and you would not recognize Davis Bar). Thus, it looks like I will be between the Devil and the Deep Blue Sea.

Today I spoke with both Doc and Fryx about K+28. Same old story. I imagine that if you wrote Fryx requesting the manuscript for publication whether he ~~was~~ finished or not, you might get it. The situation is ridiculous, before long I will have to try and borrow it to examine your point typology.

That's it for now,

Dave



Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif.  
April 12, 1966

Mr. Roald Fryxell  
Laboratory of Anthropology  
Washington State University  
Pullman, Washington

Dear Fryx,

There are now many reasons which argue for the speedy publication of the report on 45Kt28. (1) Dr. Greengo will circulate a readers draft of his Priest Rapids Reservoir report this June and publish the revised edition in the fall. He is also very near to completing the Wanapum Reservoir report; it will probably be published next winter. Both the Washington Archaeological Society and myself want the 45Kt28 report to go to press before Dr. Greengo publishes. And from your own point of view it would also make sense, since your editorial responsibilities would increase upon the publication of Dr. Greengo's reports. (2) The longer we wait the more expensive publication becomes. (3) The Grant County PUD isn't going to wait forever. (4) The Washington Archaeological Society is even more impatient than ever. In our last communication over the phone in mid December you assured me that you were sending the Society a letter which fully explained the status of the editing and would relieve me of the burden of defending my editors without any concrete information about what they had actually done or were planning to do. I duly reported this to the Society. But no one in the Society has received any word from you or from the Laboratory of Anthropology and my position has become absolutely unbearable because in the Society's eyes as author I am completely responsible for the report. (5) My father's roles as president of the Washington Archaeological Society and family head are in conflict because of my relationship to the report. The argument, which has grown considerably since December, now threatens to boil over into a family issue; this I will absolutely not tolerate. For these reasons I feel compelled to seek a speedy end to the long delay in the final editing and publication of the report on 45Kt28.

Last December you informed me that in a prior meeting with Dr. Daugherty it had been decided that the Laboratory of Anthropology could not publish the monograph until it was fully and properly edited. I agree with this completely, but I also believe that the Laboratory of Anthropology is obligated to render an editorial judgement as promptly as possible. Now I realize that your time is not always your own to spend, but if you cannot find the time to edit the report, why doesn't Dr. Daugherty edit it again himself? Or if he has not the time, why doesn't he delegate the job to someone who does?

If the manuscript is not in my hands by the first of August for the final bit of editing immediately prior to publication, the Washington Archaeological Society is very seriously considering withdrawing it. In such an event I would rework those sections of the report which do not wholly please me and



write up as much of the geology as I could if you no longer could see your way clear to preparing an appendix on the geology. The Washington Archaeological Society would then submit the manuscript to an alternate publisher. Such a decision would not wholly please me, but my obligations to the Society are such that I would have to abide by it. In any case such a decision would give the Grant County PUD their report and relieve the Laboratory of Anthropology of editorial responsibility. Let me emphasize that no decision has been made yet, and that your personal reply (and Dr. Daugherty's too) to this letter will have a great deal to do with whatever is finally decided.

In closing I wish to emphasize that I do not blame you for this unfortunate delay and that I am deeply grateful to you for all that you have done for me, both for the help which you already have given me with the 45Kt28 report and for your advice and friendship throughout the years. I hope that we can resolve the problem which is before us in an expeditious and business-like fashion and that it will not impair our long friendship.

Very sincerely yours,

Charles M. Nelson

copies to:

Dr. Richard Daugherty  
Laboratory of Anthropology

Mr. C. G. Nelson  
President, Washington Archaeological Society



706 16th Avenue East  
Seattle, Wash. 98102  
May 23, 1966

Dr. Richard D. Daugherty  
Department of Anthropology  
Washington State University  
Pullman, Washington

Dear Dick:

Under separate cover we are sending you a Xerox copy of the complete written text of the report on 45KT28 prepared in the format that Mr. Grosshans specified. It includes the report proper, 105 pages; Appendix A, 170 pages; Appendix B, 3 pages; Appendix C, 9 pages; and, references cited, 14 pages. Not included are 104 figures, 66 of which are photographs and 38 drawings. There is a second set of most of the photographs which, I understand, are or at least were at Pullman. This may be true of the drawings also. It is possible that either Dave Rice and/or Fryx know something about these. There is some additional work to be done on some of the drawings since a number of the figures are being combined and reduced. In the meantime I could supply a set of prints of the full scale originals if this would help. There are 3 tables for the report which have not been included. Also 5 tables for Appendix C in the same category.

I would like to suggest that you discuss Appendix D, Post-Pleistocene Geology of 45KT28 and Quilomene Bar, with Dr. Smith so that a decision can be reached, i.e., will Fryx write this portion of the report or should Monty do it. If Fryx is to do it, then there should be an understanding about the timing. I would be glad to do the drafting, or any thing else, if this would help.

As you probably know, Monty has some money to defray transportation costs on the choppers and plans to prepare a paper of some sort about them this summer. We will be in Pullman June 11 and 12 to get these specimens (?) on their way. We could stay over on the 13th (Monday) and take care of any problems or questions concerning the 45KT28 report. We could bring the figures over for Dr. Smith's review. At any rate please let me know what I can do to help this thing along.

Sincerely,



C. G. Nelson



Charles M. Nelson  
706 16th East  
Seattle, Washington  
July 26, 1966

Dr. Allan Smith  
Chairman Department of Anthropology  
Laboratory of Anthropology  
Washington State University  
Pullman, Washington

Dear Dr. Smith:

Enclosed under a separate cover you will find a reworked copy of the main text of the 45KT28 report. It incorporates all of the latest changes in content with the exception of those dealing with the Cayuse III subphase. The paragraphs of this section which I do plan to rework are blocked out in red ink, and Dr. Walker has a good general idea of the changes that I plan to make. With respect to the changes which have been made, pages 19, 20, 21, 22, 25, 27, 28, 29, 34, and 97 have been completely or partly replaced, and pages 98-100 have been discarded and pages 101-105 renumbered 98-102. No basic changes in content have been made in Appendix A, which has been revised three times after the editorial criticisms of Daugherty, Grosshans, and Fryxell, in that order. Since these corrections were made, I have only added a few new references and corrected minor errors of style, spelling, and numerical information, so the copy is essentially like the one you have in hand.

I have sent copies of the corrected version of the main text to both Walker and Fryxell together with comments about the portions of the report which they are most concerned. I also spoke with Fryx about two weeks ago, at which time he told me that he would be through with his editing by August 1. As long as the Washington Archaeological Society knows that reasonable progress is being made on the editing, this deadline need not be considered final. I suggest, however, that you write a brief note to my father outlining the status of the editing so that the Society knows that progress is, in fact, being made. May I also suggest that you finish your editing by September 1, so that I will have time to complete the final corrections prior to leaving for Berkeley between the fifth and the tenth of that month. Since I will be very busy with language exams, my three fields of specialization, and the preparation of a manual of African prehistory for Africa south of the Sahara, I could academically ill-afford the time necessary to work on the KT28 report this fall.

I may get over to Pullman later this summer; if so, I will drop in and see you.

Sincerely,



Charles M. Nelson



August 2, 1966

Mr. Charles M. Nelson  
706 16th East  
Seattle, Washington

Dear Monty:

Your reworked copy arrived a few days ago. I have inserted it its proper place in the manuscript.

Since I am not primarily an archaeologist, I regard my principal service as that of an editor. I have read through much of the main text and have no important suggestions, certainly none which will involve revision. I am catching a few misspellings (e.g., indiscriminantly, dessicate, occassionally, abbrassive, analogous, Kreiger), some citations that do not occur in the bibliography (e.g., Nelson 1965a; Butler 1959), an occasional punctuation slip, and a few ambiguities and awkward phrasings. This is the kind of task which cannot be properly performed until the total manuscript is in hand. When you have the sections which are marked in red in my copy rewritten and have received and incorporated the suggestions from the others who are reading your manuscript, please bring me up-to-date. By that time I shall be back from a brief vacation and two committee meetings on the East Coast and can complete the editing task. Having satisfied myself as to the quality of the report--as indeed I have--there is really no point in concluding the final editing of what I have without the remainder, for it would involve, among other things, repeating part of the task at least to no purpose.

It would seem that this plan, both sensible from the efficiency point of view and necessary in view of my imminent departure for the East, would not place too heavy a burden on your time when you receive my copy. I am marking my corrections very clearly in red ink in the margins, using standard proof-readers symbols. My suggestions and questions are also placed in the margins, bracketed to distinguish them from true corrections. It should not take you long to skim through these and to transfer those you think appropriate to your master copy. Both your copy and the one I have marked should then be sent me to allow me to compare the two. We may then have a very few points which will have to be ironed out through correspondence. But again this should not take more than a week or two. If you can get me the finished MS by September 1, I should be able to complete the copy editing by mid-month (before our academic year begins). You can then do your part and return the two copies to me for comparison. We should be busy with the printing operation by early fall.

I hope that this is agreeable to you and your father. It would appear that this is the only reasonable procedure to follow, and it should not be very time-consuming for you after your return to Berkeley.

