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THE CANADIAN RESEARCH CENTER
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The Canadian Research Center for Anthropology

As already stated in the first issue of Anthropologica, *the* Research Center for Amerindian Anthropology *owes its origin to a meeting of scholars from the fields of Missiology and Anthropology. Specially interested in the Amerindians they were eager to collect anthropological data from our canadian Indians and Eskimos.*

From its very beginning the Center attracted the attention of distinguished anthropologists and field workers. Not a few accepted to cooperate in its work, and soon, although a newcomer in the field, the Center was publishing such important works as an Eskimo dictionary, some Eskimo translations, and an ethno-linguistic questionnaire for the use of field workers. In 1955 the Center's official magazine Anthropologica *appeared for the first time. Though only mimeographed, it was, nevertheless, greeted with great satisfaction by anthropologists here and abroad.*

These few years devoted to the cause of Amerindian Anthropology has brought out the need for a broader organization embodying research in the whole field of anthropology. Consequently, the Center resolved to extend its research work along this line, welcoming scholars and field workers from all the various branches of the science of man. The Research Center for Amerindian Anthropology *has thus become the Canadian Research Center for Anthropology.* Anthropologica *remains its official organ, beginning a new series with the present issue. As previously it will be published twice a year, and will continue to give preference to canadian material, although by no means limiting itself to such.*

Joseph-E. Champagne, O.M.I.
President
The Canadian Research Center for Anthropology

Après quatre ans...

Anthropologica *fait peau neuve: cette livraison-ci, la neuvième depuis la fondation de la revue, en 1955, est la première qui soit imprimée. C'est l'occasion de nous demander ce que nous avons fait jusqu'ici et ce que nous voulons maintenant accomplir.*

Dans la première livraison, le Président et le Directeur du Centre de Recherches d'Anthropologie Amérindienne *manifestaient leur intention de publier des études ethnographiques ayant trait aux populations indigènes du Canada.* Anthropologica *était fondé pour servir de débouché aux ethnographes canadiens dont certains articles, faute de publications spécialisées, risquaient de ne pas voir le jour. Les quelque quinze cents pages de textes que nous avons publiées jusqu'ici montrent bien que nous avons suivi le programme que nous nous étions tracé. L'accueil que nous avons reçu au Canada et à l'étranger nous incite à continuer de publier* Anthropologica, *en apportant quelques modifications à sa formule.*

Récemment, l'organisme qui publie Anthropologica *est devenu* Le Centre Canadien de Recherches en Anthropologie; *ce changement veut marquer que dorénavant, le Centre et la Revue ne s'intéresseront pas exclusivement à l'ethnographie amérindienne. C'est donc avec une formule élargie qu'*Anthropologica *se présente aujourd'hui. Deux mots d'explication là-dessus: il va sans dire que les études anthropologiques sur les Indiens et les Esquimaux du Canada continueront de trouver place dans notre revue; nous ne nous ferons pas faute non plus de publier des études d'acculturation et d'anthropologie appliquée qui auront trait à ces mêmes populations. D'une façon plus générale, nous publierons des études anthropologiques qui porteront sur les groupes ethniques du Canada.*

D'autre part, le besoin se fait de plus en plus sentir d'une revue qui publie des études d'anthropologie générale, d'une revue qui puisse être, au Canada, un espèce de carrefour des *sciences de l'homme. Plusieurs de nos collègues, tant en sociologie, en linguistique qu'en psychologie sociale, croient que l'anthropologie culturelle contemporaine peut servir de point de rencontre pour de tels échanges interdisciplinaires.* Le Centre de Recherches en Anthropologie et Anthropologica *veulent favoriser ces rencontres de praticiens des sciences de l'homme et publier leurs études.*

Comme par le passé, seront acceptées les communications en anglais ou en français. Dès la prochaine livraison, nous publierons des recensions critiques et des comptes rendus des ouvrages qu'on voudra bien faire parvenir à la revue.

Inutile d'ajouter que ce sont les bons auteurs qui font les bonnes revues; nous osons espérer que non seulement les praticiens de l'anthropologie et des sciences de l'homme s'abonneront à la revue mais qu'ils lui enverront de bons articles.

Marcel RIOUX
Rédacteur en chef

A Speculative Framework of Northern North American Prehistory as of April 1959

BY RICHARD S. MAC NEISH

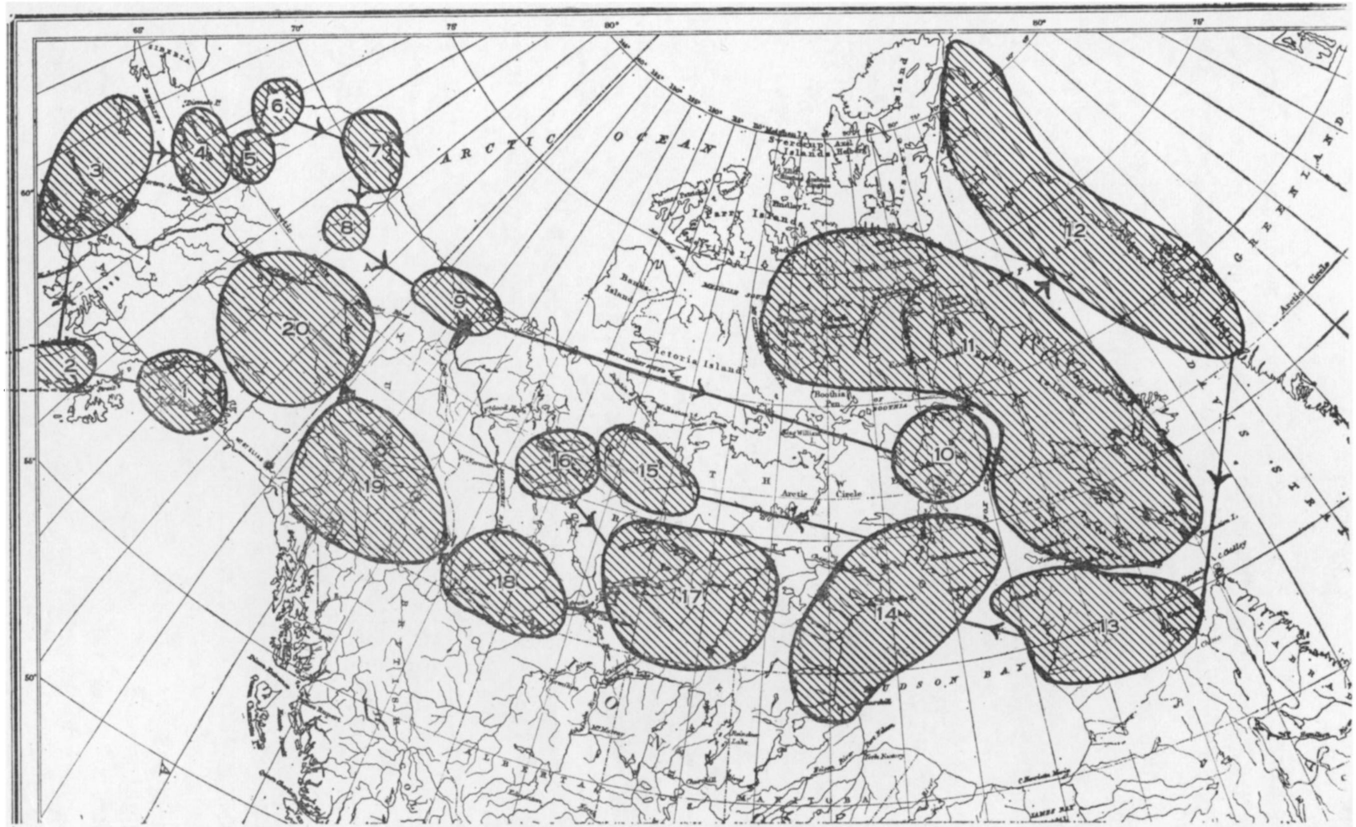
For a number of years I have kept on a wall of my office a chart of the prehistoric cultural sequences and relationships of Canada north of the sixtieth parallel. As new data appeared that chart has been supplemented and changed. I seem to have a compulsion to fill in gaps, even when the data is incomplete, and have several times tried to give a cohesive picture of Arctic prehistory to the layman. This paper summarizes those attempts. Much of the cultural relationship and temporal alignment on the chart is speculative (and probably incorrect). Generally speaking, I have felt that it was a personal matter better not shown to my archaeological colleagues. However, in response to flattering requests (including one from the editors of this journal) I am publishing the chart and attempting to explain it. Please be warned the chart is subject to change without notice, and also, not to believe as facts all that you read herein.

Now as to the chart: the vertical columns represent arbitrarily divided areas in North America, north of the sixtieth parallel. The columns running from left to right begin with the Gulf of Alaska, next to the Aleutians and the Bering Strait region, over the tundra of the Arctic to Melville Peninsula, and then across the islands of the Franklin District to Greenland. Next, they start on a more westerly movement down to the Ungava Peninsula and the southern Keewatin District, then to the area around, and inland from, Coronation Gulf. Here they enter the edge of the forest area about Great Bear Lake and the southeast part of the Mackenzie District. Next the columns go westward into the true northern boreal forest, the southwestern Mackenzie District, southern Yukon and interior Alaska. Thus the series of columns end not far from the geographical point where they started (*See* Map 1, as well as Chart).

The horizontal aspect of the chart represents time with millenia marked off by distinct lines. The placement of cultures, phases or components of the various areas in their temporal position had been delineated in part by Carbon-14 determination and geological evidence, but in many cases by mere guess. In describing the cultures and their positions in a later part of this paper, mention will be made of Carbon-14 dates and geological estimates where pertinent. Frankly, the temporal aspect is among the major weaknesses of this chart and also one of the major difficulties in accurately reconstructing the prehistory of the North American north.

Placed on the time-space background of the chart are the cultural units. Some of these units are from single components, others are phases composed of a number of closely related and very similar components. Some of these are published, others are not. I shall attempt to footnote the various names of cultures when they are mentioned so one can see what artifacts composed these units as well as investigate the nature of the units in detail. I have not attempted to indicate the many limited diffusions and influences that the phases (often of different traditions) of the same time period have on each other.

Connecting these cultural units one the chart is a series of lines composed of different symbols. These lines represent different traditions. By tradition I mean a distinct way of life as it is distinguished by different complexes of artifacts or diagnostic traits that persist in time and space. The exact process by which these traditions originate, spread, change, persist, and finally disappear, cannot be determined by the present evidence. Some of these traditions may reflect actual migrations of distinct people with distinct cultures; others may represent diffusions of distinct cultural complexes; others may result from the readaptation of a way of life from one ecological zones to another; still other traditions may derive by combination of all the above-mentioned processes as well as many others not mentioned here. Be that as it may, the origin, spread, persistance and dis-



NORTHERN ARCHAEOLOGICAL AREAS

appearance of traditions seem to be a complicated process. However, in spite of this, the concept of tradition seems useful in delineating cultural relationships in time and space in the north.

Before explaining the chart in terms of these traditions, however, it should be noted that there must exist somewhere in the north a number of early traditions which have not yet been found. These would be hypothetical ancestors to many of the so-called Paleo-Indian cultures found further south in North America. Not only are these hypothetical traditions undiscovered in the New World north, but seemingly they have not been found, or if found not recognized, in the parental region in north-eastern Asia. One can only guess that they do exist in Asia and in the north, and that as far as Asia is concerned, they were probably of Paleolithic times. Only a good deal more work in the north as well as Asia can determine what they are and if they actually exist.

The nearest thing we have to this kind of stage in the north is the poorly-defined British Mountain complex (MacNeish, 1956, and 1959) found on the Firth River, associated with peculiar Arctic soil phenomena. The peculiarity of these Arctic soil phenomena plus the lack of diagnosticity of some of the artifact types cast considerable doubt upon the authenticity of the British Mountain complex. However, this culture complex seems to be different from anything known for the region either earlier or later. Currently we know very little about its date; we do know, however, that it occured during a warm period before the advent of the Cordillerian tradition. The so-called British Mountain tradition is characterized by flake tools with adhering striking platforms that had been struck originally from large discoidal cores. Some of these flakes had been made into unifacial leaf-shaped points, knives, side-scrapers, hook-gravers, end-scrapers and central-type burins. Seemingly also, as part of this complex, are pebble choppers and large plano-convex scrapers. There are a few prismatic flakes which might actually be large crude blades but, conversely, they might also be accidental flakes that approach the form of blades. The complex as a whole has little resemblance to anything else in the north or even further south in the New World. One assemblage that resembles this culture complex is that of Malta site of central Siberia (Bonch-Osmolovsky and V. Gromov, 1936). Also, some of the materials from the Ordos region of North China (M. Bonle, H. Breuil, E Licent, and P. Teilhard de Chardin, 1928) have many similarities to the British Mountain complex. Both of these Asiatic cultures have been classified as being of middle Paleolithic times. Let me hasten to add, however, that the three, British Mountain, Ordos, and Malta, are considerable distances apart and the significance of the artifact resemblances has not been adequately assessed.

On somewhat similar ground is the Cordillerian tradition. It is represented by a number of components of the early Flint Creek phase on the Firth River on the Yukon coast (MacNeish, 1956 and 1959), as well as by a surface site collection from the Klondike site near Fort Liard in southwestern Mackenzie Distric. There are also a number of related sites southward in the Rockies, such as the Frazer Canyon site near Hope, British Columbia (Borden, in press), dated at about 8,150 years ago (S-47) and the lower levels of the Five Miles Rapid site (Cressman, in press on the Columbia River in Oregon, dated at 9,785 years ago (Y-340) as well as other early sites in Washington and Oregon (B.R. Butler, personal communication). Characteristic of this tradition are crude blades struck from large conical cores, Fort Liard laterally spalled burins, and Flint Creek multi burins, lenticular (Lerma-Like?) dart or spear points, scraper planes, large pebble side and end scrapers, end-of-the-blade scrapers, slab choppers, scale-like scrapers, and fish gorges. Many of the traits of this tradition appear in the Verkholenskaya Gora site of late Paleolithic time in central Siberia (Field and Prostov, 1937) and might indicate a cultural connection with it. There are, however, many differences and one cannot determine whether these are incidences of convergence or cultural influences or amalgamations with adjacent earlier cultures in the respective aréas. This early cultural tradition is still rather nebulously defined and it will be necessary to find and study many more archaeological components before we truly understand it.

Apparently invading, mixing with, and replacing the Cordillerian complex on the Firth River (MacNeish, 1956) and at the Kayuk site in the Brooks Range is the Yuma traddition (J. Campbell, personal communication). In many northern areas the Yuma tradition appears to be the earliest. In the Coronation Gulf zone, the Dismal 1A Assemblage (Harp, 1958) is of this tradition, as are the Franklin Tanks and Great Bear River phases (MacNeish, 1955) of the Great Bear Lake region, the Lower Terrace material from Grant Lake in the southern Keewatin District (Harp, 1959), the Artillery Lake and the Taltheilei complex of the southeastern Mackenzie District (MacNeish, 1951), and the Sandy Lake complex of the southwest Mackenzie (MacNeish, 1954), the Champagne complex of the southern Yukon (MacNeish, in press), and the Hosley type of material from central Alaska (Skarland and Keim, 1958). Characteristic of this stage would be nomadic herd hunters who used collateral and ripple flaking, basely ground, lanceolate points such as Plainview, Agate Basin, Milnesand, Angostura, and Scootsbluff-like projectile point types. Also included in this complex might be large bi-facial knives and triangular snub-nose end scrapers. Occasionally a blade or point with burin blows appears with this tradition. These may represent hold-overs or diffusions from earlier horizons or other traditions. The only dates we have on these materials are from the Great Bear River complex, and they are 4,650 4,800 and 5,000 years ago (Saskatchewan S-9, S-10 and S-5). This probably represents an end date for the tradition. The much earlier dates from the Yuma tradition in the American Great Plains seem to indicate that this tradition developed in the New World in the Great Plains and then spread northward. As has been noted, it influenced the final phases of the Cordillerian tradition. It also should be pointed out that remnants of this complex influenced (in terms of ripple and collateral chipping techniques) later traditions such as the Arctic Small Tool tradition and the earliest members of the Northwest Micro-blade tradition.

The next tradition, called Northwest (interior) Micro-blade, has a number of distinctive characteristics that include: tongue-shaped polyhedral cores (plus conical and tabular cores), a series of retoucher micro-blades, micro-blades set in an antler shaft thereby forming a cutting edge, tci-tho type scrapers, notched net sinkers, Fort Liard type burins, large dart points often notched or stemmed (except in the earlier stage when only Agate Basin-like points occur), neatly chipped large plano-convex end-scrapers, a peculiar assemblage, half of fine micro-blades and half of large bi-faces (choppers, knives, and so forth), and an economy seemingly based on lake fishing and forest animal hunting and trapping. When one attempts to study the history of each element of this tradition, many of its component parts are seen as coming from widely different sources. The micro-blades and tongue-shaped cores seem to derive from the northeastern interior of Asia; the bi-faces, burins, conical cores, and blades from the earlier Cordillerian complex already in North America; the projectile points from more southern New World complexes; the tci-tho, net sinkers and subsistence pattern may be recent inventions and adaptations which occurred while the carriers of the tradition moved into the boreal forest. In spite of the diverse background of this tradition, it did come into being and then persisted through time and spread through space.

The earliest known representative of this tradition is the Little Arm complex (MacNeish, in press) from the southwest Yukon which appears in the bottom part of a mature soil profile, perhaps representing the beginning of the post-glacial optimum some 7,000 years ago (Johnston, 1946). Possibly as early are the meagre remains from the lowest levels of Trail Creek Cave in the Seward Peninsula (Larsen, 1951), dated as 5,993 years ago (C-560) and the Kukpowruk phase from the Brooks Range (Campbell, personal communication). Developing out of this early stage, characterized by Agate Basin-like points and a predominance of tongue-shaped cores, is the Gladstone phase of the southwest Yukon (MacNeish, in press), the Pointed Mountain site materials of the southwestern Mackenzie District (MacNeish, 1954), and the Campus site remains from interior Alaska (Rainey, 1939). These in turn seem to have developed into a third stage. The Tyone site materials from interior Alaska (Irving, 1957), the Taye Lake materials from the southern Yukon (MacNeish, in press), the Fisherman's Lake complex of the southwest Mackenzie District (MacNeish, 1954), and the Lockhart River complex of southeast Mackenzie District (MacNeish, 1951) are typical representatives of this stage. The Kamut materials from the Coronation Gulf Drainage (Harp, 1958), the Selwyn and related site materials from southern Keewatin District (Harp, 1959), and the N.T. Docks material from the Great Bear Lake excavations are other possible examples (MacNeish, 1955). The last-named has been dated at 4,100 years ago (S-8). The Natalkuz Lake site (Borden, 1952) of northern British Columbia dated at 2,415 years ago (S-4), and the association of the Taye Lake complex of the southern Yukon with the overlying volcanic ash layer, dated as 1,519 years ago (C-101) hint that this tradition may have lasted until about the time of Christ.

During the latter part of the development of the Northwest Micro-blade tradition, a new tradition appears on the Arctic coast, called here the Arctic Small Tool tradition (Irving, 1957, page 47, footnote 4). Characteristic of this tradition are burins with chipped surfaces, burin spall tools, cuboid (and conical and tabular) polyhedral cores, micro-blades (usually not retouched), ripple-flaked lenticular, lanceolate and triangular endblades for arrows (or harpoons), antler foreshafts for arrows, delicate, small, neatly chipper half-moon side-blades often with ripple flaking, ovoid, semi-subterranean houses with specialized central fire place (often outlined by boulders), and an economy based on caribou hunting but supplemented by a little seamammal hunting. The earliest manifestation of this tradition is the Denbigh Flint complex (Giddings, 1951) at the Iyatayet site on the Seward Peninsula of Alaska. Carbon-14 dates indicate that this is not younger than 4,000 years ago (Rainey and Ralph, 1959) while sea level datings hint that it probably also is not older than 5,500 years ago (Giddings, personal communication). Recently Giddings found on an old high beach level in the Kotzebue Sound area another manifestation of this culture. It also appeared in the Brooks Range where Solecki (Solecki, 1951), J. Campbell, and Irving (Irving, 1953) have found similuar remains. These remains from the Brooks Range I am calling the Itivlik phase. Actually some of these sites found by various archaeologists may likely represent different stages of this single tradition. However, no one has worked this out as yet. On the Firth River, the New Mountain phase (MacNeish, 1956 and 1959), estimated to be about 4,000 years old on the basis of Carbon-14 (Rainey and Ralph, 1959) represents another part of this tradition. Here the Firth River stage with fabric-impressed and cord-marked pottery, and the Buckland stage with dentate stamped, grooved and cord-marked pottery, represent still later phases of this tradition (MacNeish, 1956 and 1959). In the Coronation Gulf region the Dismal II component (Harp, 1956) are of the same tradition. At the Alarnerk site near Igloolik, the two earliest stages which might be called Alarnerk I and II (Meldgaard, 1955) dated Rainey and Ralph, 1956) as between 3,900 and 3,000 years ago, represent a development within this tradition as do the Independence I (Knuth, 1958) and the Sarqaq remains (Larsen and Meldgaard, 1958; Mathiassen, 1958) of Greenland. The latter has been dated as from 3,500 to 2,500 years ago (Larsen and Meldgaard, 1958). A few artifacts from the Button Point site in the Franklin District (Mathiassen, 1927) and from the Nuvuk site in the Ungava Pesinsula (Taylor, personal communication) hint that this tradition also occurred in these regions. The neolithic-type burins, side-blades and projectile points (and ceramics) from the middle Lena (Okladnikov, 1955) and the Yakitikiveem site (Krader, 1952) from the interior of north-eastern Siberia suggest (if the Russian dating is correct) that some of the elements of this tradition were derived from the interior of north-east Asia. The micro-blade industry may have come from the North-west (interior) Micro-blade tradition already in North America, as might the Yuma chipping technique. The tools adapted to marine subsistence may have derived ultimately from the North Pacific tradition, which we will speak of presently. Here again is a case where a series of elements seems to have piled up in North America to form a new cultural tradition, and then moved as a unit across the entire Arctic and persisted in time.

Appearing next in the New World is a tradition termed here the North Pacific. Its diagnostic traits include the ground adze, wedge, celt, and haftings for these implements. Also occurring was the grooved muller-hammer. Ground slate dart or knife end-blades, the toggle (female) harpoon, often composite with a single basal spur, neatly made oval and rectangular stone lamps, foreshafts for harpoons with a central line hole, slender barbed points (arrows?), plain, barbed, or composite fish spears, labrets, stone and bone carving, and semi-subterranean rectangular houses with wood or whale bone walls, are other diagnostics of this tradition. The only clear picture of this tradition we have far in the New World has been found by de Laguna (de Laguna, 1934 and 1956) at Kachemak Bay in the Gulf of Alaska. Here she has defined four sub-stages of this culture as well as added to it the culture of the historic Eyak. Perhaps some of the early cultures that Borden has defined on the southern British Columbia coast (Borden, 1950) may also belong to, or be remnants of, this tradition. In terms of dating, of the material, mainly antler, has been processed for the Kachemak Bay I antler has been dated as 2,706 years ago (P-139). Some of Borden's early sites give dates of 2,900, 2,430 and 2,450 years ago (S-17, S-3 and S-18). On the basis of these estimates (done on antler which is usually conservative), I would guess the complex first appeared in the New World about 1,500 B.C. Dates of 205 and 231 A.D. (P-174 and P-102) for Kachemak III tend to confirm this estimate. Some of the projectile points in this complex, particularly the lanceolate and the lenticular forms, seem to have been derived from the earlier Arctic Small Tool tradition, but many of the elements are entirely new. I cannot but wonder if this tradition with its many new elements did not originate somewhere on the North Pacific coast of Asia. Certainly the many marine adapted tools would seem to indicate it had to come from some coastal region and we have found no likely ancestor for it in the New World.

However, one of the distinctive things about the North Pacific tradition is that it influenced a number of other cultures and gave rise to a series of other traditions in the Arctic. The first which we will discuss seems to have been a blend with the Arctic Small Tool tradition, but it became almost completely dominated by the North Pacific tradition. These remains are what I am calling the Aleutian tradition. As yet, a sufficient number of reports is not available for a clear definition of this tradition, so I will depend here on the precise albeit outdated work of Quimby (Martin, Quimgy and Collier, 1947). Seemingly characteristic of this tradition are multi-barbed bi-lateral harpoons with scoop-shaped sockets and central line holes; also characteristic are rectangular, triangular and ovoid stone lamps, rectangular pit houses (with entry ways), cylindrical earrings, bone blubs and wedges, skin boats, serrated arrow points, notched end-scrapers, and a very distinctive art style. The earliest date for this tradition is 3,018 years ago (C-409), and it seems to have continued from about that time through three stages to the historic period of the Aleuts.

Having a somewhat similar history is the Inuk tradition. Here one finds basically and Arctic Small Tool tradition that took on and often readapted to Arctic conditions a whole series of North Pacific tradition elements. Further, due to its peculiar ecological zone, it invented or formed other entirely new elements. The end results of this process is that the Inuk is distinct from both its parents. Characteristic of this tradition are semi-subterranean houses with entry ways and back beds, ground slate usus, triangular chipped or ground slate harpoon end blades, Arctic winter travelling equipment such as sleds, ice creepers and snow goggles, kayak equipment, a variety of harpoons often with side blades and often also with lateral barbs. These harpoons also usually have bow-drilled line holes. Also occurring are simple lamps either of pottery or stone and cooking vessels, in the west of pottery, while in the east of steatite. Two components, both about 3,000 years old (P-96 and P-203), seem to be transitional between the earlier Arctic Small Tool tradition and the later more definite Inuk tradition. These are the Choris site from the Kotzebue Sound (Giddings, 1957) and the Joe Creek phase from the Firth River area (MacNeish, 1956 and 1959). Both have some holdover of micro-blades but they also show some of the elements of the Inuk tradition appearing. Perhaps more definitely in the Inuk tradition are the Norton type remains from the Bering Sea area (Giddings, personal communication), Seward Peninsula and the Kotzebue Drainage, the Near Ipiutak remains from Point Hope (Larsen and Rainey, 1948), the Kayuk remains from Anaktuvuk Pass (Campbell, personal communication), and the Cliff Culture remains from the Firth River (MacNeish, 1956 and 1959). Here we have something that seems to be truly Eskimo and of the Inuk tradition, though many of the tools, especially the chipped flint ones, seem to persist from the Arctic Small Tool heritage. This general stage seems to have occurred about 400 B.C. (*See* Rainey and Ralph, 1959). After this time a rather peculiar bifurcation arose; one stem of this tradition that died out very shortly is to be found in Ipiutak (Larsen and Rainey, 1948), where seemingly many of the older Arctic tools and ways of life hung on and some of the newer elements of the tradition did not seem to take hold. While this culture persisted at Point Hope and in the Kotzebue area (at the Ipiutak site and the Krusenstern site), a more vigorous Inuk branch was developing in the Bering Sea area where it is called the Old Bering Sea (Collins, 1937) and Okvik cultures (Rainey, 1841). These seem to have occurred between the time of Christ and 300 or 400 A.D. (Rainey and Ralph, 1959). At about the end of the time period — perhaps first in the Barrows area — the Birnirk culture was developing (Ford, 1959). This begins at about 400 A.D. and seems to carry up as late as 800 A.D. and eventually gives away to Punuk culture in the Bering Sea area (Collins, 1937). During Punuk times a variant, called Thule culture, spread Inuk tradition across the Arctic as far as Greenland.

While the Inuk tradition was developing in the Alaskan area, the Arctic Small Tool tradition, particularly the eastern variety, was undergoing a change which gave rise to a new culture which we will call the Dorset tradition. (Much of what I write here is taken from conversation with W.E Taylor, though he should not be held responsible for the interpretation). In essence, the Dorset tradition seems to be composed of three traditions. It is mainly Arctic Small Tool tradition with a number of influences, direct and indirect, perhaps continual, from the western developing Inuk tradition. It also sees a few elements like side-notched points and scrapers from the Northwest Micro-blade traditions, especially from Lockhart River complex of the southeast Mackenzie and Keewatin (MacNeish, 1951). Characteristic for this stage would be micro-blades struck from polyhedral cores, either of the cudoid, tabular or conical variety. Also characteristic of this stage would be ground burin-like tools. Notched arrow points and knives occur, and there are many more chipper triangular points than lancealate. These people also had a sea-mammal adaptation, but their harpoons are somewhat different from the charcteristic Inuk ones in that they have gouged rather than drilled line holes. Houses are often rectangular and their walls are composed of extremely large rocks. They also have a series of notched and stemmed end-scrapers. All these traits plus a distinctive realistic art make this tradition very different from anything else we have seen. Its geographic extent is very limited for it occurs mainly in the Eastern Arctic and eastern coastal sub-Arctic. Recent work has demonstrated a number of stages within this culture. These stages have been determined on the Melville Peninsula and are here called Igloolik I, II, III, IV, and V (Meldgaard, 1955). In the District of Franklin there are also Dorset remains, including those found at Frobisher (Collins, 1950), and these remains seem to be equivalent to Igloolik III. In Greenland there are at least two stages: Sermermiut which is equivalent to Igloolik III as well as Ritenbenk which seems to be equivalent to Igloolik V (Larsen and Meldgaard, 1958). In the Ungava Pesinsula Taylor's unpublished data show many of the five stages that are at Igloolik; however, only three of them have been well enough defined to speak of; one is called Tyara and is equivalent to Igloolik I; a second is Tonoo which is equivalent to Igloolik II; and a third is Keeatina which is equivalent to Igloolik V. On Southampton island, the famous T-1 site Collins, 1956) seems to be equivalent to Igloolik II. Many of the sites on the Labrador coast and at the northern edge of Newfoundland (areas really not a part of this paper), seem to be roughly equivalent to Igloolik IV and V. The earliest date of the Igloolik Dorset sequence seems to be about 800 B.C. and the final date and the end of this tradition seems to be around 1,000 A.D. (Rainey and Ralph, 1959). Just what ultimately happened to the Dorset tradition has as yet not been determined, but it may very well be that this Eskimo-like culture became swamped and amalgamated with the eastward spreading Inuk culture which — as the Thule culture — seems to have left Alaska and the Mackenzie area in full force shortly after 1,000 A.D.

The final cultural tradition we shall discuss is what I shall call Denetasiro, which in an Athabascan language means, "parent of the living Dene people." Characteristic of this tradition would be tci-tho scrapers, small, side and corner-notched arrow points, unilateral multibarbed antler arrow points, detachable multibarbed fish spears, long-bone fleshers, antler daggers, antler and wooden clubs, large corner notched lance points, bone beamers, and an economy which was based on fishing with some adaptations toward trapping and hunting within the boreal forest. In the Athabascan area there are a number of phases that seem to belong to this tradition. In the southeastern Mackenzie District we have the Whitefish Lake complex (MacNeish, 1951); in the southwestern Mackenzie District there is the Spence River complex (MacNeish, in press); the Dixthada site in interior Alaska (Rainey, 1934) also seems to be of this tradition. All of these are historic or late prehistoric between the time of the arrival of the Europeans and 1,000 A.D. Only one phase seems to be any earlier and this is the Aishihik phase from the southern Yukon (MacNeish, in press). This lacks many of the bow and arrow elements and has a few more resemblances to some of the latest aspects of the Northwest Micro-blade tradition. However, the origin of the Denetasiro tradition still is undetermined. Some of its elements certainly derive from the Northwest Micro-blade tradition; where the others came from, I do not know and this remains a key problem in northern prehistory.

Summary

As is perhaps obvious from this paper, there is still a great deal unknown about Northern North American archaeology. I believe, however, that in terms of the concept of tradition most of the known sites and industries can be fitted into some sort of coherent scheme. That this scheme will be revised and the traditions supplemented and redefined is a certainty. Be that as it may, I hope this paper shows some progress toward synthesis of the rapidly accruing archaeological data and gives the reader a clear picture of what is known.

Briefly I see northern prehistory at present in terms of ten traditions (of varying validity). The earlier ones, British Mountain, Cordillerian, and Yuma are based on tenuous evidence. The Northwest Micro-blade, Arctic Small Tool, and North Pacific traditions are more solidly established and every field season sees their improvement. Except for the Aleut tradition, the later ones, Inuk, Dorset and Denetasiro seem to be on a relatively firm foundation.

However, perhaps as interesting as writing this paper itself has been the comparison of it with previous attempts to outline the prehistory of the north. When one looks at Collins' 1940 attempt, Larsen and Rainey's 1948 effort, and Collins' 1954 progress report, one is struck by the rapid and tremendous strides that have been made. In fact, it now appears that a coherent and understandable picture of Northern North America prehistory is within the realm of possibility in the not too distant future.

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Review and Assessment of the Dorset Problem

BY WILLIAM E. TAYLOR*

This paper contains a chronological summary of work bearing on the Dorset problem, one of the major problems of arctic prehistory. It is felt that such a summary will provide not only the interpretations of archaeologists, but also an historical perspective by which their colleagues may assess the archaeological progress recorded to date on this unresolved problem.

The Dorset culture occupied the Canadian Eastern Arctic and Greenland prior to the arrival from Alaska of the Thule culture Eskimo migrants abut 1,000 years ago, and after the Sarqaq culture occupation which also derived from Alaska and which terminated early in the first millenium B.C. The Dorset culture time span, as indicated by Carbon-14 dates, was more than 2,000 years. It seems to have begun very early in the first millenium B.C. and to have persisted, at least in some locales, until about 1,300 A.D. It has been suggested (Rowley, 1940) that the "Skraeling" referred to by the Viking colonists of West Greenland, were Dorset culture people. Dorset culture sites have been found as far west as King William Island, as far north and east as the northeast extremity of Greenland, and as far south as northern Newfoundland. Sites seem to be especially abundant in the general area of Boothia Peninsula — Foxe Basin — Hudson Strait. Except for those in Newfoundland, sites are restricted to what is at present a tundra zone. Comparative discussions on this prehistoric culture carry

* The writer is indebted to Henry B. Collins, Smithsonian Institution, Diamond Jenness, and R.S. MacNeish, National Museum of Canada, for critical readings of this paper. They cannot be held responsible for the writer's errors.

the investigators west to Alaska and Siberia, and south to the Great Lakes basin and the northeast Woodlands of the North America. Temporally, these discussions grope toward the Mesolithic and Neolithic periods of Siberia and the Archaic stage of the northeast Woodlands. There is, I think, no more accurate manner than this to convey the dimensions and depth of the frame of reference of the Dorset problem.

