might be in the position of divulging information against their wills. The details of the interviewing situation should be explained to each respondent before he agrees to participate, and the respondent should be in agreement with any use to which his responses are put.

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10. With the cardiotachometer in this system, the instantaneous heart rate is averaged by using a 2- to 4-second time constant. The LINC is equipped with an analog-to-digital converter.
11. Supported in part by USPHS research grants RR-00249-05 and HS-00283-03. I thank M. Spencer, W. Dennis, J. Greist, J. Keenan, H. Ludwig, A. Rasmussen, C. Slack, S. Updike, L. Van Cura, and the staff of the Programmed Medicine Laboratory for their help with this project.

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Early Racial and Cultural Identifications in Southwestern Alaska

At a time when multidisciplinary studies are found to be increasingly necessary and collaboration between scientists of different disciplinary backgrounds is recommended (*1*), Dumond's article (*2*) omits information from fields other than his own and challenges, albeit unintentionally, the concept of cooperative endeavors.

The article contains and omits pertinent information. For example, the reader is not told that the Katmai Monument investigations actually began in 1953 (*3*). The program sponsored by the National Park Service included studies of climate, flora, fauna, shore morphology, geology, and human use of the area prior to the Katmai eruption of 1912 (*4*).

The subsequent investigations had as their stated aims a concern with the relations between the Brooks River and the Columbia River and a study of the cyclic nature of salmon runs as deduced from archeological remains (*5*). Yet Dumond (*2*) states that the interior Brooks River seasonal camps were dug in order to provide a sequence representative of the Bering Sea coast occupation.

The article suggests that data collection and analysis are incomplete. There is no information on the internal spatial relations between archeologic features, artifacts, and ecofacts (*6*), and these data have not been utilized to characterize the seasonal occupations and activities for purposes of description (definition) or comparison. There is no indication that either etiological or attribute analysis of the remains was attempted. Faunal and skeletal analyses are not considered in the cultural descriptions, definitions, or comparisons.

In our opinion, Dumond's comparisons, and the inferences based upon them (*2*), rest on data that provide inadequate grounds for eliciting genetic and linguistic relationships and for reconstructing changing exploitational patterns. An example is the treatment of the B.R. (Brooks River) Gravels remains, which contain items that convince Dumond of affiliation with the Arctic Small Tool tradition. The definition of that tradition (*7*, p. 55) is based upon the occurrence of a *complex* of *diagnostic* traits including *large numbers* of microblades struck from conical cores (*8*), hafted burins with extensive retouch, retouched burin spalls used as engraving tools, many very small, bifacial points lacking stems or notches, bifacial points of medium size and knife blades without stems or notches, and scarcity or absence of grinding or polishing techniques and of pottery. The small Brooks River samples contain some bifacial bipoints, no microblade cores, rare microblades, dubious or rare burins, and no burin spall tools. The identifications of B.R. Gravels as Arctic Small Tool tradition, the people as "Eskimo," and the economy as interior-oriented are not demonstrable. A correspondence between stones and environment is assumed by Dumond but not proved (*9*).

The author postulates (*2*) that Gravels phase people migrated into the Brooks River area, *displacing* Strand phase "Indians" (again, defined on the basis of several tool types or attributes). "Non-Indian" elements in Strand (polished slate thrusting lances and open oil-burning lamps) are explained as diffusions from the contemporary Kodiak inhabitants. The identification of a migration is not convincing [see (*10*)]. Similarly, racial identification based on selected tool types is no substitute for identification based on population genetics and biometrical variation in skeletal series.

Race, language, and culture are repeatedly confused in the article. They are identified from a limited number of stone tools, types, or attributes isolated from context, not considered quantitatively, and not necessarily representative of the excavated collections. On the basis of stone implements "having triangular-sectioned stems on projectile blades, a high incidence of similar large leaf-shaped and ellipsoid bifaces, and other features in common," present at one Aleut site (2500 years old) on Agattu and on the Pacific coast of the Alaska Peninsula (in sites 5500 years old), it is proposed (*2*) that at about 4000 B.C. the Aleutian Islands, the Pacific coast of the Alaska Peninsula, and Kodiak Island were inhabited by a single people.