Sincerely,

Allan H. Smith, Chairman  
Department of Anthropology



Charles M. Nelson  
706 16th East  
Seattle, Washington  
August 4, 1966

B. Robert Butler  
Idaho State University Museum  
Pocatello, Idaho

Dear Bob,

Enclosed you will find a section of the report entitled The Sunset Creek Site Site and Its Importance in the Prehistory of the Columbia Plateau, to be published at Washington State University late this fall or winter. Since the section is an exhaustive and highly critical review of the Old Cordilleran Culture concept, I felt I should send you a copy prior to publication so that you would have an opportunity to criticise it. If you find that my facts or my representation of your point of view is incorrect, please let me know and I will be happy to make any necessary changes in the text. I would also like to know where we agree and disagree on matters of interpretation and the facts on which you base your ideas. If your arguments seem to me to be strong enough, I would change my interpretations and/or criticisms of the Old Cordilleran Culture concept, giving you credit for your ideas in the text of the report and the section on acknowledgements. Also, if you feel my basic review of evidence is essentially sound, but we are too far apart on matters of definition and interpretation for editorial criticism to be meaningful, I would be happy to publish a rebuttal as a section of the report, itself. No limit would be put on the length of this rebuttal, and tables and line figures could be incorporated if you desired; since I, myself, am over-budget on half-tones, photographs could not be included without financial help. The format of the report will be similar to that of the Llano Estacado (Wendorf 1961), employing two separate columns; single column figures should be drafted at a width of 4.5", two column figures at a width of 9.25"; no figure should exceed 13 1/8" in height, which would correspond to the height of a full page or full column/figure prior to photographic reduction. Do not number figures or tables except for purposes of correlation with your text; we would assign final figure numbers to conform with those in the report. If you decided to write a rebuttal containing figures or tables, it would have to appear as Appendix D, since all figure and table numbers have already been assigned. If it contained no figures or tables, it could appear in the main text immediately following my review or as Appendix D, whichever you wish. In any case, I would reserve the right to include a reply to your rebuttal if I felt it necessary.

If you decide to write a rebuttal please inform me of your plans as soon as possible; how you would like it to appear, approximate length, and when I could expect to receive it. We presently plan to publish in January at the latest and as early as November if possible.

I hope that this letter finds you well and that the enclosed critique does not give you ulcers or heart burn. It is designed, in large part, to initiate some meaningful discussion of your concept and its relationship to the primary data. Happy reading.

Very Sincerely,

Monty

Charles M. Nelson

copy to: Alan Smith, managing editor



Charles M. Nelson  
706 16th East  
Seattle, Washington  
September 9, 1966

Dr. Allan Smith  
Chairman, Department of Anthropology  
Washington State University  
Pullman, Washington

Dear Dr. Smith:

We were very pleased to receive your last letter and I am happy to report that the Washington Archaeological Society is satisfied with the progress of the editing. The Grant County P.U.D., to which the society frequently reports about the status of the report, was also pleased with the prospect of publication sometime this fall or winter.

Enclosed under a separate cover you will find the remaining corrections of content in the text of the report, including a re-revised section on the Old Cordilleran Culture concept and changes made in the section on the origin of the Cayuse Phase. After reviewing the section on the interpretation of the Cayuse III Subphase, I have decided to let that stand as it is, though the date for the beginning of the subphase has been moved up to 1700-1750 AD.

You will note that the treatment of the Old Cordilleran Culture concept is now much more exhaustive. After you received the original revision, it was revised again as new literature came to my attention. By this time the section had reached such large proportions that it was necessary to give Butler the opportunity to comment and/or rebut, so he was sent a copy of the text with that offer (my letter of transmittal is enclosed). By chance, Butler passed through Seattle just a day or two after this was mailed to him and so had the opportunity of discussing the review first hand. At that time he said that he would answer my criticisms in an upcoming publication on the archaeology of the Clearwater Plateau. He also provided useful criticism of my review of his work and called my attention to some new publications which might effect my arguments. As a result of these discussions, further modifications were made in this section of the report; the final edition is enclosed and replaces the comparable section which I initially sent you.

The corrections in the section on the Cayuse Phase are intended to completely replace pages 37 through 46; a modification of page 92 is also enclosed. A copy of the newly revised section has also been sent to Deward for his inspection. Though it might be expanded far beyond its present scope, I believe that it is sufficiently detailed for the purposes of this particular report and hope to expand on it in the near future.

Although I have slated many other minor changes in the text and appendix, none of these involve changes in meaning and content, but are, like most of your own corrections, matters of spelling and grammar. These corrections will be incorporated into the typescript of the report at the time I consider your editorial suggestions.

The missing bibliographic references you have noted result from the fact that the reproducible version of the references cited section does not include the references which have been added in the past six months. These have been typed as new citations have been added to the text, but we will not splice together another reproducible bibliography until I have completed my final editing and all the Appendices are in hand. There are approximately 40 references which occur in the text of the report which are not in your version of the section on references cited.

Several weeks ago, Dr. Greengo offered to run a C-14 date from the site at no expense to the Society. A charcoal sample from one of the early benched house structures in Cultural Component VII has been submitted to him and we expect the results in about one week. The date should be a useful addition to the report.

When you have finished your editing, please send the edited copy of the manuscript to me at the Department of Anthropology, University of California at Berkeley, Berkeley, California. I will then combine your and my own corrections on a single clean copy of the text and send it to my father who will prepare the final typescript. The typers copy and the typescript will then be sent to Mr. Grosshans whose staff will do the final proof reading and see that the typescript matches the corrected copy. Any errors will then be sent back to my father for correction. Then we will go to press.

If you have any further questions or comments, write to me at Berkeley, for I shall arrive there even before this letter reaches you.

Sincerely,

A handwritten signature in blue ink, appearing to read "Nancy", is written over the printed name.

Charles M. Nelson



1340 Berkeley  
Way  
1966

Dear Father—

Enclosed you will find Fig. 3 for the KT28 rept. with corrections on two of the southerly burial sites which contain CAIRN, not talus burials. Figure 2 is O.K.

Will let you know more about Africa when I find more out. It will be hot and humid, so I will need light & bright-colored clothes, some of which I should buy there, ~~as~~ where they are much less expensive.

Goodies are easy to buy and ship out of the country, so have mother save her pennies. I am told they are also very cheap if you go about getting them in the proper way.

Monty

Charles M. Nelson  
706 16th East  
Seattle, Washington  
September 10, 1966

Dr. Deward Walker  
Laboratory of Anthropology  
Washington State University  
Pullman, Washington

Dear Deward,

Enclosed in the attached envelope is a copy of the textual corrections on the origin of the Cayuse Phase, replacing pages 37 through 46. Copies were also sent to Dr. Smith.

After reconsidering the section on the interpretation of the Cayuse III subphase, I have decided to let it stand, though the dates for its beginning will be moved up to 1700-1750 AD.

I am still very interested in you're seminar idea for this winter, but do not know what rules and regulations might apply from the Berkeley end; in any case, it would almost certainly have to be through tape recording. I will write and let you know what cooks as soon as may be. In the meantime you can reach me at the Dept. of Anthro. in Berkeley.

Yours,



Charles M. Nelson



Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif.  
November 15, 1966

Dr. Allan H. Smith  
Chairman  
Department of Anthropology  
Washington State University

Dear Dr. Smith:

Just a note to urge you to proceed with the final editing of the KT28 report with all possible speed. Indeed, it would be ideal if you could send me the edited copy by the end of the first week in December so that I could prepare the final typescript during Christmas vacation. After the Christmas holidays other demands will be extremely heavy on my time and the actual preparation of the typescript could be delayed for months, as this burden has fallen to me since my father's transfer to St. Paul.

Sincerely,

*Ch. M. Nelson*

Charles M. Nelson

Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif.  
January 9, 1967

Dear Deward,

Unless my memory is slipping, you should have all of the changes in content in the text of the report; you can check your copy with that of Dr. Smith to make sure you have the corrections and additions which I sent at the end of the first week in Sept. last. Dave's comment probably refers to those corrections or to a conversation we had in which he mentioned the problem of alternative hypotheses with respect to the importance of fishing in the archaeological record and the possibility that a gradual development had occurred rather than diffusion from the coast. I replied that I though I had dealt with the evidence on fish remains and implements satisfactorily on pages 43, 44, and 45 and that there was not sufficient evidence to warrant constructing an alternate hypothesis of gradual development or evolution. I also pointed out that such a hypothesis had been proposed by Dr. Daugherty and that I had reviewed it on page 100 and 101. Dave suggested that I make some reference to this problem in the section on the Cayuse Phase, and I said that I would footnote the proper paragraph and direct the reader to page 100 and 101 in order to avoid redundancy of presentation. This I have done, but it seemed of little import to bother you with one small footnote.