Archaeological concern with the problem began in 1924, when L.T. Burwash forwarded to the National Museum of Canada a large collection of prehistoric material dug up by Eskimoes, for the most part, around Cape Dorset, Baffin Island. Shortly after this, there appeared in Denmark a first statement on the results of Knud Rasmussen's Fifth Thule Expedition, 1921-24 (1924, Geografisk Tidskrift). Therkel Mathiassen, archaeologist on that famous expedition, presented in it a preliminary description of his newly-discovered Thule culture. Diamond Jenness, working with the Burwash collection, recognized that many of its artifacts were very distinctive from the Thule culture types, but quite homogeneous of themselves. He placed this material under the title "Cape Dorset culture"[1] and published his findings in 1925. He considered it an Eskimo entity, separate from and earlier than the Thule culture.

Disagreement soon came. In 1927 Mathiassen published a full report on the Thule culture, and in it (page 165) reduced the Dorset culture to a "...peculiar, very locally-stamped phase of Thule...". He agreed that it was Eskimo, but not that it preceded Thule culture in the Eastern Arctic. In 1928, Mathiassen repeated his position, although he retracted perceptibly by noting (page 216) that the "ages" of the Dorset artifact types were not clearly comparable with those of Thule culture types.

In 1928 and 1929, Jenness published short notes on Dorset specimens found during 1927 field work in northern Newfoundland. In this Jenness suggested a relationship between Dorset and the extinct Beothuk Indians of that island.

[1] Following current common practice, Jenness' term "Cape Dorset culture" has been abbreviated in this paper to "Dorset culture".

In 1930, W.D. Strong presented his data on the Old Stone complex of Labrador. Strong cautiously suggested that the Old Stone complex might represent a basic culture stratum from which both Indian and Eskimo cultures grew. The suggestion rather implied an Indian and inland origin for the Dorset culture.

In 1930, Mathiassen denied that there was any archaeological evidence for a pre-Thule culture in the Eastern Arctic (page 595) while Birket-Smith in replying to Mathiassen's general view of Eskimo origins, claimed that a pre-Thule culture must have existed in the central regions (1930), but did not so much as acknowledge the existence of the Dorset culture.

In 1933, Jenness reviewed the Dorset problem, repeated his initial evaluation of it, and suggested a Caribou Eskimo origin for Dorset culture. In the same article, Jenness noted W.S. Wintemberg's (1939) discovery of pure Dorset sites in Newfoundland. Shortly after, Collins (1935) accepted Dorset as an Eskimo culture and cautiously accepted Jenness, view of its pre-Thule position.

Soon after, Mathiassen revised his views (pages 130, 1936), agreeing that Dorset did not arise from the Thule culture, but he rejected Jenness' suggestion of a Caribou Eskimo origin for it. Mathiassen was inclined to see Dorset as a "very Indian" culture that had influenced the Thule culture along the shores of Hudson Bay. Collins (page 373, 1937) explored the Indian origin suggestion of Mathiassen, although he construed Dorset as being older than Thule. Later he rejected it in favour of an Alaskan and Eskimo origin (1940). However, Collins still considered it probable that Dorset had been influenced by prehistoric Indian cultures bordering its area to the south. De Laguna, in 1940, held that Dorset was Eskimo, pre-Thule, and a definite contributor to the inventory of the Laurentian aspect of the northeast.

In 1939, T.C. Lethbridge reported on his collections from the Jones Sound and Buchanan Bay areas, and noted a similarity between some of his Dorset pieces and artifacts in Mathiassen's Button Point collections.

In 1940, G.W. Rowley reported on an extensive collection made by him at Abverdjar in the northwest extremity of Foxe Basin. The excellent bone, antler and ivory carvings in this collection led Rowley to reject Collin's suggestion (1937) that Dorset art was similar to the Old Bering Sea I style of Alaska. Rowley concluded that Dorset was pre-Thule, Eskimo, and suggested a beginning date for Dorset of 700 A.D.

Also in 1940, G.I. Quimby described the Manitunik culture of Belcher Islands. It was interpreted as a late occupation, circa 16th century A.D., "...built upon a Dorset influenced Thule foundation." (page 165). Since this sample has only a minor Dorset inclusion and since it was collected at random by Eskimoes, Quimby's suggestion of Dorset influence is daring and doubtful. It might well be that the sample, not the culture, was mixed.

In 1940, Jenness, writing on Old World relationships, again claimed that the Dorset culture was an Eskimo product and suggested that it was descended from an ancestor common to it and the earliest known Alaskan cultures of that day. In the light of more recent knowledge it is interesting to recall Jenness' interpretation which saw the Dorset culture separating from the common ancestor prior to Old Bering Sea I or Okvik time and spreading to the Canadian Arctic no later than the "first millenium B.C." (page 9). In 1941, Jenness described an archaeological collection from the Belcher Islands that contained a few Dorset artifacts.

Eric Holtved published in 1944 an excellent and detailed account of his excavations in the Thule district of northwest Greenland. While his work was devoted mainly to the Thule culture, he was able to show that the Dorset culture had preceeded the Thule in his area of research.

Junius Bird (1945) reported on both Thule and Dorset traits that he had found in the Hopedale area of Labrador. His view that the Old Stone complex belonged more properly in the Dorset culture than in an Indian sphere as Strong had concluded, is a surprise, at least to this reader.

In 1935 and 1936, Douglas Leechman excavated sites on the Nuvuk Islands near Cape Wolstenholme and also near Port Burwell. In reporting this work, Leechman (1943) construed Dorset as an Eskimo culture. I should like here to correct a recent error (Taylor, 1958b), an error that several others have also made, by noting that Leechman's 1936 Nuvuk work gives the first description of Dorset culture houses.

In 1946, A.C. Spaulding commented cautiously on the Dorset affinities of the Laurentian aspect and noted (page 165) the possibility that such affinities might in fact not have stemmed from the Dorset culture, but perhaps "that the Eskimo influence so apparent in Laurentian culture was exerted in the west, rather than in Labrador and Newfoundland."

With all the commentary on a possible Dorset-Indian exchange, it remained to Frederica de Laguna to provide the comprehensive and vigorous statement of the matter. As early as 1940 as we have seen, de Laguna was a considerable supporter of Jenness' 1937 views on the Dorset problem. In 1946, de Laguna wrote of Dorset as an Eskimo product that had moved from the west to the eastern Arctic prior to Thule times, and that the two had co-existed for a time later in the first millenium A.D. More significantly in this paper, de Laguna, comparing harpoon heads, leisters, chipped stone tools, and especially ground slate tools, postulated a rich Dorset culture contribution to the Indian Laurentian aspect of the northeast Woodlands and its Red Paint culture variant in New England. She thought that the exchange occured circa 1,000 A.D. and concluded that the Dorset Eskimo had adopted very little of Indian culture in the exchange. Unlike Jenness, who suggested that a Dorset migration to the eastern Arctic had occurred by 1,000 B.C., de Laguna estimated that Dorset began about 500 A.D. In her wide-ranging "The Prehistory of Northern North America as Seen from the Yukon" (1947), de Laguna repeated her general position. In this same volume (page 9) she suggested that the Sadlermiut of Southampton Island had been a descendent of Dorset culture subject to Thule influence. Collins, as we shall see, followed this idea a few years later. De Laguna saw as the western relatives of Dorset, Old Bering Sea I and Kachemak I.

In a paper read to the Third International Congress of Anthropological and Ethnological Sciences, Brussels, 1948, Birket-Smith (1951) held the Dorset to be a Palaeo-Eskimo culture that began about 200 A.D. and lasted until 1,000 A.D. He saw Dorset as an eastern, but less-developed relative of the Ipiutak culture and predicted that a pre-Ipiutak, more Dorset-like culture might be found in Alaska (page 149). As had de Laguna (1934), Birket-Smith noted that there were connecting links also between Dorset and Kachemak I of southwest Alaska. At the time of his address, Kachemak I, Ipiutak and Dorset were the oldest known cultures in their respective areas, or — at least — so many Eskimologists believed. Finally, Birket-Smith supported de Laguna's conclusions on Dorset contributions to the Laurentian and Red Paint cultures of the northeast Archaic Pattern.

In 1947 (Martin, Quimby and Collier), and again in 1952 (Quimby) a Dorset relationship with another northeastern Indian Archaic Pattern culture was suggested. In this case it was the Old Copper culture which centres around Wisconsin but is known as far east as Ontario (Popham and Emerson, 1954). Wittry and Ritzenthaler (1956) reject the suggestion of a Dorset influence on the Old Copper culture since the two carbon dates available on it are about 3,600 B.C. and 5,500 B.C., long before the earliest suggested Dorset date. They do suggest (page 261), "...that if any diffusion of these traits took place, it was northward." To me, even this is a highly unlikely suggestion since it separates the two cultures by at least 1,500 years, without mention of the spatial and ecological gaps. If the Dorset-Old Copper parallels are meaningful, it may be more productive to suggest that both have been influenced by a third as yet unknown source located perhaps in northern Manitoba. There remains the possibility that the Dorset-Old Copper typological parallels are coincidental.

In retrospect, the years between 1925 and 1948 were the exploratory years for the Dorset problem. There were very few workers and they contended with an area that was achaeologically little known, vast, and difficult of access. The Dorset culture, once defined, had to be accepted as a distinct entity, its pre-Thule position had to be demonstrated, its area determined, its artifact inventory had to be prepared and added to, its possible genetic relationships had to be postualted, explored, and debated. In time its definition, separateness, area, and pre-Thule time were accepted or determined. Its inventory was, and is being, added to. Two possible genetic connections were suggested and debated. That debate is continuing.

In the years immediately after 1948, Arctic archaeology underwent a drastic and rapid change. There was a great increase in transport facilities, especially in the Canadian Eastern Arctic and a marked increase in archaeological field work in Arctic America generally. New archaeological techniques, notably dendrochronology and Carbon-14 analysis, were applied to Arctic materials. The most prominent change, however, was affected by J.L. Giddings' reports (1949, 1951) on the very early Denbigh Flint Complex of western Alaska. This work brought new meaning to some material already in the literature and was soon followed by several reports on other early sites in Alaska (Solecki and Hackman, 1951; Solecki, 1951; Larsen, 1951, 1953[2]; Irving, 1951, 1953). For those pondering the Dorset problem it was evidence of a new depth of time to be explored, for the micro-blades, polyhedral cores, and burins of the Dorset culture had marked affinities in the Denbigh Flint Complex. It also brought a new focus on the "West Greenland Stone Age", defined as early as 1907 by O. Solberg (Collins, 1953a, page 200; 1953b, pages 34-36). Except for Collins (1935; page 335, 1937; 1940; 1953b; 1954c) this material had been either construed as a late localized variant of Thule, or ignored. Since 1948 the prehistoric picture of the Eastern Arctic has been as much in flux as the viscosity of archaeological paints permits. There have been remarkable increases in the amount of work done on Dorset sites and in the number of workers contributing to the problem.

In 1948, Larsen and Rainey presented their comprehensive theory of Eskimo origins in reporting on the Ipiutak site which

[2] Larsen's 1953 paper will provide the reader will an excellent summary of archaeological work in Alaska up to 1951.

they excavated from 1939 to 1941. As a part of their theory, Dorset is considered as a member of the Ipiutak or Palaeo-Eskimo Complex, and it is suggested that the Dorset culture was carried from Alaska to the Eastern Arctic by migration, that it had "undergone a separate development and, probably through contact with neighbouring Indians, adopted some foreign elements which have contributed towards giving it a special stamp and towards obscuring its similarity to the Alaskan basic culture." (page 184, 1948). Although Collins' perceptive criticism has raised considerable doubts about this Ipiutak theory (1954), it is significant to note that none saw fit to reject the Eskimo nature, Alaskan origin, or early time span that Larsen and Rainey suggested for the Dorset culture.

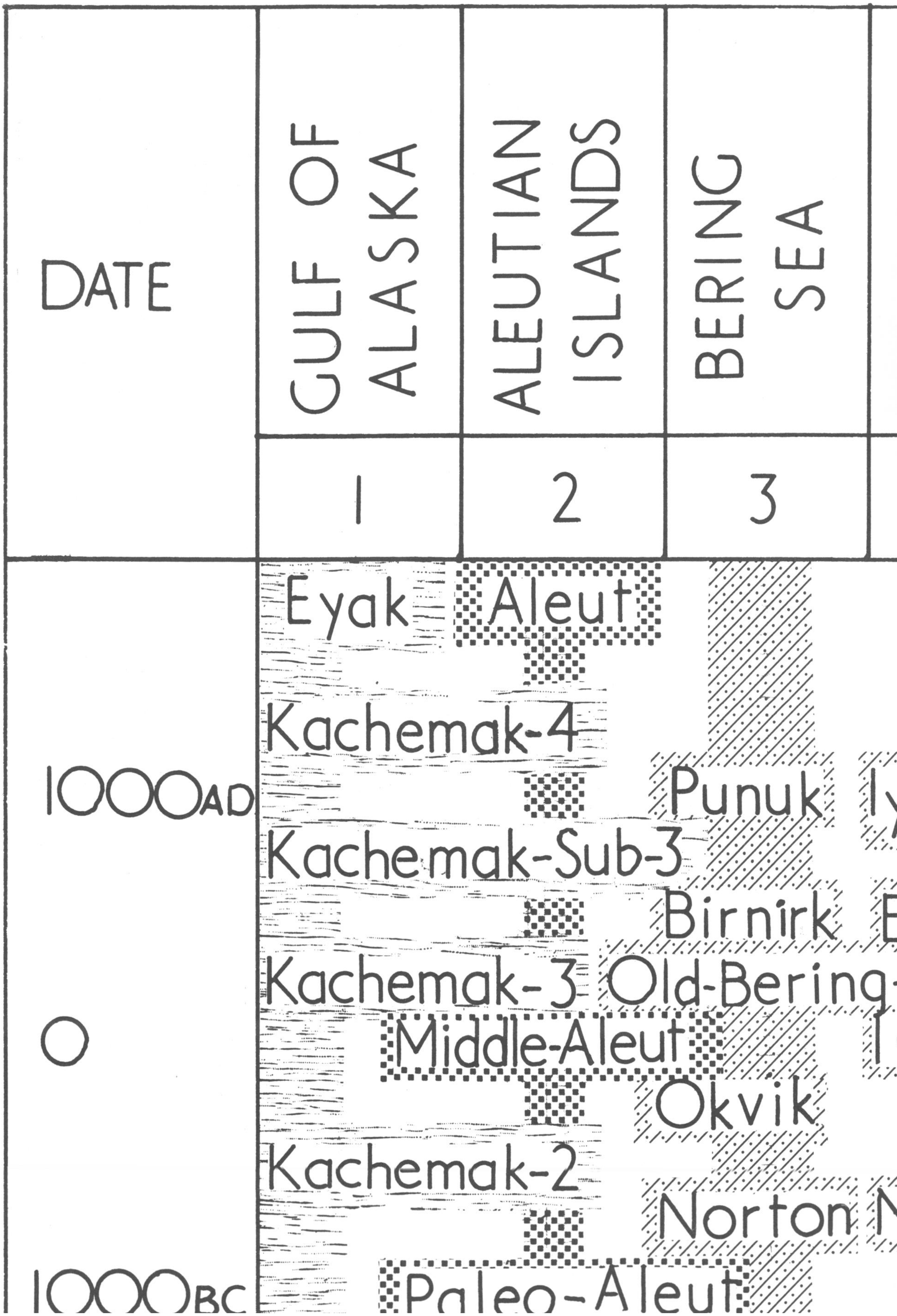
In 1948 Collins (1950) dug a stratified Dorset-Thule site at Frobisher Bay, Baffin Island, and published the first comprehensive classification of Dorset harpoon heads. This paper, so far as I know, contained the first statement of relative chronology within the Dorset culture. In 1951, Deric O'Bryan (1953) worked a mixed site on Mill Island at the western extremity of Hudson Strait. This site contained evidence that its Dorset culture occupation had marked Thule culture influence. This, then, was a Dorset assemblage definitely representative of a late stage of that culture.

In a short but lucid paper published in 1951, Collins summarized his views on Dorset origins by suggesting (page 428) that "The most likely explanation, as suggested by Jenness (1941), is that the Dorset has stemmed from the same parent trunk as the ancient Alaskan cultures. The many and fundamental differences between them, however, would indicate that the Dorset moved eastward to Hudson Bay before the Ipiutak and Old Bering Sea cultures had reached their full development."

In 1951, 1952 and 1953 Elmer Harp published very full statements of the results of his study of the Dorset culture occupation in northern Newfoundland. In this wark, Harp argued, as had several others, for an early Alaskan origin for Dorset, and he suggested "...that Dorset's first movements towards the east occurred in the first millenium A.D., probably toward the middle of that period." (page 307, 1952). He rejected any and all attempts to derive the Dorset culture from the northeast Woodlands and negated the recently published view of B.G. Hoffman (1952) that the Dorset culture was not basically Alaskan, but representative of "...an Arctic tundra and glacial lake culture of considerable antiquity in Eastern North America." (page 16).

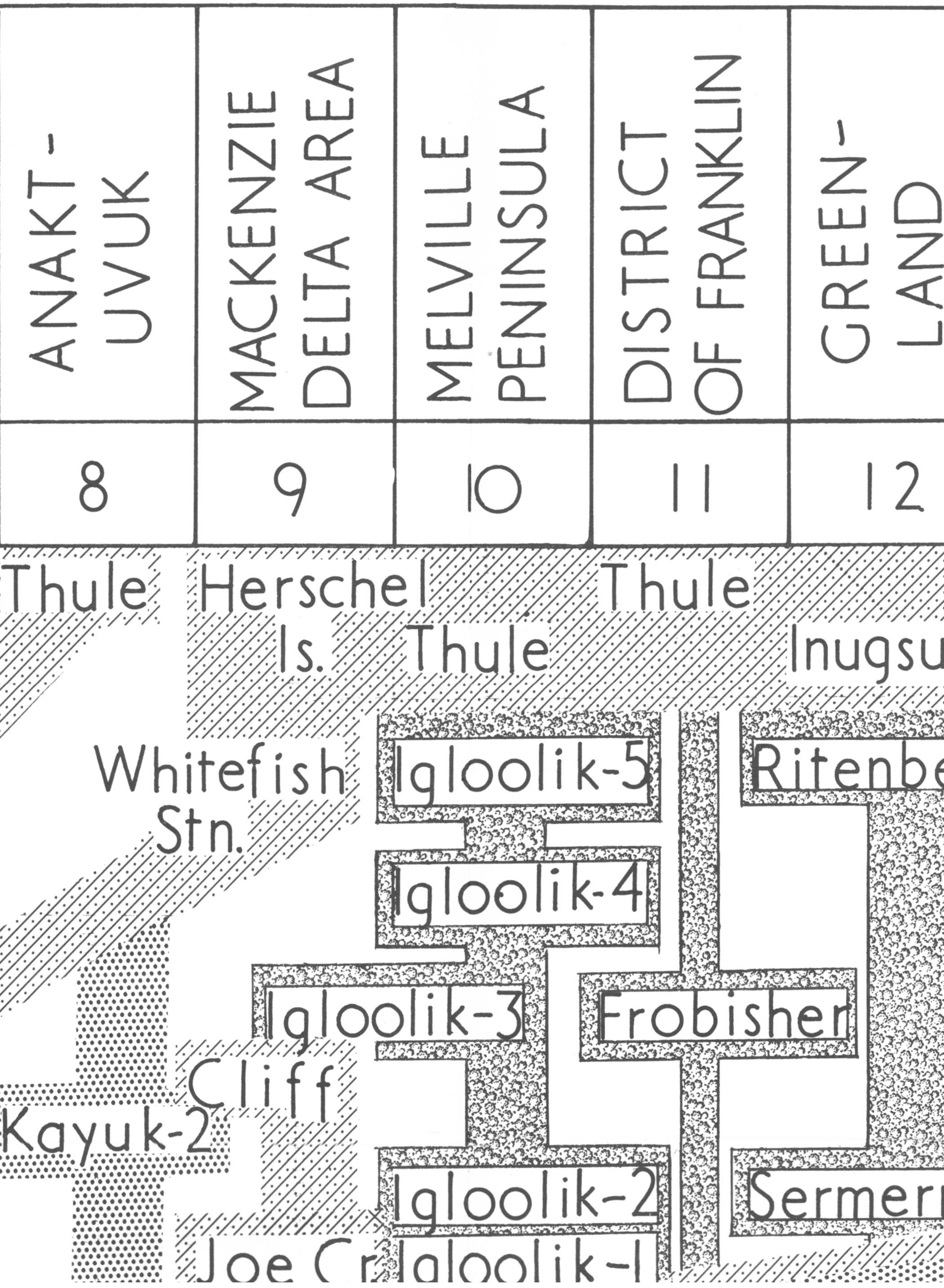
In 1951, William A. Ritchie, basing his argument on Carbon-14 dates for the northeast Woodlands, ran roughshod over earlier attempts to show that the Dorset culture was related to the Laurentian aspect of the northeast Archaic pattern. He was able to show that, on a carbon-dating basis, which gave Laurentian a time span roughly from 3,000 to 1,000 B.C., the youngest Laurentian aspect dates were at least 1,000 years older than most estimates for the origin of the Dorset culture. Consequently he rejected the possibility of Laurentian having any part of its origin in the Dorset culture and was very dubious of the reverse, a Laurentian aspect contribution to the Dorset culture.

In 1925, Jenness (page 437) had concluded that a culture older than Dorset must have occupied the Eastern Arctic but was still to be found. In 1952, Jørgen Meldgaard added that altogether new dimension to the Dorset problem for he reported on an Eastern Arctic assemblage OLDER that Dorset. Although he called it "The Paleo-Eskimo culture of West Greenland", it is generally referred to as the Sarqaq culture. Along with several other assemblages since reported, it is lumped in this paper under the general category of "Pre-Dorset". Meldgaard delineated the strong affinities of his new material with the Denbigh Flint Complex, but to the surprise of some co-workers, rejected the possibility of a Sarqaq-Dorset relationship. Both Collins (1953b, 1954b, 1958) and Harp (1952) considered the Pre-Dorset and Dorset cultures to be related one to the other, although no detailed and comprehensive argument was put forth. In a general review paper that appeared in 1954, Collins (1954b) suggested that Dorset began about 100 B.C., that it derived from the Pre-Dorset cultures of the Eastern Arctic and that, through them, its heritage could be traced to the Denbigh Flint Complex.

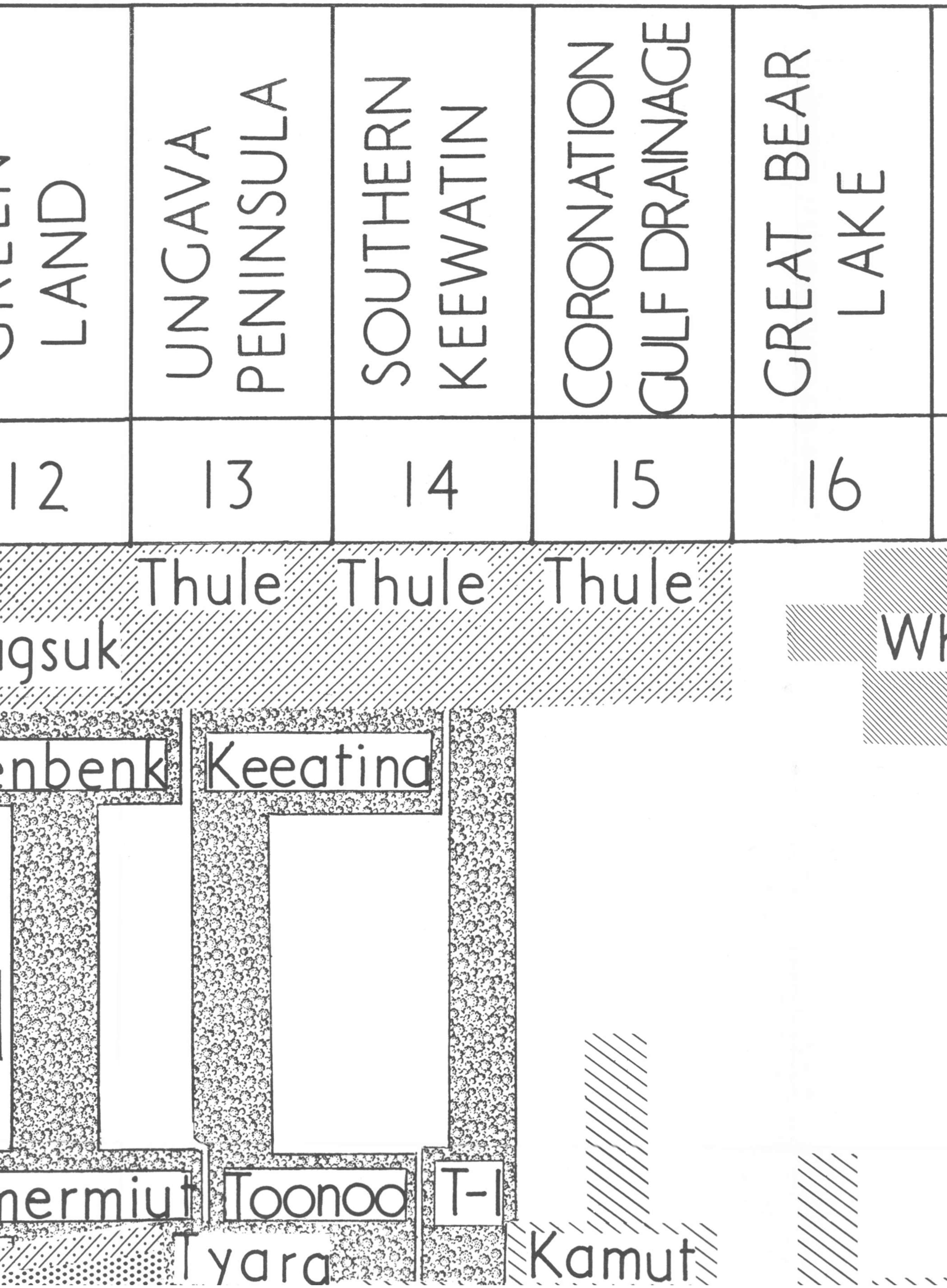


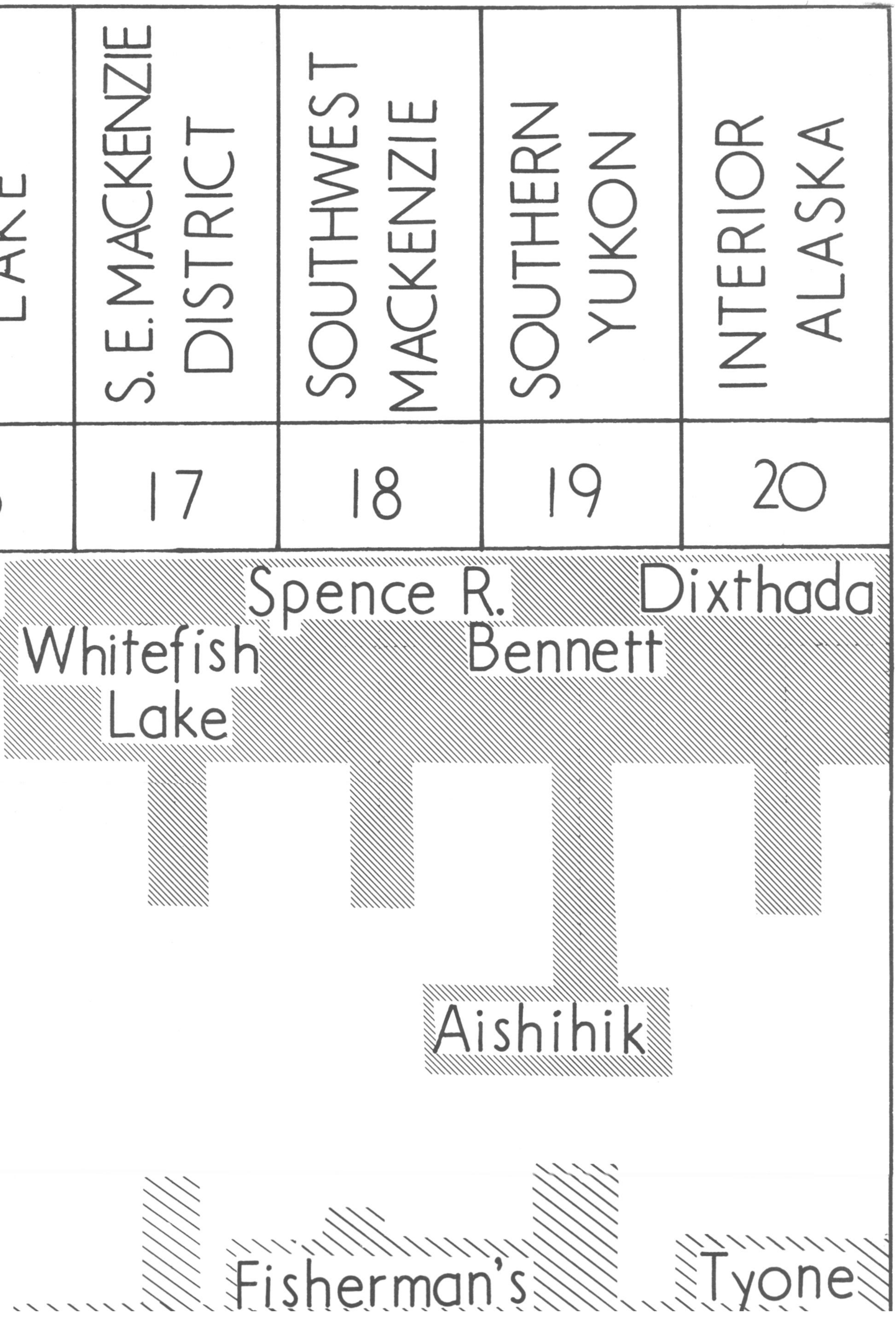
DATE
GULF OF ALASKA
ALEUTIAN ISLANDS
BERING SEA
1
2
3
Eyak
Aleut
Kachemak-4
1000AD
Punuk
Kachemak-Sub-3
Birnirk
Kachemak-3
Old-Bering-
0
Middle-Aleut
Okvik
Kachemak-2
Norton
1000BC
Paleo-Aleut

SLA	SEWARD PENINSULA	KOTZEBUE DRAINAGE	POINT HOPE	POINT BARROW	ANAKT-
	4	5	6	7	8
		Ambler		Thule	Thu
		Ekseavik	Tigara		
		Ahteut			
k	Iyatayet				
k	Birnirk	Birnirk	Birnirk	Birnirk	
ing-Sea		Krusenstern		Ipiutak	
	Ipiutak-Like				
k					
					Kay
on	Norton	Norton			
			Near-Ipiutak		

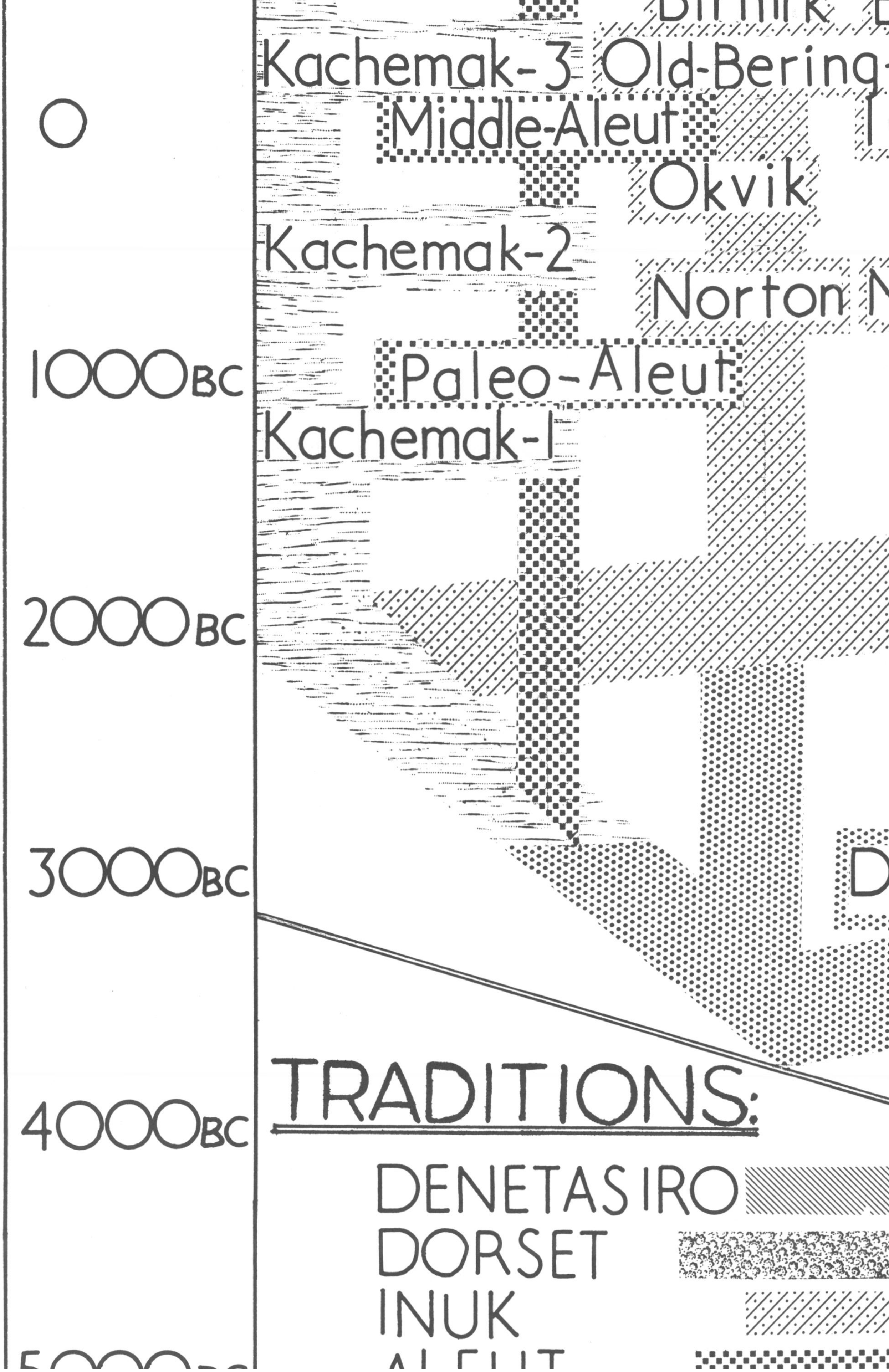


ANAKT-UVUK
MACKENZIE DELTA AREA
MELVILLE PENINSULA
DISTRICT OF FRANKLIN
GREEN-LAND
8
9
10
11
12
Thule
Herschel Is.
Thule
Thule
Inugsu
Whitefish Stn.
Igloolik-5
Ritenbe
Igloolik-4
Igloolik-3
Frobisher
Cliff
Kayuk-2
Igloolik-2
Sermerr
Joe Cr.
Igloolik-1