This identification contains errors and misleading statements. The sites are widely separated in both space and time. The compared features are selected, isolated traits, not diagnostic of either Agattu or the Peninsula sites. Furthermore, the characteristics of the assemblages in the intervening 2400 km (and 3000 years) are ignored (*11*). Published comparisons of the total assemblages of artifacts which consider major base villages or campsites in Aleut and Koniag areas consistently reveal differences between the two areas in the composition of the assemblages, in the styles of tools, in manufacturing techniques, and in local stylistic changes at all periods of occupation (*12*).

Skeletons of early Aleuts and early

inhabitants of Kodiak (obviously more relevant than tool types in population identification) are available for study and are clearly and easily distinguished from each other (*13*). No series of Aleut and Koniag skeletons suggests a single people. Furthermore, Collins (*14*) compared Eskimo skeletal series corresponding to breeding populations (rather than to a pooled sample) and found that the Pre-Koniag cannot be dissociated from the Bering Sea Eskimos, with whom on the whole they show close agreement.

In addition, Aleut and Eskimo are two different languages; their speakers cannot understand one another. Dialect formation and the larger distinction between languages depend more upon geographic and breeding barriers than upon any processes inherent in the language itself (*15*).

Three major bodies of evidence—the living people and the skeletons of their ancestors, the differences in cultural practices and manufacturing techniques reflected in their total cultural assemblages, and differences in language—indicate a long period of separation between Aleuts and all other Alaskan peoples. At the same time, the affiliation of Kodiak peoples at all periods is most likely with Eskimos.

Known geoglogical facts that bear upon the human occupation of southwestern Alaska and the Aleutians are also minimized. The recent glaciation of the Alaska Peninsula, Kodiak Island, and the Pacific side of the eastern Aleutians; the west-to-east deglaciation of those areas; the history of the Bering Land Bridge coastal configuration; and our general understanding of the ecosystem and its changes during man's presence—all are at variance with the affinities and migrations proposed by Dumond [see (*16*) and (*17*)].

Specifically, we know people did live on the coast of Anangula Island in the Aleutians 8400 years ago (according to radiocarbon dating), when sea level was perhaps 20 m below today's level. The people inevitably depended upon resources provided by the rich and stable coastal-marine ecosystem for subsistence (*12*). Anangula was part of a large island, Umnak-Unalaska (earlier the terminus of Beringia), separated by a narrow pass from the enlarged Alaska Peninsula–Unimak Island but probably connected to it by seasonal ice. Land ice prevented the migration of large land-based foraging mammals. Large glaciers were centered on the south side of the Peninsula and covered much of it, perhaps covering Kodiak and the far eastern Aleutians as recently as 6000 years ago.

Climatological, meteorological, and geological data (*18*) clearly show that the coast of Beringia from the Anadyr Gulf to the base of the Peninsula was not glaciated during the latter part of the Wisconsin stage, that marine resources were always available, that edible coastal land plants were present, and that caribou could have come to the coasts then as they do today. It is illogical to argue (*2*) that climatic changes resulting in the *expansion* of a particular habitat sparked adaptation of the Eskimos to a marine ecology at a late date, especially when we know that Aleutian and Kodiak peoples have a long history of true maritime adaptation.

Biological and cultural evidence suggests that the most likely route of entry for Eskimos into the Kodiak area (including the Pacific coast of the upper Peninsula) was across the Peninsula and that people were present by 5500 years ago, evidently not long after deglaciation. The interposition of ice [not Indians, as is proposed (*2*)], the intervening distance and accompanying barriers to easy communication, and the early adaptation of the Aleuts to that ecosystem are more likely to have been the factors that enhanced the early divergence between Aleuts and Eskimos, and to a lesser degree among Eskimo breeding populations.

Thus, Dumond's reconstruction of recent Eskimo marine adaptation and movement into the Pacific, which was demonstrably the realm of marine-adapted Pre-Koniag Eskimo hunters and Aleutian Island Aleut hunters (more than 4000 years ago in the case of the latter) is untenable (*19*).