As you probably know, Rick visited me this last weekend, at which time he mentioned the problem of multiple hypotheses and negative evidence. I asked for a specific example, and he cited my argument that the physical nature of the deposits and lack of structural remains in pre-Cayuse components support the idea that winter village emerged on the floodplain at the beginning of the Cayuse Phase. He suggested the possibility that floodplain villages may have existed and that maybe we haven't the proper areas of sites. I said that we have found early components concentrated in the beach facies of floodplain-beach series in excavations at such places as KT28, Schaae Village, and 45Col and that it was the contrast between the two geomorphic environments that I emphasized in my argument. He said that he did not remember that part of the argument and I said that I would check on it. I have and it's there on page 41, paragraphs 2 and 3, but in reading it over I have also found that I did not emphasize why a change in the area of site occupancy had to be made. The reasons are extremely obvious, but I have now added them by extending the first sentence in the following way (see paragraph 3, line 6): "...of winter villages since semisubterranean structures are extremely difficult to maintain in the structurally unstable beach deposits. Also, any structures that were maintained on the beach would be destroyed by high water during the spring and so would have to be totally rebuilt each fall instead of cleaned out and reoccupied from year to year, a common feature of Cayuse Phase villages which probably contributed to the stability of village areas. The winter occupancy of beach structures would also be hazardous since they would be subject to unpredictable winter floods. That this change took place at....." These are the sum of my reasons.

Rick also said that he objected to my use of the term migration in reference to the Salishan question and I told him that I thought I hadn't used it and that if I had I would certainly change it. He suggested the term diffusion was perfectly good for the process which I described and I agreed. In reviewing the section, I find that I used the term migration only once (paragraph 4, line 6,



page 45b) and at that time did not use it to refer to the process later described and called diffusion (p. 45, paragraph 1). Through most of the text I have used the phrase 'expansion of Salishan communities' in order to avoid use terms of process before the process had actually been described. Rick evidently read the idea of migration into the phrase. Since others might do the same, I have now taken the precaution in the introduction to the hypothesis of changing point 4 (Page 38, paragraph 1, line 17) to read "....across the Plateau by means of diffusion in a particular type of acculturative situation, an interpretation...."

This is the sum of the changes made since 1 Spet. '66. I would be grateful if you passed them on to Dr. Smith and Rick.

The questions of the use and abuse of negative evidence and alternative hypotheses are both interesting and difficult. It may be that our respective philosophies with regard to these tools is somewhat divergent. See what you think from the following.

Negative evidence is essentially evidence which does not exist, i.e. the absence of something. And, since the absence of anything is logically and philosophically impossible to prove, the use of negative evidence is always dependent on the weighing of probabilities. In the absence of hard statistical tests of probability, as for example in most of archaeology and all of the archaeological data with which I must cope, the use of negative evidence is purely a matter of judgement. I have defined negative evidence as appropriate for use when (1) no piece or pieces of positive evidence contradicts/(positive evidence is a verified fact attested to by its presence), and (2) when all available positive evidence indicates that there is a reasonable probability that the proposed piece of negative evidence in question will hold up under further testing. Thus, since the absence of structural remains in pre-canyon riverside components (a piece of negative evidence) is not contradicted by any known piece of positive evidence and is logically consistent with the geomorphic environment chosen for utilization within the site area, I admit this piece of negative evidence into the argument. Without the geomorphic evidence, the negative piece of evidence, the absence of structural remains, would be much weaker and would have to be used with much more reserve. Now, I realize that there is a good deal of room for personal judgement in the use of negative evidence with regard to the second criterion listed above; if you think I am operating outside the realm of credible probabilities with some of the negative evidence I use, please let me know specific cases and your reasoning.

The use of alternative or multiple hypotheses should be restricted to cases in which two or more hypotheses are equally or nearly equally probable. Thus, I offer an alternate hypothesis to Butler's Old Cordilleran Culture hypothesis and state why I feel my hypothesis is more probable given the known evidence; thus I present a multiple working hypothesis with regard to explaining the Cold Springs Phase, because I feel that both alternatives are equally probable given the evidence; thus I do not construct an alternate hypothesis to the Salishan expansion hypothesis because I have not been able to construct one which is nearly as probable an explanation of the archaeological and ethnographic facts as I understand them. If you have some specific hypotheses which you feel are just as probable and which explain the same set of facts that I set out to explain, I would be most interested in hearing them. I do not feel that alternate hypotheses which violate positive evidence or credible probability should be constructed and demolished in the process of constructing an hypothesis. Some people use this as a method of arguing, but in fact it contributes nothing to the probable validity or reliability of the said hypothesis simply because it does not constitute a real test of it. A good hypothesis is one which is in-



ternally consistent, logical, useful in solving some stated problem, and testable by some reasonable practice means. I believe that I have constructed such an hypothesis.

The interrelationship between hypothesis formation, the creative ability or imagination of the observer, and the observer's attitude towards the appropriate use of negative evidence is also a fascinating and pertinent problem. If all kinds of negative evidence are fair game to the hypothesis former so long as they do not absolutely violate known pieces of positive evidence, then the wildest sorts of hypotheses can be constructed and logically defended. For example, I could propose that there was a parallel, tool-using development of hominids in the new and old worlds during the early Pleistocene. Since this hypothesis is literally developed from negative evidence or speculation in the absence of any possible contradicting positive facts, it cannot be disproven, though it can be shown to be statistically highly improbable. A less far out case nearer to home is the Old Cordilleran Culture, ~~in~~ which Butler can continue to defend because it is constructed out of exceedingly pervasive positive facts and a lot of negative facts which cannot ever really be disproven even though many regard them as improbable. A good hypothesis should utilize and explain all the positive facts, utilize as few negative facts as possible, and use only those negative facts which logically absolutely necessary to the hypothesis and/or which have have a relatively high order of probability. Although it was necessary to use a good many more negative facts than is desirable, I feel that I have done a credibly good job of living up to the aforementioned standards given the state of our knowledge about Plateau prehistory.

I would be interested to know what you think of my "philosophy of science" and those portions of the report where I have violated my own cannons I would certainly like to bring into line. If your criticisms involve more basic questions or differences in point of view, I would still be interested in hearing them, though I can't promise that my mind will <sup>b</sup>e changed. I also realize that I am no expert in Plateau ethnography and may have overlooked some very important points (esp. positive evidence) which may effect the status of my hypothesis.

Please forgive the rag-draggle condition of this letter; I am in a terrible rush and don't have time to proofread or polish.

Please advise about KT28 report as soon as possible.

Oh, yes. I completely rewrote the appendix to the Asotin Report dealing with correlation between ethnographic settlements and arch. sites and brought it in line with the info. in Schwede's thesis, so you will probably want to edit again; Dave has it and I am enclosing a copy of the new correlation table which appears with it for your personal files.

Monty



chap. 5  
6-7 continues

# WASHINGTON STATE UNIVERSITY

PULLMAN, WASHINGTON 99163

DEPARTMENT OF ANTHROPOLOGY

LABORATORY OF ANTHROPOLOGY

January 16, 1967

Mr. Charles M. Nelson  
1340 Berkeley Way  
Berkeley, California

Dear Monte,

Thanks for the prompt response and the Asotin chart. Apparently, Sprague communicated, albeit somewhat unsuccessfully, some of my objections to the ethnographic section of your manuscript. First, in response to your comments on the nature of evidence negative evidence is not non-existent evidence. It is evidence that contradicts positive evidence used in support of hypotheses. Positiveness or negativeness is quite relative and determined by whether something supports or detracts from a given statement. Further, it appears that you prefer to operate in terms of polemical dialectics rather than in terms of a cool appraisal of all possibilities. You, like Butler, may soon find yourself concentrating on defense of one possibility, rather than a continuous reappraisal of all possibilities as more and more evidence comes in. A scientist demonstrates his ability, not by the ingenuity with which he defends monolithic hypotheses, but instead by demonstrating his grasp of all possible interpretations and his ability to choose the best one, considering both negative and positive evidence. Am I correct in concluding that you think the scientific spirit consists of intense advocacy of ruling hypotheses? Most hold that it is an impersonal, objective and continuous evaluation of multiple hypotheses and their theoretical matrices with a commitment to the notion that all findings ultimately must be regarded as provisional and tentative.

A second set of observations I wish to offer for your consideration concerns the ethnographic and interpretive section of your manuscript. As you must know by now, Daugherty obtained a 2700 B.P. date for one house floor in his Three Springs Bar site. He also has very serious doubts about the existence in the southern Plateau of the semi-subterranean structures you say preceded the mat covered structures identified with the ethnographic Plateau (cf. p. 39). My own information for the Nez Perces indicates that they were contemporary, the mat lodge being the main structure, and the much smaller semi-subterranean structures serving as dormitories and sometimes sudatories for the unmarried; each sex, of course, had its own structure. Further, caution is required in your use of the term "band," (e.g. p. 39). As you probably know, it has two entirely distinct meanings which you can find discussed on pp. 14-15 in Ray's Cultural Relations in the Plateau of Northwestern America. Similarly, fishing activity tended to be concentrated in the spring and fall (as you imply further down the page) and did not continue year round at the same intensity as you also imply on p. 39. However, some fishing was conducted all year long.



A more detailed discussion of fishing techniques on p. 39 would aid your entire development. For example, we now know that the Nez Perces used several different types of small fish traps, as well as large weirs built across shallower areas in larger and medium sized streams. They were constructed in two sections. The downstream section had one-way entries so that the fish could come into a pool enclosed by an impenetrable upstream section of the weir, which also extended across the entire breadth of the stream. Thus, the fish were entrapped in a pool where both dipping and spearing took place. Beyond this, of course, were the dipping platforms (both natural and artificial) and fish walls. The fish walls were used for both dipping and spearing. Similarly, a "canoes abreast," floating dipping platform was used in shallower sections of larger streams. As you must know, some dipping and spearing platforms on the banks were improved by lining the bottoms of their associated eddies with light colored stones to increase fish visibility. In other parts of the Plateau, and probably among the Nez Perces also, special bow and arrow fishing techniques were sometimes used. Of minor importance, but still present, were a limited number of gorge and line and grab hook techniques.