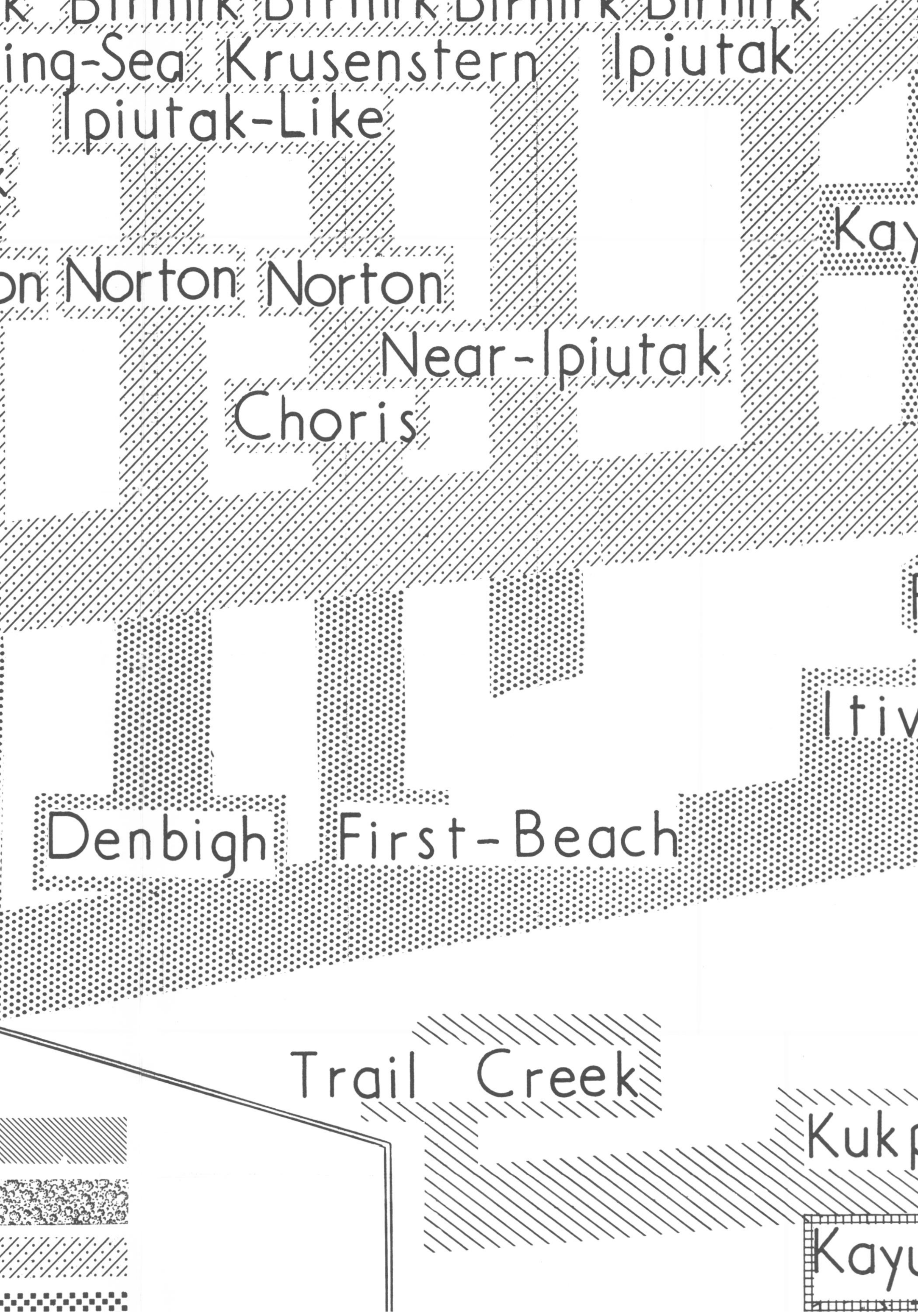

LAND
UNGAVA PENINSULA
SOUTHERN KEEWATIN
CORONATION GULF DRAINAGE
GREAT BEAR LAKE
12
13
14
15
16
Thule
Thule
Thule
Keeatina
Toonoo
T-I
Tyara
Kamut



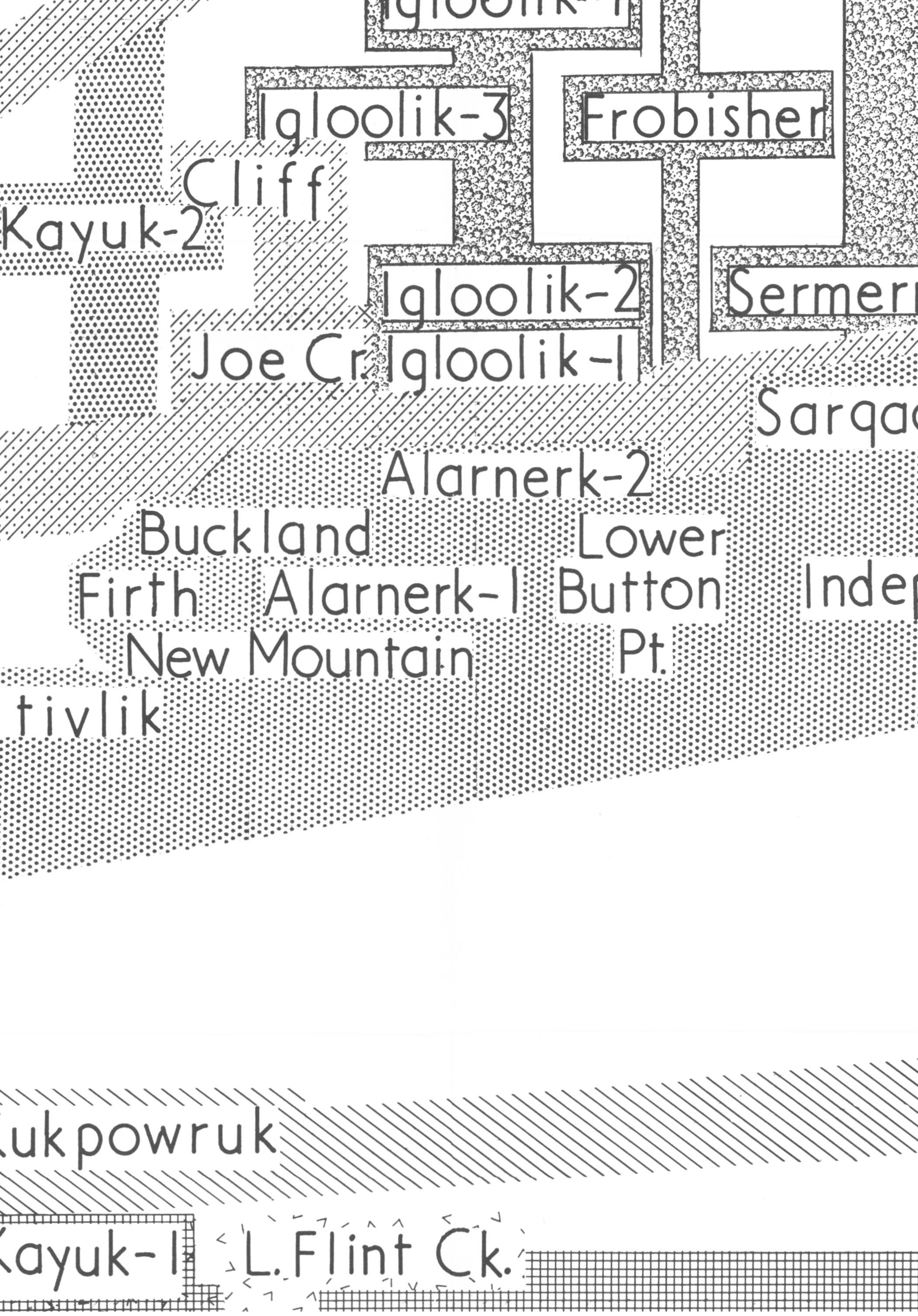
LAKE
S. E. MACKENZIE DISTRICT
SOUTHWEST MACKENZIE
SOUTHERN YUKON
INTERIOR ALASKA
17
18
19
20
Spence R.
Dixthada
Whitefish Lake
Bennett
Aishihik
Fisherman's
Tyone



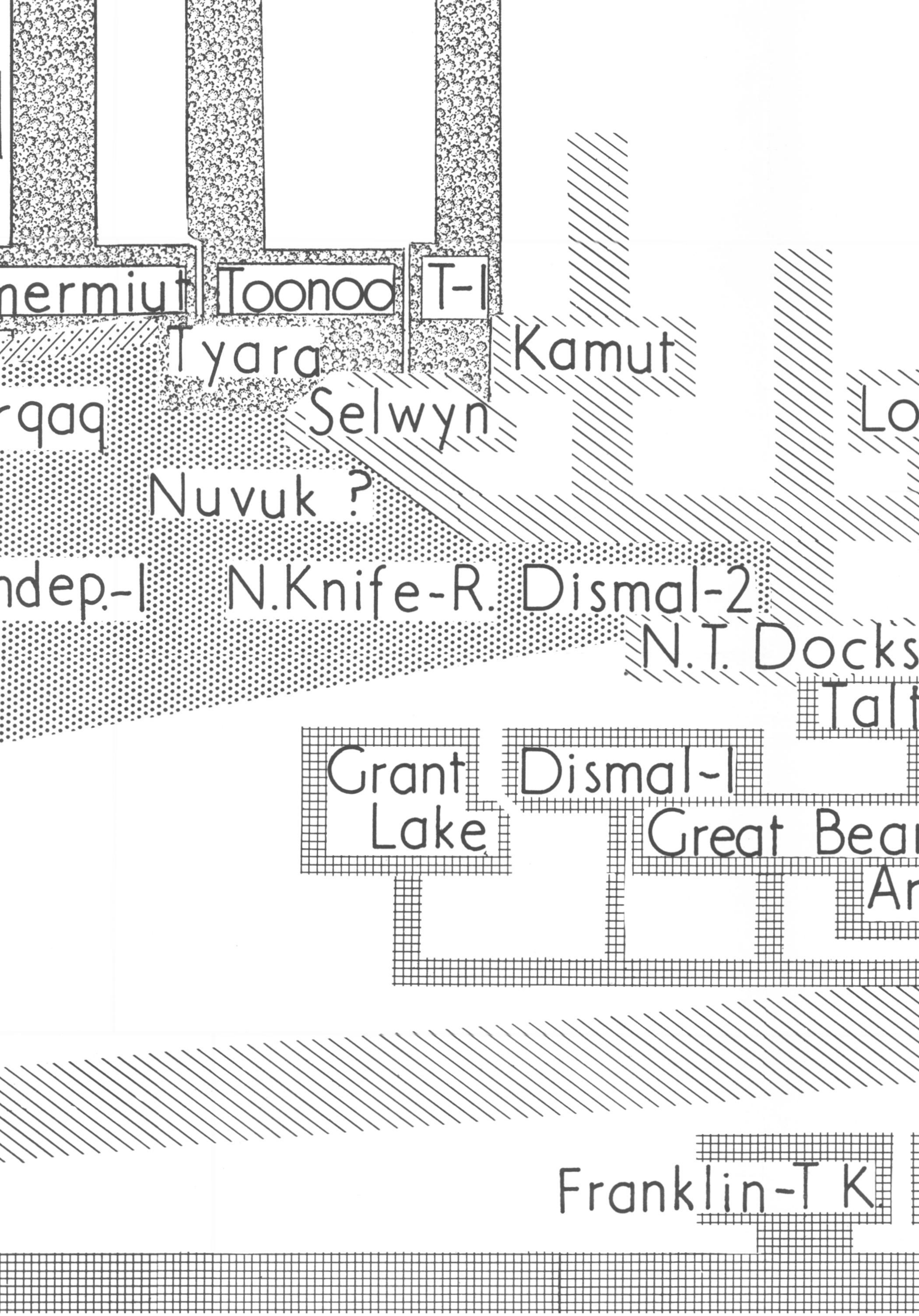
0
1000BC
2000BC
3000BC
4000BC
Kachemak-3
Old-Bering
Middle-Aleut
Okvik
Kachemak-2
Norton
Paleo-Aleut
Kachemak-1
TRADITIONS:
DENETASIRO
DORSET
INUK



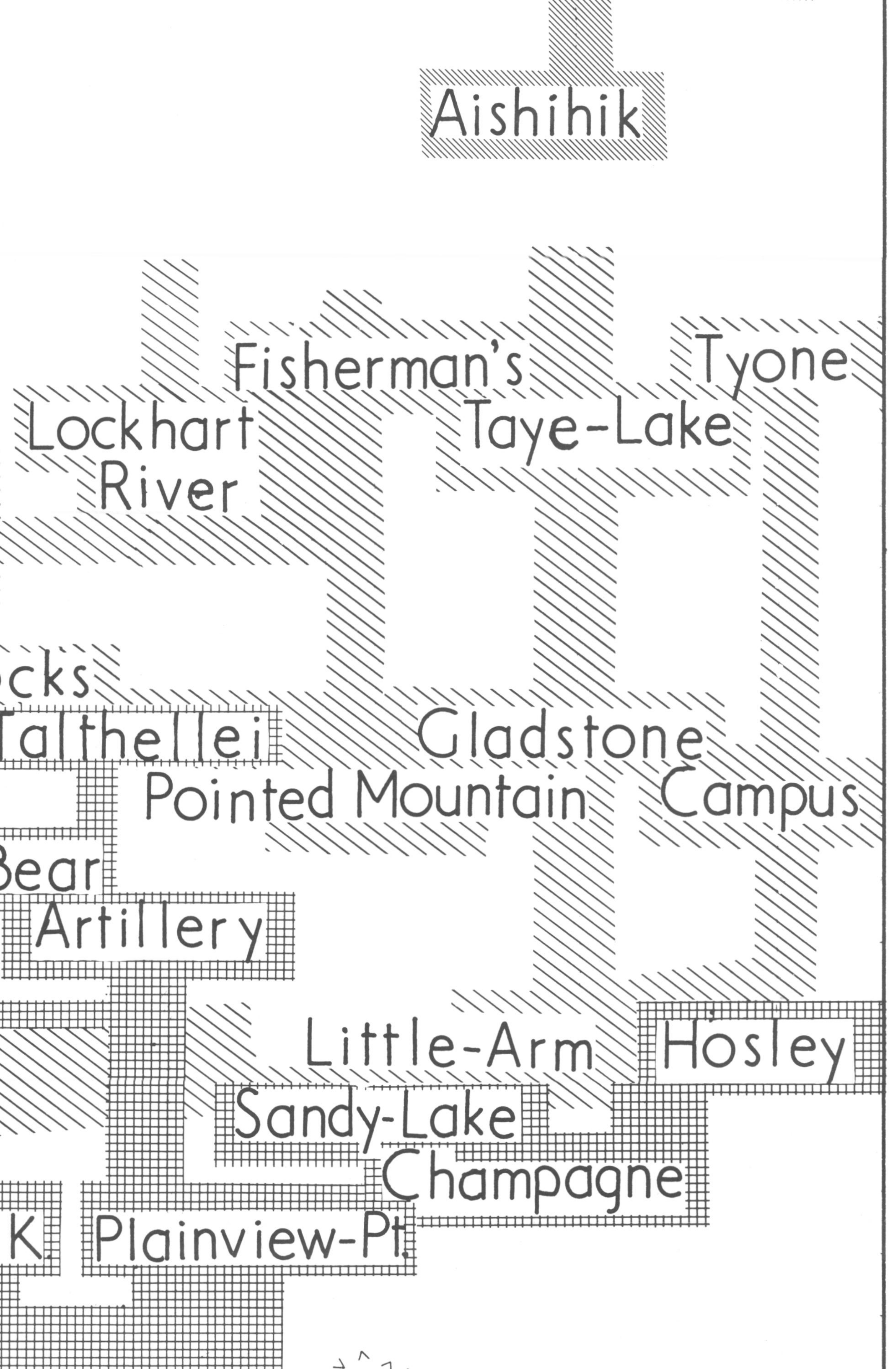
Ipiutak
Krusenstern
Ipiutak-Like
Norton
Norton
Near-Ipiutak
Choris
Denbigh
First-Beach
Trail Creek

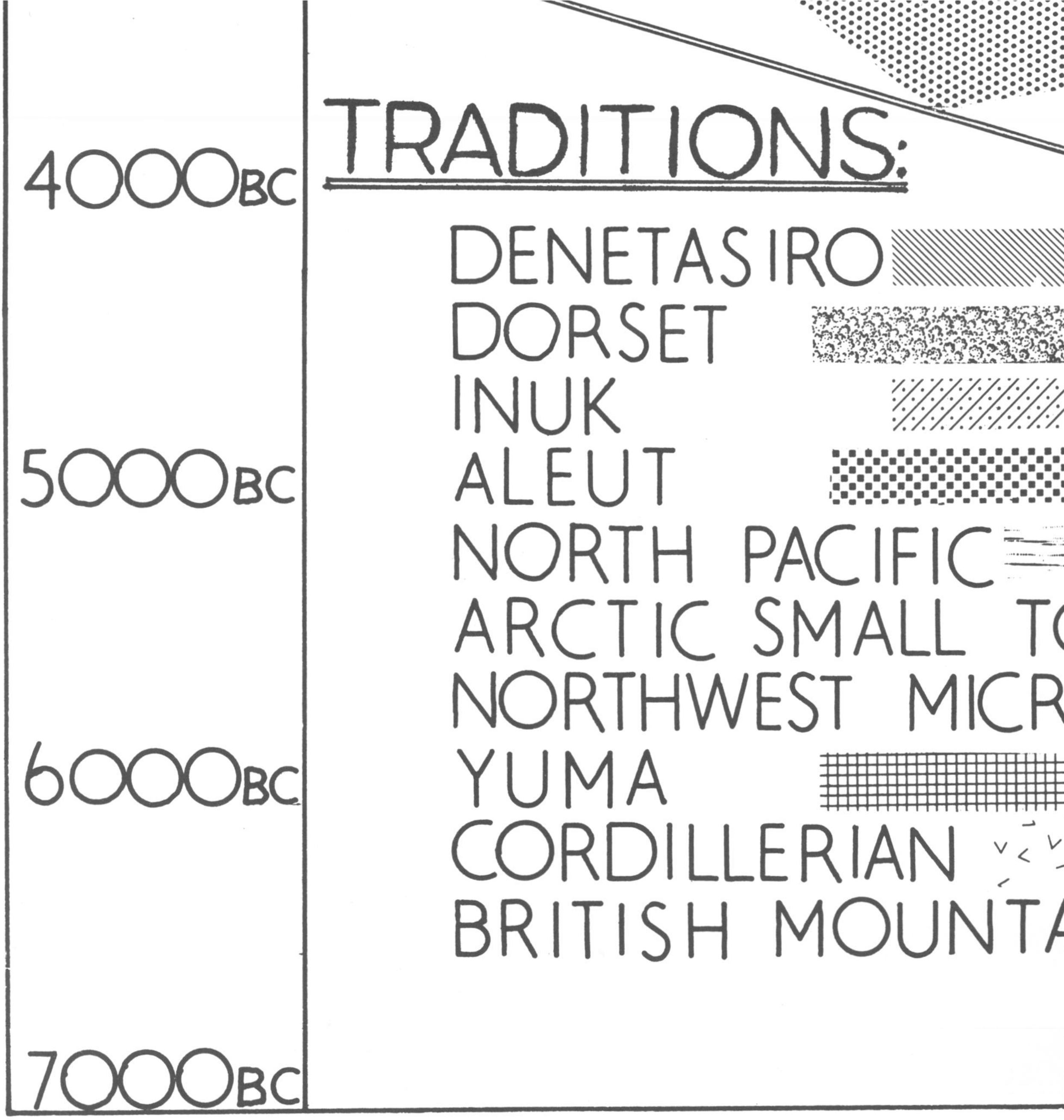


Igloolik-3
Frobisher
Cliff
Kayuk-2
Igloolik-2
Sermer
Joe Cr.
Igloolik-1
Sarqa
Alarnerk-2
Buckland
Lower
Firth
Alarnerk-1
Button
Indep
New Mountain
Pt.
tivlik
ukpowruk
Kayuk-1
L. Flint Ck.

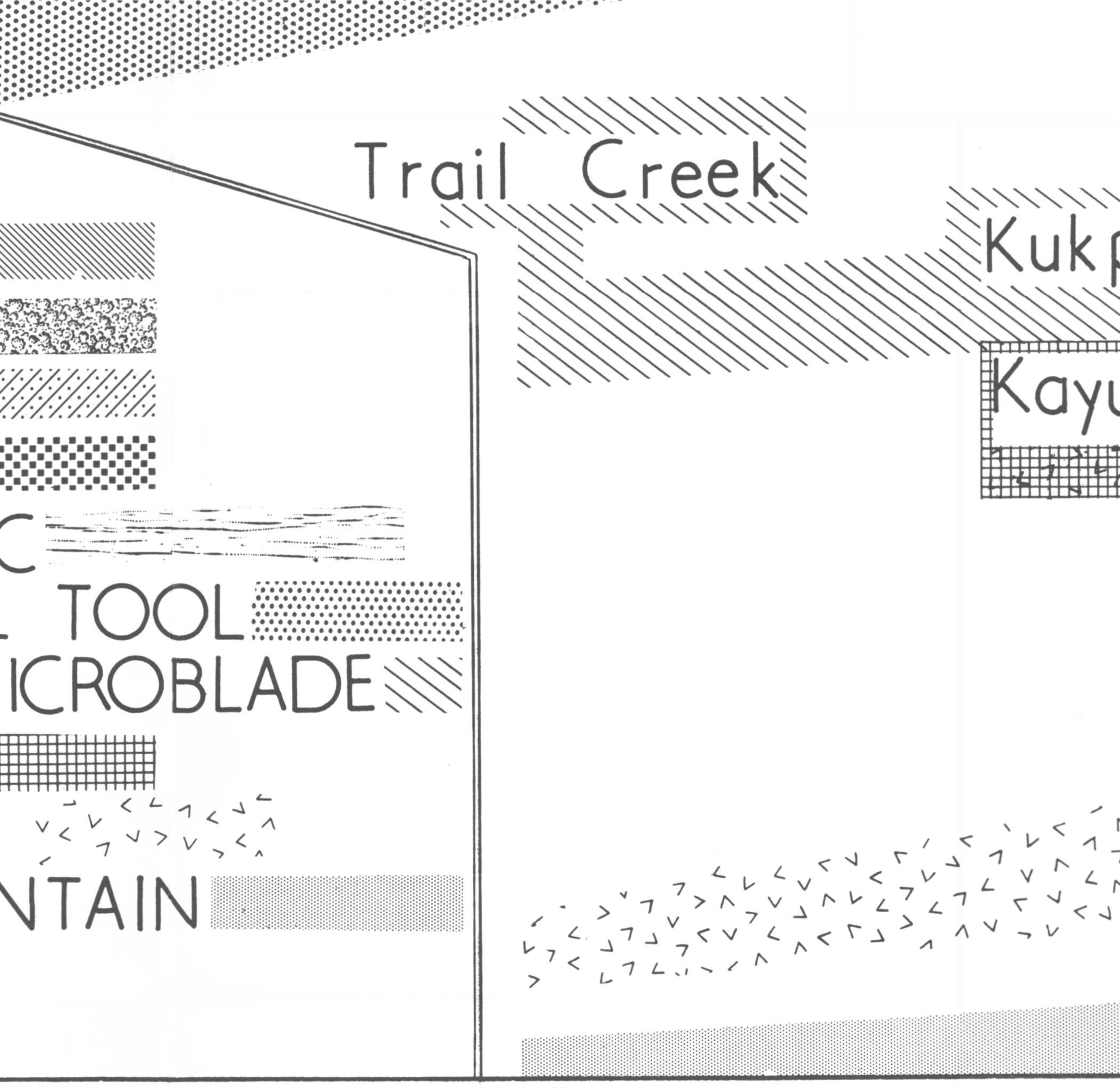


nermiut
Toonoo
T-1
Tyara
Kamut
rqaq
Selwyn
Lo
Nuvuk ?
ndep.-1
N.Knife-R.
Dismal-2
N.T. Docks
Talt
Grant Lake
Dismal-1
Great Bear
Ar
Franklin-T K.

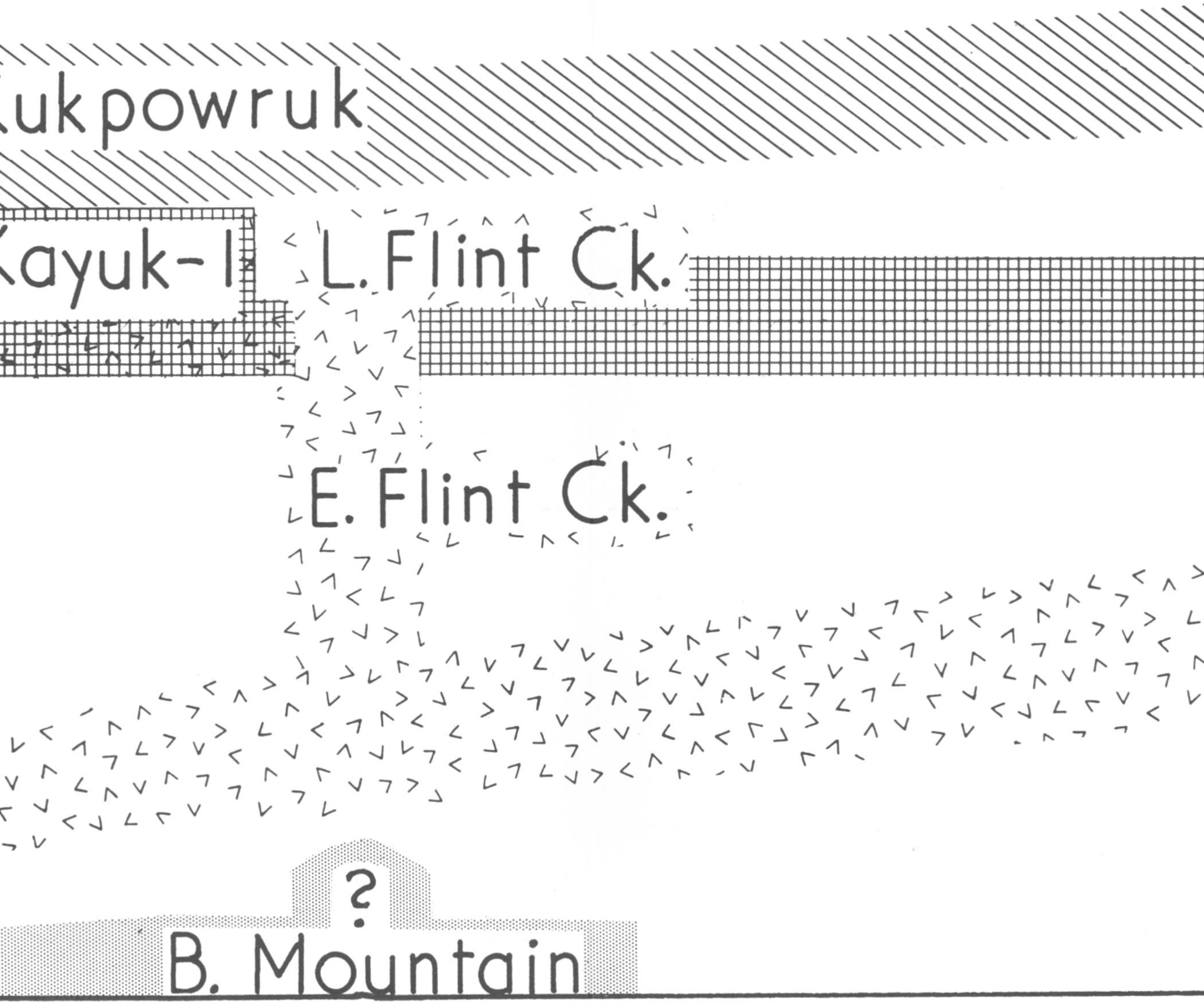

Aishihik
Fisherman's
Tyone
Lockhart River
Taye-Lake
Talthellei
Gladstone
Pointed Mountain
Campus
Bear
Artillery
Little-Arm
Hosley
Sandy-Lake
Champagne
Plainview-Pt.


4000BC
5000BC
6000BC
7000BC
TRADITIONS:
DENETASIRO
DORSET
INUK
ALEUT
NORTH PACIFIC
ARCTIC SMALL TO
NORTHWEST MICR
YUMA
CORDILLERIAN
BRITISH MOUNTA

SPECULATI



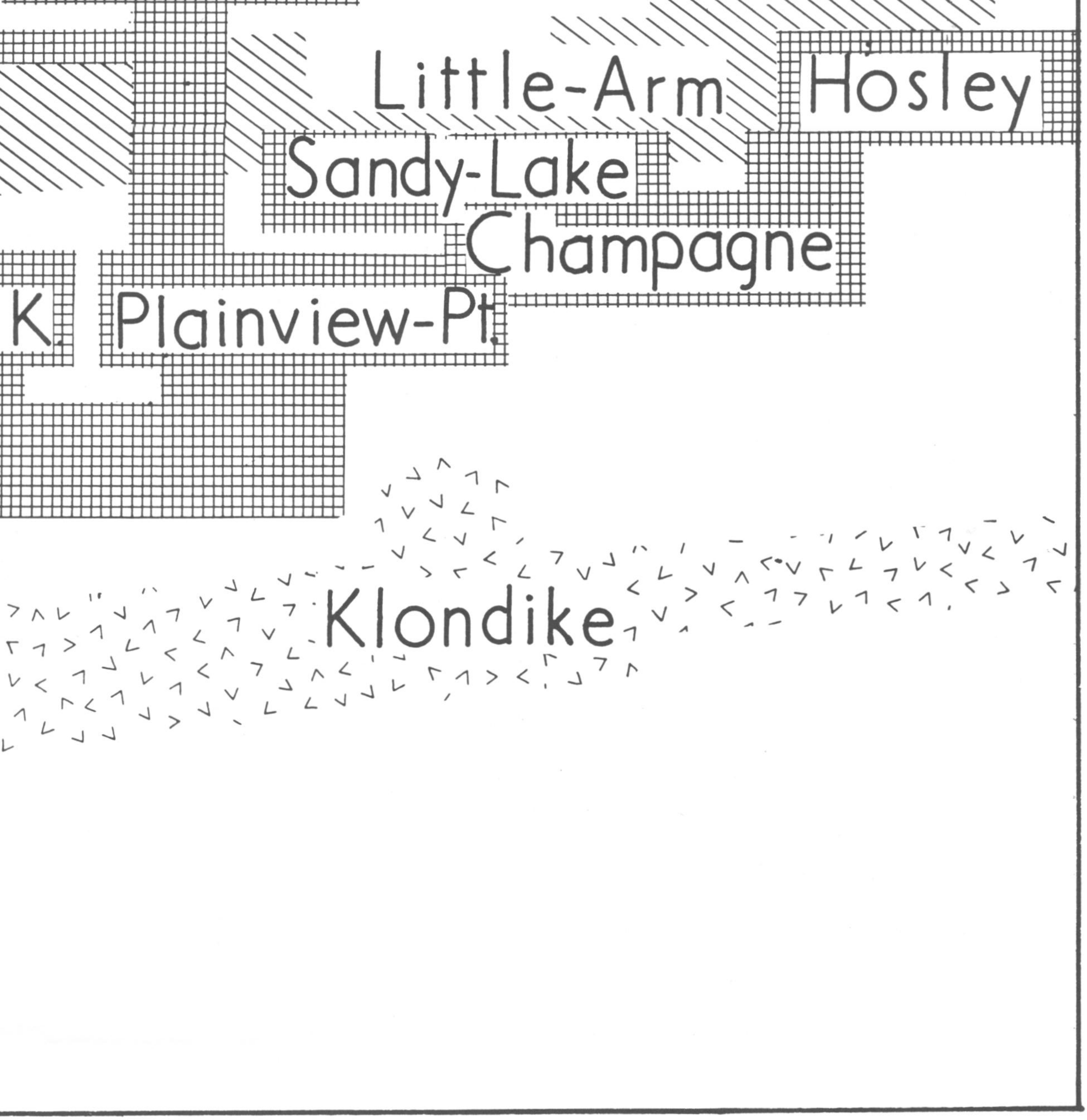
TIVE FRAMEV



EWORK OF NO

Franklin-T K.