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6. L. R. Binford, *Amer. Antiquity* **29**, 425 (1964).
7. W. N. Irving, *Arctic Inst. N. Amer. Tech. Pap.*, No. 11 (1962).
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9. Collins has noted that lack of correspondence has long been considered a major limitation in relating artifacts to their environment [H. B. Collins, *Anthropol. Pap. Univ. Alaska* **10**, No. 2, 17 (1963)].
10. I. Rouse, in "Migrations in New World Culture History," *Univ. Ariz. Soc. Sci. Bull.*, No. 27, 67 (1958); W. H. Oswalt, *Anthropol. Pap. Univ. Alaska* **11**, No. 1, 1 (1962).
11. Interestingly, Dumond in an earlier paper did make comparisons with the Chaluka site on Umnak Island and found no basis for asserting the "one people" hypothesis [D. Dumond, *Anthropol. Pap. Univ. Alaska* **12**, No. 1, 33 (1964)].
12. G. B. Denniston, *Arctic Anthropol.* **3**, No. 2, 84 (1966); J. S. Aigner, *ibid.*, p. 57; W. S. Laughlin and J. S. Aigner, *ibid.*, p. 41; W. B. Workman, *ibid.*, p. 1 → D. W. Clark, *Amer. Antiquity* **31**, 358 (19(→ W. S. Laughlin and G. H. Marsh, *ibid.* **20**, 27 (1954). Significantly, Dumond ignores the Anangula site, though in earlier papers he considered it possibly ancestral to both the Arctic Small Tool tradition and later Aleut assemblages; evidently recent reassessment of this position has led him to ignore the thousands of tools, good radiocarbon dates, and geological context of Anangula, and all their implications. [See (*2*) and D. E. Dumond, *Anthropol. Pap. Univ. Alaska* **12**, No. 1, 33 (1964); *Amer. Anthropol.* **67**, 1250 (1965)]. In 1970 Aigner recovered numerous fragments of burned sea mammal bone from Anangula, thus verifying the model of a coastal-marine orientation.
13. A. Hrdlicka, *The Aleutian and Commander Islands and Their Inhabitants* (Wistar Institute of Anatomy and Biology, Philadelphia, 1945), pp. 521 and 579.
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15. J. Greenberg, personal communication.
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17. R. F. Black and W. S. Laughlin, *Science* **143**, 1321 (1964); W. S. Laughlin, in *The Bering Land Bridge*, D. M. Hopkins, Ed. (Stanford Univ. Press, Stanford, 1967), pp. 409–450.
18. P. A. Colinvaux, *Palaeogeogr. Palaeoclimatol. Palaeoecol.* **3**, 29 (1967); ———, in *The Bering Land Bridge*, D. M. Hopkins, Ed. (Stanford Univ. Press, Stanford, 1967), pp. 207–231; R. W. Fairbridge, Ed., *The Encyclopedia of Oceanography* (Reinhold, New York, 1966); J. S. Creager and D. A. McManus, in *The Bering Land Bridge*, D. M. Hopkins, Ed. (Stanford Univ. Press, Stanford, 1967), pp. 7–31; D. M. Hopkins and T. Einarsson, *Science* **152**, 343 (1966).
19. Another aspect of this subject that calls for multidisciplinary and quantitative approaches is the increasing need for the native peoples of Alaska to document land claims. Facts, inferences, implications, levels of proof, tests and challenges of evidence, documentation, and demonstration must be reviewed carefully, with scrupulous attention to accuracy and validity because these people must go into court and prove that they and their genetic ancestors were in a particular area at a particular time, how long they were there, and what resources they used.

28 August 1970; revised 2 October 1970

Archeological research in Katmai National Monument in 1953 under the National Park Service's Katmai Project consisted of tests of four known sites by two graduate students from the University of Oregon, indirectly supervised by W. S. Laughlin (*1*). In 1954

the work was taken over by the University of Alaska, and a small site was excavated (*2*). After that year, archeological research lapsed entirely in the area.

Late in 1959, the University of Oregon was approached and offered support by the Bureau of Commercial Fisheries to conduct archeological research in the vicinity of their salmon research station at Brooks River, in the upper Naknek drainage and within Katmai National Monument, in the hope of recovering information regarding fluctuations in prehistoric salmon migrations. Thus in 1960 seasonal work was begun under my immediate field direction, work that lasted almost without interruption, although with varied financial support and with major changes in problem orientation, until the summer of 1968. That the area was one in which a small Oregon team had worked 7 years earlier was sheer coincidence.

Aigner, Laughlin, and Black are correct in their statement of the aims of the Oregon research as they stood in 1960. Unhappily, not only do they fail to mention that the research report which they cite (*3*) was published in 1962, but they fail also to mention that the same report clearly indicated that the original hypotheses were found to be untestable or inapplicable and had been abandoned (*4*). For the ensuing seasons of 1963, 1964, and 1965, the most substantial of the research support was that provided by two NSF grants, for both of which the proposals specifically were to expand the rudimentary sequence from the Naknek drainage, seen as representative of the southern Bering Sea, and to develop a second sequence from Pacific coastal sites located as close as possible to the upper Naknek drainage, *in order that the prehistoric cultural developments of the Pacific coast and of the Bering Sea coast could be effectively compared* (*5*).

