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AND

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ARCHEOLOGICAL FIELD RESEARCH

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THE NORTH ALASKA RANGE EARLY MAN PROJECT

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I. Introduction

This is the fourth in a series of annual field reports describing the archeological research of the North Alaska Range Early Man Project. It represents only one aspect of an interdisciplinary project; results of the geological and palynological research will be presented elsewhere. Since 1978, I have been responsible for the conduct of field archeology, although Dr. W. Roger Powers, my former graduate advisor at the University of Alaska and the original principal investigator for archeology, continued to act as senior consultant. Dr. Norman W. Ten Brink (Grand Valley State Colleges) provided overall direction of the project in the field, as he has for the last three years. During the 1980 field season, Mr. Samuel M. Wilson, my fellow graduate student at the University of Chicago, served as archeological research assistant.

A total of four weeks was spent in the field in 1980. Although in past years (1978 and 1979) the emphasis has been on the search for suitable geological contexts to test for pre-12,000 year old sites, this year attention turned to the actual testing, following the strategy discussed in this report. In addition to this, survey for stratified sites in the 12,000-10,000 years B.P. time range was undertaken, as the collection of new archeological data bearing on this period has become a major focus of the project in its overall effort to contribute to our understanding of native American origins. In this report I have presented a summary of the archeological objectives, methods, and results of the 1980 field season. In the concluding section, I have attempted to review briefly the current state of our knowledge about the Pleistocene inhabitants of Beringia, and ventured to make some general recommendations for future research.

II. History of Research

The North Alaska Range Early Man Project was an outgrowth of the interdisciplinary research at the Dry Creek site of W. R. Powers, R. D. Guthrie, and T. D. Hamilton. The project was launched, on a three year basis, in the fall of 1976 with the joint support of the National Geographic Society and the National Park Service. The principal investigators, Powers and Guthrie, had formulated a settlement pattern model which predicted that Pleistocene hunters would travel up the rivers of the northern foothills of the Alaska Range to exploit game concentrations in late summer and early fall, a function of delayed plant maturation at higher altitudes (1977: 18). The valleys of the foothills zone offer prominent observation points along the edges of river terraces, thus simplifying the difficult task of finding Pleistocene sites.

Of fundamental importance to the designers of the project was the location of sites in a datable geological context. Typological dating of materials older than the terminal Pleistocene was not possible because no chronological culture framework exists for Alaska prior to this time. It was assumed that the typical sedimentary context of the terrace top sites, aeolian sand and silt, would provide archeological remains not only from the terminal Pleistocene, but from significantly older sites.

During the first field season in 1977, the archeologists failed to locate any sites older than Dry Creek (ca. 11,000 years B.P.), and the assumed antiquity of the primary aeolian deposits was questioned. Was it possible that some process or combination of processes had removed these deposits prior to about 12,000 years ago? Furthermore, misconceptions about the age and nature of certain deposits in the Teklanika Valley had led to an abandonment of plans to test that area for Pleistocene sites. It was therefore decided to suspend further archeological survey until more was known about the geology of the foothills region, especially with respect to the age and origin of deposits judged to be of high archeological potential from a topographic standpoint.

In the summer of 1978, N. W. Ten Brink, who had become the principal investigator for geology, began a comprehensive study of the Pleistocene geomorphology and stratigraphy of the region. In order to contribute to the archeological perspective, I was invited to participate in this undertaking. During the course of the field season, attention was focused on two problems: the age of the primary aeolian deposits and the search for alternative contexts for future survey, should the former prove to be too young. Radiocarbon dates collected that summer were consistent with the hypothesis that the aeolian deposits generally post-date 12,000 years B.P. (Hoffecker 1979:5, Table 1).

A number of alternative contexts were considered during both the 1978 and 1979 field seasons. These included colluvial silt, frozen aeolian sediments, and various types of water-laid deposits, including overbank silt, fluvial sand and gravels, and alluvial fans. However, most of these contexts were seen to possess serious disadvantages, as discussed in the 1978 and 1979 reports. Only alluvial fans were regarded as combining both the preservational and locational requirements as a suitable context for the older sites. For the 1980 field season, therefore, the development of a detailed model for Late Wisconsinan fan sampling and some preliminary testing were planned (Ten Brink 1980).