NORTHERN PRE


Little-Arm
Hosley
Sandy-Lake
Champagne
K.
Plainview-Pt.
Klondike

REHISTORY

In 1954 and 1955, Collins (1956a, b, 1957a, b, 1958) excavated, on Southampton and Walrus Islands, in four Dorset sites. In one of these, the T-1 site, he uncovered several previously unknown artifact types and very distinctive variants of the Dorset specimens usually found. Carbon-14 dates (Rainey and Ralph, 1959) placed the T-1 occupation in the latter 600 years of the first millenium B.C. Collins described this material under the term "Proto-Dorset" culture, and so added another rung to the chronological ladder. He summarized his view of Dorset origins in referring to the Independence I culture (Knuth, 1958), Sarqaq, and North Knife River (Giddings, 1956) by saying these "might be called Pre-Dorset in the sense they represent earlier stages from which the recognizable Dorset pattern eventually emerged." (page 76, 1956a). Collins also stressed that the Pre-Dorset cultures of the Eastern Arctic have their origins in the still earlier cultures of Alaska and distant kinship to the Eurasian Mesolithic.

In these same papers, and following de Laguna's suggestion of 1947, Collins supported the idea that the Sadlermiut, the indigenous anomalous "tribe" of Southampton Island Eskimoes who were wiped out by disease in 1902-03, had been a Thule-influenced vestige of the Dorset culture. This view of the Sadlermiut conflicts with the interpretation of Mathiassen who construed them (1927, Pt. I) as a Thule culture group that had developed, because of long isolation, a distinctive variant of Thule culture.

One of the most interesting assemblages in the northeast, in terms of possible Eastern Arctic relationships, is that from the Mattawan stratum in the Frank Bay site near North Bay in Ontario (Ridley, 1954). Ridley suggested that this assemblage had some parallels with the Dorset culture, presumably because the sample included triangular and side-notched chipped endblades, a poorly-developed micro-blade industry, concave side-scrapers, and stemmed end-scrapers. Since he suggests that the material is considerably earlier than the Eastern Archaic pattern, Ridley has divided it from the Dorset culture's earliest occurrance by considerably more than 2,000 years. Keeping this rather unsupported age estimate in mind, the lanceolate and contracting stem points of this assemblage might have been noted as suggestive parallels to the Sarqaq culture. However, a recent carbon date of 970 ± 300 B.C. for the Mattawan complex (Byers, 1959) allows a change of interpretation so that on ecological, chronological, as well as typological grounds, a promising case could be made to consider this Mattawan assemblage in part as an eastern echo of the Northwest Micro-Blade Tradition (MacNeish, 1954). This hypothesis for the Mattawan complex is, I think, quite similar to that suggested by Byers (1959, page 253).

In 1954 and 1957, Meldgaard worked on an extensive site complex at Alarnerk near Igloolik in northwest Foxe Basin. On a series of raised sea beaches he found Pre-Dorset occupations and a long sequence of Dorset occupations. The Alarnerk Pre-Dorset has a great deal in common with the Sarqaq culture of West Greenland. The Dorset sequence was divided into five periods, each with distinctive artifact types. Only a most preliminary statement of this material has been published to date (Meldgaard, 1955), but the Alarnerk site is undoubtedly of fundamental importance to Arctic prehistory. Especially distinctive was Meldgaard's earliest period Dorset for, unlike usual Dorset samples, it contained a high frequency of ground slate tools. His Dorset Period II seems to be quite comparable typologically to Collins' Proto-Dorset T-1 site (Meldgaard, personal communication). Meldgaard suggested that the Dorset culture originated in Arctic Canada partly from the Sarqaq culture and partly from prehistoric Indian cultures to the south (1955). Later, at the 1956 Philadelphia meeting of the International Congress of Anthropological and Ethnological Sciences, Meldgaard announced two very important, and I think valid, Carbon-14 dates for his material. One dated his earliest Pre-Dorset or Sarqaq stage at 1,750 ± 300 years B.C.; the other date places the most recent Dorset, Meldgaard's Period V at 1,350 A.D. ± 150 years. In the same address, Meldgaard concluded that the Dorset culture originated as a result of northward migration into the tundra by Indians of the Archaic pattern of the northeast Woodlands.

In 1956, Richard MacNeish published a summary of a culture sequence represented on the Engigstciak site near the Arctic coast of the Yukon Territory. This site gave a fairly complete sequence from the Denbigh Flint Complex to recent Eskimo culture. However, none of the phases in this sequence could be demonstrated as a direct ancestor of Dorset culture. The sequence did, however, show a phase possibly ancestral to Pre-Dorset Eastern Arctic cultures. This, the New Mountain phase, is dated by MacNeish to around 2,500 B.C. The absence of a distinct Dorset parent in MacNeish's work, and in the work of others working in the same general area, is generally unmentioned by those who would derive the Dorset culture from Alaska.

In 1956, J.L. Giddings reported on a distinctive early lithic assemblage from northeastern Manitoba. This material, termed the North Knife River complex, is Pre-Dorset in time and Giddings related it to the Denbigh Flint Complex of Alaska and the Sarqaq of the Eastern Arctic.

For seven years, from 1947, Eigil Knuth collected from old sites in extreme northeastern Greenland (1952, 1954, 1956, and 1958). In 1954 Knuth had concluded his material represented the oldest culture in the Eastern Arctic, but had dated it to about 500 A.D. But with further data, Knuth was able to separate his material and describe two very old occupations called by him the Independence I and Indenpendence II cultures (1958). Independence I gave a Carbon-14 date of about 1,880 B.C., while Independence II dated about 870 B.C. The older, albeit distinctive, has affinities to Sarqaq and the Denbigh Flint Complex. More than one writer has accepted Independence I as the earliest known occupation in the Eastern Arctic or Greenland. The younger, Independence II, is in Knuth's mind, Pre-Dorset and distinctive from Dorset. On the basis of the illustrated specimens I would prefer to class it as an early Dorset sample and a close relative of Collins' early Dorset material at T-1 and Meldgaard's early Dorset material from the Igloolik area.

At the Imaha site at Payne Bay, on the west coast of Ungava Bay during the 1957 season, I had the good fortune to find a human skeleton in what was almost certainly a Dorset culture context. That it was typically Eskimo both metrically and morphologically (Laughlin and Taylor, N.D.) was no surprise to those who had long argued against an Indian origin for Dorset culture. At Payne Lake in the Ungava interior, inland Dorset sites were found for the first time (Taylor, 1958a).

In 1958, two papers reported on field work in the Disko Bay region of West Greenland. Larsen and Meldgaard's joint paper gave the results of the 1953 excavations on the Sermermiut site where Dorset culture was stratified between Thule and Sarqaq culture layers, and where the layers were separated by sterile strata. They define the Pre-Dorset Sarqaq culture and relate it to an as yet unknown Alaskan stage that existed after the time of the Denbigh complex and prior to the time of the Ipiutak and its related Near Ipiutak culture. In Larsen and Rainey's reconstruction of Arctic prehistory (1948), Ipiutak represented the first Eskimo culture and the parent of all others. Larsen and Meldgaard see the North Knife River assemblage as Sarqaq's closest Eastern Arctic relative. The Dorset component at Sermermiut was compared with Collins' T-1 site of Southampton Island and approximately equated with Meldgaard's Dorset Period III in the Igloolik area. Summarizing the Greenland situation, Larsen and Meldgaard write "...that we must count on at least five independent immigrations beginning with Independence I, then Sarqaq, which is followed by Independence II and the West Greenland Dorset, 'classical' Dorset, and the Thule culture." (page 71). Without denying the distinctiveness and the sequence of the five cultures named, the word "must" demands a caveat for, on the basis of present data, these five postulated migrations are more likely probabilities than imperatives. For all except the first and last, there are the possibilities of diffusion to or from Greenland, and of migration from Greenland to be considered in explaining relationships. In passing, let it be noted that anthropologists writing on the Eastern Arctic and Greenland have so often explained similarities by migration, and so rarely mentioned diffusion that one might wonder if something, perhaps low temperatures, prohibited cultural flow by diffusion.

Later in 1958, Mathiassen reported on the 1955 excavations at Sermermiut. In this the Sarqaq occupation is dated from the seventh to ninth centuries B.C., and the Dorset stratum to the first century A.D. The latter is only slightly younger than the minimum age estimated for the typologically similar T-1 site. Mathiassen sides with the view that the Dorset culture originated not in Alaska, but in Arctic Canada, recalling that in a paper that appeared in 1936 he "...demonstrated similarities between Dorset and some old eastern Indian cultures, and that theory has since been amplified by other workers." (page 50). With Larsen and Meldgaard, he sees Sarqaq as a derivative of early cultures in the western Arctic, and with Larsen, he rejects a cultural relationship between Dorset and Sarqaq.

In 1959, Harp published the results of his 1955 excavations in the Dismal Lake area some 60 miles southwest of Coronation Gulf. Here we are concerned with Harp's Dismal-2 microlithic complex for it is this assemblage that shows eastern affinities. Harp notes that it "...represents something of the ancestry..." (page 242) of Giddings' North Knife River material, and considerable relationship with Sarqaq. He considered its relationship with Independence I and Dorset assemblages, notably T-1, to be much weaker. As earlier, Harp again rejected any significant Eskimo contribution to the northeastern Archaic.

Later in 1959, Rainey and Ralph published a considerable part of the University of Pennsylvania radiocarbon laboratory's results. It is axiomatic that Carbon-14 dates are to be treated with caution and the authors further note "...that antler dates are erroneously young and that the discrepancy increases with age." (page 367). Their Table I (page 366) suggests this discrepancy to be on the order of ten to twenty per cent. In repeating their dates, I have identified those derived from antler samples. Collins' T-1 Dorset site, which very likely has two occupations gave five dates ranging from 675 B.C. to 103 B.C.; Meldgaard's Dorset, Stage I, gave two dates: 446 B.C. (antler) and 952 B.C. Meldgaard's Pre-Dorset culture at Alarnerk, near Igloolik, has two stages. The earlier stage produced dates of 2,000 B.C., 1948 B.C., 1602 B.C. (antler) and 940 B.C. (antler). The later Pre-Dorset stage gave a single date of 396 B.C. (antler). From the Yukon, MacNeish's New Mountain phase[3] has a presumably erroneous date of 1,250 B.C. (antler). Very likely it should date at least as early as the earliest Alarnerk date. From the Iyatayet site at Cape Denbigh, Carbon dates suggest that the Denbigh Flint Complex precedes 2,000 B.C. None, I am sure, will argue with such a statement. The middle layer, referred to as the Norton culture (previously termed an assemblage of the Near Ipiutak phase) gave three dates, by the solid carbon method, that average 403 B.C. One date for Norton culture, by the more accurate CO_2 method was 255 B.C. Giddings' Choris site (1957) which is typologically close to the Norton culture gave dates of 677 B.C., 688 B.C., and 286 B.C. These latter dates are included in view of Larsen and Meldgaard's (1958) recent view that the Sarqaq culture stemmed from an as yet unknown Alaskan stage that existed after the time of the Denbigh Flint complex but prior to Near Ipiutak or Norton culture.

At the risk of doing yet greater violence to the interpretations of Arctic archaeologists, the results of work bearing on the Dorset problem should be summarized. The Dorset culture was an Eskimo culture that spread over most of the eastern Arctic and Greenland. It outran the tundra to reach Newfoundland. Its sites are found abundantly on ocean shore locations but rarely in the interior, although this last may well be a result of inadequate searching in the Eastern Arctic interior. Temporally it is post-Sarqaq and for the most part Pre-Thule. It may have persisted in a heavily Thule-influenced form until 1902 in the form of Sadlermiut culture. It likely began about 1,000 B.C. and lasted until about 1,350 A.D. as a distinct entity in some regions. It declined after the arrival of the Thule culture from north Alaska and was replaced by that culture. Most anthropologists hold, and recent, albeit scant, concrete evidence suggests that the Dorset culture people were physically Eskimo. Dogs, the dog-pulled sled, ceramics, and the bow drill were unknown. The Dorset people made a wide range of spears, lances, harpoons, knives, scrapers and adzes utilizing chipped or ground stone blades including side blades. The microlithic tradition is a prominent component of the culture. The burin tradition seems to have been poorly developed but rubbed chert burin-like tools are typical. Soapstone lamps and pots of a variety of forms have been found. They had hand sleds, tents, and stone-sod houses, both semi-subterranean and surface. These houses were generally rectangular. The Dorset culture included a distinctive small-scale art. The people were semi-nomadic, using seasonal camps and practised a hunting economy. Available data suggest that sea-mammal hunting was the chief aspect of the food quest, although baleen whales do not seem to have been taken. Fish, land mammals, and birds were also exploited. The abundance of sewing needles in many Dorset samples leads to the suggestion of tailored fur clothing. Artifactual material from Dorset sites shows sequential change through time and these changes are currently being analyzed. The nature of the demise of the Dorset culture has not been determined and there are remarkably scant data bearing on this important problem. Nothing is known of the language spoken by the Dorset culture population and information on this point will be gained only slowly. However, L.L. Hammerich (1958), the Danish linguist, has been kind enough to hint with glottochronological support (notably Swadesh, 1952) — in a direction that appeals to me — that the Dorset culture people did indeed speak an Eskimoan language. There is another matter that, despite repeated rejection, bobs to the surface of discussions with suspicious persistence. That matter, of course, is the problem of cultural relationship between the Dorset culture and certain Archaic Pattern manifestations of the northern Woodlands. With Jenness' view (1940) that Dorset extends to 1,000 B.C. at last finding support in Carbon dates, Ritchie's negation (1951) of Dorset-Laurentian relationship has become debatable again.

There has long been, and still is, a fundamental dichotomy of views on the nature and origin of Dorset culture. This dichotomy hinges on the problem of Dorset-Archaic Pattern relationship. One hypothesis states that Dorset is basically an Indian entity that adapted to the tundra and Arctic coast, became "Eskimo-ized", after migration northward from the taiga. Proponents of this view generally see the Great Lakes Basin and St. Lawrence River Valley as the geographic source and the Laurentian Aspect of the Archaic Pattern as the cultural source. It has also been suggested that Dorset was one parent of the Laurentian Aspect. The second hypothesis sees Dorset as an Eskimo phenomenon with its home in the Alaskan, or at least, western Arctic cultures that existed prior to the time of Okvik, that is, before about 500 B.C. Exponents of this second view usually grant some minor Indian influence on Dorset culture.

So long as the Dorset culture was the oldest-known occupation in the Eastern Arctic, it was inevitable that anthropologists searched for a place of origin elsewhere and pondered routes of migration. Recognizing the artifactual content of Dorset assemblages, and the limited data of Arctic archaeology, it is not surprising that these searchings led to Alaska and northeastern North America. Both areas had produced an archaeological literature and that literature contained at least some material comparable to Dorset artifacts. However, from 1952 on, evidence has accumulated to show that Pre-Dorset cultures like the Sarqaq, had occupied the Eastern Arctic and Greenland. Even from the small samples available it was immediately evident that Sarqaq was related to the Denbigh and Denbigh-like materials that have been reported from Alaska and the Yukon Territory. There was also, I think, considerable ground to suggest a Sarqaq-Dorset affinity. With the recent additions to the literature on the Pre-Dorset cultures this latter possibility has become a probability. Nevertheless, some archaeologists working with Pre-Dorset samples have chosen to stress the differences between their materials and Dorset materials. Consequently they have rejected the possibility of genetic relationship between the two. While the two cultures are quite distinctive, there is, I think, sufficient evidence to make a strong case for the Pre-Dorset as a parent of Dorset. So far as I know, this has only been suggested hitherto by Harp (1952), Meldgaard (1955), and Collins (1956a) who as quoted above, referring to Independence I, Sarqaq of Disko Bay, and North Knife River, wrote "...all of which might be called Pre-Dorset in the sense they represent earlier stages from which the recognizable Dorset pattern eventually emerged." (page 76).

When two archaeological interpretations of a body of data are long held, well-argued, and conflicting, it occasionally happens that both have merit. Such may be the case for the two traditional views on the Dorset problem. The third and most recent view, of Dorset development in situ, may resolve the problem and reveal the merits of the earlier interpretations.

If prediction is admissable at this point, I would like to predict that in the near future all but the most obdurate will come to agree that the Dorset culture is Eskimo, that the language was probably of the Eskaleut Stock (Swadesh, 1954), and that its people were physically Eskimo. For Dorset origins, I think there will soon be sufficient data to demonstrate that the Dorset culture developed from a Pre-Dorset base with continuing influence from the western Arctic and noticeable, albeit superficial, influence from the Archaic Pattern Indian populations to the south. There may well have been a contribution to the Dorset inventory from Indian cultures east of Great Slave Lake and Lake Athabasca. Most promising in this context is the Lockhart River Complex whose side-notched points, prismatic blades, and end-scrapers remind one of Dorset types. MacNeish has estimated that the Lockhart River Complex existed sometime between 1,000 and 4,000 years ago (p. 33, 1951). The Dorset culture is strongly related to what Irving has termed the Arctic Small Tool tradition (1957). Synthesizing the interpretations of several others and speculating freely, one may discern a west-to-east geographic and chronological continuance of that tradition arriving from Siberia as the Denbigh Lithic Complex (circa 3,500 to 2,500 B.C.) and known from Cape Denbigh and the Brooks Range sites; then to the New Mountain phase (circa 2,500 B.C.) of the northern Yukon Territory; from that to the Dismal-2 microlithic assemblage on the western edge of the Canadian Barrenlands; then over a large unknown gap to the several Pre-Dorset components, beginning about 2,000 B.C. such as Independence I, North Knife River, the Pre-Dorset occupations of the Alarnerk area and the Paleo-Eskimo of West Greenland. I would suggest that it was out of these Pre-Dorset occupations that the Dorset culture grew, beginning about 1,000 B.C. The Dorset culture's affinity to the Arctic Small Tool tradition is seen in its Arctic locale, its Arctic economy, its well-developed microlithic industry, and its use, however diminished, of burins. It is distinguished from the members of that tradition by its pronounced Eskimo stamp and by its duration into the second millenium A.D. The Eskimo stamp of the Dorset culture in this hypothesis is a result of the postulated continuing influence from another major tradition that was developing in the western Arctic. It is called here Inuk Tradition[3] to avoid the theoretical implications of Larsen and Rainey's term "Neo-Eskimo" (1948). Its membership included the Okvik, Old Bering Sea, Punuk, Birnirk, and Thule cultures. The Inuk Tradition's contribution to the Dorset culture likely began as influence from such pre-Okvik Alaskan occupations as Choris, Norton, and Near Ipuitak. According to our hypothesis, these were incipient phases of the Inuk Tradition. Since the Dorset culture has extensive affinities with both the Arctic Small Tool Tradition and the subsequent Inuk Tradition, and since the Dorset way of life was distinctive, long-lasting in time, and widely spread geographically, it is concluded here that it should be considered as a distinct tradition, the "Dorset Tradition"[4], whose variant cultural forms we are only beginning to discern. The present evidence indicates a considerable division between Pre-Dorset and Dorset cultures but this likely is a fallacious impression resulting from inadequate site samples. As more sites are reported through the period 1,500 B.C. to 0 A.D., their samples might well demonstrate a more gradual change from Pre-Dorset to Dorset than that indicated by the present evidence. Canadian Arctic archaeology is plagued by a shortage of data that makes interpretation, even specultion, a risk. Such a shortage has always been the lot of archaeologists dealing with the vast Arctic area and it does much to explain the many divergent interpretations reviewed in this summary.

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[3] Rainey and Ralph (page 371-2) list it as "Early Mountain phase" of site N. VK-1. Its correct designation is as given above and the site number is NiVk-1, which is the Engigstciak site as they have identified it.

[4] R.S. MacNeish and I have found this to be an enlightening division in our current research on Arctic archaeology. For further discussion on it the reader is referred to MacNeish's paper in this volume.

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A Reappraisal of Recent Serological, Genetic and Morphological Research on the Taxonomy of the Races of Africa and Asia

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During the past fifteen years a large number of serological-genetical studies have been carried out among many populations in Africa and Asia. Many interesting facts on the distribution of ABO, MNS, Lutheran, Henshaw, Rh, Duffy and Kelly blood groups have been discovered. It has been maintained by many researchers in this field that these distributions are of great physical anthropological significance. During this fifteen-year period, a remarkable volume of reports has been published on the distributions of the various abnormal haemoglobins such as sickle cell trait, haemoglobin C, D, etc., thalassaemia, and Cooley's anaemia. It has also been asserted that the gene frequency distributions of these abnormal haemoglobins are of singular anthropological importance. It is scientifically unimportant that the data involved here are mostly blood factors; the important relevant factor in this regard is in the field of human genetics. The fundamental split in physical anthropological methodology which this blood group trend indicates, is an interest and concern with man's genotype rather than phenotype.

The proponents of the genotypical-serological school have stated eloquently and repeatedly that phenotypic studies have already been done during the past sixty years in sufficient number to warrant their termination. On the other hand, these investigators maintain that genotypical studies have not been pursued with the same zeal so that, as a result, there are large gaps in the knowledge of man's genetic nature. To these scientists phenotype, or the apparent appearance of a biological specimen, gives very little information as to the true nature of that specimen, since its appearance is modified by its environment. What is considered crucial from their point of view is the genetic potential that exists within this specimen and the mathematical reconstruction of the frequencies of the various genetic potentials, if possible, in the population whence the specimen comes. The units responsible for these various genetic potentials are called genes and can be visualized as so many pearls on a string, known as chromosomes.

The proponents of the more recent genetical method hold the view that the frequency distribution of various blood group genes within a population are of greater taxonomic value in distinguishing populations from each other, than the so-called descriptive, phenotypic, morphological, morphometric traits of the earlier scientists.

Another criticism made of the phenotypic school is that it was too frequently concerned with the products of evolution rather than the process of evolution (Washburn, 1951). This, they maintain, is now to be remedied, since the effects of natural selection and other important evolutionary processes can be grappled with effectively with the techniques of population genetics. It might be more interesting if they be allowed to speak for themselves in these matters. During the past ten years one of the most important figures representing the blood group cause is Professor Boyd, who on more than one occasion has been very outspoken on his theoretical interests in physical anthropology. For example in 1951 at the Seventh Annual Founders Lecture, Medico-Chirurgical Society of the District of Columbia, reprinted in the Yearbook of Physical Anthropology, 1952, he stated

> Races are evolved by the forces of evolution acting upon the genes in a population. Races may be distinguished by the varying distribution of various genes in the populations.
>
> There seems to be four main agencies which act to modify the gene frequencies of human populations, and thus evolve races different from those with which we started.
>
> One is the *mixture* of population which may be homogeneous or may possess different frequencies of certain genes. The arithmetic of

gene mixture has been discussed in a number of admirable papers and books. In some cases, where one population is expanding more rapidly than the other, there may exist an immigration pressure tending to push a new gene into a population that does not have it, or has relatively little of it.

Mixtures of populations which until recently were thought to differ so radically as to consistute a different species may have occurred in the past, and there is considerable reason to think that mixture between Neanderthal man and Homo sapiens occurred before the total disappearance of Neanderthal man. The present-day Homo sapiens may well possess a number of genes acquired from Neanderthaloid ancestors.

The second agency which may change the gene frequency in a population is *mutation,* which is usually the production of a new gene by the sudden change of an old gene. For most genes, this occurs with a fairly regular frequency, but it is never possible to predict in what individual it will occur. It is of course known that agencies such as radiation, colchicine, the nitrogen mustards, and certain carcinogens, may cause mutations to occur at a higher rate than is observed in natural populations.

The third agency which may alter the frequency of a gene is *natural selection,* the importance of which was brought out fully for the first time by Darwin in his great book on the *Origin of Species.* At one time not too many years ago, physical anthropologists hoped to be able to classify mankind on the basis of non-adaptive characteristics, that is, characters which were not subject to the action of natural selection. It now seems very unlikely that any such characters exist. The mere fact that numbers of individuals possess a character is an indication that it is, at least under certain circumstances, an advantage.

The fourth agency which alters gene frequency in populations is the phenomenon of genetic or random drift, or as Sewall Wright, who has studied it so extensively calls it, *random variation.* Briefly, this consists of the fact that in a small population the new generation may purely by accident not possess one or more of the genes possessed by the parent generation, or may contain them in diminished frequency. As long as the frequency does not reach zero or one (that is, one of the genes is not completely lost), random variation may again restore the same gene frequencies already present, but once the gene frequency reaches zero, something irreversible has happened: that gene has been lost forever to that population, unless it be re-introduced by mutation, or by migration and crossing. Wright has been able to show mathematically that this agency is much more important in evolution than would have been suspected from armchair speculations.

Exactly how many races we distinguish will be up to us. Taxomony is the work of man, and not of nature, and it is entirely a

> matter of expediency whether within a given species we distinguish one race, six races, or thirty races; and whether or not any of these thirty races agree with any of the six may not prove that either of these methods is wrong or that either has much advantage over the other.
>
> Having long ben interested in genetics as a result of work on the blood groups and their mode of inheritance, I have proposed a tentative classification of races based on gene frequencies. It differs but little from that proposed somewhat earlier by A.S. Wiener. Since both classifications make use chiefly of blood grouping characteristics, however, it is not surprising that they should be somewhat similar.

Spuhler's point of view is somewhat less extreme in regard to these matters. He states that

> Differences in genetic characters between human breeding populations may be catalogued in terms of differences in gene frequencies. Statistical models are available to explain change in gene frequency. In theory the process of race formation can be reduced to a single concept — change in gene frequency. Theoretically, then, population genetics provides a method for studying the relationship between, and the history of, contemporary or recently extinct populations. Empirical application of this method presupposes knowledge of a number of characters controlled by simple genotype systems showing fairly constant expressivity and nearly complete penetrance in the range of environments characteristic for human societies. Serology furnishes the best candidates for characters of this sort. Because of limitations in the application of serological techniques (for example to the vast body of data already available from the world's dissecting rooms, and from field observations by physical anthropologists), and the security provided by ever more characters employed in analysis, genetic knowledge on other widely-distributed characters, especially normal morphological variations, is required to supplement, and in some cases to substitute for, the serological data. (Spuhler, 1950)

From the above it is quite clear that Boyd's and his school's chief scientific interest is the observation of the gene frequency distribution of monogenetic-serological features. By monogenetic we mean that the biological result is determined by a single pair of alleles. This means that the mode of inheritance is that of a simple Mendelian ratio 1:2:1. Professor Boyd does not tackle the problem of polygenic features since, he states, their mode of inheritance is as yet not understood.

The biological characteristics in humans which have a monogenetic mode of inheritance are the various blood groups, the various abnormal haemoglobins, and several inherited pathologies such as brachydactylism, lobster claw, chondrodystrophy, etc. It seems rather strange that the choice of characteristics to distinguish between races is made on the basis of whether or not they have a simple mode of inheritance so that their gene frequency can be computed. Nowhere has Professor Boyd posed the question as to whether or not the characteristics he is chosing are taxonomically relevant. To put it in one phrase: *monogenic serological factors do not ispo facto have racial-taxonomic value.* Unfortunately for Professor Boyd it is the polygenic features such as skin colour, hair texture, nose shape, lip thickness, which have the greatest taxonomic value. And why is it necessary to understand the mechanism of inheritance if one is concerned with the question of distinguishing between the various racial groups which, Professor Boyd states, is one of the chief aims of physical anthropology?

There are several other inherent inconsistencies in his system. In his book *Genetics and the Races of Man* (1950), he reconstructs the racial history of Africa, Asia, Australia and the New World on the basis of the gene frequency distribution of the various blood groups. However, as we have seen in the above quotation, he points out that these gene frequencies are subject to change due to hybridization, mutation, natural selection and random variation of genetic drift. All geneticists agree that in small populations, the last-mentioned factor, namely genetic drift, can be the cause of rather rapid changes in gene frequencies. If this be true, how then can Professor Boyd construct the racial history of the world on the basis of gene frequencies which are known to be variable due to the above-mentioned processes causing change? This is the reason 'par excellence' why the serological genetical system in racial taxonomy is even more inconsistent and logically untenable than the phenotypical methodology. Weidenreich as long ago as 1946 and Hooton in 1956 very clearly expressed their dissatisfaction with Boyd's views. These criticisms have been so flagrantly ignored that the relatively complete citation below is justified. In Chapter IV of his book *Apes, Giants and Man,* Weidenreich states,

> Since there is no correlation between racial characters (such as have been used by the anthropologists) and blood qualities, and in

addition, since an anthropological race is not determinable by one such character alone, the anthropologists were right in neglecting the serological criteria. All the more so since their system is based on the principle of the geographical restriction of the racial characters. However, the distribution of the blood qualities, as it is today and as it is supposed to have been in the past, gives no convincing evidence of a strictly geographical distribution, despite all claims of the serologists to the contrary. Charts have been constructed by the serologists to show the routes along which blood qualities have spread over the globe. Of course, blood qualities must have travelled, for man, their bearer, has travelled; but in which special race-disguises the qualities were concealed, it is impossible to determine, simply because there is no strict correlation between race and blood group, as stressed by the serologists themselves. Boyd, who is fully aware of these difficulties, confines the real anthropological worth of the blood qualities to their fitness as a guide through the maze of racial history, particularly in regard to early mankind. But even with this reservation, morphological features, as they have been preserved in the skeleton, are certainly more elucidative for this purpose.

Relation between Blood Quality and Hair Color,
Tested on 1,152 Individuals
(In Percentage)

Hair Color	A	B	AB	O
Blond	44.2	8.6	2.3	44.9
Brown	42.0	9.6	3.0	45.4
Black	44.5	7.8	3.6	44.1

It has been shown that the apes — particularly the anthropoids — possess, in principle, the same blood groups as man. This being so, the quality of the blood must be of very ancient character, and therefore, a very old heritage. The distribution through mankind may have occurred long before the morphological characters chosen for the anthropological classification of today were developed.

Should, however, anthropologists yield to the demands of the seroanthropologists and accept the blood qualities as essential criteria for the classification of modern mankind, there is no other way of incorporating them in the anthropological system than to subdivide each of the acknowledged racial groups according to the special blood qualities recognizable in them. But if this were done, a new difficulty would arise. In late years, additional blood qualities have been discovered. The group A itself has been subdivided into four subgroups; and new groups have been added to the already existing list, namely

Frequencies of the Blood Groups: O, A, B, and AB

Populations	O	A	B	AB
Eskimos	80.7	12.9	2.4	4.0
Argentines	59.0	28.0	18.0	2.0
Bantu Negroes	53.2	18.6	24.5	3.6
Giliaks	50.0	27.4	14.5	8.0
Germans (Eifel)	46.5	44.8	5.2	3.5
Germans (Baden)	38.1	48.1	10.9	2.9
Dravidians	24.3	27.5	36.8	11.4
Egyptians	24.0	32.0	30.0	14.0
Koreans	17.9	36.6	33.7	12.5
North American Indians	91.3	7.7	1.0	0.0
Australians	55.0	38.0	5.9	1.1
Negroes (Congo)	45.6	22.2	24.2	8.0
Swedes	43.0	42.0	8.0	7.0
Ainu	11.6	29.3	34.1	25.0

the groups M_1 and M_2, N_1 and N_2 and Rh, etc. Not less than 2,560 kinds of human blood are now serologically distinguishable. It is not known, so far, whether and in what frequency all these groups are distributed over the populations of the earth. Should all of them occur in all thirty-eight anthropologically distinct races and subraces, we would have 92,780 different racial groups — not including the "constitutional" types and their combination with the "anthropological" and serological ones. The main qualities of all these groups would agree with the demands of geneticists and serologists.

Hooton maintains in his book *Up From the Ape*

In spite of the accurate knowledge we possess of the genetics of various blood group substances, their control by very few genes, their allegedly "non-adaptive" character, their permanence in the individual, and their supposed independence of environment, we can hardly discard the ordinary anthropological criteria of race in favor of serology. The reconstructions of primitive races and prehistoric migrations that are based upon serology (at least as respects the standard blood groups) are even more speculative and implausible than those that result from the study of skulls and bones. And, as regards contemporary man, I am afraid that scientists, together with the entire lay population of *Homo sapiens,* will persist in distinguishing Negroes, Mongoloids, and White by observing their visible and distinctive morphological combinations rather than by depending upon serological tests. We shall continue to call a gorilla a gorilla and a chimpanzee a chimpanzee, even if they belong to the same blood group.

Similar Blood Group Frequencies in Physically Diverse Peoples

People	Place	Gene Frequency p	q	r
Eskimo	Labrador, Baffin Land	.318	0	.682
Aborigines	W. Australia	.306	0	.694
Pygmies	Ituri River, Belgian Congo	.227	.219	.554
Russians	Perm	.231	.203	.573
Iranians	Persia	.237	.235	.553
Zulu	South Africa	.157	.122	.730
Berbers	Tunis	.159	.127	.707
Whites	Agnew, Cal.	.172	.105	.733
Melanesians	N.E. Pantari Is.	.173	.108	.723
Buriats	Mongolia	.158	.264	.578
Bambarra	W. Sudan	.180	.225	.598
Hindus	United Provinces	.190	.272	.550
Manchu	Mukden	.198	.249	.556
Orochi	Saghalien, Japan	.190	.270	.552
Gypsies	Puspokladany, Hungary	.178	.265	.573
Armenians	Tiflis	.334	.112	.559
Micronesians	Kusaie, Carolines	.300	.125	.585
Egyptians	Alexandria	.338	.116	.553

In November 1955, Professor Boyd recanted his views at the American Anthropological Association meetings in Boston. He stated that he was wrong, but not for the reasons that have been mentioned above. The reasons he gave were that the ABO blood groups which he had thought were non-adaptive features, in other words, unmodifiable by the environment, were found to be correlated with certain diseases. Buettner-Janusch makes the following interesting remarks on this subject.