This intent was achieved. In addition to the article in *Science* (*6*) to which Aigner, Laughlin, and Black now raise objections, both aims and results have been presented and discussed at a number of national professional meetings, and the results have been published in summary (*7*). Although research results are often reasonably the subject of argument, it is rather surprising to encounter categorical statements regarding the *aims* of research coming from people who have never had the slightest connection with it.

Their objections to my interpretation of the research results themselves range from suggestions that my article was less than a complete site report (which comes as no surprise), to suggestions that I have ignored relevant environmental data, that I have conceptually confused race and language and culture, and that I would not really know an Arctic Small Tool if I saw one. From this shotgun approach I conclude that their object is to discredit the entire interpretive analysis given in the article and that they prefer another construction (*8*). Here I simply respond to a few of the complaints that might be taken by some readers to be serious.

1) *Race, language, and culture are confused.* I have not discussed the evidence of physical anthropology since, in an earlier paper, I concluded that the published data from southwestern Alaska are simply not adequate to permit detailed comparisons valid over considerable spans of time between areas of Aleut and Eskimo speech (*9*). In the article under consideration (*6*) I did not discuss physical variation at all and could hardly have confused race with either language or culture. The terms "Aleut" and "Eskimo" I have used consistently and consciously *only* to refer to speakers of specific languages. The term "Indian" indicates the speaker of a non-Eskaleutian language, for by definition American natives who are neither Eskimos nor Aleuts are Indians. The method I have used to infer linguistic continuity from the archeological record has been explicitly explained elsewhere (*10*); needless to say it does *not* involve the use of "a limited number of stone tools, types or attributes isolated from context, not considered quantitatively, and not necessarily representative of the excavated collections." The relationship hypothesized between people of the T. Alder phase of the Alaska Peninsula and of Krugloi Point on Agattu Island is no exception, as an examination of the publication in which the hypothesis was presented will show (*11*). To assert otherwise is to misrepresent what is present in the literature.

2) *The B.R. Gravels phase is misinterpreted.* The analytic collection of the Gravels phase from Brooks River numbers 839 implements recovered in situ from seven locations, forming one of the larger collections of Arctic Small Tool materials from Alaska. Implement types by which the Small Tool tradition is defined (*12*) are all present. These materials have been discussed at national professional meetings (*13*), have been seen at first hand by interested colleagues who work in the area north of the Alaska Peninsula where such materials are found, and are accepted as pertaining to the Small Tool tradition by those same colleagues, who include the archeologist who first formally defined that tradition (*14*).

All of the implements were recovered from a stratum immediately postdating a layer of volcanic ash deposited about 3800 years ago (*15*), and they have been dated by seven radiocarbon determinations on associated charcoal that range from 3900 ± 130 (sample I-1629) to 3052 ± 250 (sample I-1159) years ago. The analytic collection of the earlier B.R. Strand phase proceeds from in situ deposits beneath the same volcanic ash layer; these have been dated by four radiocarbon determinations on associated charcoal that yielded ages from 4430 ± 110 (sample I-1946) to 3840 ± 130 (sample I-1630) years (*16, 17*). The only typological continuities between the B.R. Strand and the B.R. Gravels phases are in categories of small but haphazardly made scrapers that are so broadly defined that they are present in collections from all periods. The Strand phase practices of chipping relatively crude bifaces from basalt and of polishing long lance heads of slate are replaced by the Gravels phase practices of pressure-flaking small delicate implements of chalcedony, of pressing bladelets, and of striking small burins. There are no collections with intergrading forms.

If this transition does not represent a complete population replacement, then it surely represents the most amazingly rapid technological revolution known to prehistory.