Since 1978, the archeological research objectives of the project have moved along two seemingly separate paths. The most important of these has been the search for appropriate pre-12,000 year-old contexts, but of growing significance has been the problem of sites in the 12,000-10,000 years B.P. time range. Both the results of the Dry Creek excavations and the discovery and dating of the Moose Creek site in 1978 have unequivocably confirmed the existence of a bifacial point technology in this period. The recognition of such a technology in Pleistocene Beringia is a relatively recent development, and one with potentially important implications for the peopling of the New World. In order to interpret the relationship of the point technology to the previously known microblade assemblages of Beringia (the Diuktai and Denali complexes), new sites in this time range must be discovered. Thus, additional testing of the terminal Pleistocene age aeolian deposits was also planned (Ten Brink 1980).

III. The 1980 Research Design

The archeological research design originally formulated for the summer of 1980 underwent substantial revision immediately prior to the beginning of the field season. This was due to two new interpretations with regard to the Pleistocene geology of the Nenana Valley. Thus, a wholly new research strategy was implemented in 1980 which resulted in what I believe to have been one of the more productive phases of archeological research in the history of the project.

The original field season objectives of the project, outlined by Ten Brink (1980:1-2), included the testing of both aeolian terrace top sediments and alluvial fan deposits. A brief reconnaissance of the Tonzona Valley was also proposed. The primary goal of the 1980 field work was the formulation of a strategy for locating archeological sites pre-dating 12,000 years B.P., based on the assumption that Late Wisconsinan alluvial fans constituted the most promising geological context for such sites. These fans were known to represent undisturbed deposits of sufficient age; they were believed to provide a generally favorable location for sites. The chief problem defined in the spring of 1980 was how to proceed in testing the often large (several km^2) and only intermittently exposed fan sediments.

Discussions with N. W. Ten Brink less than two months before the beginning of the August field work convinced me, however, that our plans required significant changes. A series of radiocarbon dates had been obtained from the uppermost alluvial units of several Nenana Valley fans which indicated that the Wisconsin age deposits in these fans were capped by Holocene alluvium. Deposits of similar age and origin have been described from the Riley Creek age (Wisconsinan) fan at Dry Creek (Thorson and Hamilton 1977:172), but the presence of some coarser-grained sediments in the uppermost alluvial layers of fans examined in 1979 (Hoffecker 1979) and their substantial elevation above the Holocene river level appeared to minimize this possibility. Ritter (1980) has developed a model to account for these late fan-capping events.