> Despite the widespread belief among anthropologists and others in the usefulness of nonadaptive traits and neutral genes in classifying human populations and working out the course of human evolution, it has been difficult to defend the concept of a neutral gene since 1930 when Fisher published "The Genetical Theory of Natural Selection." Boyd (1953) pointed out what difficulties this point of view made for physical anthropology. That the associations noted above between blood groups and disease may be due to ethnic stratifi-

cation in the samples tested is, of course, a necessary caution when one is working with large samples drawn from populations of mixed origin. This point can be, and has been, overemphasized (Wiener, 1943; Wiener and Wexler, 1956). In the study by Clarke and his associates already cited, some care was taken to show that in the association between duodenal ulcers and group O the stratification argument cannot possibly hold, and similar considerations apply to the cases of the relation of group A to stomach carcinoma, diabetes mellitus, and pernicious anemia. It is worth noting in detail the three points Clarke makes. First, the original population susceptible to ulceration must have a group O frequency larger than 60 percent. No such group has been reported from the continent of Europe. Second, the association has been found in a number of European countries. Third, the association between blood type A and gastric carcinoma implies, according to the ethnic argument, a second stratification in Great Britain, and one which runs in an opposite direction to the geographical incidence of the disease. We should counter with the question, all right, where did the original ethnic differences in blood group frequencies come from? It is time to turn the question around in this way and search for the reasons for ethnic, population, and even village differences such as Ceppellini showed (1955). (Buettner-Janusch, 1959)

Let us examine some of the serological data, gathered in Africa by qualified haemotologists for the purpose of explaining

The Distribution of Non-Metrical Features in Some Negro and Negroid Races of Africa

Race	Hair Texture	Hair Colour	Eye Colour	Skin Colour
Hamitomorph	curly to frizzly	black	light brown to dark brown	light brown to dark brown
Nilohamitomorph	frizzly	black	dark brown	dark brown to black
Bantomorph	frizzly	black	light brown to dark brown	light brown to black
Nilotomorph	frizzly	black	dark brown	black
Congomorph	frizzly	black	dark brown	light brown to black
East African Mulattomorph	curly to frizzly	black	light brown to dark brown	light brown to dark brown
Nigeromorph	frizzly	black	dark brown	dark brown to black

(East and Oschinsky, 1958)

The Distribution of Metrical Features in Some Negro and Negroid Races of Africa

Race	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hamitomorph	tall	small	long	long	medium	Dolicho-cephalic	Leptorrhine to mesorrhine	thick
Nilohamitomorph	tall	small	long	long	high	Dolicho-cephalic	Meso- to platyrrhine	thick
Bantomorph	medium	medium	medium	medium	medium	Dolicho- to mesocephalic	Platyrrhine	thick
Nilotomorph	tall	small	medium to long	long	high	Dolicho- to mesocephalic	Platyrrhine	very thick
Congomorph	medium to short	large	short	short	low	Meso- to bra-chycephalic	Platyrrhine	thick
East African Mulattomorph	variable	large	short to medium	short to medium	medium	Meso- to bra-chycephalic	Mesorrhine	medium
Nigeromorph	medium	medium	—	—	high	Mesocephalic	Platyrrhine	thick

Key: (1) Stature; (2) Sitting Height relative to Stature; (3) Total Upper Extremity Length relative to Sitting Trunk Height; (4) Total Lower Extremity Length relative to Sitting Trunk Height; (5) Forearm Length relative to Upper Arm Length; (6) Head Breadth relative to Head Length; (7) Nose Breadth relative to Nose Height; (8) Lip Thickness.

(East and Oschinsky, 1958)

some rather remarkable anthropo-geographical problems, centering around the origin and the differentiation of various Negro sub-races. The Negro stock, or 'la grande race négroïde' exhibits many interesting local geographical varieties, as is also the case among caucasoids and mongoloids. [see tables above]

The tall, lean, thick-lipped, very broad-nosed Nilotomorphs with their coal-black skins and long extremities, represent one extreme in the typological range of variation in contrast to the little pygmies, characterized by extremely short stature, short extremities, thin lips, light-brown skin and peppercorn hair distribution. Between these two typological extremes in Negro variation are the Hamitomorphs, Bantomorphs, etc., whose morphological features can be read in the above table. Some of the old problems which morphological-morphometrical methods attempted to solve and which serology has taken on, are the following:

1) Are the Nilotomorphs and the Congomorphs the primordial Negroes since typologically they have the blackest skin, the thickest lips, the broadest noses, and the highest degree of prognathism?
2) Are the Hamitomorphs and the Bantomorphs intermediate types in a Negro evolutionary sequence, or are they ancient hybrids between the Caucasoids and Negroids?
3) Are the recent Negro-White hybrids or Mulattomorphs found in Zanzibar and on the East African coast similar biologically to the ancient hybrids?

Among the most important of the recent serological studies in this field is that of Lehmann and Raper. They found an inverse relationship between sickle cell trait and Hamitomorphic biological admixture, and drew the following conclusions from their data

> ...We wish to do no more than indicate how our results bear on physical anthropology. The incidence of the sickle-cell trait is uniformly low in the pastoral, Hamitic-tongued tribes, with the single exception of the Teso.
>
> The Nilotic tribes... are remarkably homogeneous with regard to sicklaemia, excepting only the Madi...
>
> Unlike the two previous groups, a wide variation is seen among the Bantu tribes. The incidence of sicklaemia appears to be inversely

proportional to the contact the various tribes have had with their most recent Hamitic invaders. Thus the trait is least common among the Bairu, who have lived for generations as helots to the ruling class of Hamitic conquerors, the Bahima, on the best pastoral land in Uganda. Next come the Banyaruanda, Banyora and Batoro, whose traditions and customs testify to prolonged contact with the Bahima, and whose aristocracy possesses Hamitic features. The contact of the Baganda, Bakonjo and Barundi with Hamitic peoples in recent times has probably been less direct and made by way of their neighbours, the Banyoro, Batoro and Banyaruanda.

Distribution of Sicklaemia in Uganda

Language Group	Tribe	Number Examined	% of Sicklaemia
	Bahima*	166	2.4
HAMITIC	Sebie	124	0.8
	Suk	128	3.9
	Karamojong	156	3.2
	Teso	416	17.8
	Lango	278	27
	Acholi	141	27
	Jaluo (Luo)	130	28
NILOTIC	Lugbara (Lubgwara)	120	21
	Kakwa	101	25
	Alur	114	25
	Jonam	109	26
	Madi	109	3
	Bairu (Bahiru)	139	2
	Banyaruanda (Bahutu)	496	8
	Banyoro	91	12
	Batoro	120	12.5
	Baganda	740	19
BANTU	Bakonjo	102	18
	Barundi	108	19
	Bakenyi	88	26
	Basoga	241	29
	Bagishu	207	30
	Baamba	140	45

* Although the Bahima now speak the Bantu language of their subjects, they have — for obvious reasons — been included in the Hamitic group.

The Basoga and Bagishu, two closely-related tribes, live remote from the track of the Hamitic peoples. The South Basoga to whom the accompanying table refers, live in the swampy country between Lakes Victoria and Kyoga. The Bagishu live on the western slopes of Mount Elgon. There is no history of direct contact with Hamitic tribes, though tradition records hostile relations with the Jaluo of the neighbouring plains, themselves a tribe of high sickle-cell incidence.

The Baamba are a secluded tribe living west of the Ruwenzori Range. They are composed of the Babullibulli and Bamwezi, who differ in language and in some physical characteristics, but show the same incidence of sicklaemia. They claim to be autochthonous, and of all the tribes we have investigated these pigmoids are furthest removed from the Bahima in physical appearance." (Lehmann and Raper, 1949)

Allison *et al.* make the following claims on the basis of the gene frequency distributions of the various blood group systems

Collected Gene Frequencies: Kenya

	Kikuyu	Luo	Mixed Series: Luo & Kikuyu	Massai
O	0.6882	0.6881	0.7145	0.7095
A_1	0.1113	0.1127	0.1077	0.1148 (A_1 + A_2)
A_2	0.0661	0.0362	0.0435	
B	0.1343	0.1629	0.1342	0.1756
MS	0.2088	0.1010	0.1642	
Ms	0.3799	0.4068	0.3619	
NS	0.0841	0.1285	0.1124	
Ns	0.3272	0.3637	0.3614	
R_1 CDe	0.0654	—	0.0730	
R_2 cDE	0.0645	—	0.0992	
R_o cDe	0.6159	0.8174	0.5550	
$R^u{}_o$ cD^ue	0.0942	0.0474	0.0399	
r cde	0.1287	0.0415	0.2045	
R′ Cde	0.313	0.0469	0.0142	
R″ cdE	—	0.0469	0.0142	

> The blood groups of the Amba (Baamba) therefore do not show such a difference from those of their neighbours as was expected when their study was undertaken. The difference between them and the Kikuyu (Akikuyu) and Luo is not appreciably greater than the difference between the latter two tribes.
>
> Taking all blood group systems into consideration, it is surprising how slight are the differences between the Nilotic Luo and the Eastern Bantu Kikuyu. From the present study and the results of Elsdon-Dew (1939) the tentative conclusion can be drawn that the East African native population (apart from the Massai and other Hamitic tribes) is on the whole fairly uniform. (Allison *et al.*, 1952)

The above quotation is an example of the kind of unwarranted generalization based upon a few characters which is all too frequently met with in the reports of some of the serologists. As the writer has already pointed out in 1954,

> The blood group distribution of 5,000 East African Bantomorphs (Elsdon-Dew, 1939) and 808 South African Bantomorphs (Pijper in Wiener, 1948) are extremely uniform. In East Africa among the various Negro races the situation is rather complex. The Hamitomorphic Bahima and the Bantomorphic Baganda have practically the same blood group gene distribution. The Bantomorphic Akikuyu of Kenya, when compared with the Baganda who are anthropometrically similar, have distributions which are more different than one would expect within groups belonging to the same race. As already has been mentioned above, the Nilotomorphic Luo and Lango have a rather similar blood group gene distribution to that of the Congomorphic Baamba.
>
> Since the intra-racial blood group distributions of the East African Bantomorphs seem to be heterogeneous and the inter-racial blood group distributions in many cases are homogenous, one has little choice but to conclude that the blood group data alone are insufficient as sole criteria of racial differences in East Africa.

and again in 1958,

> Distribution of the Percentage Sicklaemia and Blood Group Genes of the Various East African Racial Groups according to Lehmann and Raper (1949), Elsdon-Dew (1950), Allison *et al.* (1952), and Hiernaux (1954)

Racial group	Tribe	Sicklaemia	Blood Group Genes A	B	O
Hamitomorph	Bahima	2.4	11.3	7.1	81.3
	Batutsi	1.4	12.6	5.6	81.7
Bantomorph	Baganda	19.0	10.2	6.6	82.9
	Bahutu (Uganda)	8.0	—	—	—
	(Ruanda)	5.2	17.4	13.7	68.7
	(Urundi)	11.8	17.4	13.7	68.7
	Akukuyu	—	17.7	13.4	68.8
Nilotomorph	Luo	28.0	14.9	16.3	68.8
	Lango	27.0	16.3	17.1	64.1
Nilohamitomorph	Massai	—	11.5	17.6	71.0
	Teso	17.8	13.4	13.0	72.2
Congomorph	Baamba	45.0	18.1	12.8	69.1

Hiernaux (1954) found that the Rhesus-factor incidence in both the Hamitomorphic Batutsi and Bantomorphic Bahutu showed no significant difference. The same author also discovered that the incidence of sicklaemia in the Ruanda Bahutu was less than half of that of the metrically very similar Urundi Bahutu.

Rhesus Factor Incidence in the Batutsi and Bahutu Tribes

Factor	Batutsi	Bahutu
R.O	63.3	68.0
R.1	6.9	4.7
R.2	5.6	5.9
R′	2.2	1.8
r	21.7	19.3

On the basis of the above data it is difficult to determine the various racial affinities of these tribal groups since sickle cell trait gene frequency distribution does not agree with ABO gene frequency distribution, which in turn does not agree with Rh gene distribution.

Lehmann has recently found a high incidence of sickle cell trait among the Veddoids of southern India. This has led to a change of point of view in regard to his previous reliance on sickle cell trait as a diagnostic feature in African physical anthropology. He now believes that the Rhesus gene Rh_0 (cDe) is more useful in solving problems of African racial affinities. He remarks the following,

> Unlike high Rh_0 frequency the sickle-cell trait is only patchily distributed in Africa. The highest incidences have been reported from East Africa where they varied from 30 to 45% in some Bantu tribes. Other Bantu tribes in East Africa have lower incidences of the trait and in Uganda this seems to go parallel with the degree of dilution by recent Hamitic-speaking invaders. If one considers the highest incidences only, one finds that they fall towards the west; at the coast they are 20 to 25%. They also decline the further one goes south and are about 10% in Northern Rhodesia, to become practically nil in South Africa. The Bushmen have no sickle-cells at all, although their Rh_0 frequency is high. Similarly, the 'yellow' Pygmies of Central Africa are virtually free of sickle-cells. Thus the trait is not a universal African feature and one may suggest that it entered the Continent with Veddian blood well after the African races had established themselves. (Lehmann, 1953)

There is much to quarrel with in this last statement. First, it has not been proven that the Bantu tribes of East Africa have been very much influenced by the Hamitomorphic Bahima and Batutsi. Second, the Pygmies of Central Africa are not yellow and some of them do have sickle cells. Third, a trait does not have to be universal to be diagnostic. In fact, no diagnostic trait is universal in any racial group or in any geographical area. Fourth, the fact that sickle cell trait is found among the Veddoids of southern India and southern Arabia does not necessarily indicate which way it diffused or that it diffused at all. Why could it not have arisen in both places as mutations? And finally, is it anthropologically enlightening to lump together groups as distinct as Bushmen, Pygmies and Nilotomorphs simply because they agree in a single character? The Bushmen have a high percentage of mongoloid eyefold but no scientific morphologist would claim on the basis of this single character that they are related to the Mongoloids of eastern Asia.

Allison (1954) has pointed out that there is selective advantage in being heterozygous for S-haemoglobin in malarious areas. The researches of Hiernaux and others indicate quite clearly that this cannot be so. The Batutsi, the Bahutu, and the Batwa all inhabit the same malarious area and yet have very different sickle trait gene frequencies.

Neel (1958) and Livingstone (1957) advanced the view that haemoglobin C and haemoglobin S (Sickle cell) are not found among certain "paleo-negroids" of West Africa. The term "paleo-negroid" is very inaccurate since it implies that the typologically negroid groups in question are automatically the descendants of the primordial African Negro. There is no evidence for this whatsoever. If a group typologically embodies a cluster of morphological Negro variations which are at one end of the range of variation, it does not necessarily follow that this group is the descendant of the ancestors of the total Negro racial stock. By the same logic we might advance the view that the primordial Negro ancestor was typologically intermediate between Caucasoids and Negroids, or Mongoloids and Negroids. In my case the absence of S among these particular "Paleo-negroids" proves nothing concerning its antiquity or recentness in negro populations.

Pales and Linhard (1952) in their investigations of sicklaemia in French West Africa have found that the Hamitomorphic Peuls have a higher rate of sicklaemia (11%) than a group of neighbouring Sudanomorphic tribes who have percentages ranging from 6 to 8%. On the basis of the Uganda data one would have expected the opposite.

Although morphological traits may be apparently more modifiable by environmental influence, they do not lose their distinctiveness and diagnostic value for the groups which they distinguish. Although they are not mutually exclusive in their distribution among the various races, they do not exhibit the high degree of inconsistency that the various blood group systems indicate. Mahalanobis, Majumdar and Rao (1949), East and Oschinsky (1958), and Pollitzer (1958) have shown that the use of D^2 analysis provides a satisfactory estimate of racial affinities. In the study by East and Oschinsky, the resulting estimates of the 'distance' separating the various racial groups was not at all consistent with the serological data. Pollitzer, on the other hand, states that he found "satisfactory agreement of the two methods" (Pollitzer, 1958). Pollitzer is of the opinion that the agreement is due to hybridization, but states that there is no reliable estimate of "chromosomal relationship between morphological and other genes", which can be made on the basis of his data. He further states that racial analysis should include "a large battery of morphological and genetical traits." As we have already seen, a very large battery of traits can create as much confusion as a single trait.

Physical anthropologists have yet to develop valid scientific criteria which are biologically relevant to studies of the racial affinities of populations. Until these valid criteria are found, morphological analysis and such statistical tools as Mahalanobis' D^2 and Penrose's Size and Shape analysis will have to serve as temporary transitional techniques.

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La référence aux valeurs dans les sciences de l'homme

PAR FERNAND DUMONT

> "La science, ce n'est pas ce qui est écrit dans les livres. Pour qu'il y ait connaissance, il faut qu'en présence d'un système axiomatique... (l'esprit) continue de percevoir plus ou moins sourdement tout l'étagement des significations, qu'il se sente capable de les susciter à chaque instant et, si l'on peut dire, de régistrer différemment, et plus ou moins richement, le même texte, faisant tantôt résonner une multitude d'harmoniques et tantôt le réduisant à sa ligne la plus nette et la plus sèche".
>
> BLANCHÉ: *La science physique et la réalité,* 81-82.

Le problème des valeurs dans les sciences de l'homme semble se poser dans deux directions généralement considérées indépendamment l'une de l'autre. D'une part, le savant prétend étudier "objectivement" les valeurs qui inspirent les hommes; cette intention peut être considérée, sommairement, (Kolb), selon une double atitude: l'étude de la valeur comme "chose" à expliquer ou la considération de la valeur comme moyen d'explication. D'autre part, le chercheur est très préoccupé d'éliminer ce qu'il appelle les "jugements de valeur"; selon certains, les raffinements techniques à venir finiront bien par faire disparaître ces interférences importunes; selon d'autres, les jugements de valeurs sont inévitables dans nos disciplines et il semble tout aussi difficile de les éliminer que d'éviter les collusions parentes des théories authropologiques avec les idéologies ou la conception de l'existence propre à tel ou tel chercheur.

Notre première ambition, dans cette étude, est de dépasser ces discussions que nous jugeons plutôt superficielles et de nous situer dans une perspective où l'étude des valeurs et les jugements de valeurs s'enracinent dans des difficultés communes.

Pourtant, une solution radicale du problème s'oppose à une tentative comme celle que nous poursuivons dans cet essai. Elle sera, implicitement ou explicitement, remise en question tout au long de notre propos, mais il importe sans doute de la dénoncer dès le départ, comme un procédé grossier pour dissoudre des difficultés complexes et pleines d'intérêts.

A un regard superficiel, la présence multiforme des valeurs dans la structures des sciences anthropologiques est incontestablement gênante. Des traditions convergentes dans ces diverses disciplines, ont sommairement discrédité, au nom d'une objectivité confusément conçue, toute référence de ce type. Sur ce point comme sur bien d'autres, les tentatives pour définir des points de départ définitifs ont toujours été assez bien accueillies par les hommes de science. Encore aujourd'hui les essais pour éliminer *radicalement* la référence aux valeurs nous sont, pour ainsi dire, sympathiques; ils sont en tout cas florissants.

Dans la mesure où il est possible de résumer une épistémologie largement inavouée, on peut dire que, dans la perspective positiviste, seulement le directement observable doit être retenu[1]. Sans rappeler le sort du positivisme dans les sciences physiques, il faut souligner la difficulté essentielle à une telle position: quel est le statut de *l'observable* dans les sciences anthropologiques? A quelles charnières, *précisément délimitées,* l'observable se dégage-t-il du non-observable? Le positivisme devrait pouvoir enraciner ses arguments dans une pareille discussion. Sans cela, il n'est qu'une prise de position a priori, il n'est qu'un obstacle supplémentaire à l'observation critique des démarches effectives qu'épousent les praticiens des sciences de l'homme quand ils travaillent.

Le postulat méthodologique en quelque sorte fondamental utilisé par les tenants de l'idéologie positiviste semble consister, en gros, à réduire la valeur au jugement de valeur. Ce postulat montre bien la parenté qui existe, au niveau des attitudes

[1] Entre beaucoup d'exemples, voir la déclaration de M. Franz Adler citée par KOLB, *The Changing prominences of Values in Modern Sociological Theory;* dans BECKER et BOSKOFF, *Modern Sociological Theory in Continuity and Change,* pp. 100-101.

du chercheur, entre les deux perspectives sur le problème des valeurs que nous distinguions au départ; sur les deux plans, il permet de s'orienter vers une solution apparemment acceptable. Au niveau de l'étude des valeurs, celles-ci, définies comme des opinions, deviennent des faits comme les autres. Quant aux jugements de valeurs de l'homme de science, il suffira de les distinguer des jugements de réalité, puis de trier soigneusement les uns et les autres: ce qui permettrait d'épurer la pensée du savant par une opération relativement facile de sarclage. Sur les deux plans, on se trouve ainsi doté d'une objectivité à peu de frais.

Mais les valeurs ne sauraient être situées exclusivement au plan de la raison. Comme l'écrit un philosophe, Louis Lavelle: "La valeur n'apparaît que là où la personne s'engage intérieurement, là où elle sent, où elle veut. L'axiologie est une sorte de métaphysique de la sensibilité et du vouloir". Et encore: "La considération de la valeur oblige le moi à s'interroger non plus sur la représentation mais sur l'existence"[2]. Compte-tenu du langage hâtivement philosophique de Lavelle ("métaphysique", "existence"), ces remarques indiquent bien que se référer aux valeurs, c'est atteindre un aspect fondamental de la spécificité des conduites de quelque type qu'elles soient[3].

Il nous fallait dénouer, dès le départ, la doctrine positiviste. Les considérations sommaires qui précèdent n'ont pas du tout pour but de lui en substituer une autre. Nous croyons, au contraire, que les sciences de l'homme sont, sur le plan des intentions d'objectivité, encombrées de doctrines empruntées. Il est temps de nous débarrasser de ces idéaux (souvent laissés pour compte par les autres sciences) pour reconnaître, dans leur spé-

[2] Louis LAVELLE, *Traité des Valeurs*, tome I, pp. 25-26.

[3] Il est bien certain que les réflexions de Lavelle mènent à des considérations philosophiques qui sont marginales par rapport aux préoccupations scientifiques. Par exemple, parlant des empiristes, Lavelle souligne l'aberration qui consistait à confondre la valeur avec l'opinion sur la valeur "alors que l'on n'avait jamais confondu la vérité avec l'opinion sur la vérité" (cf. p. 27). Ce dernier type d'argument a peu de sens au niveau de l'analyse positive. Mais si, à propos des valeurs, les cheminements de la philosophie et des sciences divergent, la remarque que nous citions au texte semble bien recouvrir la perception des valeurs communes à l'une et à l'autre démarche considérées à leur point de départ.

cificité, les démarches effectives de notre pensée[4]. Pour tout dire, il est possible, après tout, que le positivisme représente un idéal valable: sur ce point, nous n'éprouvons aucune envie de le discuter; la question relève alors paradoxalement des valeurs qu'entretiennent les groupes d'hommes de science. Mais, dans la situation présente de nos sciences, on discerne un pluralisme épistémologique que l'on ne saurait, sans mystification, insérer dans une doctrine aussi cohérente et aussi simple; nous essaierons d'ailleurs de montrer que ce pluralisme est un élément authentique de richesse et de profondeur. Et surtout, il importe de cerner d'abord ce qui constitue le phénomène essentiel: comment le traitement des valeurs par les sciences de l'homme s'enracine dans la perception plus globale des valeurs et comment il s'en dégage[5].

*

*   *

Plutôt qu'à un idéal à priori, les réflexions qui précèdent nous renvoient donc à une description des façons très diverses dont s'opère la référence aux valeurs dans les sciences de l'homme.

[4] On aura ainsi reconnu que ces propos et ceux qui vont suivre se situent moins sur le plan de la théorie proprement dite que sur celui de l'épistémologie. Notons incidemment que cette distinction — dans les sciences de l'homme tout au moins — est peut-être plus arbitraire qu'il ne paraît à prime abord. Pour la psychologie, M. Piaget et d'autres ont montré les liens très étroits qui unissent théorie de la connaissance et théorie psychologique. Nous avons essayé, pour notre part, dans un article beaucoup plus modeste et qui se situe dans une toute autre perspective — à partir des difficultés actuelles de la théorie — de montrer qu'il en est sans doute de même pour la sociologie (cf. "Du sociologisme à la crise des fondements en sociologie", dans *Recherches et Débats*, Paris, déc. 1958). Et des travaux comme ceux de M. Guillaume en linguistique, de M. Febvre en géographie, de M. Granger en économique, etc. nous incitent à croire que la situation est semblable (nous ne voulons pas dire identique) dans bien d'autres sciences de l'homme.

[5] Ainsi situé, le problème devient très complexe. Ainsi M. Ruyer (un philosophe) écrit: "un jugement de valeur qui constate un autre jugement de valeur n'est pas un jugement de valeur" (*Philosophie de la valeur*, p. 52). C'est vrai — mais aussi, ce n'est pas si simple: la constatation du jugement de valeur renvoie, en un arrière-plan, à la perception de la signification des valeurs dont participe le jugement d'autrui; et en ce sens, détecter un jugement de valeur chez une autre personne, ce n'est pas tout à fait la même chose que de poser un jugement de réalité. Vaines subtilités, pensera-t-on peut-être; mais les idéologies scientifique les plus grossières prolifèrent sur la méconnaissance de ces difficultés.

Dans un premier survol, s'offre une constatation simple: la place faite aux valeurs, dans la recherche, est très inégale selon les diverses disciplines.

En histoire, le champ est d'autant plus libre que les concepts (au sens strict) sont rares et utilisés dans un sens peu rigoureux. La réflexion contemporaine sur l'histoire (Weber, Aron, Marrou, etc.) a mis en évidence la part très grande de la subjectivité de l'historien dans la reconstitution du passé. Dans l'impossibilité de saisir directement les valeurs caractéristiques des temps écoulés, l'historien part nécessairement de sa propre perception du présent qu'il projette pour ainsi dire sur le passé, ou plutôt sur les documents qui nous ont conservé un stock disponible de passé: de ce processus, se dégage la position du problème et même la démarche méthodologique. Les "faits" apparaissaient à l'historien positiviste comme des données fournies par la critique interne et externe, préalables à toute construction; ils sont pour nous l'aboutissant d'une sélection et d'une élaboration par l'historien — en somme, *comme la conclusion d'un processus complexe de valorisation.*

C'est dans l'axe d'une pareille réflexion que Marx Weber est allé jusqu'à écrire: "C'est sans fin que se transforme le cours de l'événement imprévisible qui va au-devant de l'éternité. C'est toujours à nouveau et sous d'autres aspects que se posent les problèmes de culture qui émeuvent les hommes, et le champ reste donc variable de ce qui, dans le courant infini de l'individuel, reçoit pour nous sens et signification et devient un individu historique, comme sont variables les rapports de pensée sous lesquels il est considéré et posé en objet de science. Ainsi, les principes des sciences de la culture resteront changeants dans l'avenir sans limites tant qu'une sclérose de la vie de l'esprit ne déshabituera pas l'humanité, comme en Chine, de poser de nouvelles questions à une vie inépuisable". Et Weber essaie de montrer comment toute tentative pour élaborer un système des sciences de la culture "ne peut que rassembler pêle-mêle, les points de vue multiples, spécifiques, disparates, sous lesquels la réalité se présente à nous chaque fois comme "culture", c'est-à-dire devient signifiante dans ce qu'elle a de plus particulier"[6]. Nous essaierons de bloquer, plus loin, cette épistémologie intempérante; laissons-lui pour le moment, toute sa vigueur d'obstacle, toute la force avec laquelle elle constate la présence de la valeur au cœur même de la pensée historique et, plus généralement, des sciences de la culture.

En ce qui regarde le problème qui nous occupe, la psychopathologie, ou plus généralement la psychologie clinique, peut être rangée tout près de l'histoire. Là aussi s'est opérée une réaction très vive contre la réduction de la maladie au biologique et contre le simple alignement des symptômes dans une chaîne causale. Alors que l'on se contentait trop souvent de considérer la maladie comme une entité à identifier dans un catalogue nosographique, on insiste souvent maintenant sur la nécessité, pour le psychologue, de comprendre le sens de la maladie pour le malade. Ainsi, deux mondes des valeurs, celui du malade et celui du psychologue, sont mis en perspective et le diagnostic s'opère au sein de cet étrange dialogue. Des épistémologies ont poussé, ici encore, sur cette prise de conscience de la spécificité de la clinique. Parallèlement à la pensée de Weber que nous évoquions tantôt, c'est ici une position comme celle de Jaspers qu'il faudrait rappeler avec son opposition tranchée entre "compréhension" et "explication". Voici d'ailleurs un texte curieusement similaire à celui de Weber que nous avons cité: "En psychologie et en psycho-pathologie, pas de théories dominantes. Dans ces sciences, un systèmes théorique unique est impossible, ou n'est du moins possible que comme construction personnelle"[7]. Ce mouvement s'est même élargi du côté de la biologie pour gruger la part faite par Jaspers à l' "explication": il suffit de rappeler l'œuvre de Goldstein[8]. Acceptons, ici encore, de donner à cette doctrine toute sa puissance d'extension.

[6] Cité par MERLEAU-PONTY, *Les aventures de la dialectique*, pp. 35-36.

[7] JASPERS, *Traité de psychopathologie générale* (trad. fr., Alcan, 1933), p. 16. Pour l'essentiel de la distinction entre "explication" et "compréhension" en psychiatrie, cf. particulièrement pp. 24-25.

[8] GOLDSTEIN, *La structure de l'organisme* (trad. Buckhardt et Kuntz), p. 313: "La connaissance biologique est l'acte créateur toujours répété par lequel l'idée de l'organisme devient pour nous de plus en plus un événement vécu, une espèce de "vue" au sens de Gœthe, vue qui ne perd jamais contact avec des faits très empiriques".

Histoire et psychologie clinique: on peut penser que, dans les deux cas, *à la limite, l'univers scientifique procède littéralement de l'univers des valeurs.* Pour les fins de la description première que nous opérons, il semble bien que nous puissions considérer ces deux univers comme définissant le pôle extrême de ce phénomène de la référence aux valeurs que nous examinons. Donner d'abord toute sa force à l'affirmation de cette référence, comme nous l'avons fait en invoquant Weber, Jaspers, Goldstein, c'est risquer — comme nous le laissions entendre — d'accorder aux épistémologies qui l'assument actuellement un crédit qui dépase leur portée réelle; mais ce sera par ailleurs, une excellente façon de les remettre ultérieurement en question.

Par rapport au premier pôle ainsi délimité, d'autres disciplines peuvent être placées à l'autre extrême — qu'elles contribueront aussi à définir. Nous retiendrons les cas de la micro-économique et de la linguistique structurale.

La micro-économique fait peu de place aux valeurs parce qu'elle s'est édifiée sur une psychologie élémentaire: celle d'un homme absolument logique. La visée des valeurs chez le sujet est ici rigoureusement un calcul. Pourtant l'aspect significatif du comportement économique n'est pas entièrement éliminé. Le processus d'identification aux valeurs d'autrui (auquel nous avons fait allusion, cf. note 5) est supposé par une proposition telle que: "l'entrepreneur vise à maximiser son profit" — ou par l'identification de l'analyse marginale avec la valeur d'utilité chez le sujet économique. Il est bien instructif, sans doute, que, sur ce point, l'objectif que Pareto se donnait d'éliminer les raisons psychologiques des choix économiques en substituant les "courbes d'indifférence" à l' "utilité" se soit avéré comme illusoire — les courbes apparaissant précisément comme les indices de discrimination entre niveau d' "utilité".

Le cas d'une certaine linguistique structurale nous mène plus loin encore. Nous songeons à Bloomfield et à l'Ecole non-mentaliste de Chicago et à l'Ecole danoise de Hielmslev. En réduisant la langue à un processus de communication, les tenants de cette perspective éliminent, en somme, la référence au contenu et, par là même, la référence aux valeurs. Sommes-nous, dès lors, en un secteur privilégié des sciences de l'homme où, enfin, le problème encombrant des valeurs serait absent? Il ne semble pas. Observons que, au moment où s'affirmait cette perspective, au sein de la linguistique considérée cette fois-ci globalement naissait une méta-lingustique (dont une des sources privilégiées peut sans doute être cherchée dans les écrits de Benjamin Lee Whorf) et où, cette fois-ci, le contenu, et donc la référence aux valeurs occupent toute la place. C'est là un phénomène de *délégation,* à l'intérieur d'une problématique globale, qui est plein d'enseignements épistémologiques sur lesquels nous reviendrons avec attention. Notons, pour l'instant, que ce qui nous apparaissait de prime abord comme une élimination de la valeur ne peut être défini comme tel qu'à l'intérieur d'un secteur bien délimité d'une science particulière, en l'occurence, la linguistique structurale; dès que, *à propos d'un même objet* un peu global (ici, la langue), on replace ce procédé partiel dans le contexte des procédés voisins, la référence aux valeurs réapparaît.

Ces deux cas semblent nous indiquer un second pôle du phénomène global de la référence aux valeurs dans les sciences de l'homme. Ils font apparaître cette référence comme éminemment "construite" et comme refoulée, pour ainsi dire, dans un aspect bien délimité de la problématique d'ensemble. De sorte qu'il semble légitime de caractériser l'extremum ainsi délimité par rapport au premier en inversant le qualificatif: dans la micro-économique et dans la linguistique structurale, *l'univers des valeurs procède littéralement de l'univers scientifique.*

Ces deux pôles extrèmes nous semblent suggérer un véritable spectre épistémologique qui nous fournirait la première description globale que nous cherchions. En effet, il ne serait pas difficile de ranger, par rapport à ces pôles concrets, les autres sciences de l'homme: l'anthropologie culturelle, qui établit, au cœur même de ses investigations, des constellations de valeurs définissant les cultures; la sociologie, qui couple des valeurs culturelles sur des variables moins "significatives" (démographiques, par exemple) ou réduit les valeurs aux supports ou aux processus de valorisation; la psychanalyse, dont le vocabulaire souvent emprunté à la biologie ("force", "pulsions", etc.) traduit l'intention d'effacer la référence aux valeurs, mais qui suppose, par ailleurs, dans la notion de transfert, un dialogue entre des conduites significatives; la psychologie expérimentale, qui tâche de réduire la conscience au comportement, mais qui remanie aussitôt cette position du problème, à l'intérieur même du behaviorisme (Tolman, Kantor), pour y introduire des perspectives téléologiques.