3) *Anangula is not discussed.* The present evidence that the Arctic Small Tool tradition arrived on the Alaska Peninsula suddenly from the north 3800 years ago and the recent discoveries of blade and wedge-shaped core industries in the Alaskan interior, such as in the Akmak complex at Onion Portage (*18*), change the framework in which the Anangula collection must be viewed. Although I accept provisionally a littoral adaptation by the people of Anangula, and although I am sensitive to the feeling of archeologists who have examined both Anangula and Akmak artifacts that there was no immediate connection between those peoples (*18, 19*), it still seems clear that in a gross typological sense the presently known Alaskan stone industries that compare most closely to that of Anangula are

from the interior. On the other hand, the 4000-year period between the occupation of Anangula and the appearance of ancestral Aleuts at Chaluka on Umnak Island is bridged by little more than the fact that people of both sites lived on the seacoast and probably ate what they found there (*20*). That is, unless the 1970 field work has introduced some significant new elements, there are presently *no* racial, linguistic, or material cultural grounds upon which an argument for continuity between Anangula and Aleuts can convincingly be based. The most that can be said is that there is no direct evidence that such continuity was absent.

Therefore I choose to remain in the position I took in the article now under discussion (*6*, reference 25), that, on the basis of present evidence, it is not possible systematically to relate the Anangula finds to other archeological material from southwestern Alaska.

4) *Nonarcheological data have been ignored.* I have confessed to not using physical anthropological evidence and have indicated why.

Linguistics: The degree of divergence between Eskimo and Aleut has been understated by Aigner, Laughlin, and Black; Eskimo and Aleut have the taxonomic status of separate language *families*. This, together with archeological data, led me to suggest in 1965 (*10*) that the common ancestor of Eskimos and Aleuts lived around 6000 years ago. Rather than minimize this divergence, as Aigner, Laughlin, and Black seem to suggest it would, the construction now at issue would *increase* the estimate of elapsed time since Eskimos and Aleuts were a single people.

Geology: The major reference given by Aigner, Laughlin, and Black is an article by Black (*21*) in which he indicates that "there is meager evidence that deglaciation of the Alaskan Peninsula proceeded from west to east," and that "most of Kodiak Island and the main Alaska Peninsula were deglaciated significantly later than western Umnak Island." But in the same article Black says that "one literally can read into the record any particular sequence of events that is desired." The last statement emboldens me to accept the (nongeological) evidence from radiocarbon dated pollen profiles from peat bogs on Kodiak and Afognak islands that glaciation ended there by about 9000 years ago (*22*) and to accept a radiocarbon determination of 9100 ± 220 (sample I-1628) years ago for the beginning of the postglacial pollen sequence preserved in a bog on the Pacific coastal strip of the Alaska Peninsula itself, near Kukak Bay (*17, 23*). It also encourages me to accept the geomorphological evidence from the upper Naknek drainage, 100 km away, where, when the level of ancestral Naknek Lake was 9 m higher than the present surface, waves deposited in beach sands charcoal that yielded a radiocarbon age of 7360 ± 250 years (sample I-1160); yet waves of the same lake, when the earliest proglacial level was 26 m higher than the present lake surface, had cut a deep terrace in the terminal moraine of the last major (Iliuk) glacial advance of the Wisconsin (*17, 24*).

It encourages me, that is, to conclude that the best evidence now available is that which indicates that the upper portion of the Alaska Peninsula was substantially deglaciated by 8000 or 9000 years ago. If deglaciation of the Peninsula indeed proceeded from west to east, ice would scarcely have been a barrier to Pacific coastal peoples at 4000 or even at 6000 B.C.

Thus the model of Eskimo-Aleut relationships (*25*) that Aigner, Laughlin, and Black espouse should take cognizance of at least two elements: (i) A new people arrived from the north to inhabit the Bering Sea side of the Alaska Peninsula after 1900 B.C., and a comparison with archeological sequences throughout the area of Eskimo speech suggests that these people were probably speakers of ancestral Eskimo. (ii) By 6000 B.C. the glaciers of the upper Alaska Peninsula may well have been little larger than they are in A.D. 1970.