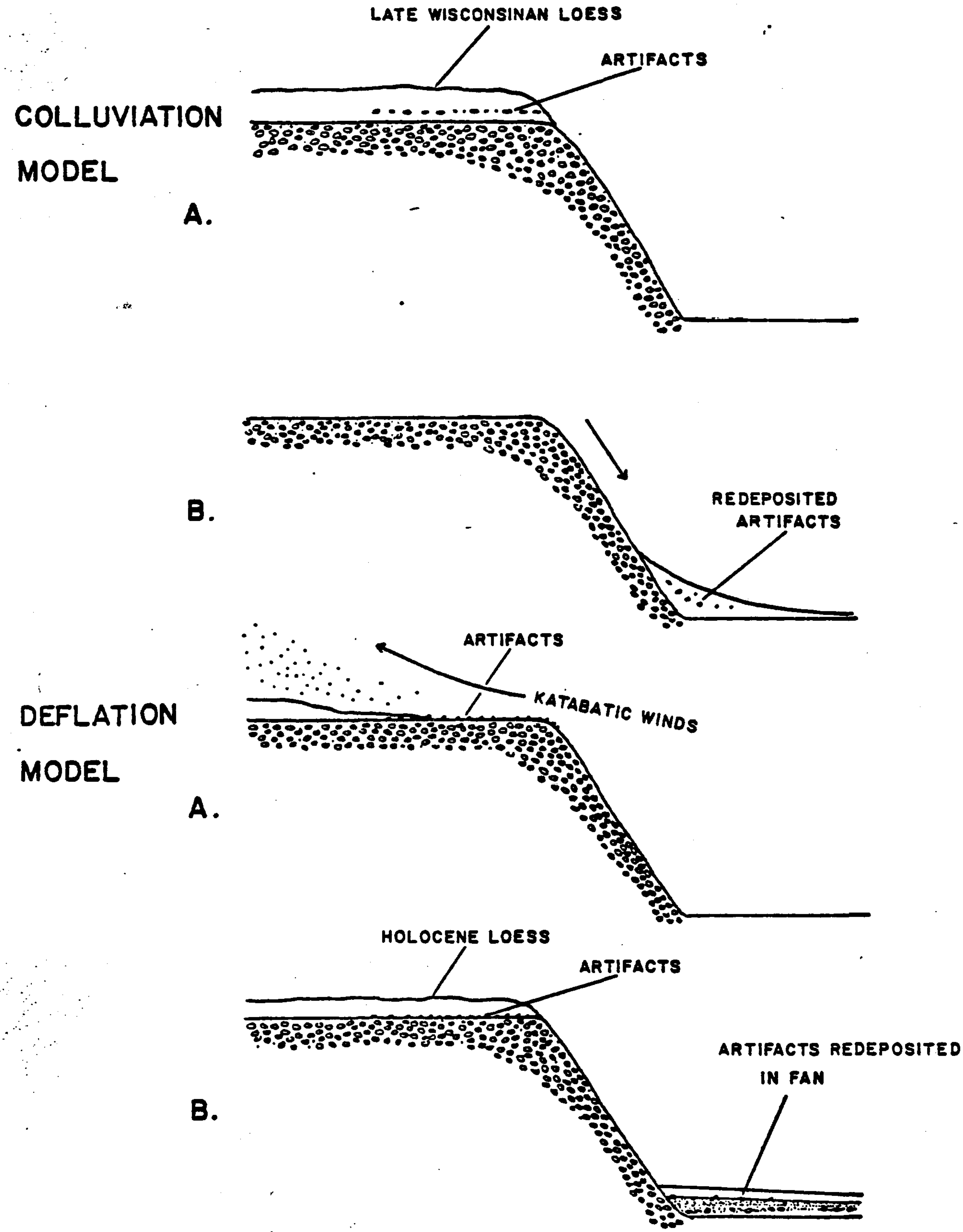
The Holocene alluvium overlying the Late Wisconsinan fan sediments presents a new obstacle to the testing of these deposits for sites pre-dating 12,000 years B.P.. As most of the fan testing would require digging downward from the present-day surface (due to limited exposures), archeologists would find it necessary to remove an additional meter or more of overburden (below the covering loess mantle) in order to reach units of sufficient age. Considering the fact that we lack information on any locational biases for sites on side valley fans, our probability of recovering any artifacts would seem to be significantly reduced, unless compensated for by an exceptional commitment in manpower. Furthermore, Ritter (1980) has hypothesized that all of the upper fine-grained units in the fan sequences represent post-Wisconsin events. If this is correct, archeological testing would be confined to the gravel units below, a less favorable context for preservation and discovery. Thus, the fan testing strategy in its original form was abandoned.

Discussions with Dr. Ten Brink touched on another geological question of great importance to the Early Man survey: the absence of primary loess deposits pre-dating 12,000 years B.P.. In previous field reports (Hoffecker 1978, 1979), emphasis has been placed on the presence of deep reworked silt sections in the Toklat-Sushana system and elsewhere in the foothills. The widespread evidence for mass-wasting processes in Alaska in general has been stressed, and colluviation has been proposed as a likely explanation for the absence of older, undisturbed loess in the foothills. Ten Brink (pers. com.) now suggests that deflation - at least in the case of the Nenana Valley - offers a better explanation of the data. This valley, although the most thoroughly explored of all areas in the project area, has never yielded any deep silt sections that might be interpreted as reworked loess. Moreover, Wahrhaftig (1958: 63) has commented on the extremely high wind velocities which are characteristic of the valley, as suggested by evidence for wind transport of moderately heavy fragments of schist in the Nenana Gorge area.