Chacune des sciences de l'homme ferait ainsi référence aux valeurs selon deux perspectives inégalement importantes pour chaque discipline: l'univers des valeurs donnant naissance à l'univers scientifique, et inversement. Nous sommes là, il nous semble, devant la dialectique la plus globale à laquelle donne lieu l'évocation des valeurs dans nos sciences. Nous en verrions volontiers une démonstration supplémentaire dans le fait que certaines disciplines ont admis la super-position de deux modèles d'analyse axée en définitive l'un et l'autre sur les deux pôles que nous avons cru détecter. Ainsi, rappelant des réflexions de Guthrie, Ducasse et Singer, M. Tilquin écrit à propos du schéma du comportement caractéristique de la psychologie behavioriste: "Pour Singer, ...la reconnaissance de la finalité n'est pas contradictoire avec l'assertion du mécanisme: des choses prises collectivement peuvent avoir des propriétés (finalité), qu'elles ne possèdent pas prises distributivement (mécanisme). Cette remarque de Singer nous conduira à reconnaître que les propriétés, aussi bien téléologiques que mécaniques d'un système sont des propriétés *formelles*. Le behaviorisme téléologique se dit concret et s'oppose au behaviorisme physiologique comme à un behaviorisme abstrait. En réalité le behaviorisme téléologique est téléologique parce que molaire, le behaviorisme physiologique est mécanique parce que moléculaire"[9].

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[9] TILQUIN, *Le behaviorisme. Origine et développement de la psychologie de réaction en Amérique*, p. 505.

A partir de cette position globale du problème, deux voies paraissent s'offrir à la réflexion: ou bien nous contenter de cette première dialectique et la traduire un peu platement en terme d'antinomie fondamentale de la pensée dans les sciences de l'homme; ou bien, considérer cette dialectique comme une simple saisie première de l'univers des sciences humaines et tenter de la dépasser; nous tenterons d'abord d'opérer ce dépassement à partir d'une critique de l'épistémologie — courante dans la philosophie de nos sciences — qui oppose "compréhension" et "explication" et à laquelle nous avons déjà fait allusion en citant Weber et Jaspers. Cette critique, notons-le aussitôt, ne sera pas, pour nous, que négative. Nous avons élaboré un spectre épistémologique dont le pôle positif était donné par la référence débordante aux valeurs: nous avons même donné à ce pôle toute sa force d'affirmation en le définissant à travers des doctrines dont la tonalité polémique n'est pas douteuse. En critiquant les épistémologies qui opposent "explication" et "compréhension", "nature et valeur", et qui (nous le verrons) finissent par accorder la priorité à la compréhension, nous serons amené, non pas à supprimer le spectre que nous avons construit, mais, au contraire, à donner une plus grande consistance à la dialectique qu'il engendre en la tournant pour ainsi dire vers le dedans, vers le pôle négatif qui en définit la fermeture.

Avant d'introduire à des considérations plus générales, abordons ces épistémologies par deux exemples qui nous paraissent fort caractéristiques.

Dans un petit livre très suggestif[10], M. Lagache a essayé de montrer que les théories et les méthodes de la psychologie oscillent entre deux positions: faire de la psychologie une science de la nature ou une science de l'homme. La tendance "naturaliste" vise à l'élimination de la conscience et au traitement des faits psychologiques comme s'ils étaient des choses; pour elle, les éléments sont à étudier d'abord; elle vise à élaborer des lois; le véritable substrat psychique est de nature organique; elle rejette les valeurs comme éléments non scientifiques. La ten-

[10] Daniel LAGACHE, *L'unité de la psychologie* (Presses Universitaires de France, 1949).

dance "humaniste" considère plutôt les faits psychologiques comme des "expériences vécues", des "expressions"; elle pose dès l'abord l'antériorité du tout; elle ne cherche pas des lois, mais tente d'établir des "types idéaux"; par le fait même, elle fait large place aux valeurs; la conscience et l'inconscient sont au centre même de la recherche. M. Lagache souligne qu'il est à peu près impossible de faire rentrer sous chacune de ces tendances, des théories ou des doctrines bien déterminées; il s'agirait plutôt de deux inspirations qu'on peut légitimement isoler comme telles, mais que l'on retrouve parfois à l'intérieur d'une même théorie (dans la psychanalyse par exemple). Finalement, M. Lagache voit la synthèse et l'unité de la psychologie dans la clinique, c'est-à-dire — est-on tenté de penser — dans la "compréhension".

La pensée de M. Merleau-Ponty sur la sociologie nous paraît très proche de celle de M. Lagache pour la psychologie. Dans un article remarquable[11], où il s'inspire profondément et parfois littéralement de Husserl, il veut essentiellement montrer la présence de la philosophie au centre même des préoccupations du sociologue. A cause même de son objectif, il atteint plus radicalement que M. Lagache l'opposition de "l'explication" et de la "compréhension" que nous voudrions finalement cerner: d'où l'intérêt très vif de cette réflexion pour notre propre dialectique. La pensée profonde de l'auteur paraît pouvoir se résumer dans la dichotomie qu'il pratique incidemment entre "l'idéalisation du fait brut, qui est l'essentiel", à son avis, du travail du savant et "le déchiffrement des significations qui est sa raison d'être" (cf. p. 52). Toute l'étude de M. Merleau-Ponty est dirigée contre ce qu'il appelle — beaucoup trop brièvement — "l'objectivisme". Celui-ci écrit-il, "oublie cette ...évidence que nous ne pouvons dilater notre expérience des rapports sociaux et former l'idée des rapports sociaux vrais que par analogie ou par contraste avec ceux que nous avons vécus" (p. 52). Et encore: "Sous le nom collectif de science, il n'y a rien d'autre qu'un aménagement systématique, un exercice

[11] MERLEAU-PONTY, *"Le philosophe et la sociologie"*, dans *Cahiers internationaux de sociologie*, vol. X (1951), pp. 50-70.

méthodique — plus étroit et plus large, plus et moins clairvoyant, — de cette même expérience qui commence avec notre première perception" (p. 55). Dès lors, la tâche essentielle du philosophe consistera, une fois la science faite, à reconstituer l'essentiel de la visée qu'elle suppose, *à révéler* littéralement au sociologue le fondement de sa propre pensée. Ce qui nous intéresse, ici, au haut point, c'est: 1) l'antinomie, vite établie, entre *objectivation* et *compréhension;* 2), la réduction, également rapide, de celle-là à celle-ci. Il ne reste plus qu'à penser (et c'est bien là semble-t-il, la position essentielle de l'auteur) que savoir positif et phénoménologie, explication et compréhension, conduisent à l'idée d'un enveloppement réciproque[12].

Nous n'avons pas du tout l'intention de montrer que les analyses de M. Lagache et de M. Merleau-Ponty sont fausses. Elles sont cohérentes, au contraire, avec la première description de notre problème que nous avons tenté. Il y a bien, comme veut le montrer M. Lagache, une intention naturaliste et une visée humaniste dans les sciences de l'homme; il y a bien, dans la sociologie, une "réification", une "objectivation" de la perception du social par le sociologue et elle appelle une démystification, comme le veut M. Merleau-Ponty. Mais justement, situer l'analyse de la pensée scientifique à ce niveau, n'est-ce pas édifier davantage une sorte de psychologie de la science bien plutôt qu'une épistémologie? N'est-ce pas faire originer l'analyse bien davantage dans les attitudes du savant que dans la raison scientifique — ou tout au moins brouiller l'interférence des deux? La remarque vaut sans doute pour M. Lagache; mais elle atteint aussi M. Merleau-Ponty en ce sens que celui-ci remet en question bien plus le réalisme du savant que ses procédés. On comprend comment, en ce sens, ces deux œuvres constituent une excellente dénonciation de certaines idéologies scientifiques — en particulier d'un certain positivisme que nous

[12] Nous pourrions étendre l'analyse que nous venons de faire à la pensée, énormément plus élaborée, de M. Merleau-Ponty sur la psychologie (cf. ses deux thèses *La structure du comportement* et la *Phénoménologie de la perception)*. L'intention de réduire l' "objectif" à la signification y est manifeste. Il était plus intéressant de saisir d'abord cette réduction au plan de la sociologie où, sans aucun doute, elle atteint le niveau critique où elle devient, de façon décisive, contestable.

avons nous-mêmes dénoncé; mais elles risquent, du même coup, de rester à ce niveau.

Ces analyses ne risquent-elles pas elles-mêmes de donner lieu à de nouvelles idéologies? On doit se demander, par exemple, si le relativisme de Weber n'est pas une idéologie tout aussi pernicieuse que le positivisme et s'il n'est pas — dans une autre direction — l'exploitation d'une confusion inhérente aux sciences humaines dans leur état actuel; la dualité établie par M. Lagache et même celle que discerne M. Merleau-Ponty risquent de conforter la source même de la confusion. Par exemple, la psychanalyse peut être lue dans une perspective mécaniste ou dans une perspective "compréhensive": il y a là un brouillage tragique dans la lecture d'un univers scientifique que ni M. Lagache, ni M. Merleau-Ponty ne contribuent à éclairer; ces auteurs risquent, au contraire, en résorbant finalement l'antimonie dans la "compréhension", de consacrer les perspectives trop globales qui inspirent ces déchiffrages contradictoires[13].

Nous sommes ainsi acheminés vers notre critique essentielle. Pour que les réflexions de MM. Lagache et Merleau-Ponty puissent constituer une analyse épistémologique valable de la dualité qu'elles indiquent, il faudrait qu'elles puissent montrer comment la compréhension sort littéralement de l'explication — et *vice-versa*. Or ce n'est pas ce qu'elles font. Elles réussissent tout au plus à discréditer le "naturalisme" ou "l'ob-

[13] Ainsi, dans un important ouvrage récent (*La psychanalyse d'aujourd'hui,* sous la direction de Nacht, 2 vol. 1959), M. Fain écrit (tome II, p. 508): "La psychanalyse utilise avec la neurologie et la psychiâtrie des termes communs tels que inhibition, répression, structure. Il est aisé de se rendre compte que le sens donné par la psychanalyse à ces locutions est non seulement le même que celui que lui donne la physiologie mais que les phénomènes décrits par ces disciplines différentes s'apparentent étroitement." — Mais, dans son étude sur la clinique psychanalytique, M. Bouvet déclare de son côté (ibid., tome I, pp. 43-44): "Si la clinique psychanalytique n'est pas originale dans sa description "immédiate" des désordres de la vie psychique, et si nulle part ces descriptions ne diffèrent radicalement de celles qu'ont pu établir les cliniciens éminents de la psychiatrie classique, c'est au moment de la "compréhension" qu'elle jette une vive lumière sur le fait psychopathologique". Il semble que l'on saisisse ici, sur le vif, chez des auteurs qui ne polémiquent évidemment pas entre eux, deux lectures fondamentalement différentes de la même science. Au niveau de la science, on voit comment les réflexions de MM. Lagache et Merleau-Ponty permettent de détecter cette antinomie sans arriver à la dénouer autrement qu'en privilégiant l'un ou l'autre des deux pôles.

jectivisme". L'œuvre entière de M. Merleau-Ponty est particulièrement éclairante en ce sens. Dans sa *Phénoméologie de la perception*, et dans son étude sur la *Structure du comportement*, il remet finement en question le naturalisme de la psychologie expérimentale, mais il ne réussit pas à sauvegarder *les concepts* de celle-ci à l'intérieur de sa phénoméologie; qu'il proteste, par ailleurs, que la phénoménologie n'abolit pas la science psychologique[14], cela ne modifie guère le résultat final. Dans l'étude sur la sociologie que nous avons citée comme exemple, les généralisations sociologiques sont présentées, à la limite, comme n'étant littéralement, en définitive, que la généralisation de l'expérience sociale du sociologue. Or, même si cela était exact, il reste que la sociologie interpose entre le sociologue et son objet d'étude un montage conceptuel — et, pour reprendre une expression courante chez M. Merleau-Ponty, "objectiviste" — dont il faudrait expliciter le sens autrement qu'en en faisant un barrage à dissoudre pour parvenir à la "compréhension". Même si tout aboutit à la compréhension, il faudrait comprendre pourquoi ces montages conceptuels constituent *une étape nécessaire* dans la dialectique que l'on prétend reconstituer: on ne saurait prétendre sauvegarder la science comme parallèle à la philosophie qu'à ce prix. L'hommage incident aux théories extrémistes de la science ne saurait faire illusion: exalter, par exemple, la notion de "comportement" chère aux behavioristes sous prétexte qu'elle ouvre la conscience sur le monde[15] ne peut être considéré que comme un compliment fait par la phénoménologie — dont la notion centrale est, on le sait, l' "intentionnalité" — à une théorie scientifique incidemment parente; elle n'assume pas l'usage effectif de ce concept *dans la science*. En somme, toute l'ambiguité de la pensée de M. Merleau-Ponty — et, dirions-nous, de la phénoméologie en général — par rapport aux sciences

[14] Par exemple: "Husserl n'a donc rien contre une psychologie scientifique. Simplement, il pense que l'existence et le développement d'une telle psychologie posent des problèmes philosophiques, dont la solution importe à la psychologie elle-même, si du moins elle doit sortir des impasses". MERLEAU-PONTY: *Les sciences de l'homme et la phénoménologie*, fasc. 1 (miméographié), C.D.U., p. 12.

[15] Voir, par exemple, MERLEAU-PONTY, *La structure du comportement*, p. 2, note 2.

de l'homme, pourrait être soulignée par le rapprochement de deux textes. Le premier est souvent cité avec faveur: "Les déterminations numériques de la science repassent sur le pointillé d'une constitution du monde déjà faite avant elles" (*Phénoménologie de la perception*, p. 348). Le second se trouve à la fin de *La structure du comportement* (nouv. éd., p. 240): "On veut... égaler la conscience à l'expérience entière, recueillir dans la conscience pour soi toute la vie de la conscience en soi". La tentation nous guette alors de faire comme si les montages de la science étaient tout entiers contenus dans la conscience "en soi" et n'étaient qu'une feinte — et non une démarche nécessaire — pour parvenir à la conscience "pour soi". Or s'il est, à notre sens, un postulat incontestable de l'épistémologie des sciences, c'est que celles-ci constituent une dialectique *nécessaire*. Sans ce postulat, le philosophe pense *à propos* des sciences; il ne parle pas de science.

Encore une fois, nous ne discutons pas ici la validité de la phénoménologie comme entreprise spécifique. Mais, dans son dialogue incessant avec nos sciences et dans sa tentative pour en élucider les fondements, elle est une épistémologie hâtive. Elle risque d'être une philosophie paresseuse des sciences anthropologiques. Après les critiques que nous avons formulées, le spectre épistémologique proposé plus haut fait apparaître l'opposition de "l'explication" et de "la compréhension" comme *une épistémologie de l'alternance*, comme un va et vient entre des pôles trop éloignés l'un de l'autre; c'est là, sans doute, ce qui permet la résorption du processus d'ensemble dans une "compréhension" qui efface les moments de l'objectivité ou du naturalisme. Et par là, il s'agit d'une dialectique sans consistance.

Nous serions beaucoup plus près de la spécificité de la pensée dans les sciences humaines si nous passions de cette épistémologique de l'*alternance* à une épistémologie du *déplacement*. Notre spectre épistémologique nous oriente déjà en ce sens: essayons de l'indiquer brièvement.

Nous avons vu comment, à l'intérieur d'une science déterminée, se produisaient des phénomènes de "délégation" (en linguistique, en science économique, en psychologie expérimentale). Cette constatation peut être élargie: ces délégations se produisent entre les sciences. Ainsi, si M. Parsons peut construire, à propos de la sociologie, ce que M. Bourricaud appelle "une axiomatique de l'action quelconque", c'est parce qu'il renvoie à l'anthropologie l'étude du contenu et des valeurs des conduites. C'est là un procédé curieusement analogue à celui que nous avons discerné *à l'intérieur* de la linguistique. Le phénomène inverse pourrait être explicité dans l'*histoire des religions*. Rien de plus curieux, à cet égard, que le *Traité* de M. Eliade[16]: malgré son titre, il n'y est pas question d'une *histoire* des religions, mais d'une analyse de la perception des valeurs religieuses à travers les matériaux que fournit l'histoire des religions. L'antinomie méthodologique est d'ailleurs bien marquée, par exemple, par un passage comme celui-ci: "Les modalités du sacré révélé par le christianisme sont en somme plus correctement conservées dans la tradition représentée par le prêtre (fut-elle violemment colorée par l'histoire et par la théologie) que dans les croyances du village. Or, ce qui intéresse l'observateur, ce n'est pas la connaissance d'un certain moment de l'histoire du christianisme, dans un certain secteur de la chrétienté, mais la religion chrétienne elle-même. Le fait qu'un seul individu, dans tout le village, connaisse le rituel, le dogme et la mystique chrétienne, tandis que tout le reste de la communauté les ignore et pratique un culte élémentaire imbu de superstitions... ne présente ici du moins, aucune importance"[17]. Ce procédé méthodologique est fort intéressant pour notre propos: pour atteindre les valeurs religieuses dans leur spécificité, le savant renonce aux niveaux d'étagement (ici, les groupes sociaux) qui risquent de disperser cette spécificité et à une temporalité qui menace aussi de la dissoudre. Nous constatons, sur le vif, le phénomène exactement inverse de celui que nous avions détecté pour la linguistique structurale: cette fois-ci, c'est l'explicitation de valeurs spécifiques qui renvoie à une problématique plus large où la perspective naturaliste aurait un sens.

[16] Mircea ELIADE, *Traité d'histoire des religions* (Payot, 1953).

[17] *Op. cit.*, p. 19. Le fait qu'un phénoménologue (au sens large) ait précisément cité ce propos pour justifier que son analyse intentionnelle se limite à peu près aux écrits théologiques confirme, il nous semble, nos propos de tantôt (cf. DUMERY, *Critique et religion*, p. 184, note 1).

Nous saisissons ici radicalement pourquoi une épistémologie de l'alternance est possible — mais, du même coup, pourquoi elle est insuffisante. Elle pense tout réduire à la compréhension et aux valeurs, alors qu'il ne s'agit que d'un déplacement. Déplacement dans les deux sens, — notre dernier exemple vient de le montrer: plus le refoulement de la valeur est radical plus les procédés naturalistes s'étaleront dans la problématique complémentaire; et inversement.

Dans ce contexte, l'anthropologie culturelle offre, nous semble-t-il, un cas extrêmement intéressant et en quelque sorte privilégié. Sa situation, dans le complexe des sciences de l'homme, montre que non seulement existent ces vastes déplacements que nous avons cru discerner à l'intérieur d'une même science, mais que ces procédés de délégations peuvent se dérouler entre les sciences et au niveau spécifique de la référence aux valeurs. Abordant des cultures autres que la sienne propre, l'anthropologue a le sentiment fort vif de ce qu'il appelle le relativité des valeurs; et le fait qu'il étudie habituellement de petites cultures l'incite à saisir "des ensembles signifiants" mieux et plus encore que le sociologue. Il tire de ce procédé une leçon d'objectivité — non pas seulement pour sa science, mais aussi pour la sociologie et la psychologie. Ici, le procédé d'objectivité par rapport à la valeur consiste non pas à réduire celle-ci, mais à déplacer l'objet dans lequel on la saisit. C'est cette variation qui consistue très précisément un procédé implicite d'objectivité pour les autres sciences de l'homme. Incidemment avec l'anthropologie, nous commençons vraiment à apercevoir comment le problème de la référence aux valeurs ne doit pas être considéré seulement à l'intérieur de telle ou telle discipline particulière; elle opère de façon spécifique dans la totalité que forment les sciences de l'homme. Nous l'indiquerons aussi en évoquant, une fois encore, l'historiographie. Celle-ci, nous l'avons relevé, fait une très large place à la reférence aux valeurs. Or, la plupart de nos actuelles sciences de l'homme ont été, à un moment ou l'autre, des sciences historiques. Encore maintenant, l'histoire forme, pour ainsi dire, l'arrière-plan de nos disciplines: l'économique s'accompagne d'une histoire économique, la sociologie d'une sociologie historique, la linguistique d'une linguistique historique, etc. Tout se passe comme si chacune de nos sciences déléguait encore l'histoire à la multiplicité des significations que comporte sa matière propre.

Il nous paraît possible de résumer ainsi l'essentiel de ce qui devrait constituer le point de départ d'une réflexion épistémologique sur la référence aux valeurs dans l'univers des sciences de l'homme: si on considère cet univers dans son ensemble, on constate que les valeurs, "poursuivies" sur un plan se réfugient ailleurs dans une problématique complémentaire ou dans la problématique d'ensemble.

Dans toute cette étude, nous avons évoqué — sera-t-on tenté de penser — des exemples un peut trop vastes. Au plan de la position globale de la question où nous nous sommes placé, il fallait respecter ce qui constitue un critère fondamental de l'épistémologie, à notre sens: saisir les diverses problématiques scientifiques à de divers niveaux; dans ce débrouillage que nous avons sommairement opéré, il fallait essayer, sans doute, de ne pas perdre de vue le problème d'ensemble et même d'indiquer implicitement à quel plan il se situe.

Nous nous sommes engagés ainsi, il nous semble, dans deux voies de réflexions.

D'une part, nos indications sur le déplacement de la référence aux valeurs indiquent que les réflexions axées sur "la compréhension" rendent un mauvais service à la philosophie des valeurs; elles consacrent la primauté de celles-ci, mais dans le nuage prématuré du "mystère" que M. Gabriel Marcel oppose trop facilement aux "problèmes". Les mystères sont des paravents à moins qu'ils ne soient cernés, traqués au terme d'une très longue recherche — dont les sciences de l'homme constituent un excellent prototype. Dans ce contexte, bien loin que ce soit l'enveloppement final dans la "compréhension", c'est le déplacement incessant des valeurs dans les problématiques des sciences de l'homme qui est littéralement un hommage aux valeurs; il en rend la perception de plus en plus fine, de plus en plus nuancée parce qu'il en multiplie les visées, en remanie sans cesse l'intuition, remet celle-ci continuellement en question par des procédés.

Nous nous engagerions ainsi dans une des démarches qui nous paraîtraient fécondes pour la constitution d'une anthropologie philosophique que, pour notre part, nous appelons de nos vœux. Pour l'instant, ce n'est pourtant pas ce chemin que nous voulons suivre. Nous voudrions rester sur le plan de l'épistémologie et constituer la contrepartie des critiques que nous avons faites aux philosophies hâtives de la compréhension. Du même coup, nous nous orienterions vers un autre niveau de la problématique des valeurs dans nos sciences, vers l'analyse plus fine des procédés de références; nous pratiquerions ainsi le passage de la référence générale aux valeurs — considérée jusqu'ici globalement — aux modes d'étude dite "objective" des valeurs dans les sciences de l'homme. Nous aurions alors à raffiner le spectre épistémologique présenté ici. C'est ce que nous tenterons de faire dans une prochaine étude où nous voudrions essentiellement faire apparaître, au cœur de ces procédés de déplacement dont nous avons parlé, la genèse des concepts scientifiques.

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Notes of a Psychologist Fieldworker*

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Anthropological findings make interesting reading for clinical psychologists. The accounts of anthropologists who lived for years with "savages" and "primitive people" are read with admiration as testimonies of hardships scientists will undergo to obtain their material. There is a shade of envy mixed with the admiration for the clinical psychologist's research setting is usually the campus of a university, the hospital and the school. His informants are university freshmen, patients and children going to school. Of the anthropologists' methods of gathering data, the psychologists know little except that it must be a difficult procedure.

This paper is an account of a clinical psychologist who, as he continues investigations of methods of thinking, ventures into an isolated and perhaps less primitive culture than his own. The experiment conducted in the field is of no significance to the pursuit of my subject — thoughts and armaments of a psychologist to anthropology in the raw.

Contemplation of field work in a strange culture raised intense feelings of apprehension and displeasure, mitigated only by the writer's enthusiasm for mountain climbing and other outdoor activities. The apprehension was not connected with the prospects of travel and lack of physical comfort; nor was there concern about living with strange people. The misgivings were concentrated around the ignorance of anthropological "know-how." How does one contact strange people? How does one elicit their cooperation? How will they take to being tested? Will their responses make sense? How can one who is relatively ignorant of anthropological techniques undertake a task so inti-

* This study and field trip was supported by a research grant from United States Public Health Service.

mately related to it? Training in anthropology instills in the student the notion of eventual field work in a radically different culture. Training in clinical psychology readies the student to work entirely in his own culture.

Anthropologists, friends as well as strangers, were encouraging and extremely helpful. With their cooperation and active support, an area and a tribe were selected: a group of Athabascan-speaking bush-Indians in the Northwest Territories in Canada. A few months later, the author, his wife and four-year-old son were established at their headquarters. The community contained a Royal Mounted Police Station, a Communication Station, a Hudson Bay store, a Roman Catholic Mission and Hospital, two more missions, and some independent training establishments. Some Indians had settled around these institutions. The services of an interpreter had been secured with the aid of an anthropologist who had worked previously in this area with the same individual. While some resident Indians were interviewed, the bulk of the subjects consisted of individuals who lived in the bush. The experimenter and his interpreter took to the river to accomplish this

The following account, written in the first person, is an excerpt of notes made in the long hours spent on the river in a canoe and represents on the spot impressions rather than orderly thought and mature conclusions.

When visiting the Indian village I was impressed by the many ways in which the Indians were like the people in my home town. Of course, they did not resemble individuals from "the University tribe" of which I am a member, nor did they resemble members of "the patient tribe" with whom I have considerable experience. Rather, the Indians reminded me of that tribal group, the "people from the other side of the tracks" among whom I have some friends. Visiting with friends from "the other side of the tracks" tribe, I am aware of differences. I experience some tension due to these differences. But the likenesses between me and people who live on the other side of the tracks are much greater than the differences between us. I know that they have different values, occasionally different customs and ceremonies; still I can understand the differences, can cope with them to my satisfaction and theirs. The differences are there but I and they have the equipment to live with them.

The Indians appeared so much like some tribes in my community that I started wondering. I had no reason to doubt that the cultural system reported in the anthropological literature was part of their lives, but it seemed to exert only a small influence in their daily existence. Something akin to the cultural myth of the "pioneering spirit" in my home community — a historically well documented saga, extolled on TV, in children's books and western movies, etc. However, the pioneering myth is hopelessly outdated and survives only in nooks and crannies of the modern industrial West. The important *current* myths governing the behavior of the West are rarely written about, are rarely the subjects on TV shows. In the same fashion, has the anthropologist overemphasized the historical aspects of cultural constructs in the Indians' lives? Do the cultural data represent ineffectual memories experienced at the fringe of existence or do they indicate the presence of a powerfully operating mainspring of on-going behavior? Are the myths, told by older folks, shrugged off by children as pleasant unrealities, or do they serve them as guiding stones when important decisions affecting life and livelihood are to be made? Anthropologists must have encountered great difficulties defining to what degree the ancient cultural determinants represent a focal or peripheral factor in the Indians' present social organizations.

The determination of whether a factor is central or peripheral in the existence of an individual is an important one in clinical psychology. Like the anthropologist, the psychologist obtains two types of data. He learns what an individual thinks and feels his life is like, what his experiences in the past have been, and how he thinks events of the past are reflected in his present life and behavior. The clinical psychologist may or may not agree with the individual's self-evaluation .It is the psychologist's task to relate an individual's history to his present behavior, with the understanding that the focal points of motivation are ever shifting and changing in time. What was most important, most vital to an individual yesterday may be of little motivational impact today.

Psychologists assume that they can evaluate an individual's life pattern and deduce a motivational hierarchy. Whether this claim represents wishful thinking, good guessing or justified theory is an open question. Psychologists act as if they were able to create a hierarchical structure of motivations which indicates what is focal and what is peripheral in an individual's motivational field. It is assumed that this hierarchy is capable of explaining present behavior, as well as predicting future behavior.

Psychologists deal with the problem of assigning quantitative values to a motivational variable primarily in two ways — artistically and theoretically. It is great artistry to sense intuitively the hierarchical relationships of an individual's motivational variables. The theoretical approach draws upon the notions of the orderliness of personality growth and development, upon theories of personality dynamics and the relative strength of drives — social, cultural, physiological and psychological.

An example of a point in theory would be represented by the concepts of sexual identification. It is postulated that with few exceptions all individuals in our culture learn by progressive differentiation, due to social pressure, to assume a sexual role and to derive satisfaction from it. The child accomplishes this goal but slowly and in the course of years. At times, the process of progressive identification is a source of great anxiety, confusion and disturbance. Various sequential steps of the process are postulated. The process is considered terminated when successful identification is said to have taken place. Failure in identification, it is held, will become focal points in an individual's motivational structure. We assume that success and/or failure in sexual identification will occupy a central position in any and all individuals in our culture. Many focal motivational forces will be linked to this development.

Psychological theory is yet inadequate to yield more than tentative answers and is presented here only as a sample of psychologists' thinking in meeting this problem. Like psychology, anthropology has been slow to develop an adequate system by which to assess the significance variables have upon the present behavior of organisms — be the organism an individual or a society.

It soon became evident that the life of the Indian could not be understood unless one took into consideration those social institutions which exerted considerable influence on his existence. The Hudson Bay Company, the Royal Mounted Police and the Roman Catholic Church, to name only the most important ones, do exert on the Indians a set of conditions which influence profoundly every aspect of their activities. One, therefore, has to understand these social institutions, their histories, as well as present organization and policies, in order to comprehend the world of the Indian.

The clinical psychologist in a similar way needs knowledge of the present social environment of the individual he is dealing with, be it a patient or an experimental subject. This understanding of the social environment is derived from the data and theories of social psychology and/or sociology. The clinical psychologist is often in difficulty when he needs to integrate his own data with data of a sociological nature. There exists at present no consistent body of theory by which the transition from the individual to social data can be accomplished. Clinical psychologists need sociological concepts but they can only be borrowed; there is no way by which they can be assimilated and amalgamated, at present, in clinical psychological theory.

I ask the probably naive question whether cultural anthropology does not face a similar problem in the transition between anthropology and sociology. If it does, it faces, like clinical psychology, the task of unifying principles with ancillary fields. The absence of inifying principles makes communication difficult between professions. The anthropologist centers his interest and convictions around culture and the clinical psychologist his around the individual personality, group interaction, etc. A try at unifying these two principles would be a necessary and possible first step.

When meeting the Indians as an experimenter, my reactions ran the gamut from exhilaration and animosity to exasperation and inertia. They let me wait hours and days without any rhyme or reason, or so it appeared to me. They were unwilling to part with information, they were often suspicious. At all times I was the one who had to adjust to them, not one of my subjects adjusted to me. All this appears to be a necessary condition of successful field work. There were times when things went smoothly, my work progressed, my curiosity knew no bounds. My notations at the top of the data sheet indicate my feelings at the time of the experiment. It easily can be seen that my data, in some instances, were richer, more exhaustive; my subjects more informative than at other times. At these other times, perhaps, I had spent two days chasing my subjects unsuccessfully. My data sheets would clearly contain an indication of my frustration. The data obtained under these conditions were very biased usually. It helped, in the evaluation of the data, to know the conditions under which they were obtained. Not only I, the experimenter, but my interpreter, too, was a variable instrument. There were days when he was superb; there were others when he was a reluctant and only partially adequate instrument. As time progressed and the end of my experiment approached, field work had worn me thin. I became a "so-so" instrument. I was hungry, filthy, uncomfortable and a bit bored. My feeling for the Indians and their interminable concern for subsistence forced me into psychological distance, as a method of self-protection. The last part of my data must be scrutinized with this in mind. The instrument of the experiment, in this case the experimenter, had deteriorated to the point of doubtful usefulness. The latter data should be either eliminated or reported with a special warming.

Again, clinical psychology and anthropology meet a similar problem. Both in their activities, the one with patients and the other with informants, are not only experimenters, they also are part of the investigating tools. The psychological and anthropological field investigators like all others are not totally obpective observers; they are deeply human and carry this quality into their observations. The human error is a limiting factor and a magnificently enriching variable at the same time. How to decrease the variable, human error, while keeping intact the richness of observations born in subjectivity is a source of grave concern in clinical psychology. The best solution known, at present, consists of the examiner possessing considerable self-knowledge. He must know his subjectivity to the last detail in order to supply curbs in one instance (human error) but to let himself go where his intuition leads him using a semiconscious trail or inquiry. Considerable training is given to the clinical student in self-observation and self evaluation. To my knowledge this aspect of training is not stressed in an anthropologist's background. The freedom of controlled subjectivity has little in common with the state of freedom from neurotic pain which can be gained by undergoing an analysis or some form of psychotherapy. The freedom gained in psychoanalysis may, but need not result in free-flowing, controlled subjectivity. The goals of psychoanalysis are determined by the analysand's individual needs which often do not coincide with the personality criteria required of a good anthropological fieldworker. It would represent a fascinating and formidable task to create a special form of analysis designed to alter only those aspects of a worker's personality which come into play in the field work process, while leaving the rest of his personality unchanged.

Short of this improbable type of analysis, other means can be used to insure that the person in the field is suited to his task. Religious groups selecting candidates for missionary work use psychometric and psychological examinations in order to avoid field work failures. Similarly, the budding anthropological student should be guided, with the help of psychological selection procedures, into the most suitable area of his functioning as determined by his needs and capacities. Anthropologists, like all other people, should use their neuroses to best advantage and fight them in their analysis only when their problems become crippling or painful.