Given this plethora of fishing implements for the Nez Perces, and probably most of the Plateau, one can legitimately question the archaeological evidence you advance in support of the existence of weirs. So far as I can see, the evidence you advance probably indicates an increasing reliance on fish, but in no way does it indicate progressive adoption of weirs. Clearly, all the fishing implements you have evidence for are known to have been used without weirs or traps. Invoking the progressive adoption of weirs as a principal cause of the Salishan expansion and the concomitant emergence of the winter village pattern might be regarded by some as an example of reliance on "negative" evidence (i.e., nonexistent) as you define it in your letter. Incidentally, the expansion of the Interior Salishan speech community, according to Elmendorf's latest findings (cf. S.W.J.A. 21,1:63-78), was well underway (perhaps by as much as 2500 years) by the probable date you give for the emergence of the winter village pattern. Here we have a good illustration of your disregard of what I would term negative evidence. You fail to grapple with this major discrepancy and thus produce a monolithic hypothesis containing a serious non sequitur, at least if Elmendorf is correct.

Another probable example of this failure to consider negative evidence concerns your somewhat unreasonable implication that before the emergence of the winter village pattern which you associate with Salishan speakers, the preceding inhabitants of the region did not pass the winters in river valley settlements. In view of what we know about temperature and general climatic differences between the uplands and the lowland river valleys, such inhabitants would indeed have been fools not to utilize the river valleys during the colder months. Even assuming that they were primarily hunters and that they did no winter fishing, this movement into the river valleys is still a reasonable conjecture because of the very well known winter movement of the game down into the lower elevations. You must deal with such potentially negative evidence in order to achieve an adequate analysis.



Related to the foregoing requirement is a need to justify your additional assumption that the depressions you say indicate concentrated winter villages also indicate a sharp population increase. It might well mean only a change in the type of structure and/or associated settlement patterns. I also detect a possible non sequitur in your statement in the third paragraph on p. 43 to the effect that "there were no significant changes in the biophysical environment" when I compare it with your statement in the third paragraph on p. 41 that "The fact that this took place at approximately the same time as a shift in the geologic regimen may be coincidental or indirectly tied to the conditions which brought about the emergence of the Cayuse Phase." A further objection might be raised to your failure in the latter instance to describe the "...shift in the geological regimen...."

Given Daugherty's recent 2700 B.P. date on a "pit house" floor, the discrepancy between the Frenchman Springs root processing technology may not be all that much before the postulated emergence of the winter village pattern. This brings me to an additional alternative interpretation you failed to consider. It strikes me as very reasonable that addition of intensive root gathering to a predominantly hunting economy might well produce a surplus sufficient to permit substantial sedentariness during winter months. The many tons of roots regularly taken and successfully stored in cache pits for as much as several years in the ethnographic Plateau period is a bit of negative evidence you dismiss all too quickly in my opinion. Clearly, roots are as reliable as salmon and are known to have been the very last type of stored food resource to be exhausted in the late winter among Plateau societies of the ethnographic period. Likewise, I find curious your statement in the first paragraph on p. 44 that "...it is doubtful that this (increase in the overall utilization of roots) involved a perccapita increase in the consumption of roots." Clearly, an increased individual consumption of roots would have nothing to do with a population increase. What you glided over here is the fact that an increased utilization of roots would have provided a very effective basis for population expansion, particularly because of their storability, even though there had been no per capita increase in root consumption. You must do something about the confused logic and implications of this statement.

An additional interesting bit of negative evidence you failed to consider relates to your discussion (p. 44) of variations in the number of fish bones in the several sites mentioned. The presence of large numbers of fish bones at Five Miles Rapids and Cold Springs may be accounted for by the practice of leaving salmon bones at the site of the fishery. You describe this practice in the fourth paragraph, but fail to refer back to your assertion in the third paragraph to the effect that the frequent fish bones at Five Miles Rapids and Cold Springs were exceptional and by implication, not reflective of the general situation. Inasmuch as such areas were associated with major fisheries used by many Plateau groups, the large number of bones may reflect general and regular, not exceptional use of salmon. For example, the ethnographic Nez Perces and other eastern Sahaptian speakers regularly exploited Columbia river fisheries and brought back to their home territory tons of dried and boned salmon. I enjoy the way you deal with the many factors that could have influenced deposit of salmon bones in the fourth paragraph. Perhaps you intended that the reader evaluate paragraph 3 in terms of the implications in paragraph 4. If so, this must be made clearer to him.



I also appreciate the way in which you deal with the evidence for nets and their functions in the first paragraph on p. 45. Your objectivity here, however, contrasts with other parts of the ms. and probably stems from the fact that it does not support your hypothesis. For example, you fail to be as scrupulously objective with the evidence you advance at the end of this paragraph for the existence of weirs and traps. As I have noted elsewhere, the evidence you advance for traps and weirs is actually nonexistent. Their presence is strictly inferential.

In the third paragraph on p. 45a I would caution that when faced with starvation, Plateau peoples regularly employed a number of emergency procedures. Among them were use of the cambium layers of certain trees, tree moss, and trips down the Columbia as far as Vancouver and Willamette Falls to intercept the salmon runs early. We also know that winter fishing, sometimes beneath the ice, was common. Likewise severe winters usually meant that the game became more, not less, available. Deep snows and cold temperatures made snow shoe hunting very profitable among the Nez Percés. The fact that cache pits could preserve foods for several years also should not be forgotten. My principal point here is simply that while failure of a resource such as salmon undoubtedly was a serious matter, the people were not devoid of emergency measures probably sufficient to tide most of them over the difficult period. Thus the occasional "population decimation" you hypothesize in paragraph three may be too strong a term. Characteristically, this overstatement appears in a context where your ruling hypothesis could be questioned.

In the third paragraph on p. 45b, you invoke Beardsley (1956) out of the blue to summarize the section. Irregardless of whether the summary is tenable, he should have appeared very early in this section and the summary should be yours alone. Likewise, I find clumsy the placement of your "Expansion of Salishan Communities Across the Northern Plateau" on pp. 45b-45d. It might fit better somewhat further back in the section dealing with "The Ethnographic Pattern." Further, in the second paragraph on p. 45c, I would like to see you deal with the possibility that increasing trade with the coast may have been a function of the Chinook commercial expansion, rather than simply a function of a Salishan expansion.

In the second paragraph on p. 45d it is also necessary that you show why one could not view the maritime fishing techniques as modifications of inland techniques. This is necessary since some have postulated with good evidence, e.g. Cressman, that the coast was settled after the interior. Also making this necessary is the conclusion drawn by Suttles and Elmendorf that the Salishan speech community parent to the Interior and Coast Salishan speech communities was itself inland and probably riverine in orientation. I would also like to know what implications for your hypothesis you see in Jacobs' observation that fairly recently some interior Salishan speaking groups were replaced by Sahaptin speaking groups. I believe he locates this along the speech boundary presently separating the various Yakima groups from the Interior Salishan groups to their immediate north. I shall refrain from commenting further on your model for expansion of Salishan communities until I hear from you.



Frankly, I find your analysis extremely exciting and only wish we could get more archaeologists to abandon the traditional, naive "tool types" and "environment" approaches, for the more difficult but infinitely more significant, higher level approach you employ. I would be deeply saddened if you did not persevere in this attempt. Further, you probably are very close to the historical truth in much of your analysis. As you have seen, my objections to your analysis concern strategy primarily. Your enthusiasm for a monolithic ruling hypothesis leads you to over-interpret your evidence and to underrate, and even ignore, perfectly reasonable alternative interpretations and negative evidence.

I remain very interested in your ms. and hope to hear from you soon.

Sincerely,

Deward E. Walker, Jr.  
Asst. Prof. of Anthropology

DEW/ha



Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif  
February 15, 1967

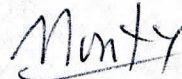
Dr. Allan Smith  
Chairman  
Department of Anthropology  
Washington State University  
Pullman, Washington

Dear Dr. Smith:

Enclosed is a copy of a bibliography of references which should cover all of the citations in the KT28 report. I hope it makes your job a little easier.

I am working on Deward's suggestions as my spare time permits, but the brevity of the Winter quarter (only eight weeks long) dictates a tight schedule. It is probable that I will not be able to get the additions and corrections to you until the middle of March, during spring vacation. These corrections will only involve that part of the text which deals with the interpretation of the Cayuse Phase, i.e., pp. 36-46.

Sincerely,



Charles M. Nelson

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Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif.  
April 10, 1967

Dr. Allan H. Smith  
Chairman, Department of Anthropology  
Washington State University  
Pullman, Washington

Dear Dr. Smith:

Enclosed you will find a complete copy of the text of the 45KT28 report containing all corrections and additions which have been made to date. Rewritten sections which will not correspond to the copy which you presently have include the main text, pp. 36-46, and the bibliography. Other corrections and additions are made in red ink with the exception of a few typed insertion sheets. Green numbers along page <sup>margins</sup> refer to the approximate placement of figures and correspond to the figure numbers used in the text.