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References and Notes

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2. W. Oswalt, *Anthropol. Pap. Univ. Alaska* **4**, No. 1, 23 (1955).
3. L. S. Cressman and D. E. Dumond, *Research on Northwest Prehistory: Prehistory in the Naknek Drainage, Southwestern Alaska* (Department of Anthropology, University of Oregon, Eugene, 1962). This report, never circulated in unpublished form [although cited as unpublished by Aigner, Laughlin, and Black; see *National Union Catalog, Author List, 1963–1967* **13**, 48 (1969)], includes results under NSF grant G-12964, L. S. Cressman, Principal Investigator.
4. In a similar vein, reference 11 of the comment by Aigner, Laughlin, and Black draws attention, pointedly and misleadingly, to a paper of mine published in 1964, on the grounds that it did not take cognizance of a collection excavated by an Oregon party in 1965. Again, comments in their text that would have been more nearly descriptive of the B.R. Gravels phase as it was sampled before 1962 are applied without explanation to that phase as it is now represented after additional work in 1963, 1964, 1965, and 1967.
5. L. S. Cressman and D. E. Dumond, "Prehistory of the Northern Alaska Peninsula," NSF proposal for grant GS-79 (1962); D. E. Dumond, "Prehistory of the Northern Alaska Peninsula: A Continuation," NSF proposal for grant GS-655 (1964). Research in 1967 and 1968 was somewhat differently oriented.
6. D. E. Dumond, *Science* **166**, 1108 (1969).
7. Annual meetings in 1965 of both the Society for American Archaeology and the American Anthropological Association; D. E. Dumond, *Arctic Anthropol.* **5**, No. 1, 82 (1968).
8. In 1967, Laughlin hypothesized that the "ancestors of the Aleuts and the Eskimos actually lived on the [southern] coast of the Bering Land Bridge and that the ancestral Eskimos were forced to withdraw as the water levels rose while the Aleuts [first known at Anangula] remained in place" (see *19*).
9. D. E. Dumond, *Anthropol. Pap. Univ. Alaska* **12**, No. 1, 33 (1964).
10. ——, *Amer. Anthropol.* **67**, 1231 (1965).
11. ———, *Proc. Int. Congr. Anthropol. Ethnol. Sci. 8th 1968* **3**, 102 (1970).
12. W. N. Irving, in *Arctic Institute of North America Technical Paper*, No. 11 (1962).
13. For example, the annual meeting of the Society for American Archaeology in 1968.
14. For example, H. G. Bandi, *Eskimo Prehistory* (Univ. of Alaska Press, College, 1969); J. L. Giddings, *The Archaeology of Cape Denbigh* (Brown Univ., Providence, R.I., 1964); W. N. Irving, *Proc. Int. Congr. Anthropol. Ethnol. Sci. 8th 1968* **3**, 340 (1970); R. S. MacNeish, *Pap. Robert S. Peabody Found. Archaeol.* **6**, No. 2 (1964). As a matter of fact, the first tentative identification of the first Gravels finds (of 1960) was made in early 1961 by the late J. L. Giddings during a visit to Oregon; it was confirmed the following year by Helge Larsen of the Danish National Museum, on the basis of a much larger collection.
15. Dated by radiocarbon determination on peat surrounding it in a bog, at 3860 ± 90 years ago (sample Y-931) [M. Stuiver and E. S. Deevey, *Radiocarbon* **4**, 250 (1962)]. The ash deposit is identifiable wherever it is found near Brooks River; see M. Nowak, thesis, University of Oregon, Eugene (1968).
16. M. A. Trautman, *Radiocarbon* **6**, 269 (1964).
17. J. D. Buckley and E. H. Willis, *ibid.* **12**, 87 (1970).
18. D. D. Anderson, *Acta Arctica* (Copenhagen, 1970), fasc. 16.
19. W. S. Laughlin, in *The Bering Land Bridge*, D. M. Hopkins, Ed. (Stanford Univ. Press, Stanford, 1967), pp. 409–450.
20. ———, *Science* **169**, 1107 (1970).
21. R. F. Black, *Arctic Anthropol.* **3**, No. 2, 7 (1966).
22. C. J. Heusser, *Late-Pleistocene Environments of North Pacific North America* (American Geographical Society, New York, 1960), especially p. 144.
23. Two determinations from peat higher in the same bog are 7670 ± 350 (sample I-1627) and 4360 ± 115 (sample I-3113) (*17*). An unpublished determination on peat from a Takli Island bog of 4.2-m depth (yielding a pollen profile dominated throughout by alder), in which by laboratory error a sample from a depth of 1.9 m and a sample from a depth of 3.3 m were combined, was 5580 ± 210 (sample I-1939) years ago. All of these serve to support the early dating of the single basal sample.
24. E. H. Muller, "Geology of the Brooks River Archeological Sites," unpublished (1963).
25. At the risk of appearing ungracious I recall Laughlin's remark that his model of 1967 (*8, 19*) "needs considerably more evidence, . . . and it needs to be examined and tested in company with alternative hypotheses." Amen.

6 November 1970