An anthropologist with a pleasant and socially approved neurosis, which manifests itself by his need to talk often in a friendly way with a lot of people and thereby to receive many silent demonstrations of affection, perhaps, should not punish himself too hard by doing anthropological field work under conditions of social isolation. Only when his gregarious self is also in need of doing field work under conditions of isolation will he get into emotional difficulties. This unhappy anthropologist may have two choices. He may wisely decide to become a theoretician or he may decide to undergo an analysis to deal with his conflicts. He may come out of the analytic process with the insight that he has cumbersome, infantile aspirations; therefore, give up field work, or he may emerge with his neurotic gregariousness reduced to the point where he could tolerate field work (which is different from enjoying field work). Any number of other solutions to conflicts may be dealt with successfully in his analysis; he may turn out a much happier man, but still unable to tolerate the rigors of field work. There are many anthropologists who have the "right" kind of neuroses qualifying them to go into field work and to enjoy it. The problem is one of *selecting*, not one of *creating* a right type.

Related to the above point of individual tolerance is the problem of interviewing techniques. At first, when interviewing Indians, I tried to be exact in my formulations, requiring from them exact responses. I believed not unlike an experimenter sitting in an experimental room in the university facing an experimental rat or a college freshman. This search for exactness yielded highly unsatisfactory results; my subjects were "cold," and so was I. Only gradually did I realize that I was behaving contrary to my own training. When examining a non-middle class subject, I know how to make him feel comfortable. I know how to make my subject be free and trusting. I know by his posture when he relaxes; his speech tells me by its hesitation when he has conflicts; his pauses fill in the story; his breathing is smooth when he feels at ease. When I used my tried-and-trusted knowledge of therapeutic interviewing on my informants, the results were startling. I suddenly knew what went on in an interview. From the subject's physical reaction, I often knew whether he liked or disliked a particular question — whether the answer was given with hesitation or in full cooperation. In evaluating the experimental responses, I knew more than the translator's report. I felt I had knowledge as to the reliability of the response.

Anthropoligsts would benefit, it appeared to me, from training in what is known to the clinical psychologist as non-verbal communications. Perhaps it would be worthwhile to recommend opening courses in interviewing techniques to interested anthropologists. The purpose of the introduction should not be to instruct the dynamics of unconscious verbal behavior, but rather to familiarize the student with the telling signs of content above and beyond the spoken word.

As an afterthought it occurred to me that I know of no anthropological field report stating at the beginning the emotional attitude of the fieldworker toward his experiences and physical surroundings. It may be valuable to know how good the instrument was when reading its product.

When interviewing my first subjects, I behaved in the quaint and genteel ways of a middle class professor. I demonstrated my good upbringing by sending my interpreter ahead of me into a tent in order to inquire whether my presence was acceptable. In short, I behaved the same way I would back home — I first inquired whether or not my presence would be disturbing. My translator behaved rather oddly to my experience. He never knocked at the tent; he never coughed, announcing his presence. He usually walked into a tent, took off his shoes and came directly to the point: "A white man-doctor wants to talk to you — ask you questions." After a few days of poor returns in terms of interviews, he took me aside and told me that the Indians thought I was haughty. Then interpreted my "politeness" as an attitude of "they were not good enough." I changed my ways, adapted my interpreter's ways and got used to walking into any tent directly without preliminaries. It worked well. Repeatedly I was told by the Indians that I was the first white man ever to come into their tents. It made my work easier.

From many anthropologist friends I have been told that they carefully refrain from going *after* their information, that they rather wait for their informants to come to them. Thusly, the data yielded by the informants, they assure me, were less subject to bias because they were given voluntarily. Relying only on volunteers introduces a bias into the sample of informants. The informant who is motivated for one reason or another to talk to the white man often represents an exception to the many who want to have no dealings whatsoever with the white man.

Should one go after informants thus biasing his data; or should he sit back and wait for informants thus biasing his data? Clinical psychologists face a similar problem in dealing with a patient, a source of a great deal of research information. Provided the clinical psychologist uses only data from patients coming voluntarily to him, he will bias his sample. Many individuals demonstrate the same or similar symptoms as our patients do but they are never contacted by psychologists. Some clinical psychologists, recently, have carried their investigations into the community, interviewing individuals in their homes. Such investigations have been difficult because of the subjects' resistance to yield psychological information about themselves. The data, therefore, may represent considerable resistance related bias. Psychology, as well as anthropology, deals with the problem of data and sample bias.

There was one thing I strongly felt: none of the tests that clinical psychologists use are of any value in the investigation of non-westernized cultures and/or individuals. It would lead too far to indicate here but a few of my objections.

It is generally accepted that intelligence tests cannot be applied across cultures. It is equally accepted that the assumtions underlying the concept of personality differ from culture to culture. As all personality tests are based on one personality theory or another, which in turn is validated in our culture only, it follows none of our present personality tests can claim validity outside of our culture. The few attempts to revalidate personality tests in a non-western culture have been fairly successful. To my knowledge no such attempt at revalidation of the Rorschach Test has taken place prior to its use by anthropologists and/or psychologists in their specific field work.

Let us take as an example of revalidation problems the concept or projection itself. When we present an ink blot to a subject in our own culture and we say to him: "Tell me what you see in it, what it reminds you of, what it makes you think of" — we are implying a number of conditions. People in our culture are used to make-believe, they are trained in making differentiations between real and unreal. When a subject says: "It reminds me of a bat," the examiner and the subject "know" it is not a real bat. Both know that they have in common a frame of make-believe, tacitly assumed by all members of our culture, that something in the ink blot is like a bat which is not a real bat. We must recall that some primitive people reacted at first to a projected movie by throwing objects at the villain on the screen.

It appears as if the assumption — "We know that something is not real, but we both can act as if it were" — cannot be made outside our own culture. Some individuals in our own culture act as if the bat were a real bat. We, therefore, conclude that such a person in our culture is deviant. We infer that the individual fails to deal properly with "as if" conditions. This, in turn, we interpret as ego deficit. If many individuals in our culture respond with bat to a given ink blot, they all do so in reference to an "as if" condition. If many individuals in another culture respond with bat, little inference is permitted until we have established that the "as if" condition is part of their response. If it is not, and there is some evidence to support this belief, the response "bat" means and implies different interpretations than the ones we can make in our own culture. The Rorscharch can be used as a projective test if the condition "as if" can be assumed to exist in the testing situation. There are other basic assumptions which must be verified before the Rorschach Test can be used in a different culture.

On its own, we all know that the Rorschach Test is not a reliable instrument. The test is only as good as the individual who interprets it. The good interpreter brings to his task a vast knowledge of noncodifiable Rorschach information. He registers not only the subject's responses, the time delay, but also his comfort, or anxiety while responding. The examiner who has tested many subjects has knowledge of clinical cues, of pathological insertions, of disturbed response sequences and many other indications of why the subject reacted in a given fashion. All the data which the examiner receives and evaluates, which are not part of the Rorschach response proper, are culture laden to such a degree that they cannot be used as and to interpretations. If these are not used on the other hand, the Rorschach is reduced to being a questionable source of information.

On the danger of being called overconservative, old-fashioned and what is worse, anti-Freudian, I must say that I don't believe that psychology can offer any personality theories which would have meaning outside our own culture. A paper destined for publication in a psychological journal will deal with this problem more exhaustively. Let me present here one sample of my doubts.

I observed that nearly all Indians giggled at the mentioning of the word "angry" — but they had no difficulty describing and discussing angry behaviors. In our own culture, individuals who have deep feelings about "angry" usually don't like to talk about it. Here we had a group of people who do not like to be angry but who, at the same time, do not mind talking about it in a rather relaxed and easy manner. On another occasion, I observed two Indians fighting. Many Indians surrounded them. Assuming that the group experienced repressed fears and anxieties about the overt aggression, according to our personality theories, they should have reacted in a predictable manner. The group should have manifested signs of repulsion, excitations and, consequently, should have attempted to prevent the fighting. Neither or these proved to be the case. The Indians watching the fight took little action. They thought in general that fighting was not a good idea; it disrupted relationships, created bad conditions, but they also recognized that fighting did occur and could not be avoided. What cannot be avoided cannot be condemned, they seemed to reason. I could not help but think that the Indians consciously disliked aggression, saw no sense in it. Nowhere could I find evidence that they denied or repressed their aggression.

Theirs was an adaptation to aggression resulting in genuine indifference to it, something to be avoided if possible. I speculated a bit about this and how it possibly could have come about that some investigators inferred that the Indians' apathy was related to their repression of hostility. Yes, the Indians are apathetic and there are many reasons for it: economic, social and cultural. The Indians dislike aggression. Aggression and apathy often are related in our own culture.

Let us take a look at this phenomenon in our own society. The problem of aggression is of central importance in our society. (Is it central in Indian culture?) Individuals cope with aggression in a variety of methods, i.e., counter-aggression, flight, retreat, acting out; the aggression can become self-directed and many more coping methods are known. Should an individual for intrapsychic or environmental reasons be unable to deal with his aggression, he presumably represses these feelings. They now emerge as hidden aggression, depression, reaction formations, sublimation and other defensive maneuvers. Apathy is related to depression and thusly often encountered as a symtom in individuals who have problems with aggression. Could it be that observers, having noted few indications of overt aggression among the Indians, having also noted their apathy, concluded that the two symptoms were related to each other, aggression being the common denominator? If so, the observers simply superimposed the dynamics of individuals in our culture upon observations made in another culture. The observers, born and bred in western white culture, cannot grasp that different cultures developed personality dynamics different from their own. They cannot see and accept that in a given Indian culture aggression represents a peripheral phenomenon which can be adjusted to with indifference, avoidance and caution.

To the western white observer the word apathy is the proper descriptive word because it represents an aspect of *his* adjustment armamentarium. In our culture the optimal solution consists of compromise between opposing conditions; few indeed are the situations which are absolutely unalterable in our society. The Indians, on the other hand, are surrounded by what appears to them as unalterable conditions. The possibility of adaptation by accepting the source of the frustration as an unalterable condition, to be lived with, is alien to our way of thinking and, therefore, not adequately stressed. Similarly, it is my personal observation that when Indians refrain from aggression, it does not represent an unconscious process, and it would have to be an unconscious process to permit *interpretation* like regression, reaction formation and others. It could be equally substantiated that the Indian's attitude toward aggression is a conscious one. The ego may have evaluated aggression as a useless, panic-creating, society-disruptive factor. The ego, therefore, may be formed in such a way as to consciously avoid aggression-laden situations. Other cultures in history held similar views.

What is regarded as a first step in the direction of understanding personality dynamics is the necessity to make observations which are *not* based on any assumption stemming entirely from our own. Perhaps observations should be made by investigators who have had special training in psychological theory and its limitations. Even then it will be difficult to reduce the built-in cultural blinds but intellectual insight can be used to advantage to pierce the blind spots of perception. Field observations made by clinically trained investigators, capable of differentiating between conscious and unconscious processes, between expression and projection, between reaction formation and direct reaction to a situation, will yield a new and different understanding of foreign cultures. It is not too surprising that when western cultured field investigators (untrained clinically) present their collected material to western cultured psychologists (clinically trained), they will end up agreeing that some of our personality dynamics can claim to have universal validity. Whether they do or not is a question we will only answer when differently trained observers will yield less biased results.

Anthropologists and psychologists share the problem of their "boundedness." The anthropologist, in dealing with other cultures, is bound by his own cultural structure; the psychologist, in dealing with individuals, is bound by his own personality determinant. The two disciplines are interdependent in their need for freedom from bias. The psychologist has some tools to make the anthropologist a better instrument for gathering data; the anthropologist, on the other hand, can tell the psychologist about his bias in constructing theories of personality. Unfortunately, this necessity for cooperation is far from accepted and/or practiced.

It was the fact that doubts were created in my own theories that I think was the most fruitful result of my field work experiences. Parenthetically, my data bore out clearly the cultural hypothesis of my experiment.

Working in the field I now know what culture means when an anthropologist talks about it. The power of the subjective experience of this concept, by living it, is overwelming. All clinical psychologists would profit from having experienced cultural differences. There must be other and less cumbersome ways to experience culture than to live among Indians. This is a hope born out of my personal discomfort in doing field work. Intellectual enjoyment and satisfaction can mitigate but not undo hardship. I salute with respect my anthropological brethren who go out and do field work as a matter of course (without complaining about it in their publications).

Psychologists and anthropologists need to get together, not only because they share interests but because they have common purposes and problems. I do agree to the division of labor: let the anthropologist go out and get the data in the field, have the psychologist keep company with a rat to convert observations into theories. Let both of them have their pleasures and pains — but let them get together in their data and problems.

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Notes on Great Whale River Ethos *

BY JOHN J. HONIGMANN

AND IRMA HONIGMANN

THEORY AND METHOD

In *Culture and Ethos of Kaska Society*[1] ethos is defined to cover the "emotional aspects" abstracted from artifacts and behavior (i.e., from culture) together with the inferred motivational states in which those aspects are assumed to be rooted. In other words, an attempt is made to explain the emotional quality of an activity, thought, or artifact in terms of psychological drive theory, the drives themselves being regarded as acquired in childhood and through later learning.

Two considerations suggest the advisability of a slightly different as well as simpler conceptualization of ethos. First, there seems to be no great difference between the so-called emotional aspects of culture and the quality of culture which Kroeber would group under the heading of "style"[2] or what

* Field work among the Great Whale River (P.Q.) Eskimo was done during the summers of 1949 and 1950. The aid of the Wenner-Gren Foundation for Anthropological Research is gratefully acknowledged.

A paper similar to the present one was completed in 1954. It was accepted for publication by a social psychology journal, whose permises were destroyed by fire shortly thereafter. The manuscript was lost, a fact which we did not learn until the late summer of 1958. Instead of resubmitting the original version, of which we possessed a carbon copy, the first-named writer decided to make certain revisions incorporating his recent thinking concerning the ethos concept.

[1] HONIGMANN, John, J., *Culture and Ethos of Kaska Society* (Yale University Publications in Anthropology, no. 40, 1949), pp. 13-14.

[2] KROEBER, Alfred L., *Style and Civilizations* (Ithaca, Cornell University Press, 1957).

Weakland calls "form"[3]. That which an ethnographer observes when attending to the emotional aspects, style, or form of socially standardized cultural content is a *quality* attached to certain data. This quality, which recurs from one social situation to another, is apprehended in relatively direct fashion[4]. Expressing it verbally, therefore, is apt to be a difficult and, from the standpoint of subsequent verification, hazardous task.

As already indicated, the quality called style, form, or emotional tone may be referred to one or more underlying motivational states. Here the second of the two considerations referred to becomes relevant. In themselves motives, needs, or drives inferred as underlying the emotional aspect of culture possess slight explanatory value. To ascribe the non-aggressive, highly deferent character of Kaska social relationships to a dominant motivation of deference adds little or nothing to the original characterization unless the motive is in turn related, via a hypothesis, to some independant (or antecedent) condition[5]. Reflection indicates, however, that in many cases such hypothetical relationships can be traced even while ignoring the intervening motivational variable. One then deals directly with the quality or emotional aspect, not with the motive. That is to say, the form or quality of cultural content is related hypothetically to some independent factor. This procedure we propose to follow whenever possible in the present essay. Eliminating motivation from an analysis of ethos simplifies the problem of verification. A subsequent observer may still have difficulty in ascertaining that the particular quality previously designated is indeed present. But in order to complete the test of reliability he does not also have to make an inference to the same psychological drive that was specified earlier.

[3] WEAKLAND, John H., Family Imagery in a Passage by Mao Tse-Tung (*World Politics*, vol. X, 1958, pp. 387-407; pp. 389-390.) Margaret Mead early called attention to the importance of studying the highly implicit qualities associated with behavior. See her paper "More Comprehensive Field Methods" (*American Anthropologist*, n.s., vol. XXXV, 1933), pp. 1-15.

[4] SCHAPIRO, Meyer, "Style." In Kroeber, A.L., Ed., *Anthropology Today* (Chicago, University of Chicago Press, 1953).

[5] BOLLES, Robert C., "The Usefulness of the Drive Concept." In Jones, Marshall R., Ed., *Nebraska Symposium of Motivation* (Lincoln, University of Nebraska, 1958).

What has been said should not be construed as a rejection of motivational theory or the concept of character structure. For certain problems in culture and personality research these have proven to be very valuable analytical tools. But we suggest that often characterization of cultural syle (we prefer to continue using the word "ethos") can be done without having direct recourse to motivational constructs.

Although we shall attempt to relate certain features of the ethos of the Great Whale Eskimo to particular aspects of child rearing in the community, obviously we did not study the early socialization of our adult subjects. Field work confined itself to paying attention to current socialization routines. It is assumed that similar routines obtained during the childhood of our adult subjects. On the basis of this assumption we relate, *in the form of hypotheses,* certain stylistic features of adult behavior to contemporaneous child rearing routines. Strict and unbiased testing of such hypotheses would seem to demand either (A) predicting adult response tendencies from early socialization sequences alone, or (B) studying adult personality and from these data retrospectively predicting the course of early socialization. To some extent, but in far from controlled fashion, both types of hypothesis-testing entered into the preparation of this paper. For example, nursing among the Great Whale River Eskimo means that the child is breast fed only in response to a brief persistent demand. The session of suckling lasts not for the 10 - 20 minutes recommended by American pediatricians[6] but one or two minutes at a time[7]. If the child still frets when removed, it usually receives another brief turn at the breast. This pattern suggests the prospective prediction that fretting and complaining will constitute established techniques of coping in adult Eskimo personality. Search of our data and memories produced no support for that hypothesis. Usually, however, we worked from two directions while inter-

[6] SPOCK, B., *The Pocket Book of Baby and Child Care* (New York, Pocket Books, 1946), p. 34-35.

[7] HONIGMANN, John J., and HONIGMANN, Irma, "Child Rearing Patterns among the Great Whale River Eskimo" (*Anthropological Papers of the University of Alaska,* vol. II, 1953, pp. 31-50).

preting our data. That is, we tried to match certain qualitative patterns of adult behavior with certain known features of bringing up children. When this matching utilizes hypotheses which have previously been advanced in psychology or anthropology we venture to speak of contributing toward the confirmation of such propositions. Is there any need to point out that our psychogenetic formulations are tentative? That is, we offer hypotheses only and expect that all of them may not be supported. Experience in culture personality research has indicated very clearly that it is most difficult to formulate viable connections between adult culture patterns and learning situations in early and later childhood. When statistical techniques are employed in such endeavors, correlations tend to be small. Or else, the probability that the finding is not due to chance remains dissappointingly low. Apparently anthropology still lacks the key for dynamically relating later-life to early-life culture patterns. One solution that some students have adopted in the face of this difficulty requires paying attention to emotional or stylistic *congruence* between early parent-child and later adult-adult sequences of interaction. We have not chosen to follow this method. Instead, we propose to examine how the Great Whale Eskimo ethos is learned in the formative years of life and is later supported by other personality variables and cultural arrangements. Little attempt is made to describe systematically early socialization routines. The reader desiring a comprehensive description of childhood events among the Great Whale Eskimo is referred to another publication[8].

Six more or less overlapping stylistic features of Eskimo culture have been abstracted for analysis in this paper. The behavior of this community is characterized by:

A. A frank, friendly, genial, and rather spontaneous demeanor in interpersonal relations, to which is related, prior to marriage especially, an easy relationship with members of the opposite sex and a capacity to form deep emotional attachments.

[8] *Ibid.*, p. 75.

B. A confident and optimistic approach to at least the ordinary problems of existence.

C. A narcissistic idealization of the self along with a strong feeling of personal responsibility for one's actions.

D. A relatively quick vulnerability to hurt and to frustration that may be related to a capacity for empathy.

E. Rejection and avoidance of aggression.

F. Flexibility with regard to many procedures (but not to the point of disorderliness or undependability) accompanied by a relative absence of magicality.

ANALYSIS OF THE ETHOS

A. *Friendliness.* In contrast to Indians of the Northern Forest whom we have studied, the Great Whale Eskimo in interpersonal relations do not appear aloof and constricted, not even in their relationship with strangers. The smile, so often commented upon by visitors to the Eskimo, is well-nigh habitual. Games allow the release of some hostility, but in all but small boys it is a camouflaged by a smiling exterior. An easy risibility further symbolizes the quality we are seeking to express. Friendliness links the sexes. Men and women play ball together from childhood. Even mature adults continue occasionally to take part in the ball game. All this is in sharp contrast to the separation of the sexes customary among northern forest Indians, among whom considerable tension or confusion marks intersexual contact except, of course, in certain kin relationships. The Eskimo also readily and closely identify with partners in social interaction. An example of the deep attachments formed and also of the capacity to reveal feeling came when one of us bade farewell to an elderly informant who wept quietly as he shook hands. Some shyness toward strangers, particularly whites, certainly exists but in degree it is less inhibiting than among, say, the Cree-speaking Indians of Great Whale River. A young man introduced to a new role when asked to guide two white sportsmen to a fishing site exhibited, according to our journal, "the utmost complacency, reminding one of Mary when she first took [her] job here. Whatever confusion he feels is not obvious and one is tempted to see him as the most poised of individuals." Summing up these and other data, the Eskimo in interpersonal relations are emotionally expressive with behavior marked by geniality, that is, friendly openness. They adapt smoothly to new people, maintaining considerable outward poise. (We do not imply that the Eskimo is never confused about what strangers expect of him.)

The Eskimo child is explicitly taught to wear the genial smile that identifies him to visitors of the Arctic. But, we hypothesize, his friendly interest in people extends back further than to the age of learning to smile. The relationship of Eskimo parents to children may be called "complementary," indicating that each partner in the relationship plays a different role "and the two roles are conceived as complementing each other[9]." The baby in the Eskimo household is a cynosure, a focus of pride and pleasure, but he is also considered to be helpless and dependent. Therefore, it is felt that he deserves nurturance and devotion. The parents' and relatives' adoration and pride become componets of this attention. The early demonstrative affection and devotion develop a confident, friendly approach to the world that never becomes wholly dislodged. Furthermore, in the day-to-day example of adults, older children witness an imitable frank and open demeanor. There is not the tense, suspicious reaction to strangers with which the Cree Indian child grows up and that is reinforced by frightening accounts of "white trappers," *Otcipweak* (strangers), and "bogeymen." The strong emphasis placed on sharing unequally distributed resources gives further encouragement to warmth in interpersonal relations. Widely ramifying kinship ties must also be counted on in seeking to understand the emotional quality we describe, together with the small size of the group. The latter allows opportunity for considerable personal knowledge of neighbors to develop on the basis of which behavior may accurately be predicted. Toilet training, while relatively firm, is gradual and

[9] Mead, Margaret., *Male and Female* (New York, Morrow, 1949), p. 64.

gives no evidence of being traumatic. Education is not a process in which the young person learns demands that are beyond his tastes or capacities to perform. Discipline tends to be permissive rather than restrictive. All of these patterns we suggest, are part of a syndrome of child-rearing procedures capable of bringing out the quality of friendliness. Significantly, the illegitimate and orphaned suffer by comparison to their more normally reared peers. Not only are they reared differently but in two or three cases of which we have information their spontaneity seemed limited, although that was by no means invariably the case.

The popularity which the Eskimo enjoy among Eurocanadian and American traders and travelers can be related to their genial charm as well as to the compatibility of an easy and cheerful responsiveness with Euroamerican tastes. Administrators, missionaries, and traders in the James Bay district — an area including the southern shores of Hudson Bay and the coastal settlement of James Bay — nearly unanimously profess greater fondness for the Eskimo than for the Indians. It has been suggested that "when children are lumped together as all inferior in status or strength to adults of both sexes, then sex differences are minimized."[10] The Eskimo parents' complementary relationship to young children involves such a disregard of sex. The other term in the equation also characterizes Great Whale River Eskimo social relations. Around puberty, Eskimo parents mildly attempt to introduce some segregation of the sexes, but hardly to the strength and explicitness found among the Nunivak[11]. Sexual experience, however, is not condoned before marriage, an attitude that may owe more to the diffusion of Christianity than to the persistence of corresponding aboriginal values. However, in the almost nightly summer ball games, adolescent youngsters of both sexes wrestle, struggle, and tease with clear sexual overtones. It is, of course, also true that early experience rarely teaches the child that men are

[10] *Ibid.*, p. 75.

[11] LANTIS, Margaret, "The Social Culture of the Nunivak Eskimo" (*Transactions of the American Philosophical Society*, vol. XXXV, 1946, pp. 152-323), p. 261.

fundamentally different from women, that women are dangerous, or that close contact with men is menacing. Literature indicates the Great Whale Rivers Eskimo girl's easy relationship with men is a pattern found elsewhere in the Arctic and one that has made the Eskimo vulnerable to sexual exploitation by white visitors. Eskimo young men are, presumably, trained to respond to the girl's friendly casual approach with reciprocial behavior that stops short of sex — unless the situation invites deeper intimacy. Males who have been reared to expect that refusal or avoidance will be clearly indicated by the girl[12] may find the Eskimo girl's behavior puzzling. It is clear that American men have found it easy to seduce Eskimo women even without use of gifts or liquor[13].

Part of the Eskimo's ability to manage easily with other people is related, perhaps, to his readiness to be appeased for a slight. Accepting a gift that is designed to erase hard feelings neither promotes shame nor signifies weakness but cements a disrupted relationship and gives pleasure. Possibly the utility of appeasement as a technique of interpersonal relations is related to an early environment in which parents (especially the mother) are deeply reluctant to allow the baby to manifest prolonged discomfort. Fretting quickly brings some response: a brief session at the breast, distraction by pointing out some interesting feature in the environment, or a ride upon the mother's or sister's back during a walk outdoors. Accustomed to being appeased the child becomes an adult who is able to appease and expects appeasement.

B. *Confidence*. Illustations of this quality include the confidence with which young men set out to hunt small game or to fish. An informant who had received a seriously incapacitating internal injury to his knee hopefully expected to be able to travel by kayak and to be able to hunt seal from that vessel.

[12] MEAD, Margaret, "The Application of Anthropological Techniques to Cross-National Communication," (*Transactions of the New York Academy of Sciences*, ser. II, vol. IX, 133-152), p. 139.

[13] See, for example, MARSHALL, Robert, *Arctic Village* (New York, Literary Guild, 1933), pp. 265, 268-270.

Also in point here is the unprotesting manner in which the men in the community once accepted an administrative order denying them flour and sugar in family allowances in favor of ammunition[14]. About that incident one of us has written: "One generalization concerning Eskimo character thereby received some confirmation, namely, that the Eskimo accept their fate with little distress and without protest." We would now express that idea somewhat more positively. But attention must also be called to the uneasiness voiced by one man who said: "The ammunition is no good if there is nothing to shoot. You can always eat flour." Underneath apparent optimism ambivalent attitudes sometimes lurk. Serious illness provokes some helplessness that in turn is probably related to the slight development of therepeutic techniques in Eskimo culture. There is also a readiness confidently to solicit the help of somebody more capable — like the manager of the Hudson's Bay Company or his wife. Little tendency to postulate terrifying supernaturals exists. Formerly the *tungait* menaced man but now they are probably no longer evil; one informant thought that perhaps they too have learned the teaching of the missionaries. In fact, Christian teachings appear to have considerable confidence-promoting value. Optimism represents a covert behavior pattern which also reveals the quality we call confidence. Optimism is probably supported by the custom of communal sharing as well as by other cooperative relationships.

Genetically speaking, the Eskimo's confidence would seem to be traceable to the reassurance, devotion, and affection that go into the child's early experiences. Although the feeding pattern does not encourage long suckling, there is not the slightest danger that the baby's persistent demand for food will ever be ignored. Mother and young baby are rarely separated, hence the child is readily able to control his environment by simple vocal techniques. Mouth play is abundant, freely tolerated and may compensate the child for the brief sessions at the breast. Learning that he can get pleasure by his own efforts further

[14] HONIGMANN, John J., "An Episode in the Administration of the Great Whale River Eskimo" (*Human Organization*, vol. X, no. 2, 1951, pp. 5-14).

encourages the emergence of optimism. In the absence of traumata occasioned by abrupt weaning, emotional rejection, or conditional love, the individual rarely has his grounds for optimism shaken in the early formative years. The child learns of his parents' reluctance to hurt him and to leave his needs unattended. Psychological depressions occur among the Eskimo and sometimes lead to resignation (see E), but this behavior may be considered as reflecting a limitation of the security system rather than as being in contradiction of the stylistic feature we call confidence.

C. *Narcissim.* The brief cool summer and long winter make warmth an important requisite in clothing. Eskimo men appear to value clothing for little beyond its function of covering the body to insure warmth and other practical advantages. Less than women do they seem to desire cleanliness in clothing. Women not only value the cleanliness of their garments but also dress with the intention of augmenting their social selves. They use dress to idealize themselves. What may be called a narcissistic preoccupation appears further in girls' and women's use of simple ornaments, their careful hair braiding, and readiness to use make-up if it becomes available. Quite possibly, men more than women obtain narcissistic satisfaction through activity, including dancing. Hunting, especially when successful, helps a man to idealize himself in the eyes of the community. Through his own responsibility he has achieved a socially desirable act that also enhances his conception of himself. Women's traditional roles do not function in this way. The narcissistic tendency, as it appears in feminine dress and adornment and in the feeling of achievement stemming from successful performance of certain males roles, constitutes, like friendliness, another point at which the ethos of the Eskimo culture approaches values in Euro-american culture.

The genetic *anlage* of this stylistic feature of behavior may lie in the frequent approval and praise with which the child is remarked. Eskimo youngsters win praise in the family for achievements which they can control as well as for responding in expected fashion to stimuli imposed by adults. For example, the mother expresses pleasure when a young child contributes assistance to the household. She also praises lavishly when she adorns him with a new garment. Approval and praise (like the encouragement given to learners in the Euroamerican middle class) probably function to encourage effort and optimism. They may also encourage reactions in which the self is used as an instrument possessing social-stimulus value.

For other sources of the responsibility which we subsume under narcissism, we look to other aspects of the early social environment. For example, the organized games of the people place a great deal of stress on achievement, or on getting something through, show of skill. Such play would seem to encourage the attitude to which we refer. It has been hypothesized that a sense of individual responsibility is a function of socialization in independent nuclear families[15]. Life in a self-standing nuclear family, in other words, may help the individual develop a heightened awareness of himself as a distinctive person, who possesses a predictable stimulus effect in interpersonal relations. The Great Whale River Eskimo, like many other Central Arctic Eskimo, possess this pattern of social structure. The correlation between nuclear family organization and the display of an individualized sense of personal responsibility, therefore, tends to be confirmed by our data.

D. *Touchiness.* The picture of the Eskimo as friendly, optimistic, and confident in his own resourcefulness, although manifested in speech and other acts, must be modified sufficiently in order to accommodate somewhat contrasting empirical data. Sibling jealousy early conflicts with a confident, friendly style of interpersonal relations. In later life, resentment and easy vulnerability to hurt are other behaviors which bear the same stamp.

The Eskimo quickly reacts to hurt and frustration. He has been trained to enter social relations with manifest friendliness showing in his person. Presumably he also expects to be accepted by persons with whom he comes into contact. Sym-

[15] MEAD, Margaret, "The Family in the Future." In Anshen, R.N., Ed., *Beyond Victory* (New York, Harper, 1942).

metrical role behavior in the community generally reinforces these expectations after the model of a regenerative circle. When, however, occasionally the expectations are not fulfilled, then equanimity quickly collapses. A child reacts to a sign interpreted as rejection with a ready flow of tears. Later, as an adult, he will withdraw from similar stimuli with shame. A widow accused of illicit sex relations with a married man in the comunity took to her tent not to make a public appearance all summer. She actually stayed in her dwelling most of the day for two months. Her alleged lover remained at his coastal camp, visiting the settlement only briefly. Sibling jealousy and aggression toward siblings are further indicators of what may be called low frustration tolerance in the Eskimo child. However, aggression is soon displaced as a reaction to frustration, a displacement easy to understand in view of the strong sanctions employed against any show of hostility. Perhaps related to his touchiness is the generally sensitive personality make-up. To this the Eskimo's ready empathy into *another* person's plight also belongs. People hearing or witnessing a predicament readily register their sympathy and appear to feel the other's emotion. Tact reveals something of the same quality and is employed in order to avoid hurting somebody's feelings. In our home Mary, the girl who worked for us, sought to sit in her accustomed place after serving our and her own dinner. Annie, a friend, occupied the position, however, We wondered how the situation would be handled, perceiving that the visitor was taking no move to leave. Mary touched Annie's arm, indicating a desire that Annie shift over. At the same time, however, she partly held Annie back, so that the latter would not take the gesture as a sign to leave the house. This showed considerable tact.

The Eskimo's early experience gives little preparation for, or training in, rejection and frustration. In his rearing he has been provided with few hard knocks, such as, apparently, are useful to build up some vulnerability to hurts. Our thinking parallels that of Kardiner[16], who points out that a child lacking

[16] KARDINER, Abram, *The Psychological Frontiers of Society* (New York, Columbia University Press, 1945), pp. 345-348.

experience with restrictions and accustomed to nearly unmitigated gratification will not learn strength in the face of trial. The Great Whale River data tend to confirm his hypothesis. Ethnographic material from elsewhere also tends to corroborate the proposition. Arapesh indulgence probably exceeds that which is found among the Eskimo. In the Arapesh, too, we find that the loving trust developed in the early setting becomes "an attitude that can be shattered at a blow because no blows are received in childhood to habituate the growing child to ordinary competitive aggressiveness in other."[17]

E. *Deference*. Human relations at Great Whale River, at least after age 12 or 13, are marked by an even, deferent tone and are disturbed by no serious outbreaks of aggression (an exception being repeated wife-beating on the part of one young man). Our data suggest that in southeastern Hudson Bay, readiness for violence was once no less developed than among other Eskimo of the Central Arctic[18]. The tendency persisted until relatively recent times in the Belcher Islands, lying off the coast of Great Whale River. In the absence of ready aggressive responses, tension in interpersonal relations is sometimes revealed by a forced smile — the same gesture to which so many positive associations are conditioned. If overcome in a fight or tussle while playing ball, Eskimo children also readily burst into tears, probably revealing more a response to frustration than actual pain. Sulking constituted the response to aggressive acts initiated by Indian youths in the case of one, otherwise not especially shy, 10 year old Eskimo lad. At Great Whale River, boys openly vent hostility against Cree Indian lads, who reciprocate in kind. However, adults who catch sight of children fighting reprimand the activity heartily, using some of the sharpest scolding to be witnessed in adult-child relations. The expletive "It is horrid!" is hurled at the fighting child. The strength of the inhibition which is developed appears to be pro-

[17] MEAD, Margaret, *Sex and Temperament* (New York, Morrow, 1935), p. 137.