The totally rewritten portion of the main text corresponds to the major interpretive section on the Cayuse Phase. Changes were made to include more recent references, to include a series of older references previously unavailable to me, and to incorporate many of Dr. Walker's useful suggestions. As you will note, the new source material has enabled me to produce a far more sophisticated essay on the origin of the Cayuse Phase, but it has also meant about 20 new pages of text.

Changes in the bibliography are due to the addition of several new sources and to changes in form, especially where series or institutions have changed their names since the initial drafting of the bibliography.

You will also note that there are many changes in numbers of artifacts in the component catalogs and in Appendix A. This is due to a wide variety of reasons including: (1) the finding of seven level bags from which tools had not been completely catalogued, (2) discovery of three minor groups of artifacts previously not recorded in Appendix A (core tool frags., possible burins, and hammerstone fragments), (3) complete reclassification of all the tools as a double check on Appendix A, (4) the redesignation of two levels from Cultural Component VII-undesignated to VIIIA, (5) discovery of an error which showed some tools as deriving from VIIE instead of VIIF, (6) discovery of five mislabeled tools, and (7) discovery of previously unnoticed typographical errors in the text. These have resulted in numerous minor changes of tool class frequencies, but they do not affect the interpretive sections of the report. They have also created some minor changes in some of the tool distribution tables; corrections have been made and corrected copies will be sent to you shortly by my father.

Other changes include minor notes on spelling, syntax, form, and the like, as well as the addition of current references, recently published C-14 dates, and minor corrections and additions of content.



The inclusion of new data has made it necessary to revise Figure 1, while receipt of ~~of~~ a new and much better photo from the USGS is also causing us to redraft Figure 3. When this has been completed (which should be very soon), father will send you all of the original drawings as well as Figs. 2 and 3. Since you have prints of the artifact photos, these will not be sent unless you request them from my father (address 502 Portland, St. Paul, Minnesota).

I have just learned that I will be spending the summer working with Merrick Posnansky in Uganda, probably from early in June until sometime in September. It is therefore imperative to finish the KT28 editing (at least in so far as it concerns me) by the end of May. To this end I hereby give you my express permission to make any editorial changes of spelling, syntax, and form which do not involve alterations in basic meaning or content. I am willing to rely completely on your judgement in making such changes; they may be incorporated into the text without my immediate knowledge.

If there are any changes of content which you feel should still be made, please advise me of these as soon as possible so that we can thrash them out before I leave for Uganda.. If you are satisfied with the content as it stands, proceed to publication with all possible speed.

I have sent Deward a copy of the revisions on the Cayuse Phase so that he can advise you on his opinion of my revisions.

I do not know the state of Fryx's appendix, but will leave that problem to you. If Fryx for some reason cannot contribute the appendix, simply strike references to it out of the text, which embodies a sufficient description of the stratigraphy for the purposes of the report if needs be.

My father is no longer in a position to type corrections or copy for the text, and I do not have access to an Adler typewriter with a carbon ribbon here in Berkeley. Even if I did, I would not have the time to do the job because of my school work. If possible, I suggest that money be found to retype the text in Pullman, something which would also give you a free hand at changing the format of the publication if you so desired.

Well, I think that about does it. If you have any burning questions, please don't hesitate to call collect. I am usually home in the evenings.

Sincerely,

*Marty*

Charles M. Nelson



April 1967

1340 Berkeley Way

Dear Father,

Glozer's no longer exists as far as I can tell--went out of business a couple of months ago and I haven't been able to trace its stock yet. Holmes Book Store is large, but not particularly good for anthropology--no BAE or that sort of thing. They do have a small section on Rail Roads, however. I also checked several other stores for the book you wanted, but no one has seen any copies for years.

Enclosed is a copy of the letter I sent Smith with the KT28 report. Deward has already replied to the section which concerns him, saying that he has passed it on to Smith with approving comments. But I have not yet heard from Smith.

Please sent slide cammera, ring flash, power pack/as soon as possible; cable release, bellows, have not got keys and access to museum collections and want to take advantage of it immediately.

Got to run now

Monty



Charles M. Nelson  
1340 Berkeley Way  
Berkeley, Calif.  
April 10, 1967

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Chairman, Department of Anthropology  
Washington State University  
Pullman, Washington

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Sincerely,

*Marty*

Charles M. Nelson



1340 Berkeley Way  
April 25, 1967

Dear Father,

Enclosed you will find the corrections for Figure 1, which I have altered by excluding some of the less important sites and including some more impo. ones. The number of spaces (52) and general order of things is the same as before so that changes will not cause total redrafting. I have added site numbers where this is possible; if no site no. is present, than it is not published.

Yes, return the 9 X 9's to Rick in Pullman.

Have not been able to locate the Cross ball point pen, but I have lots of fillers; do you want them?

Enclosed is strobe sinc cord--it was the wrong one; I have purchase the proper type, so don't worry.

I will retain my NSF stipend while in Uganda, but am having trouble in getting all of the subvention funds that I wanted for travel expenses. Will let you know how things come out. Will also try to route myself through St. Paul on the way to Uganda, so don't get nervous and send me things just yet.

I have just purchased a Munsell Color book so that I'll have that when I go, but I haven't been able to get a template for taking cross sections. Do you still have the literature on it that we got last summer. If so order one if you can so that I have it before leaving.

Got to study French.

Marty



502 Portland Avenue  
Saint Paul, Minnesota 55102  
May 1, 1967

Dr. Allan H. Smith, Chairman  
Department of Anthropology  
Washington State University

Dear Dr. Smith:

Monty has asked me to send you the reproducibles of the figures and tables for the Sunset Creek Site report in addition to the other figures mentioned in his letter to you dated April 11, 1967.

The following figures are sized to 9-1/4" x 13-1/8" which are to be reduced to 6-1/2" x 9-1/4" for publication:

Figure 1. Location of Important Sites and Surveys Cited in Text.  
Tracing.

Figure 2. Oblique View of Quilomene Bar and Environs. Photograph.

Figure 3. Site Locations on Quilomene Bar. Photograph.

Figure 4.

On one plate:

\*Figure 5. Stratigraphic Section in the House Pit 15 excavations Taken Parallel to the River Bank.

\*Figure 6. Stratigraphic Section in the House Pit 15 Excavations Taken Perpendicular to the River Bank.

\*Figure 10. Schematic Profile in the House Pit 13 Area.

On one Plate:

\*Figure 18. Schematic Relationships between House Floors in the House Pit 15 Area.

\*Figure 19. Subcomponents in the House Pit 15 Area.

On one Plate:

\*Figure 20. East-West Stratigraphic Section Through Subcomponents VII B and VII F.

\*Figure 21. East-West Stratigraphic Section Through Subcomponents VII C and VII H.

\*Figure 22. North-South Stratigraphic Section Through Subcomponents VII C and VII H.

On one Plate:

\*Figure 23. East-West Stratigraphic Section Through Subcomponents VII D and VII E.

\*Figure 24. Plan View of House Pit 7.

On one Plate:

\*Figure 26. House Pit 1, Map of Excavations.

\*Figure 27. Profile at Southern Lip of Subcomponent VII G.

\*Figure 28. Feature Complex at Northern Edge of Subcomponent VII G.

On one Plate:

\*Figure 29. Stratigraphic Section at the Northern Edge of Subcomponent VII I.

\*Figure 30. Reconstructed North-South Stratigraphic Section Thru Subcomponent VII K.

\*Figure 31. Stratigraphic Section at the Southern Edge of Subcomponent VII J.

\*Figure 34. Schematic Development of Stemmed and Leaf Shaped Projectile Points.

\*Figure 35. Schematic Distribution of Chipped Stone Projectile Points.

\*Figure 36. Schematic Distribution of Stemmed Projectile Point Types during the Cayuse Phase.

Figure 42. Stemmed and leaf-shaped projectile points, miscellaneous forms.  
Photograph, scale 1:1, negative # C 10, letter designations corrected.

Figure 57. Scrapers, Type Variant 3B. Drawing, scale 1:1, sized to appear in text [single column].

Figure 58. Scrapers, Styles 1 and 2. Drawing, scale 1:1.

Figure 60. Bifacially flaked tool. [3 views] Drawing, scale 1:1.

Figure 61. Possible microblades. Drawing, scale 1:1.

Figure 62. Possible microblade cores from Cultural Component III.  
Drawing, scale 1:1.

Figure 65. Cobble scraping planes. Drawing, scale as indicated [2:1].

Figure 83. Bone and antler pins from Subcomponent VII c. Drawing (pencil), Scale 1:1

Figure 84. Composite harpoon valves, Type 1 and Forms 1. Drawing (pencil), Scale 1:1

Figure 87. Incised digging stick handle from floor level of Subcomponent VII B.  
Drawing (pencil), scale 1:1.

Figure 89. Flaking implements. Photograph, scale 1:1, letter designations corrected.

Figure 94. Anthropomorphic figure of fossil ivory for Subcomponent VII H.  
Drawing (pencil), scale 1:1.

Figure 95. Antler comb. Drawing (pencil), scale 1:1.

