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Sir Daniel Wilson: Canada's First Anthropologist

BY BRUCE G. TRIGGER

RÉSUMÉ

L'article présente d'abord Sir Daniel Wilson, premier anthropologiste canadien. Suit un essai de classification et d'évaluation de ses écrits: quatre volumes et environ 45 articles portant sur l'archéologie, l'ethnologie et l'anthropologie physique.

Sir Daniel Wilson was a many-sided individual who developed new interests throughout his life, without ever entirely losing sight of older ones. In Canada he is honoured as a Scotsman who came to Toronto in 1853 to take the chair of History and English Literature at University College and remained to become the second president of the college and the first president of the reorganized University of Toronto.[1] Wilson wrote at least three books dealing with British history — two of them early and the third based upon early work.[2] In the field of English literature he produced *Chatterton* (1869), a biographical study, and *Caliban* (1873), dealing with the creative imagination of Shakespeare as applied to supernatural beings. Both of them, if the charge cards in various libraries are any indication, are not totally ignored today. In addition he published a volume of poems entitled *Spring Wild Flowers* (1875) and a 254 page memoir of William Nelson, the head of the Edinburgh publishing house (1889). Although an eminent scholar, both Wilson and his con-

[1] Our biographical data come from two articles and from Daniel Wilson's own writings. The more important article is in "Review of Historical Publications Relating to Canada", 5:199-217, and includes an almost complete bibliography of Wilson's works. The second article is in "Dictionary of National Biography, vol. 62.

[2] Oliver Cromwell and the Protectorate (1848); Memorials of Edinburgh in the Olden Time (1848); Reminiscences of Old Edinburgh (1878).

temporaries regarded his primary life's work as a battle "for the maintenance of a national system of university education in opposition to sectarian or denominational colleges" (Harris, 1900:62, 90-91). Largely through his efforts the foundations were laid for the University of Toronto's future greatness. Both in his lifetime and today Wilson's achievements as an administrator and teacher are recognized and respected.

In this paper we aim to concentrate on yet another aspect of Sir Daniel Wilson — his anthropology. In addition to his "professional" writing in the fields of history and English literature, Wilson managed to publish no less than four books and 45 articles devoted to archaeology, ethnology, and physical anthropology. Half way through his life he was recognized as one of the leading prehistorians in Europe, and afterwards anthropology remained "his favourite study" as his daughter, Sybil Wilson, described it in her introduction to his last and posthumously published book (Wilson, 1892:vi).

Yet, if specific aspects of Wilson's work as an anthropologist are remembered by a few specialists, his work as a whole remains virtually unknown — and perhaps nowhere more than in Canada itself. I remember hearing Wilson's name mentioned only twice in connection with anthropology during the years I was an undergraduate at Toronto. The first was a brief aside praising his work in a lecture given by Edmund Carpenter; the second was a question asked by a don, in the course of an informal discussion in the Sir Daniel Wilson Residence, regarding the sort of work he did. I recall that after a brief silence someone suggested that Wilson's work in anthropology was generally regarded as that of a dilettante. This impression was already part of the climate of opinion concerning Wilson that existed in Toronto at the time of his death, and was one even shared by some of his friends. Since then, unfortunately, there have been no attempts to appraise the value of his work, an undertaking that would at least have some interest in terms of the history of anthropology. I will concentrate primarily upon Wilson's writings, since I have had neither the space nor the materials at hand to undertake an extensive biographical study of the man himself. After describing his writings in roughly chronological order, I will attempt to evaluate them, in terms of their historical setting and lasting value.

Daniel Wilson was born in 1816 in Edinburgh, where he was educated at the old High School and the university. In 1837 he went south to try his fortune working in London as a writer and an engraver — the two vocations being an early indication of his versatility. In 1842 he returned to Edinburgh where he continued to write with some success, although in later years he was greatly distressed by the unauthorized identification of some of his early work. At the same time he pursued a hobby that was to lead to his publication of *Memorials of Edinburgh in the Olden Time* in 1848, and eventually to his more important work on Scottish archaeology. *Memorials of Edinburgh* began, in his own words, "not with the pen, but the pencil" (Wilson, 1872:1, ix). For some years many of the quaint and the historic buildings of old Edinburgh had been disappearing, as growing prosperity led to a desire for improved sanitation and "urban renewal". During these years Wilson explored Edinburgh making pencil drawings of its old buildings. When such buildings were torn down he also tried to record whatever of note might be found in the course of demolition. Wilson's most interesting find was the rediscovery of St. Margaret's Chapel which was built by the daughter of Edward the Confessor and today is a prized attraction of Edinburgh Castle. For many years it had stood unrecognized, and at the time of Wilson's investigations it was being used as a store room for gunpowder (*Ibid,* 1872:2, 128; 1863:2, 405). In 1848 Wilson published a large number of his best sketches accompanied by a rambling account of interesting and picturesque events in the history of the city. The text forms a fitting accompaniment to the pictures, which are invariably of high quality both technically and artistically, and today are an important source of information concerning old Edinburgh. Wilson retained his interest in Scottish history throughout his life, and after coming to Canada was delighted to find and publish for the first time an accurate description of the crozier of St. Fillan, an ancient Scottish relic which had been brought to this country in 1818. When Wilson found it, it was in the possession of a farmer, Alexander Dewar, whose forefathers had been given charge of it at Bannockburn.[3]