Given the importance of the Wisconsinan loess problem, one objective of the 1980 field work was clear: to devise a test for the deflation hypothesis. At the same time, I realized that it would be possible to examine the loess/gravel contact zone in various locations around the Nenana Valley with little additional effort beyond that which we had originally intended to allocate for our continuing survey of sites in the 12,000-10,000 years B.P. range. By digging our test pits down to the bottom of the primary loess, and carefully scrutinizing the contact zone and the upper few centimeters of gravel, a pre-12,000 year-old surface could be tested for artifacts. In the absence of mass-wasting processes, such artifacts should remain on this surface (see Figure 1).

One of the most important features of this survey strategy is that it solves the problem of where to look in terms of likely top-

Figure I. Alternative Hypotheses for Loess Removal.



ographic situations for sites. Focusing on the surface beneath primary terrace top aeolian sediments permits us to concentrate on those locations which have proven so productive in the past: points along the terrace front where side valley stream incisions occur. Sites like Dry Creek, Panguingue Creek, and Moose Creek are all in this type of topographic situation. The strategy does not, however, immediately solve the problem of obtaining a datable context. Artifacts recovered from the contact zone are likely to pre-date 12,000 years B.P. and they will clearly post-date the age of the terrace. Nevertheless, an unconformity ranging from several thousand to more than several hundred thousand years normally exists between the base of the loess and the top of the terrace gravels. In order to properly date artifacts from the contact zone, a further step must be taken.

It is under these circumstances that the alluvial fans once more become useful. As Professor Karl W. Butzer (Departments of Geography and Anthropology, University of Chicago) has observed, the terrace fronts where these sites are located are subject to continual erosional processes, and some artifactual material is likely to be redeposited at the base of the terrace slope periodically. If outwash or alluvial deposits are accumulating at the slope base, the artifacts will be incorporated into these sediments, and will rest in a potentially datable context. Barring substantial transport by stream action, they should be recoverable directly below the terrace edge on which they were initially discovered, or close nearby. These localities (side valley stream cuts) are typically characterized by alluvial fan deposits. This two-phase testing strategy thus appears to solve both the problem of site location and datable context.

As I have noted already, the other chief objective of the 1980 field research, the testing of primary aeolian deposits in the 12,000-10,000 years B.P. range, could be pursued simultaneously with our implementation of the new pre-12,000 year-old site survey strategy. This was a fortunate arrangement, as I had hoped to devote as much time as possible to working in deposits of this age. The previous field report (Hoffecker 1979), as well as another recent paper (Powers and Guthrie, n.d.), have outlined an important archeological problem in this time range: to explicate the chronological relationship between the point and microblade technology now known to have been present in interior Alaska during the terminal Pleistocene. Existing data from the Nenana Valley and scattered information from elsewhere in Beringia suggest that the point technology may be slightly older (at least to the east of the Lena Basin), but this remains uncertain. The Nenana Valley, which possesses thick aeolian stratigraphy from this time period, is an ideal area for attempting to resolve this problem.

IV. Results of Field Investigations

The 1980 summer field season was a relatively short one for the archeologists of the North Alaska Range Project, consisting of only four weeks (July 30-August 26). Nevertheless, interesting results were obtained. With the exception of a one-day reconnaissance to the Tonzona Valley (via helicopter), all work was confined to the Nenana Valley.

It was only at the end of August, when Dr. Ten Brink was available to work with the archeologists, that a field test was performed in an attempt to falsify the deflation hypothesis. I did not feel that it was necessary to test the hypothesis before we began our contact zone sampling, because, as noted previously, the latter requires little time and effort beyond that which we had already committed to the testing of terrace top aeolian deposits. In addition to this, we already possessed a considerable amount of negative evidence which was consistent with the hypothesis.

In order to test the hypothesis directly, I assumed that our alternative hypothesis, colluviation, was correct, and attempted to predict locations where colluviated Wisconsinan sediments might be observable. Areas where such deposits could have been subsequently removed by stream action had to be avoided. Thus, we concentrated on a location approximately 2 km. south of Panguingue Creek, on the north side of the Stampede Trail, where the rear of the Healy age (pre-Late Wisconsin) outwash terrace rests against the slope of an ancient Nenana Gravel terrace. Here, if the colluviation hypothesis was correct, we would expect to find: 1) undisturbed Late Wisconsinan loess (as these deposits should not be removed from the back of a level terrace in the absence of stream activity), 2) reworked Late Wisconsinan silt (derived from the slopes and front of the Nenana Gravel terrace above it), and 3) undisturbed Holocene aeolian deposits (with possibly additional reworked Holocene aeolian material from the slopes and front of the Nenana Gravel terrace). In short, a substantial body of sediment should lie at the base of of the Nenana Gravel scarp, on the Healy outwash surface.