[18] BULIARD, Roger P., *Inuk* (New York, Farrar, Straus and Young, 1951); FREUCHEN, Peter, *Eskimo* (New York, Grosset and Dunlap, 1931), and RUESCH, Hans, *Top of the World* (New York, Harper, 1950).

portionate to the intensity with which aggression is put down by adults in children. We do not, of course, suggest that the Eskimo feel no hostility. Evidence to the contrary cannot be ignored. The point, however, is that the overt expression of the hostility does not appear readily and sometimes is strenuously inhibited. A man was seen literally to turn his back and flee from a situation that raised his hostility to what he may have perceived to be a dangerous level.

In a community with so little tolerance for aggression it is not surprising to find the quality of deference expressed in the care taken not to generate aggression in interpersonal relations. One consequence of this wariness or tact is a social atomism characteristic, too, of other food-gathering people[19]. For example, in the case of a young wife-beater, considerable criticism of his conduct circulated behind the husband's back but little if any reached the man directly. The same deference appears in parent-child relations. While parents said they sometimes hit children, we only rarely observed any such manifest pattern. As a result the child cannot inconsistently learn the use of violence by being himself the target of such emotionally toned behavior.

F. *Fexibility*. By this term we refer to a relaxed mode of procedure and tolerant attitudes toward demands of living. Such a quality turns out to be apparent in many cultures that lie outside of the Euroamerican area. While not as at extreme as the disorderliness of the Aymara of Chucuito described by Tschopik[20], the Eskimo of Great Whale River reveal basically the same lack of perfectionism, compulsiveness, or rigidity. Compared to the Kaska Indians[21], however, the Eskimo might

[19] HONIGMANN, John J., *Culture and Ethos of Kaska Society* (op. cit.), p. 266; also see HALLOWELL, A.I., "Some Psychological Characteristics of the Northeastern Indians. In Johnson, Frederick, Ed., Man in Northeastern North America (*Papers of the Robert S. Peabody Foundation for Archaeology*, no. 3, 1946), p. 207.

[20] TSCHOPIK, Harry Jr., "The Aymara of Chucuito, Peru" (*Anthrological Papers of the American Museum of Natural History*, 1946, vol. XLIV, pp. 137-308), p. 184.

[21] HONIGMANN, John J., *Culture and Ethos of Kaska Society* (op. cit.), p. 271.

be classified as relatively orderly, particularly with regard to the value they set on the cleanliness of clothing or dwelling.

We were impressed with the Eskimo's flexibility as manifested in a display of passive resignation toward disasters; for example, toward food shortages brought on by several days of bad weather that prevent hunting. In his vague theories of causation, also, the Eskimo reveals a lack of systematic, orderly thought. Misfortune or illness is often ascribed neither to sorcery, fate, nor God's will. Events just happen — a theory compatible with the proclivity for resignation since as an explanation it does not constitute a premise from which a coping technique — either threat or prayer nor anticipatory hope for a change of luck — might follow. With regard to questions of motivation, the reply, "Don't know," is characteriscally used by informants. People do not pursue hypothetical explanations. Casualness is also manifested in that the people seem to posit few absolutes and adhere to few necessary connections between phenomena. In other words, they are relatively non-magical[22]. The emphasis on Sunday as a day of rest is the strongest exception to this pattern.

Flexibility is absorbed in an easy-going, often indirect, noncompulsive learning process which is localized in a social environment that reflects no rigid organization. Yet, in some respects, the Eskimo household is ordered and such ordering cannot fail to be observed by the growing child. Food, for example, does not lie available to free access but is served only at particular times of the day by the mother. Toilet training is not severely inculcated, but a system of elimination does exist and the child early comes to realize that there are appropriate places for elimination. While mother resign themselves to a child's will in many things, they rarely do so with regard to aggression of selfishness. These facts suggest that flexibility in the child's early environment is not as high as it theoretically might be. The degree of flexibility in Eskimo culture is correlated with relatively, but not wholly, permissive circumstances

[22] WILSON, Godfrey, and WILSON, Monica, *The Analysis of Social Change* (Cambridge, University Press, 1945), pp. 89-90.

obtaining in early life. Early permissiveness, in turn, would appear to be predictably associated with relatively simple community organization and a subsistence system based on food gathering plus trapping.

SUMMARY

Great Whale River Eskimo culture is characterized by interpersonal warmth and friendliness, optimism toward ordinary problems of existence, as well as by idealization of the self and display of a sense of personal responsibility. Behavior reveals limited tolerance of social rejection or frustration; that is, interpersonal relations are marked by touchiness. A quality of deference also pervades social relationships. In many procedures people adopt a relaxed and casual manner of approach to problems of living.

These stylistic features of behavior, it has been recognized, overlap. It is assumed that they are also functionally dependent on certain interrelated patterns of child rearing as well as on other designated patterns of socially standardized behavior.

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Two Attempts at Community Organization among the Eastern Hudson Bay Eskimos

BY ASEN BALIKCI

The decrease of the useful fauna in many parts of the Arctic has created new problems in the acculturative process of the Canadian Eskimos. In areas where Euro-Canadian establishments were developed, inter-ethnic tensions and various disfunctional processes have appeared. Almost everywhere the dependence of the natives on the local trading posts has increased. Many solutions to the "Eskimo problem" have been suggested. One hears about relocating south the native populations of the Canadian Arctic, or concentrating them around a few mining centres, or rapidly developing various cottage industries. More conservative observers feel that the Eskimos, with the help of some Euro-Canadian agencies, may develop new forms of ecological adaptations and establish satisfactory symbiotic relations with the Euro-Canadians. It appears, however, that these objectives can be realized only if new and superior forms of social organization are established among the Eskimos. This paper attempts a description of such efforts in two Arctic locales, Great Whale River and Povungnituk (Eastern Hudson Bay.) The data presented here have been collected during two summer trips to these regions, in 1957 and 1958 respectively.

Great Whale River

Three major acculturative phases can be established for the Eskimos living in the area between Cape Jones and Richmond

Gulf. (For a detailed history of the region, see Honigmann, 1952, and Balikci, 1957).

1) The first and middle part of the 19th Century were characterized by contacts of varying intensity with the whalers and the Hudson's Bay Company outpost at Little Whale River, leading to the gradual increase of trapping, the introduction of metal tools and firearms with some important changes in the sealing, fowling and caribou hunting techniques.

2) During the last quarter of the past century until the 1950's, the Hudson Bay Company's activities in the area grew in importance aiming mainly at the stabilization of the fur trading system. While whaling for commercial purposes came to a stop, trapping was steadily developed. The large caribou herds were soon exterminated with the help of the newly-acquired firearms. This brought to an end the seasonal inland migrations of Eskimo hunting groups. The Eskimos came to rely increasingly on the various commodities imported by the Hudson's Bay Company, such as foodstuffs, clothing, tent material, tools, etc. The role of the local trader was important; he controlled the debt system, encouraged trapping and provided occasional employment. Conversion of the Eskimo to the Anglican faith meant a quick rejection of the traditional beliefs and curing, and adoption of the Eskimo syllabary.

3) During the 1950's, extensive changes took place in the ways and the organization of Eskimo groups. Several new agencies entered the area and paramount among these were a construction enterprise and officers of the Department of Northern Affairs and National Resources. In the following paragraphs, a picture of the relations between the Eskimos and the various Euro-Canadian agencies during the summer of 1957 will be presented. This description is essentially limited to the symbiotic relations of the two ethnic groups. Contrary to Keesing's cultural definition of a symbiotic situation which refers to borrowing of traits where two cultural systems maintain contact (Keesing, p. 388), the term is used there to denote all social interactions not motivated simply by the desire of sociability.

Prior to the development of an important construction program at Great Whale River, the Eskimos traditionally inhabiting the coast between Cape Jones and Richmond Gulf lived in small isolated groups usually at the mouths of the larger rivers. Most, if not all the members of such a group, were bound by kinship ties, and in several communities a headman, usually an older and successful hunter, enjoyed considerable prestige. He directed the seasonal movements of the camp and had the last word in the use of the boats. Among some groups, however, no hunter with outstanding prestige was to be found. In winter the trading post was visited regularly for supplies and for a few weeks in summer all the groups concentrated at Great Whale River. Trading was conducted individually by each hunter and never trough the headman. In the camps important patterns of collaboration existed during some collective hunts and in sharing food. While members of different groups visited occasionally, no significant intergroup collaboration existed, except during starvation periods when food gifts were made.

During the summer of 1955, numerous employment opportunities were made available to the Eskimos at Great Whale River. This led to the concentration of all regional Eskimo groups near the trading post. A large number of natives were hired individually as laborers. Soon after, for various reasons, many of the laborers abandoned their jobs and returned to hunting. Thus the large Eskimo community was split into two occupational groups: laborers and hunters. The marked differences in income produced some mild tensions between the two occupational groups. Hunting decreased rapidly and fresh meat became scarce. Certain prostitution practices and some epidemics allegedly brought by the Euro-Canadians were instrumental in creating a feeling of hostility directed against the white workers. Soon a Northern Service Officer was appointed to cope with the many administrative problems originating in this complex acculturative situation. He acted as an employment agent and an accountant for the natives, distributed relief and family allowances, and was kept informed about the activities of the hunters. He was faced mostly with individual cases, and when dealing with collective problems, addressed an assembly of most, if not all adult, male, Eskimos. The R.C.M. Police constable looked after the actual distribution of the relief, kept the vital statistics, and also acted as an adviser to the natives on many occasions. The male nurse regularly visited the Eskimo tents tending the sick and encouraging individual or family sanitation practices. The Anglican missionary conducted services in the local church supposedly for the whole Eskimo community. He also practised small group and individual preaching, while trained catechists conducted group prayers in the tents.

The increased monetary income of the native laborers stimulated the sales for cash at the counter of the trading post. Trading became increasingly anonymous and was conducted generally between the Eskimo women and the store's clerks. The contacts between the trader and the Eskimo men became less intense, the debt system was partially abandoned, and the status of the trader was considerably diminished.

To sum up, these interactions reveal that the Euro-Canadians considered Eskimos either as individuals or as family heads, but very rarely as representatives of a larger grouping. Two examples may suffice. The headman of a group of hunters tried to obtain some ammunition on relief on behalf of his people only to be accused by the latter of not sharing the supply equitably. Another headman voiced an opinion regarding the activities of the prostitutes, considering himself as spokesman for the whole community. A further investigation revealed that he was representing only himself together with a small circle of relatives. Cases like these led the administrators to call all the adult Eskimos when problems of collective interest were to be discussed. The headmen, however, continued to exercise their traditional and limited authority over their groups in regard to the use of the boats and the seasonal migrations in the case of the hunters.

Different factors led the Northern Service Officer to encourage the creation of an elected Eskimo Community Advisory Council. Principal among these factors was the necessity to institutionalize a system of communication between the Administration and the Eskimos. While the former could easily call on individual natives, the discussion of collective problems was a difficult matter and it was hoped that a Community Advisory Council could more easily sound the natives and bring already prepared answers to the Administrator. Furthermore such a Council was to evolve gradually into a sort of self-government, able to take decisions concerning all the Eskimo groups at Great Whale River. Thus a superior level of socio-political integration was to be achived in the native society through the democratic process. For some time, however, the functions of the Council were to remain only advisory and consultative. The administrator retained all executive authority.

Summoned by the Administrator, the Eskimos cheerfully elected six individuals, (three headmen, one interpreter, and two natives of some prestige.) Of the six only two had any authority over their groups. One group was represented by two councillors while several others had no representative at all.

After its constitution the Council had been apparently consulted on various subjects such as control of dogs, choice of foods to be served in the mess hall, over-time pay, and income tax (Bond, VIII). The main test took place in a few weeks, on a problem of dog food distribution. The Council was asked by the Administration to organize the equitable distribution among all the Eskimo families of the considerable kitchen refuse thrown out daily by the Euro-Canadian mess. The refuse was used as dog food. The mess managers have been throwing the refuse in the community dump where it was individually colleted by the Eskimos according to their needs. The managers felt that it was preferable to have the refuse removed directly from the kitchens at regular hours. The Council decided that P., a representative of a large group, should be in charge of the removal and distribution of the refuse. P. immediately chose two of his relatives for the execution of the task. Soon after, the Eskimos noticed that, instead of sharing the dog food among all the tents, the two men stored it in the camp of P. Objections followed and the Council modified its decision. Two groups were to perform the task alternately. For a few days the program operated smoothly. Two boys of each group regularly removed the refuse. Soon, however, one of the boys went hunting and did not arrange for replacement. His companion was unable to proceed alone and the whole operation was first temporarily, and later, completely dropped. The Community Advisory Council never met again. What were the factors instrumental in the failure of the Council?

a) The Council was created first in a shallow frame, without any specific function or a long range program of activities. Only a quick explanation by the Administrator preceded the election of the councillors, and a year later the few Eskimos who still knew about the Council were unable to give any information about its objectives. These informants perceived the Council as an artificial creation of the Administrator who called the sessions at will and only whenever he had a problem to discuss.

b) The problems the councillors had to deal with were either not understood or of little interest to them and the solutions were obviously to the advantage of the Euro-Canadians.

c) The Administration was apparently ignorant of the absence of any symbiotic inter-group relations among the natives. The Great Whale River Eskimos show essentially a family level of integration with some important forms of collaboration at the group level such as sharing food and the use of imported boats. Division of labor, except along sex and age lines was unimportant and, whenever present, due to Euro-Canadian influences. The dog-food distribution program called for a certain amount of economic specialization and inter-group collaboration. It was naive to believe that this could occur within the Eskimo community without the supervision of an Euro-Canadian agency. The program might have succeeded if certain remuneration of the dog-food collectors was envisaged. No such measures aiming at the development of some organic solidarity among the Eskimos were considered.

d) The loose character of the headman-group and the headman-headman relations were equally misunderstood by the Administration. Traditionally the headman derived his prestige from his superior hunting and trapping skills and his knowledge of the ecological conditions of the country. In a wage economy he found himself to be just another laborer, and sometimes not the best one. In the acculturative situation prestige became attached to income and possessions and the already loose authority of the headman suffered. As one informant described a headman, "P. speaks only for himself." The recent unequal distribution of wealth produced new jealousies and tensions between individuals, headmen, and different occupational groups. This reduced the possibility of inter-individual and inter-group collaboration in the Council and in the community.

e) The representation of the groups in the Council was unequal. This stimulated arrangements benefiting one group, led to the objections of the other groups, and discouraged planned collaboration.

Povungnituk

The history of the Eskimo-European relations in the Povungnituk area falls into four interactional periods.

1) The first period, roughly similar to the corresponding phase at Great Whale River, lasted a quarter of a century longer. The influence of commercial whaling on the ways of the Povernitormiut remains uncertain. The trading post at Little Whale River was visited only once a year, in spring, and trapping did not seem to have been intensively practised. Firearms largely replaced the bow and arrow and the long spring harpoon late in the 19th century. The conversion to Christianity of all the Eastern Hudson Bay Eskimos took place almost simultaneously around the 1880's.

2) The second or adaptive period covers the end of the last and the first three decades of this century. A trading post was established in the area, trapping was intensified, the decrease of the caribou herds produced some important changes in the seasonal migrations and increased the dependance of the natives on the trading post. The role of the trader became very important. He interfered in the migrations, encouraged trapping, acted as petty justice officer, gave medical treatment, helped during starvation periods, procured some wage employment, and through the individual debt system, controlled the prestige structure of the community.

3) The third period was one of crisis and ended with the beginning of the present decade. The almost complete disappearance of the caribou together with the decrease of the other useful species reduced considerably the supply of fresh meat. Trapping was not sufficiently productive, it failed to increase the cash income and stimulate consumption of larger quantities of imported foodstuffs. Periods of hunger became more frequent. Messianic beliefs appeared, reflecting an atmosphere of crisis due to the decrease of the caribou and the seals.

4) This last period of social reorganization extends to the present time. It is characterized mainly by the concentration of almost all Eskimo groups around Povungnituk Bay, the development of soapstone carving for commercial purposes, the increase of government relief under various forms, the creation of group accounts, and the arrival of a Roman Catholic missionary. In the following paragraphs, a more detailed picture of the symbiotic interactional system at Povungnituk will be presented, drawing from data collected by the author during the summer of 1958.

The natives remembered vividly the difficult pre-war and post-war years when people died of starvation in the northern and central camps. Trapping provided them with a very irregular income, pelts worth only $3,222 were sold to the trader in 1951-52, while two years later their total income from trapping amounted to $39,361. An Euro-Canadian informant remembered that as late as 1955, the natives were beging for food and seemed intensely preoccupied with the possibility of starvation. In recent years, family allowances and relief to the needy families and individuals distributed by the Federal Government through the local trader, made available to the natives larger amounts of ammunition, clothing and imported foodstuffs. The economic conditions of the community, however, were radically changed by the development of carving for commercial purposes. Stimulated by the trader, the Eskimos increasingly devoted themselves to the carving of small animal or human figurines of soapstone representing mostly traditional subjects. All carvings were priced and purchased by the trader. Soon carving became the dominant activity in the area, the total sales of carvings increased from $740 in 1952-53, to $38,000 in 1955-56. Povungnituk came to be known as a "carving community." The Eskimos were freed from the necessity of intensive hunting. They did not have to remain any longer near their traditional hunting and trapping grounds and settled around the post. Most of the groups had headmen and formed compact clusters of tents. As at Great Whale River, the group collaboration patterns inherited from the traditional and the fur-trading periods, continued to function (food sharing and common use of boats under the supervision of the headman). A reasonable supply of fresh meat had to be assured and most of the poor carvers continued trapping. Povungnituk Bay was not a good hunting or trapping area and the hunters and trappers had to travel long distances, in summer or winter, to their traditional hunting grounds. This necessitated a larger number of sea-worthy boats powered by motor engines and an increase in the amount of supplies necessary to the trapper along the trail. Individual hunters could not easily face these new expenses. A collective solution to the problem had to be found. The local trader suggested and succeeded in establishing the following program. The seven or eigth Eskimo groups who lived in relative isolation along the coast prior to their concentration at Povungnituk, were to establish group accounts at the trading post. A fraction of all earnings was to go to the group accounts. With the funds thus accumulated some larger goods for collective use such as boats and new engines could be obtained. Only elected group leaders had authority to spend the funds. No community, supra-group account was envisaged.

The system worked successfully. Following many informal consultations, many of the headmen who enjoyed some prestige among their kinsmen and others were elected. Soon several canoes, new engines and larger motor boats were acquired. The contribution, freely accepted by the group-members, fluctuated from 5 to 35 cents for each dollar earned by the members, depending on the importance of the goods to be purchased. The system was flexible and allowed for larger or smaller savings. Under the control of the group leader, some funds were used to purchase gasoline, ammunition and supplies for the needy trappers. Needless to say, through their supervision of gasoline purchases, the control of the group leaders over the boats was strengthened generally. The Eskimos had to ask permission before using the boats. Soon tensions occurred between headmen and some hunters who were refused supplies from the group accounts. Another headman who monopolized the use of a boat was bitterly criticized. Similar tensions brought the splitting-up of a group with two separate camp accounts and the formulation of plans for acquisition of personal canoes by some Eskimos. It should be noted, however, that these tensions never endangered the functioning or the existence of the system which was perceived by the natives as highly beneficial. While minor purchases were left to the discretion of the group leader, any major decision concerning the group accounts was taken after many informal consultations and discussions. No formal meetings of the group members took place. This fitted well into the traditional Eskimo pattern of group decision making. No explicit attempt was made by the Euro-Canadians to organize a community council of group leaders. Occasionally the group leaders were called to meetings and asked to answer questions of interest to most of the members. It is relevant that numerous Eskimos were present at these meetings and replied individually. Outside the sphere of group accounts the group leaders had little or no authority over the other Eskimos, except in traditional situations.

Conjointly an attempt was made by the trader to establish an exchange system between carvers and fishermen. The system was intended to provide a compensation for the continual gifts of fish by the hunters to the specialized carvers. The fish was to be purchased by the trader and later sold to the carvers. Thus the traditional sharing patterns were to give way to formalized inter-Eskimo trade relations, reflecting the recent occupational differentiation within the community. The system functioned successfully for some time under the close supervision of the trader. When his control was relaxed, the natives quickly reverted to their traditional sharing practices.

A third organizational project was invented by the local Missionary. Under his leadership a carver's association was founded. This new cooperative intended to sell carvings directly in Southern Canada, with the help of the Mission. Some intermediaries could be eliminated and the profit margin to the Eskimo increased. The members showed a great interest in the new institution, assembled once a week and collectively priced each carving. All carvings were examined, discussed, and individual styles compared. Good sculptures brought considerable prestige to the carver and some competitive attitudes appeared. A successful carver, himself a group leader, was chosen as president. He failed to exercise any authority and during the group deliberations behaved as an ordinary member. Odd jobs connected with the carving trade were regularly remunerated with the funds of the association.

In the following paragraphs, the factors which contributed to the success of the previously described organizations will be listed.

a) The new forms of collaboration connected with the group accounts were at the group level with some similarity to the interactional patterns developed during the fur-trading period for the acquisition and use of imported vessels.

b) Both the group accounts' system and the carvers' association were created in view of very specific objectives. Far from constituting shallow structures, they existed only in function of their aims which were executive rather than consultative. Furthermore, these objectives were perceived by the Eskimos as highly beneficial to them.

c) The group account system was flexible. It allowed for the formulation of either immediate or long-range projects and variable contributions. The initiative for these projects came from the Eskimos and not from the Euro-Canadians. The natives felt that the group accounts were "their business" and took keen interest in them.

d) The objectives of the new structures (acquisition of boats, providing the needy families with imported supplies, sale of carvings at reasonable prices) were of central importance to the Eskimos, solving problems of economic relations with the

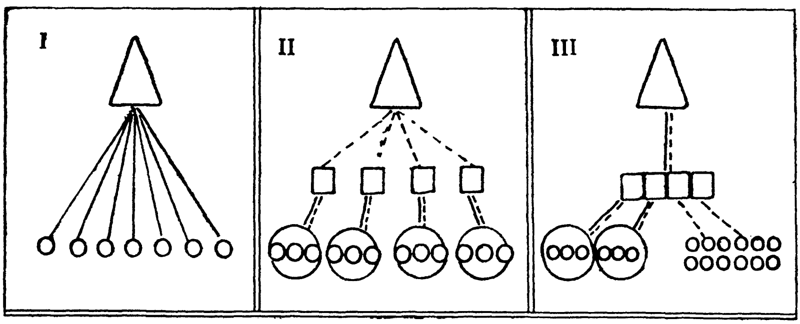
Euro-Canadian agencies that the simple traditional structures of Eskimo society could not face.

e) The group account system was suggested to the Eskimos at the end of a period of crisis when the natives were willing to accept changes. It was a timely answer to their pressing desire to acquire imported goods.

f) The new structures were not only initiated but continually supervised by the Euro-Canadians. The trader, however, controlled the system as such, without interfering with the projects formulated by the Eskimos.

Conclusion

Three recent organizations described above can be presented very schematically as follows: (the charts refer to the actual functioning of the systems and not include normative elements.)



△ Euro-Canadian Initiator

□ Eskimo headmen

O Eskimo members of organizations

– – – – Consultative relations

——— Superordinate-subordinate relations

Comparing the three, it is clear that they represent systems of increasing complexity. System I is simple enough, if reflects superordinate-subordinate relations between the Euro-Canadian initiator and the members of the Sculptors' Association. System II indicates the Trader's rôles as community accountant and stimulator, the general absence of formalized inter-leaders' relations, and both the consultative and superordinate-subordinate relations between leaders and group members. System III is characterized essentially by a formal grouping of community representatives, which is a step in advance over system II and represents an integrative effort at a higher level, and by the consultative relations between some councillors and a large portion of the Eskimo community.

Certain normative statements concerning the functioning of these organizations can be listed:

1) It is assumed that the Eskimos of these two communities cannot create by themselves any supra-group structures or spontaneously establish formal group-headman relations. Thus the catalyctic activity of an Euro-Canadian organizer seems necessary. A superior type of social organization may succeed better if, during an initial period, it is closely supervised and stimulated by the initiator. Within the established framework he should allow for the formulations of projects by the natives. His role should become increasingly permissive, without completely abandoning control over the functioning of the system. The nature and the duration of the control exercised by the initiator should depend on the complexity of the system. Complex organizations necessitate a strong leadership and supervision.

2) New structures, representing a superior level of sociopolitical organization, have better chances of success when created for the achievement of specific tasks and when these tasks are well understood by the natives and concern some vital local problems, perceived as such by the Eskimos. Paramount among these, appear the Eskimo-Euro-Canadian economic relations. Thus the trader seems to be an ideal organization initiator. He is the local link between the Eskimos and the Euro-Canadian economy and the "vertical" integrator of the community. (The initiator should direct his efforts both along the "vertical" and "horizontal" organizational axis as distinguished by Dr. R.L. Warren.)

3) Before establishing the chart of the projected institution, the homogeneity of the traditional groups, the headman-group relations and the recent occupational differentiations should be

realistically analyzed and taken into consideration. In localities where a higher level of socio-political integration is necessary, some gradual development of the planned institution may be appropriate. Thus after the natives have been thoroughly familiarized with system II, it is possible to conceive the successful creation of a formal grouping of headman characteristic of system III.

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Note sur la notion d'idéologie

PAR MARCEL RIOUX

Récemment, Léon Dion[1] publiait un excellent article sur l'idéologie politique et l'analyse fonctionnelle de la réalité socio-politique; les remarques qui suivent ne veulent être qu'un prolongement de cet article. Nous ne voulons pas discuter l'hypothèse que Dion formule mais examiner brièvement la nature de l'idéologie et les relations que les phénomènes qu'on appelle idéologiques entretiennent avec la culture et la structure sociale.

Si l'on passe en revue un certain nombre de définitons de l'idéologie, on se rend compte qu'elles oscillent entre deux pôles: scientifique et existentiel. Parmi les positions les plus cohérentes, axées sur le pôle scientifique, on peut citer celle d'un certain nombre d'anthropologistes et celle des logiciens positivistes. C'est ainsi qu'Ayer[2], qui illustre la position des logiciens, propose d'utiliser le critère de vérifiabilité pour distinguer entre jugement de réalité et jugement de valeur; toute proposition qui ne peut être vérifiée scientifiquement est une pseudo-proposition. Comme l'idéologie contient toujours les deux espèces de jugement, elle irait, si l'on employait le critère de vérifiabilité d'Ayer, rejoindre la métaphysique au rayon des choses insignifiantes. C'est l'opinion d'Arnold Brecht: "Je ne vois pas comment la science ne pourrait pas ne pas appeler "idéologies" tous les principes, les évaluations et les maximes concernant la fin et les moyens et qui ne sont pas des théories scientifiquement établies"[3]. L'idéologie équivaudrait, selon ces critères, à toute production mentale qui ne serait pas scientifique. Quant à la conception que se font de l'idéologie certains anthropologistes, elle est aussi large et aussi

[1] DION, Léon, "Political Ideology as a tool of Functional Analysis in socio-political dynamics: an Hypothesis" *The Canadian Journal of Economics and Political Science*, Vol. XXV, No. 1, Feb. 1959, p. 47-49.

[2] AYER, A.J., *Language, Truth and Logic* passim (London, 1936).

[3] BRECHT, Arnold, in *Social Research*, Vol. 24, No. 4, p. 484.

cohérente que celle des positivistes sans toutefois se fonder sur un critère de vérifiabilité. Fried écrit: "... les idéologies expliquent à l'homme pourquoi il existe, d'où il vient et ce qu'il devrait être; elles lui expliquent le fonctionnement de l'univers, quelles relations il doit entretenir avec le milieu et les fins de la culture."[4] Honigman énonce à peu près les mêmes idées en ajoutant que l'idéologie comprend aussi le monde des valeurs. Du point de vue de ces anthropologistes, la question de la vérité scientifique ou existentielle de l'idéologie ne se pose pas; ce pan de la réalité doit être décrit, aussi objectivement que la technique et l'organisation sociale; l'idéologie se présente comme une catégorie aussi vaste que celle de superstition, au temps où les anthropologistes étaient ethnocentristes.

Les sociologues, d'autre part, emploient généralement le mot d'idéologie pour désigner une sorte d'illusion, de jugement biaisé. Merton, par exemple, écrit: "Aussi longtemps que l'attention a été centrée sur les déterminants sociaux de l'idéologie, de l'illusion, du mythe ou de la croyance non-vérifiée, la sociologie de la connaissance ne pouvait pas naître."[5] L'idéologie étant un des thèmes les plus importants de la sociologie de la connaissance, qui étudie les facteurs existentiels qui influent sur la connaissance, les sociologues se sont préoccupés de vérité existentielle beaucoup plus que les anthropologistes. Mais il y a, entre eux, une autre différence. Les anthropologistes que nous avons cités, et la plupart de leurs collègues, s'accordent à penser que l'idéologie fait essentiellement partie de la culture; au contraire, dans les définitions des théoriciens politiques et des sociologues, l'idéologie est liée à la structure sociale; cette liaison serait même la caractéristique principale de l'idéologie. Arthur Child, suivant en cela, Manheim et Lukacs, écrit: "... nous avons constaté que l'idéologie devient possible, d'une façon ou d'une autre, quand existe une société divisée en classes."[6] Idéologie est entendue ici dans le sens où la plupart des sociologues l'emploient, c'est-à-dire un système de croyances qui se donne comme un système

[4] FRIED, Morton H., *Readings in Anthropology*, Vol. 11, p. 404, 1959.
[5] MERTON, R.K., *Social Theory and Social Structure*, p. 459, 1957.
[6] CHILD, Arthur, *The Problems of the Sociology of Knowledge*, thèse du doctorat, Berkely, Université de Californie, 1938, p. 36.

de vérités empiriques alors qu'en réalité il ne s'agit que des jugements de valeur d'une classe socio-économique qui défend ainsi, consciemment ou inconsciemment, ses intérêts de classe. Est-ce à dire qu'il n'y aurait pas de phénomène idéologique (au sens sociologique) avant l'apparition des classes sociales? Fried et Honigman parlent d'idéologie globale, de vision du monde bien individuée et différente de celle des autres sociétés; idéologie employée dans ce sens-là est presque synonyme de culture. A ce sujet-là, Sartre fait une réflexion qui va dans le même sens que ces remarques. Revenant d'U.R.S.S. où il avait assisté à une rencontre d'écrivains venant des deux côtés du rideau de fer, il écrit dans l'Express du 19 octobre 1956: "Il faut un moment d'adaptation pour comprendre ceux (les principes) des autres et l'on se trouve vraiment en présence, en U.R.S.S., de ce que j'appelle une idéologie et une culture, ce qui pour moi est la même chose." Toute culture globale est idéologique au sens restreint du terme parce qu'elle présente deux des caractéristiques principales qu'on associe à l'idée d'idéologie: aliénation et rationalisation. Toute culture est essentiellement sélective, c'est-à-dire qu'elle ne choisit de la réalité que ce qui est compatible avec son système de valeurs, ou mieux elle interprète la réalité selon ses catégories explicites et implicites; chaque culture ne donne de la réalité qu'une image fragmentaire et c'est en cela qu'on peut dire qu'elle représente au départ une aliénation. D'autre part, pour se maintenir en équilibre chaque culture rationalise ses choix pour mieux les justifier; comme dans toute rationalisation, chaque culture, pour défendre sa vision du monde, qui est toujours fragmentaire et particularisée, mêle les éléments de réalité et de valeur. Ce n'est pas seulement avec la naissance des classes sociales qu'apparaît l'idéologie mais avec l'apparition de la culture humaine.

Quelle que soit la notion d'idéologie qu'on examine, globale ou restreinte, la notion de *l'autre* tient une grande place dans la formation de l'une et l'autre espèce d'idéologie. Dans les sociétés tribales et folk, *l'autre* qui se dresse enface de *nous* global de ces sociétés peut être la nature, le surnaturel ou une autre société globale; l'idéologie globale aura pour fonction de justifier l'existence de ce *nous* global face à ces *autres*. L'hétérogénéité du personnel et des valeurs apparaissant avec la division poussée du travail (même dans les sociétés tribales, n'aperçoit-on pas un embryon d'idéologie interne dans les relations entre les sexes et les classes d'âges) *l'autre* se présente comme un groupe ou une catégorie sociale à l'intérieur même de la société globale. C'est alors qu'on commence à douter de la bonne foi de *l'autre* qui, sous certains aspects est *même* puisqu'il participe à la même culture globale; c'est là qu'apparaît l'idéologie restreinte.

Ces deux espèces d'idéologie font partie intégrante de la culture. Dion distingue entre idéologie sociale et idéologie politique sans montrer explicitement comment elles se distinguent. Pour ceux qui voient dans la réalité sociale deux dimensions, la dimension culturelle (action) et la dimension proprement sociale (groupe), tout phénomène idéologique, parce que comportant un élément d'évaluation (action) appartient, tant par ses aspects cognitifs qu'affectifs, à la dimension culturelle de la réalité. L'idéologie que Dion appelle politique et qui semble se rapprocher de notre idéologie restreinte est liée plus étroitement à la structure sociale que l'idéologie globale; d'autre part, les classes sociales deviennent des sous-cultures qui continuent de participer à la culture et à l'idéologie globales d'une société; l'idéologie restreinte, axée sur la distribution du pouvoir à l'intérieur de la société globale, reste partie intégrante de la dimension culturelle; cette participation est médiatisée par l'organisation sociale qui, elle-même, joue un rôle de charnière entre la structure sociale et la culture des sociétés globales.

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