[3] The crozier, described as "a priceless relic of the Celtic Church" was returned to Scotland by the Dewar family in 1877 and is now in the National Museum of Antiquities of Scotland, Edinburgh, (Wilson, 1863, vol. 2, p. 477).

Memorials of Edinburgh was reprinted in 1872, and Wilson published a revised edition in 1891. In addition he wrote a shorter and more popular account of the Edinburgh of his childhood and the past, which was published in 1878 (see 2). In this latter book Wilson tells how his interest in the civic history of Edinburgh led to his interest in prehistory. The exploration of Edinburgh revealed not only medieval buildings but much earlier material. Wilson had been interested in comparative anatomy for some time and in 1850 read a paper to the British Association entitled "Inquiry into the evidence of the existence of primitive races in Scotland prior to the Celts".[4] Concerning this preliminary study of skeletal differences he later noted: "It is amusing now to recall the undisguised incredulity with which a theory was then received which has since met with universal acceptance as a mere truism necessarily involved in greatly more comprehensive assumptions" (Wilson, 1878:2, 140-141).

Meanwhile, Wilson was pressing ahead with a much more ambitious scheme. In 1846 he was elected a Fellow of the Society of Antiquaries of Scotland and in 1847 he became an honorary secretary of the association. In this capacity he began to visit sites and correspond with interested people throughout Scotland in order to prepare the first "consistent and comprehensive" study of Scottish archaeological remains. The result was his publication of the *Archaeology and Prehistoric Annals of Scotland* in 1851, eight years before Darwin's *Origin of Species* and before Boucher de Perth's paleolithic finds were accepted in Britain.

It is important to remember that at this time serious prehistoric archaeology scarcely existed except in Scandinavia (Daniel, 1950:54). It is therefore significant that in this book Wilson squarely broke with the traditions of British antiquarianism, which was dominated by an intense concern with classical remains and a sterile desire to identify all other archaeological remains as the relics of historically known peoples. By way of contrast with his own approach, he described previous work in Britain as follows:

> A very simple theory sufficed until very recently, for the classification of all... British antiquities. Whatever was rude and barbarous...

[4] Brit. Assoc. Rep. 1850: pt. 2, 142-146.

> was native and Druidical; whatever manifested skill, invention, or any progress in the arts was Phoenician, Roman, or Danish! Britain was tacitly assumed to have been sunk in the lowest state of barbarism, until humanized by the bloody missionaries of Roman civilization. But such ignorant assumption will no longer suffice (Wilson, 1851: 1, 353).

And more pointedly:

> The name of Dane has in fact for centuries been one of those convenient words which so often take the place of ideas, and save the trouble and inconvenience of reasoning (Ibid.:1, 353).

Wilson correctly saw the work of Christian Thomsen as "the foundation of archaeology as a science" (*ibid:*1, chap. 1), and an escape from the impasse in which the "fog and flood" archaeology of the previous century had resulted.[5] Following Thomsen's general scheme, he divided his book into four sections devoted to the ages of Stone (Primeval), Bronze (Archaic), and Iron, and to the Christian period. Within each section individual chapters were given over to various classes of data — tombs, fortifications, dwellings, weapons, vessels, ornaments, art, religion, and domestic life. In the latter part of the book historical data supplemented the record of material culture. While keenly aware of the difficulties which a lack of historical data imposed, Wilson nevertheless felt that the study of archaeological assemblages offered the hope of being able to learn at least something about the more perishable aspects of culture.

> We need not despair of learning somewhat of the early Caledonian, of his habits, his thoughts, and even of his faith, when we are able to refer to so many specimens of his handiwork and inventive design; and retain some relics of his ruined temples, and abundant illustrations of his sepulchral rites (Wilson, 1863:1, 486).