The surficial deposits revealed by the test trench excavated to the level of the gravels (which Dr. Ten Brink believes likely to be of Healy age on the basis of heavy staining on the undersides of the clasts), were not consistent with this expectation. Only 245 cm. of fine-grained deposits were uncovered, and the stratigraphy appeared generally similar to primary aeolian sections observed elsewhere around the valley. The uppermost 40 cm. were poorly sorted and contained small pebbles; Dr. Ten Brink thought this unit likely to be colluvial in origin, and it may represent some Holocene slopewash. Regretably, organic material was not found in sufficient quantities for radiocarbon analysis. Nevertheless, I regard the test as having produced significant additional support for the deflation hypothesis. It is difficult to explain the absence of large accumulations of sand and silt in this situation, other than by deflation.

For the first time since the summer of 1977, archeologists of the North Alaska Range Project resumed the active search for pre-12,000 year-old sites during the 1980 field season. A total of fifteen localities in the Nenana Valley were tested for the presence of artifacts in the previously described contact zone between the terrace top aeolian deposits and the underlying gravel surface (Figure 2). Virtually all localities tested were in the type of topographic situation used as a focus in earlier surveys: the front edge of Healy or pre-Healy age terraces, where incised by side valley streams. Even well known localities such as the Dry Creek site were rechecked for material in the contact zone, which might have been overlooked in the past.

No artifacts were recovered from the lowest levels of the aeolian deposits, the gravel surface, or the uppermost 10 cm. of the gravels. The last of these was carefully examined for the presence of small flaking debris on the assumption that such material would be trapped in the interstices of the gravel in the absence of, or during the removal of, an overlying sand or silt unit.

The negative results of this testing are, in my view, hardly conclusive at this point. In order to retrieve a sufficient amount of data for making any reasonably reliable generalizations about human habitation in the Nenana Valley prior to 12,000 years B.P., further testing must be performed. A large number of attractive localities remain to be tested, especially on the higher, older terraces (which were being used here in the terminal Pleistocene, as illustrated by the Moose Creek site). In addition to this, many localities already tested should be revisited in order to increase our sample size.

Simultaneous testing of post-12,000 year-old aeolian deposits occurred in those localities chosen for sampling of the contact zone, and significant results were obtained. Four new stratified sites were discovered (Browne, Walker Road, Slate Creek, and Usibelli, shown in Figure 2). Although no radiocarbon samples have been recovered from these sites at present, our existing knowledge of the aeolian stratigraphy of the Nenana Valley seems sufficient for assigning tentative, approximate dates on the basis of the relative stratigraphic position of the artifacts. One of these sites, Browne, appears to be of late Holocene age. This is the first site to be located at the northern end of the valley. Slate Creek pos-