Figure 101. Perishables from Sites 2, 4, and 7. Drawing, scale 1:1---  
Photographic reduction may be necessary.

\*Table 1. Vertical Distribution of Stemmed Projectile Points.

\*Table 2. Vertical Distribution of the Type Varieties in Type 6, The Columbia Plateau Corner Notched.

\*Table 3. Vertical Distribution of Utilized Flakes.

Tables 4, 5, 6, 7, and 8, for Appendix C. Reduced to single column width.

\*Indicates that a full size negative is being furnished for these figures.



Dr. Allen H. Smith

--3--

May 1, 1967

The statement Monty makes with regard to further typing is basically true--being a department head (chairman) with a system responsibility (Minnesota to California) I have less time available with suitable continuity to permit any large project. However this particular publication is one that I wish to see completed--there is an obligation to the PUD and to the WAS--so I am ready to assist in any way that I can. Let me know what I can do to help.

Sincerely,



C. M. Nelson

cc: C. M. Nelson



502 Portland Avenue  
St. Paul, Minn. 55102  
May 23, 1967

Dr. Allen H. Smith, Chairman  
Department of Anthropology  
Washington State University  
Pullman, Washington 99163

Dear Dr. Smith:

Fortunately Monty read his copy of my letter to you dated May 1, and reviewed the enclosed prints of figures and tables. He pointed out that Tables 1 and 2 were an earlier version which had been subsequently revised. I have brought the full scale drawings of these tables up-to-date and am enclosing a reproducible and print of each. It is entirely possible that the reduced scale negatives you have are of the latest issue. At any rate the full scale prints are the very latest.

I might explain that there is apparently some material handled by the movers that was mis-filed, mis-laid, mis-something-or-other: net result mis-sing. We are still finding things and I know that sometime when we least expect it, these tracings along with other missing things will be found.

Sincerely,

C. G. Nelson

cc: C. M. Nelson



502 Portland Avenue  
St. Paul, Minn. 55102  
September 13, 1967

Mr. David Rive  
Department of Anthropology  
Laboratory of Anthropology  
Washington State University  
Pullman, Washington 98163

Dear Dave:

With reference to your letter of August 26th concerning the Asotin Report, my records show that the original tracings were sent for the following:

- Figure 1. Site Locations in the Asotin Dam Reservoir.
- Figure 9. Characteristics of Seasonal Camp Sites in the Asotin Reservoir.
- Figure 10. Characteristics of Burial Sites in the Asotin Reservoir.
- Figure 12. Schematic Representation of Fish Walls in Plan View and Cross Section.
- Figure 27. Archaeological Evidence for Nez Percé Settlements.
- Table 1. Level Data from Test Pit 1, 45AS12.
- Table 2. Level Data from Test Pit 1, 45AS35.
- Table 3. Level Data from Test Pit 1, 45AS37.
- Table 4. Level Data for Test Pit 1, 45AS47.
- Table 5. Level Data for Test Pit 1, 10BP1.
- Table 6. Distribution of Chipped Stone Artifacts.
- Table 7. Distribution of Chipped Stone Projectile Points.
- Table 8. Flaked Stone Artifacts by Site.
- Table 9. Pecked and Battered Stone.
- Table 10. Measurements of Cobble Implements Tabulated by Percent of Total.
- Table 11. Distribution of Ground Stone, Bone and Antler, Shell, Perishable, and Nonaboriginal Artifacts.
- Figure 2. Sites in the Asotin Reservoir tabulated by site feature.

Table 10 was the only not submitted in the original. In lieu of this a photo reduction of a print was made and this was marginal. In going through the file the original has been found and this is being sent under separate cover.

In the same container are some tracing of 45KT6 which may be of some use should you get around to writing this site [up].

Kay and I are coming out west and will be in Waitsburg by the week-end of October 7 & 8th. If you have an expedition going up to the Ben Franklin Reservoir, we might be able to join your group. Could you let us know? We're leaving St. Paul September 26th. At any rate we'll try to stop in Pullman on the way to Spokane.

Until later,

CGN



September 27, 1968

Editor Fryx:

Per Monty's request am sending you under seperate cover all of the negatives of the figures for the KT28 report. With this you should have all of the original reproducible material that we have. Enclosed is a copy of the List of Figures which has been marked to show those negatives sent on to you.

Am anxious to see the Sunset Creek Report in print. If there is anything I can do to help, please let me know.

Sincerely,



September 27, 1968

Editor Fryx:

Per Monty's request am sending you under seperate cover all of the negatives of the figures for the KT28 report. With this you should have all of the original reproducible material that we have. Enclosed is a copy of the List of Figures which has been marked to show those negatives sent on to you.

Am anxious to see the Sunset Creek Report in print. If there is anything I can do to help, please let me know.

Sincerely,

CGN

C. G. nelson

502 portland avenue

st. paul, minn. 55102

January 15, 1969

Dr. Roderick Sprague  
Department of Sociology & Anthropology  
University of Idaho  
Moscow, Idaho 83843

Dear Rick:

With reference to the publication of the report on 45KT28, Sunset Creek Site, I called Jack Parks, who is the proper "archaeological" contact in Grant County PUD at Ephrata, Washington and discussed the publication of the report on the Sunset Creek Site.

These are some of the facts or points that emerged from this discussion:

All of the accounts pertaining to the construction of Wanapum Dam, including the salvage archaeological funds, have been closed out. However, the fact that Resolution No. 1092 is still outstanding in the amount of somewhere between \$1000 and \$1200 gives us a favorable opening. Since there has been a change of the guard, i.e., new manager some new commissioners, etc., Parks advised that we should give the thing the hard sell rather than brashly announcing that we want our money. The president of the W.A.S. should therefore write to the P.U.D., attn.: Mr. Jack Parks, refer to the letter of agreement and reopen the subject. Copies of the resolution and letter of agreement should accompany the letter. A statement of the funds spent should be included, the balance of which is being requested for support of the actual publication. There should be a recitation of the chronology of delays, procrastination, projects of higher priority pre-empting editorial work, etc., on the report. The editorial reviews by Smith, Daugherty(?), Fryxell, Walker, yourself, and whoever else worked on it. The confrontation that ultimately resulted in NARN acquiring the manuscript and publishing rights should be included. A definite publication proposal should be made. A sample format should be presented along with a statement concerning the type, method of printing, specification of the paper, quality of half-tone reproductions, approximate length of publication, number of copies to be produced, some cost data, i.e., unit cost at different quantity levels. The P.U.D. wants 400 copies, the W.A.S. wants 150-200 copies [they will say], the author want X number [he will say], and NARN wants their usual quantity. The cost figures



Dr. Roderick Sprague

January 15, 1969

above could support a claim that the number required in excess of the NARN quota, 400 at the unit cost which will pay for 600, cannot be furnished for the \$1200 contribution and that an additional amount of money is needed. This, of course, should only be done if there is a genuine need for additional funding. The difference in printing costs from 1963 to 1969 could be cited as a factor.

It may be good strategy for you [Walker] and Del Nordquist to work out the details and discuss these informally with Jack Parks before framing the proposal. It may be well to have the Editor of NARN make a statement concerning the manuscript and the contribution it will make, etc., etc. With an appropriate amount of work I feel [that] the money will come. Parks is on our side and he will go for any bundle that is reasonable.

Am enclosing copies of some of the correspondence which may give you a feel for what has preceded the present effort. Again, if there is something further I can do, please don't hesitate.

A short note about the conference at Tuscon--Kay and I enjoyed it very much. In terms of 'interaction,' it was great. The bead merchant of Victoria, B.C., is Desiré de Tremandau. He works out of his home, so a check of the phone directory or Information should develop this. He is beyond his 70's. A field trip to the Provincial Museum at Victoria would give an opportunity to interview Desiré.

Sincerely,

C. G. Nelson

cc: C. M. Nelson  
Department of Anthropology  
University of California  
Berkeley, California 94720

Del Nordquist  
10421 26th SW  
Seattle, Wn. 98146

# WASHINGTON ARCHAEOLOGICAL SOCIETY

WASHINGTON STATE MUSEUM

4037 - 15TH AVENUE N. E.  
SEATTLE 5, WASHINGTON

NEW ADDRESS

WASHINGTON ARCHAEOLOGICAL SOCIETY  
P. O. Box 84 University Station  
Seattle 5, Washington

March 26, 1969

Professor Deward E. Walker, Jr.  
Department of Sociology and Anthropology  
UNIVERSITY OF IDAHO  
Moscow, Idaho 83843

Dear Professor Walker:

Mr. Charles G. Nelson and I had a long discussion about the 45 KT 28 Report of "Monty" Nelson, which I understand from Dave Rice, is on the way to you. Dave has collected the MS. plates, etc. for your consideration.

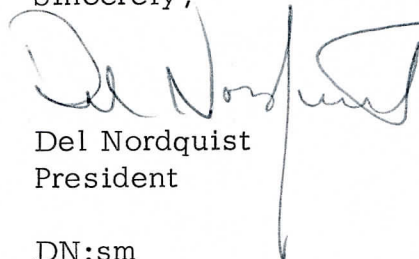
It is my understanding that the author will be in St. Paul in early April, departing on the 25th for Africa, should you wish to contact him about it. Monty (Charles M.) did a final revision not too long ago, also preparing the MS. for two-column, photo-ready reproduction if this is of interest to you. I have no idea whether your copy is identical. There have been some last minute changes so perhaps you might find it advisable to write or call him.