While Wilson postulated an "eastern cradle of our race" (*ibid:*introduction, and p. 300) (in his early works this was probably as much a biblical as an historical concept) and noted that there was trade between Britain and the Mediterranean in early times, he was strongly opposed to attributing all cultural development in the north to influences from the south. He even suggested that bronze might have been invented in Britain where copper and tin occur together (*ibid:*1, 310). While later research makes

[5] For an explanation of this term see Daniel, GLYN, The Idea of Prehistory, pp. 13-37.

this last suggestion highly unlikely, it is worth remembering that the smelting of iron was probably an invention of Anatolia rather than of the Middle East.

While using the three-age scheme, Wilson carefully noted that there were considerable differences in detail when the material from Scandinavia was compared with objects from the corresponding period in Scotland. He concluded that the development of culture in each area was sufficiently different to render individual sequences worthy of study in their own right. Although he did not attempt to define culture units, as opposed to stages, within Scotland during the prehistoric periods, he was able to do this with the material culture of the Danes and the Anglo-Saxons.

Scotland, unlike France, had been covered by glaciers and its earliest artifacts are of Mesolithic (Azilian) age.[6] Hence Wilson's material offered no prospect of dramatic discoveries about man's earliest past such as characterized the work of Boucher de Perthes, and he was unable to catch sight of the great antiquity of man, although he went further in doing so than did most British scholars of the day. He noted that in Scotland prehistoric man and some kinds of prehistoric animals had existed together, and that there had been notable changes in the shoreline since that time. But as yet, this deeply religious scientist was unwilling to contradict publicly the Bible as the "most commonly received chronology" (Wilson, 1863:1, 24, 227) — a point we shall return to later. Even as late as 1863 he apparently did not fully appreciate the difference in age between his own Mesolithic finds in Scotland and the Brixham cave discoveries in England.[7]

In *The Prehistoric Annals of Scotland* Wilson also showed something of the practical turn of mind that he was to turn to such good account in Canada. The proposals which he makes suggest some of the lines along which his career as a prehistorian might have developed had he remained in Scotland. For example, Wilson noted that the comparison of pottery might form the basis of a more sensitive chronology than any then known, but that at

[6] A Short Guide to Scottish Antiquities, (3rd ed.), Edinburgh, National Museum of Antiquities of Scotland, 1962:6.

[7] Note, however, that by this time Wilson had not been in Britain for almost a decade (Wilson, 1863: vol. 1, p. 122).

the time there was not enough material available to make such a study possible. He requested readers to save the pottery which they dug up and not to throw it away as was the usual practice among antiquarians of the time. He also urged the systematic display of artifacts at the British Museum, organized on the basis of Thomsen's system, and advocated the repeal of the Treasure Trove law in Scotland, which he demonstrated had resulted in the destruction of large numbers of artifacts made of precious materials. Data which he had collected revealed that less than ten of some 200 gold relics known to have been found in Scotland could be located at the time of his study. He also solicited a letter from Jens Worsaae describing the benefits which had resulted from the repeal of a similar law in Denmark.

Although in only a few years the novelty of Wilson's research was to be pushed into the background by the upheaval resulting from the theory of biological evolution and the discovery of the paleolithic, his work was important in helping to establish prehistory as a science in Britain and in replacing the antiquarian traditions of the past. Although proper techniques of archaeology remained to be developed by General Pitt-Rivers and Flinders Petrie, Daniel Wilson was one of the foremost European archaeologists of his time, and was a man well qualified to take part in the exciting events that were to transform European archaeology in the years to come.

Very soon, however, Wilson made a decision which was to alter the course of his life. In 1848 he had been an unsuccessful applicant for the position of librarian for the Faculty of Advocates in Edinburgh. In 1853 he was offered, and accepted, the chair of History and English Literature at University College, Toronto.

Although the rest of Wilson's life was devoted increasingly to administration and teaching, the move to Canada did not end his interest in archaeology. Nevertheless, it did change the direction of that interest. Wilson was quick to appreciate that in the wilderness of Canada there was a situation of great value to a prehistorian. For him, the pioneers who were pushing back the forest constituted a society similar in many ways to that of northern Europe in ancient times. He recalled that in the reign of David I (A.D. 1124-1153) and even later, Scotland had

been a land where beavers were trapped and their skins exported, even as they were in Canada in his own day (D. Wilson, 1878:2, 152). On the other hand, the Indians were a living example of what European societies must have been like still earlier. Contact with these Indians familiarized Wilson "with a condition of social life realizing in the living present nearly all (he) had conceived of in studying the chroniclings of Britain's prehistoric centuries" (Wilson, 1863:1, xv). This aided him in the preparation of the second edition of the *Prehistoric Annals of Scotland* (1863). Moreover, in the general opening up of the New World, Wilson saw an unparalleled opportunity to study both the interaction of the peoples of Europe and Africa with new environments, and the processes by which different cultures came into contact in the widest variety of different circumstances. He foresaw that a study of contemporary changes in the New World would result in a more general understanding of the nature of culture contact, race mixing, and adaptation to environment and that this in turn would aid scholars to understand the culture history of the Old World.