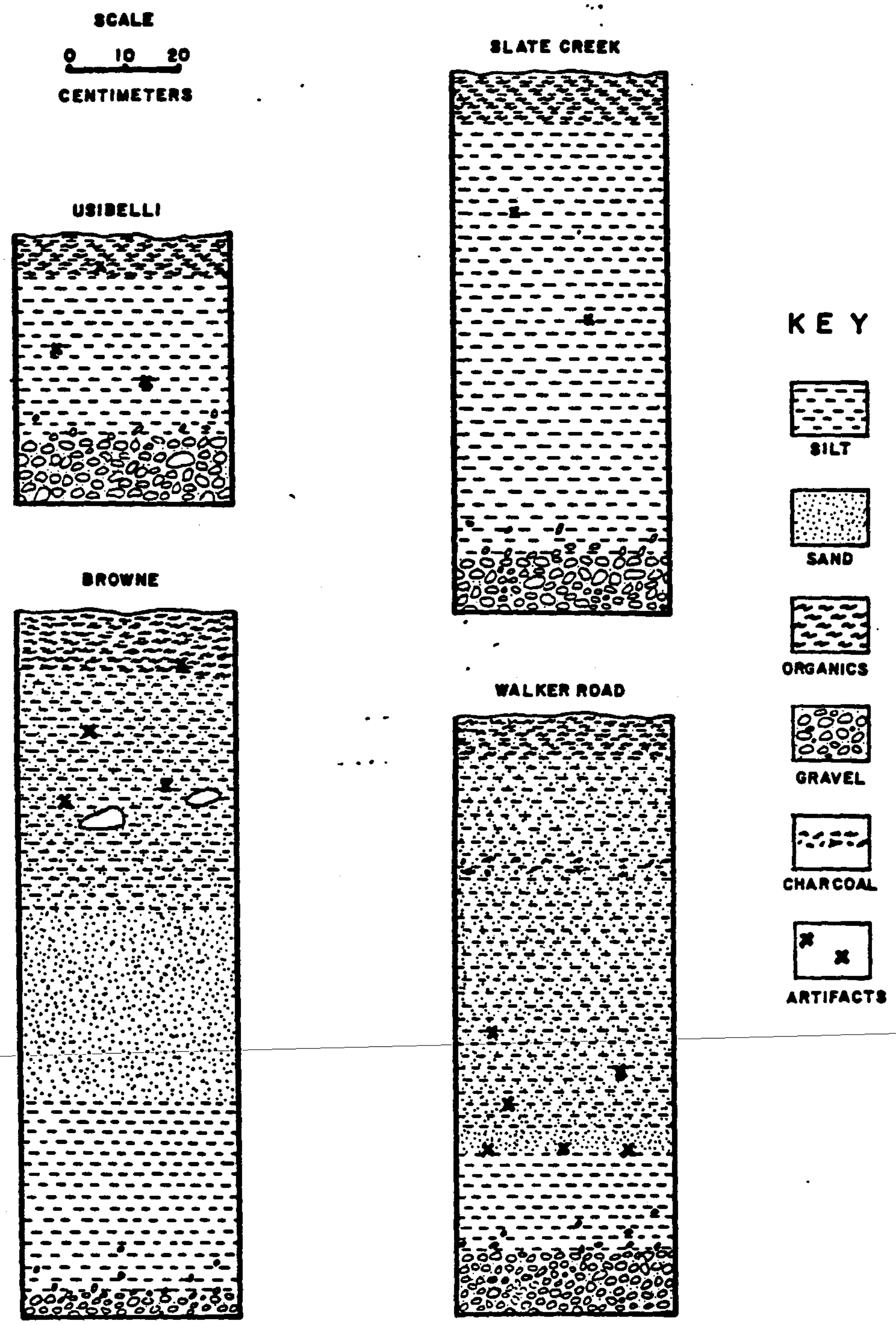


Figure 3. Stratigraphy of Sites Discovered in 1980.

sesses at least two components, one of which may be mid-Holocene or earlier in age. The Usibelli site may contain as many as three components, the lowest of which seems likely to be of terminal Pleistocene age. A small triangular chert point recovered from this level bears a strong typological resemblance to the small bifacial point and point fragments in Component I of the Dry Creek site. At the Walker Road site artifacts were recovered as low as 80 cm. below the surface of a 98 cm. thick sand/silt section; no diagnostic materials were found here. Further study of these last two sites seems likely to shed new light on the microblade and point technology of the 12,000-10,000 years B.P. period.

Some observations on sampling strategy and field methods during the 1980 survey should be made. In contrast to previous years, both north and south sides of stream cuts were tested, as it seemed to me that the earlier emphasis on south-facing (i.e. north side) terrace edges, because of their superior autumn and winter exposure to the sun, was not justified when the absence of arboreal vegetation prior to the early Holocene was taken into account. A revised approach to digging test squares was employed in 1980 as well. In the past, the total number of squares (averging approximately 1 m^2 each) has been maximized at the expense of their individual rates of excavation. Methods such as rapid troweling, skim-shoveling, and even outright shoveling were used. However, as the bulk of archeological material in the Nenana Valley (not as measured by volume, but quantity of artifacts) consists of small flaking debris, a more careful troweling technique seems preferable, despite its limiting effect on the total number of squares dug.