The complete report has about 200 pages of text and 110 pages of plates, about six of which will be fold outs. Most of the plates require half-tone reproductions.

In terms of our arrangement W.A.S. will apply, in concert with you, for additional funds to cover the proportionate costs of the 250 copies we would like to get for our membership. I'll wait until you can give me your printer's estimate of costs for that number.

Keep me posted as to your plans for NARN. We'll follow your advice as to when and how to reapproach GRANT CO. P.U.D.

Sincerely,



Del Nordquist  
President

DN:sm



Charles M. Nelson  
502 Portland Ave.  
St. Paul, Mn 55102  
April 25, 1969

Deward E. Walker, Jr.  
Editor, NARN  
Sociology/Anthropology  
University of Idaho  
Moscow, Idaho 83843

Dear Deward,

Dave has told me of the various copies of my manuscript which he recovered from various offices around WSU. As I have not seen any of the editing done potentially by Fryx and others in the last year in these copies I am sending you a clean version of the text with a couple of minor additions and corrections. From this you should be able to sort out the most modern copy and work from there.

You have my permission to do what you think best with regard to syntax and spelling just so long as content and meaning are not changed.

I have not included comment on Caldwell and Mallory's Snake River bit as I feel the text itself is comment enough. Their conclusions are so tentative and so vague that a specific comment would require 10 or 15 pages and still be rather beside the point when all was said and done. I have, however, included comment on Sanger's recent theory (1967).

What are the arrangements for author's copies?

I hear you're going to a very nice position at Colorado. Congratulations.

Would you please rework the acknowledgements to include yourself, Sprague and any others who have contributed in recent years? Thank you.

Best Wishes

Marty

May 20, 1969

= Dear Dave:

Will I see you at Ellensburg? We'd better. Incidentally I'm learning to use an IBM electric and it fouls me continually. You'll see shortly, or have you already.

You have received a copy of Grant Co. PUD with the July 1, deadline. on publishing 45KT28.


*rather* I wrote the same to Walker. He claims he can't get anything out *by then* — ~~by September~~, which naturally loses all the money. At the same time he asks for ~~more~~ another \$500 to ~~fat~~ fatten the kitty, for no avail since he can't get the paper out before September.

As I see it, unless you can come up with something else, NARN is out.

Walker seems disturbed by PUD's request for a colorful cover. Why must all Univ. publications be so damnably dull in appearance? I'm for something interesting whosoever reads it. We might get a few more looking at this stuff, which is often pretty dry anyway.

Let's give it a round in Ellensburg, but I'm ready to look elsewhere if Chuck concurs. Otherwise it's dead as far as PUD is concerned.

See you,

A handwritten signature, possibly "De", written in dark ink.



GREAT  
NORTHERN



Moscow 83843

Great Northern Railway Company / 175 East Fourth Street / Saint Paul, Minnesota 55101 / Phone 612 224-5588

Engineering Department

1969

DAVE:

As promised enclosed are  
prints of Fig 7 (E11), 8 (C7),  
9 (C12), 11 (C6), 12 (C11), 13 (C3)  
and 14 (C5). Also a Xerox copy  
of the table of contents.

Enjoyed our visit. If I  
can help in any way please  
let me know. I appreciate  
your efforts on behalf of  
Sunset Creek as does Monty  
Kay Del and others.

As ever

Chuck - Nelson.

June 16, 1969

Mr. Jack J. Park  
Senior Engineering Assistant  
Grant County Public Utility District  
P.O. Box 873  
Ephrata, Washington 98823

Dear Mr. Park:

The Washington Archaeological Society is in the process of billing your district in the amount of \$1,200 for costs in the publication of C.M. Nelson's report "The Sunset Creek Site and Its Place in Plateau Prehistory." This report is based on excavations at site 45KT28 in the Wanapum Dam Reservoir area. The report is extraordinary in that it not only contains a detailed description of the results of excavations at 45KT28, but also presents one of the few syntheses of Plateau prehistory. The manuscript contains about 200 pages with typescript on both sides, about 50 half-tone plates, 50-60 line drawings of which a few are foldouts. The estimates for printing 400 copies run \$1,700+.

The WSU Department of Anthropology has agreed to publish the report in its series Reports of Investigations on the assumption that some of the \$500+ difference between funds available (\$1,200) and the cost of publication (\$1,700+) can be absorbed by the District's purchase of 100 copies of the printed report at the cost of publication (about \$4-4.50 per copy). Since the report cannot be published by July 1st the University has billed the Washington Archaeological Society for \$1,200 and will encumber these funds for the present time and print the report in the course of the summer. Copies should be available by 1 September.

The distribution of the 400 copies of the report is proposed as follows: 150 copies to the archaeological society @ no charge, 20 copies to the author @ no charge, 10 copies to the District @ no charge, 100 copies available to the District @ cost of publication (about \$4-4.50 per copy), and the remaining 120 copies will be sold through WSU in order to defray the additional printing costs.

Enclosed is a sample copy from the WSU series Report of Investigations. The cover of Nelson's report will be artistic and based on photos and line drawings of artifacts from the corpus of the report. I suggest that the District prepare some kind of statement regarding its participation in the publication of the report on letter-head stationery for inclusion in the opening pages of the published report.

I hope that the District will find this plan and procedure satisfactory.

Sincerely yours,

David G. Rice  
Research Assistant

Enclosure.





# WASHINGTON ARCHAEOLOGICAL SOCIETY

P. O. BOX 84 • UNIVERSITY STATION • SEATTLE, WASHINGTON 98105

August 14, 1969

Dear Dave:

Everything is in your hands !

Meaning: I hope you got the letter from Mr. Gillette of Grant County PUD left at your home -- and as of yesterday we sent \$1200 to Pullman. I stated in my letter to Miss Caroline "What's her name" supervisor of Duplicating and Mailing that the money was from WAS (and PUD) for the Nelson monograph and that PUD would give \$ 500 more upon receipt of their 100 copies. (I can change this if you see necessary) Further, I restated that 150 were earmarked for us. You were named as our official contact so its up to you now.

It is good to get this thing wound up and I'm sure the Nelson's will too. I hope they are fully aware of your part in this, the finalizing, footwork, etc. In anycase I'll make a point of it the next time I see Chuck.

He called Wednesday before he left for the East. I'm going to put in a form relating to designating 45SN100 as a "Historic site" worthy of being preserved and noted in the state register. How this is a historic site, I don't know. It was Greengo's idea and he's on the board so must know something I don't.

Let us know when we can get the pictures of Marmes for our next ARCHAEOLOGIST. If there are unpublished things, best. How about these: the skull of the most complete remains; artifacts, together if possible; the flooded site. I can fill in between. If there is anything very special of activities or persons, o.k.

Sincerely,



# UNIVERSITY OF IDAHO

MOSCOW, IDAHO 83843



*Department of  
Sociology/Anthropology*

LABORATORY OF ANTHROPOLOGY

December 15, 1969

change → to R.W. Gillette

Mr. E.B. Gibbons, Manager  
Public Utility District of Grant County  
P.O. Box 878  
Ephrata, Washington 98823

Dear Mr. Gibbons:

On behalf of the Washington Archaeological Society it is my pleasure to present the report by Charles M. Nelson entitled "The Sunset Creek Site (45-KT-28) and Its Place in Plateau Prehistory" in fulfillment of the Letter of Agreement with your District dated June 12, 1962. This report provides the first really comprehensive view of Plateau prehistory and its bases that is available. The District is commended for its support of the research and publication of this work.

This report is of further special significance. It is an example of the constructive role that non-professional archaeologists can play in contributing to our limited knowledge of northwest prehistory. With the growing public interest in American antiquities, works of this kind should be promoted and encouraged inasmuch as they provide legitimate and valid outlets for the expression of amateur enthusiasm and dedication within a still limited but rapidly growing profession.

This report is the result of interaction of many individuals, and it is exceptional that these views found their way into print. The elements essential for an understanding of Plateau prehistory have been discussed by many researchers, but have rarely been documented on paper, and they have never been synthesized in this manner. The basic text of the report was written in 1962-1963. It was supplemented and revised into the present draft by early 1967. Unfortunately, editorial problems have delayed its publication until now.

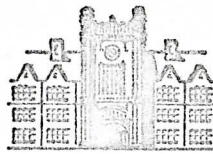
Sincerely yours,

David G. Rice  
Assistant Professor  
of Anthropology



UNIVERSITY OF IDAHO

MOSCOW, IDAHO 83843



*Department of  
Sociology/Anthropology*

LABORATORY OF ANTHROPOLOGY

June 8, 1970

Dr. Robert E. Greengo  
Department of Anthropology  
University of Washington  
Seattle, Washington 98105

Dear Bob:

Here is a report that I think will interest you--"The Sunset Creek Site (45-KT-28) and Its Place in Plateau Prehistory," by Charles M. Nelson. I am forwarding this copy to you in advance of our regular distribution in the hopes that it may be of some assistance to you in organizing and interpreting the Priest Rapids-Wanapum material for your National Park Service reports.

Best regards,

A handwritten signature in cursive script, appearing to read "Dave".