> It has been my fortune to become a settler... in the midst of scenes where the primeval forests and their savage inhabitants are in process of displacement by the arts and races of civilised Europe. (This is one of) its great transitional eras: with its native tribes and its European and African colonists in various stages of (cultural and physical) mutation, consequent on migration, intermixture or collision. Man is seen (on the grandest scale) subject to influences similar to those which have affected him in all great migrations and collisions of diverse races. Some difficult problems of ethnology have (hereby) been simplified in my own mind; and opinions relative to Europe's prehistoric races, based on inference and induction have received striking confirmation (D. Wilson, 1876:1, 4-5).

Throughout his life Wilson eschewed extreme theories of migration and diffusion, and like his contemporary, Adolf Bastian, although in a less extreme form, he tended to attribute widely separated similarities to the operation of common human abilities under similar conditions. He became convinced for a number of sound, though general, reasons, that the culture histories of the Eastern and Western Hemispheres were for all intents and purposes separate. His first reason was the great variety of Indian languages and their lack of any obvious connection with those of the Old World. Secondly, despite the general racial similarities

between Indians and Mongols, the amount of physical variation among New World peoples also seemed to suggest a rather long separation. Finally, neither the archaeological record nor the available ethnology suggested close connections (*ibid,* 1876:1; 12, 13). Most alleged similarities seemed to Wilson more likely to have resulted from parallel development. Significantly, Wilson never put much trust in the presumed geological proofs of the antiquity of man in America that were being put forth in the second half of the nineteenth century in an effort to emulate the discoveries being made in the Pleistocene deposits of the Old World (*ibid,* 1876:1; 47-63). Wilson noted correctly that many of these finds had proved false and that most of the rest were very doubtful. As a result of these observations, Wilson was able to conclude that the study of New World prehistory gave "promise of disclosures replete with interest in their bearings on the secrets of the elder world" (*ibid,* 1876:1, 16). With the New World as an example of independent culture development, it would be possible to detect what was inherent in the course of such development and what was unique. Thus Wilson came to feel that a study of his adopted land was of significance for his first field of interest.

Soon Wilson was devoting much of his spare time to the history and ethnography of the New World. In the summer vacation of 1855 he made the first of several difficult trips as far as Fond du Lac on the shores of Lake Superior, where he examined the Indian copper mines. He reported his findings concerning their mode of operation and the copper artifacts in the *Proceedings of the Society of Antiquaries of Scotland* and in volume one of the new series of the *Canadian Journal.*[8] Wilson realized the antiquity of the use of native copper and noted that the presence of pure copper in a natural state had allowed its use by simple hunters and gatherers without smelting — a situation that was wholly unparalleled in Europe. The culture history of the New World was thus not only independent of the Old, but in some ways not even roughly parallel. Lewis Henry Morgan was later to make the same point when he indicated that the lack

[8] For the reference to these articles see WRONG and LANGTON, op. cit., p. 212.

of domesticable animals in the New World altered the later stages of cultural development for the two hemispheres (Morgan, 1877). It is to Wilson's credit that he, a European, should have made the same observation so much earlier. On the other hand, Wilson was properly skeptical of contemporary speculations about a "lost Indian secret" for hardening and tempering copper, and he had Henry Croft of the University of Toronto analyse a number of native copper implements which were found near Brockville, Ontario. His tests, which were some of the earliest made on native copper tools from the New World, found no signs of artificial hardening, although it should be noted that this myth has persisted to the present (Griffin, 1961:x, 118-123). A fondness for laboratory work was characteristic of Wilson. Already in 1850 he and his brother, George Wilson, had analysed the metal in some bronze axes from Peru and prehistoric Europe.