The 1980 field season also involved a one-day geoarcheological reconnaissance of the Tonzona Valley, located beyond the western-most border of Mt. McKinley National Park. This area was believed, as described in the research proposal (Ten Brink 1980:3), to have the potential of providing the project with undisturbed pre-12,000 year-old aeolian deposits in attractive topographic situations. Several exposures were profiled and radiocarbon samples were collected.

V. Recommendations for Future Research

The successes of the 1980 field season are encouraging and they suggest that the study area of the North Alaska Range - specifically the Nenana Valley - continues to hold great potential for resolving problems in the Pleistocene prehistory of Alaska, Beringia, and perhaps the whole New World. I believe that this area has already, and will in future years, contribute to our knowledge of both the periods before and after 12,000 years B.P.. In the concluding section of this report I would like to review some of the existing pertinent data on each, and propose ways in which the North Alaska Range Project might collect important additional data bearing on the archeological problems.

To a large extent the discovery of pre-12,000 year-old sites was the prime objective of the project at its beginning. I am inclined to believe at present that its most significant positive contribution will be in the 12,000-10,000 years B.P. time range. However, ongoing work in the Nenana Valley will certainly provide some useful information on the problem of pre-12,000 year-old sites.

It is neither necessary nor desirable to review here all of the purported pre-12,000 year-old sites in the New World. It is sufficient to observe that controversy surrounds most of these (Haynes 1971, Lynch 1974, and others), even though it is almost impossible to discount every one. Their overall paucity stands in curious contrast to the Old World, especially during the Upper Paleolithic period. Moreover, if people were present in the New World prior to Clovis times, they might have been derived from coastal populations along the North Pacific rim. It is, therefore, by no means certain that interior Alaska was inhabited prior to the appearance of the Denali assemblages, roughly 12,000 years ago.

Soviet research in Siberia has produced some interesting results relevant to this problem, although many important questions remain unanswered. It is clear that the Lena Basin was inhabited throughout much of the Late Wisconsinan (Sartan in Siberian terminology), by a population which manufactured tools highly similar to those of the Denali Complex. Mochanov (1969) has termed this technology "Diuktai" after the type site on the Aldan River. Although Abramova (1979) has recently challenged the earliest dates for the Diuktai "Culture", it seems that these and typologically similar assemblages from eastern Siberia date well into the last glaciation. East of the Lena Basin, beyond the Verkhoyansk Range, the picture is different. Here there are only a handful of early sites, all dating to no more than about 13,000 years B.P.. Mochanov (1977) has also lumped these sites into the Diuktai technology, although at sites like Berelyokh, Kukhtuy III, and Ushki Lake, other forms of technology appear to be present (Dikov 1977, Hoffecker 1979).

In the Soviet Far East, there are also signs of non-Diuktai technology at sites like Osipovka and Kumara (Powers 1973), but this material remains to be adequately dated.

It is apparent, therefore, that we currently lack convincing evidence for the occupation of both western and eastern Beringia prior to the terminal Pleistocene, or approximately 13,000 years B.P.. It is not difficult to imagine why hunting bands might colonize Beringia at this time. The deterioration of full glacial conditions was probably, through its effect on the large mammalian grazers, a cause of increasing disequilibrium between the human population and its resources. The grassland refugia supported by cool glacial valleys like the Nenana (Ager 1975) would have provided a strong attraction for these big game hunters.

However, while the Laurentide and Cordilleran ice sheets might have presented a barrier to movement into the heart of the New World prior to this time, no such formidable physical barrier appears to have blocked a passage into Beringia. Even the glaciated Verkhoyansk Range might have been bypassed in the north or in the south (Chard 1974:43-44). Thus, researchers are confronted with a perplexing problem: if the existing negative evidence from both sides of Beringia accurately reflects a complete absence of human occupation until the terminal Pleistocene, how can this be explained? The answer might be a subtle, ecological one, not readily testable with the available data. Although the large mammalian fauna were present, perhaps some relatively small but essential resource was missing. As Guthrie (pers. com.) observes, the late colonization of Arctic Canada may provide an analogous situation.