David G. Rice  
Assistant Professor  
of Anthropology

DGR:ty



June 8, 1970

Mr. Paul J. F. Schumacher  
Chief, Archeological Investigations  
U.S. National Park Service  
450 Golden Gate Avenue, Box 36025  
San Francisco, California 94102

Dear Mr. Schumacher:

It is my pleasure to transmit a report on "The Sunset Creek Site (45-KT-28) and Its Place in Plateau Prehistory," by Charles M. Nelson. This report summarizes work conducted by the Washington Archaeological Society in the Wanapum Dam reservoir area between 1957 and 1963, particularly at site 45-KT-28. The research and publication of this report was sponsored by the Public Utility District of Grant County, Washington, under Resolution No. 1092, dated June 12, 1962.

I think that you will find this report unique in northwest prehistory. No other site report is as complete or as graphic. Few reports offer such comprehensive coverage of the literature (up to 1967 when the manuscript was completed). No report to date has dealt as much with the problems and syntheses of northwest prehistory as does this work. It is a worthy contribution to our knowledge of the Plateau.

Sincerely yours,

David G. Rice  
Assistant Professor  
of Anthropology

Trustee, Washington  
Archaeological Society

DGR:ty

cc: Del Nordquist, Editor-W.A.S.  
R.W. Gillette, Manager - Grant Co. P.U.D.  
C.G. Nelson, 502 Portland Ave. St. Paul, Minn.



A LETTER IN CONFIDENCE TO PAUL J. F. SCHUMACHER

Moscow, Idaho  
June 8, 1970

Dear Paul:

I think you should know that putting out Nelson's report on "The Sunset Creek Site" has not been easy. The text of the report was written while Nelson was an undergraduate at WSU (1963-65). Nelson submitted an early draft of the report to Fryxell for editing in 1964. When I entered graduate school at WSU in fall 1965 I was asked by the Nelson family to look after the manuscript. Periodic checks with Fryxell revealed that nothing was being done, and in fact, Fryxell indicated to me and told to others that he was purposely delaying editing and publication of the report "because of Monty's attitude" and because "a student paper cannot be the first in a new series" (Fryxell was at that time editor of Reports of Investigations and a new series was being contemplated). Meanwhile, Nelson sent several letters to Fryx from Berkeley which were never answered and I was offered vain promises. When I spoke with Daugherty about this problem he said, "There is nothing I can do, Fryxell is a professor just like I am, and I can't tell him what to do." The report languished.

In 1966 Deward Walker took an interest in the manuscript and heavily edited the ethnographic and interpretative portions of the report. The remainder of the report had by this time been transferred to Allan Smith for editing. This was a period of fragmentation of the manuscript -- some was with Fryxell, some with Walker, some with Smith; Sprague and Leonhardy at different times examined the manuscript; and when these sections came back together the sum of the parts did not equal the whole.

Monty's rewrite on Walker's comments constituted the last revision of the manuscript before its publication. But still, there was no move in the department to publish the report. When Leonhardy became editor of Reports of Investigations I finally gained some support to publish the work (1968).

My increasing influence in the department reached a point in 1968 where I was able to talk directly with Allan Smith about publication. He agreed that by that time the report had been sufficiently edited, and he felt that the department had a responsibility to publish it. With Smith's support I began the task of gathering together the scattered parts of the report. While working on the Marmes report during summer 1968 I accidentally found a packet containing all the negatives to the Nelson report in the back of one of Fryxell's file cabinets where it had been lost one or two years earlier. Other bits and pieces kept drifting in until spring 1969: text, figures and plates were together again.

Early in 1969 Walker offered to publish the report in NARN, but this fell through because of high cost estimates and other compromises. Leonhardy gave me the go-ahead to publish in the WSU series as long as I could pay for it by outside means.

In May 1969 I received word from the Grant Co. PUD (on June 12, 1962 they authorized expenditure of \$1700 for writeup and publication of the report; Monty used \$500 in 1962 for writeup) that they were closing all accounts on the Wanapum-Priest Rapids projects and that after July 1, 1969 no funds would be available. Also, I found that the cost estimate for publication exceeded the amount available by at least \$500.

Through long distance negotiations with the PUD and the WAS I was able to increase the original grant of \$1700 to \$2200. Next, it was possible to arrange for a transfer of funds from the PUD to the WAS and then to WSU so that the account could be saved by July 1. Allan Smith was instrumental in making this possible.

During fall 1969, concurrently with the Asotin Report, I did the editorial work of rewriting sections of the manuscript, putting together an abstract, bibliography, illustrations, deciding upon a cover, etc. In January 1970 I proofed the mats and the machinery of publication began to grind. Finally, June 1970, it is out.

Now, two down (Asotin, Nelson), and one to go (Marmes).

Sincerely,

A handwritten signature in cursive script, appearing to read "Dave", followed by a horizontal flourish.

David G. Rice



June 29, 1970

Mrs. Mona Bedall, Treasurer  
Washington Archaeological Society  
P. O. Box 84 University Station  
Seattle, Washington 98105

Dear Mona:

Re: "The Sunset Creek Site (45-KT-28) and Its Place in Plateau Prehistory," by Charles M. Nelson. The total cost of publication for 400 copies amounts to \$1,663.65. Last year about this time the WAS paid \$1200 to Duplicating and Mailing at WSU. The balance due, therefore, is \$463.65. I have asked Duplicating and Mailing to bill the Society for that amount.

One remaining obligation of the Society is to provide some hardbound presentation copies of the report to the PUD. I am having 12 copies of the report bound by V. J. Languille and Co. of 611 W. 3rd Avenue, Spokane, Washington. I will instruct them to bill the Society for the cost of binding. The amount is expected to be about \$40.00. When this is done and the reports are delivered, the Society will have met its contractual obligations with the Grant Co. PUD.

Sincerely yours,

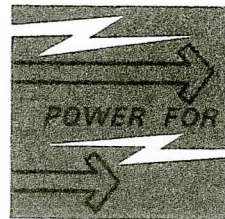
David G. Rice  
Assistant Professor  
of Anthropology

Trustee Washington Archaeological  
Society

DGR:jw

**PUBLIC UTILITY DISTRICT  
OF GRANT COUNTY**

P.O. BOX 878 • EPHRATA, WASHINGTON 98823 • PHONE SK 4-3541 • CODE 509



September 23, 1970

Mr. David G. Rice  
Assistant Professor of Anthropology  
Department of Sociology/Anthropology  
University of Idaho  
Moscow, Idaho 83843

Dear Dave:

This will acknowledge receipt of the 100 copies of the archeological report delivered by John Mattson last week. Everyone I have shown the report to is extremely pleased not only to have the report, but for the quality of the printing. We are going to restrain distribution of the report until we have received the hard bound copies and have made a formal presentation to our Commissioners.

Will we have the only copies of the report available for library or individual acquisition? If so, could you give us an idea on how you normally handle distribution to library or interested public agencies, etc.

I am still looking forward to the opportunity of discussing with you the display of some of the artifacts. Thanks again for sending the report and seeing through to their publication.

Very truly yours,

A handwritten signature in dark ink, appearing to read "Jack J. Park". The signature is fluid and cursive, with a large, sweeping initial "J".

Jack J. Park  
Director of Interpretive Services

JJP/peb



October 13, 1970

Mr. Jack J. Park, Director  
Interpretive Services  
Public Utility District of Grant County  
P.O. Box 878  
Ephrata, Washington 98823

Dear Jack:

Today I sent ten hard bound copies of "The Sunset Creek Site" to your office. They are packed in two boxes and should be received by the end of the week. With their receipt all contractual obligations between the Washington Archaeological Society and your District should be fulfilled.

With regard to the distribution of the reports: The WAS has 150 copies of the report for distribution to its membership. The District has 100 copies for distribution as it sees fit. Twenty copies will be sent to the author. Of the remaining copies I have made a complimentary distribution to members of the profession, State Library, University Libraries, and the National Park Service for research, reference, or review. Any remaining copies will be sold at \$5.00 per copy by the Editor, Reports of Investigations, Laboratory of Anthropology, Washington State University, Pullman, Washington 99163.

When I have had a chance to discuss the matter of artifact display with the Nelson's, I will be back in touch with you.

Sincerely yours,

David G. Rice  
Assistant Professor  
of Anthropology

DGR:ty



October 22, 1970

Dr. Fred Plog, SAA Review Editor  
Department of Anthropology  
U.C.L.A.  
Los Angeles, California 90024

Dear Sir:

I wish to submit the following publication for review in American Antiquity:

The Sunset Creek Site (45-KT-28) and its place in plateau prehistory.  
Charles M. Nelson. Washington State University, Laboratory of Anthropology, Report of Investigations, No. 47, Pullman, Washington, 1969. vii + 423 pp., 104 figs., 8 tables, 3 appendices. \$5.00

Since the author was conducting field work in East Africa at the time the manuscript was edited and published, I cannot take responsibility for errors in content. The form and style of this report was my editorial responsibility, however.

It is requested that Dr. Deward E. Walker, Jr., Department of Anthropology, University of Colorado, Boulder, Colorado, be invited to write the review.

Sincerely yours,

David G. Rice  
Assistant Professor  
of Anthropology

DGR:ty

Enclosure