Given our present inability to explain this phenomenon, the current lack of evidence for pre-terminal Pleistocene sites may be simply a function of poor sampling. On both sides of the Bering Strait, this area remains sparsely settled and generally undeveloped industrially, and this is an important consideration in evaluating the number of site discoveries. Continued testing of pre-12,000 year-old deposits in Alaska appears, therefore, to be warranted. Both the research being undertaken in the Porcupine River area by E. J. Dixon and D. C. Plaskett of the University of Alaska Museum, and the Nenana Valley work should complete a thorough testing for the presence of older sites. It would be difficult to explain the absence of pre-terminal Pleistocene sites in both these areas, especially the Porcupine drainage with its numerous caves containing Pleistocene large mammal remains; firmly based negative results would constitute important evidence on the question of interior Alaska's earliest inhabitants.

A wholly different type of problem remains to be solved in the 12,000-10,000 years B.P. range. The existence of an Alaskan point technology in this time period, in addition to the well-known Denali microblade assemblages, has been demonstrated at the Healy Lake site (McKennan and Cook 1970), and possibly in the Tangle Lakes area (West 1973). Shallow stratigraphic contexts have, I believe, limited the usefulness of these data. The excavations at the deep, well-stratified sites of Dry Creek and Moose Creek have provided a basis for separating the two technologies, and have also confirmed the presence of lanceolate points in the 12,000-10,000 years B.P. period, the latter having been justifiably questioned previously (Anderson 1978).

Points are absent in Siberian Diuktai assemblages, except at the end of the sequence, at Diuktai Cave itself (Mochanov 1977). What then is the relationship between the two technologies?

In the previous field report (Hoffecker 1979), the Nenana Valley point assemblages and the non-Diuktai assemblages from northeastern Siberia were tentatively isolated as a new complex, and the suggestion was made that it might represent a separate ethnic group. This speculation was inspired by the evidence that the point technology is older than the microblade technology in Beringia, despite the long history of the latter in the Lena Basin, and that, at sites like Ushki Lake and Dry Creek, there appears to be a sudden replacement.

Discussions with Professor W. Roger Powers have convinced me, however, that defining a new complex is premature at present. It is probably best to consider them as two technologies, the interrelationship of which remains unclear because of insufficient data. The relationship need not be one of separate ethnic groups. In fact, an equally plausible case may be made at this stage that the points and microblades represent a complex response to terminal Pleistocene environmental changes. Invoking the points which appear at this time in Diuktai Cave (in the Lena Basin), and observing the rough contemporaneity of the technologies across Beringia, we could postulate the development of a new hunting device such as a throwing stick, requiring dart-point heads, in the context of the Diuktai microblade technology. Despite the apparent redundancy of point and microblade technology, it should be noted that both occur in the same assemblages on both sides of the Bering Strait during the Holocene. Hypotheses involving other processes of technological change are also conceivable.

Not until an adequate number of new, stratified, datable occupations bearing the two technologies are discovered and excavated will the chronological relationship become clear. If they are found to have a contemporaneous or overlapping temporal distribution, detailed lithic analyses might further clarify this relationship. I believe that the Nenana Valley possesses a potential uncommon in Alaska for solving this problem. The deep aeolian stratigraphy and large number of attractive topographic locations provide much opportunity for finding the necessary sites, as the 1980 field season has demonstrated.

Perhaps the most interesting product of the research conducted over the last few years by the North Alaska Range Project has been the growing recognition of a possible technological progenitor to Clovis. By this observation, I am not making any specific typological comparisons between Clovis points and the scanty materials from Alaska and Siberia, but merely noting that the existence of a Beringian point technology* in the terminal Pleistocene is significant. The Clovis point technology need not, of course, be derived from an older point industry; it may represent a local innovation. However, the presence of the Beringian points - reaching as far west as Diuktai Cave and dating, perhaps, no earlier than about 13,000 years B.P. - lends additional plausibility to the "Late Entry" hypothesis of Haynes, Martin, and others. We now possess a credible Old World source for Clovis, either a functional variant of the

*(including lanceolate forms)

Diuktai technology, or an independent complex. In the years to come, I believe that the Nenana Valley sites will occupy an important place in American archeology.

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