In 1862 Wilson published *Prehistoric Man,* which appeared in a second edition in 1865 and was revised and enlarged in 1876. As its subtitle explained, the book was intended as an introductory study of the origin of civilization in the Old and New World and it was in fact one of the first attempts to synthesize the culture history of the New World. As such it was a worthy successor to Scotland's first important contribution to the study of the native cultures of the New World, William Robertson's *The History of America,* published almost a century earlier (Hoebel, 1960:x, 648-655). Well aware of the meagreness of his data Wilson commented "we must be content to limit speculation to the confines of existing evidence and aim at clearly discriminating between fact and fancy" (Wilson, 1865, 2nd ed.: 255).

Wilson believed, unlike many of his contemporaries but as most anthropologists do today, that primitive man has the same intellectual capacities as civilized peoples, and he refused to consider the state of a culture's development to be an indication of the intellectual ability of its members. To Wilson the savage appeared unintellectual only because much of his time was spent trying to remain alive. He noted, rather quaintly, that the wise man Job must have lived a life little better than that of most Indians (Wilson, 1876:1, 36). Man's great distinguishing characteristic was his ability to accumulate knowledge. Man alone has

an "inner world of thought by means of which he is capable of searching into the past; anticipating the future" (Wilson, 1865: 121). Although Wilson pointed out that his knowledge of recent discoveries in Europe was limited to articles which were generally available, he was by this time aware of some of the paleolithic finds. While he believed in the generally progressive nature of cultural evolution and saw man's golden age to be in the future, not in the past, he did not accept the idea of either unilineal or unidirectional evolution. He objected especially to the uncritical use of principles of biological evolution in trying to understand cultural development, holding that the latter was best conceived of as a process of learning (Wilson, 1876: 1, 36). He believed it possible for a society to pass from a more advanced to a more primitive state and indeed held pioneer life in America to be one (albeit probably temporary) instance of such regression.

Wilson, who was familiar with many of the available accounts of the Spanish conquest of Mexico and Peru, perceived that the cultures of the New World ranged from simple to complex. Although much of *Prehistoric Man* dealt with eastern North America and in particular with a classification of "Mound Builder" remains in this area, he attempted to deal with the full range of this variation. Despite his limited data, Wilson's judgment concerning the degree of culture attained by the most complex societies of the New World is strikingly modern and shows the advantages which he derived from his familiarity with Old World material. Wilson felt, as Lewis Henry Morgan did later, that Prescott and others like him had exaggerated the degree of civilization in Mexico. Indeed he referred to Prescott's book, certainly too harshly, as "an historical romance". According to Wilson his picture of Peru and Mexico had been distorted by the Spanish adaptation of native history and tradition to European conceptions, a tendency which Prescott furthered by identifying Indian institutions with those of Europe (*ibid,* 1876:1, 242). But he certainly did not agree with Morgan that the Aztecs and the Iroquois Indians of New York State were comparable, and he later openly criticized Morgan for exaggerating the degree of social and political development among the Five Nations (Wilson, 1876:1, 263-264, and 1892:174-175). Wilson maintained that the Inca and Aztec civilizations, which he saw as developing more

or less independently of one another, were more directly analogous with the early civilizations of the Old World such as Egypt and Babylonia. While he admitted that the art of these early civilizations could be equal to that of Greece and Rome, he characterized the societies as a whole as belonging to a "stage of advancement where art and even science contributed to sensuous craving for magnificent display but are of little avail for mental and moral progress" (Wilson, 1876:2, 10). Although Wilson, like most of his generation, had a strong faith in the improvement of man, he was not devoid of a sense of cultural relativism. Again and again he took pains to point out that the cruelty commonly attributed to the Caribs, Papuans, and Aztecs should be regarded as a sign of "primitive vigour". Similar behaviour he pointed out had characterized the Vikings and Hungarians in earlier times, and all too often the people who despise modern primitive tribes for their cruelty, admire much the same kind of behaviour in their own ancestors. On the other hand, an institution like the Inquisition was more reprehensible since it had been "grafted onto" an advanced society (*ibid*, 1, 247). Modern students of Andean culture history will be interested to learn that Wilson considered the numerous juxtaposed microclimates in that area to have been an important factor in the development of trade and civilization (*ibid,* 1, 343, 344). Wilson's search for parallel situations in the Old and New Worlds continually resulted in novel and interesting analogies. For example, to make the point in his discussion of the Aztec penetration of central Mexico and the destruction of Toltec civilization, he calls to mind the analagous Anglo-Saxon invasion of Roman Britain. What emerged from Wilson's investigation of New World civilizations was that, despite many important differences in the process, mankind in both hemispheres had independently tended to develop a higher civilization. Like many of his contemporaries Wilson regarded the Indians as being of east Asiatic origin, having entered the New World by way of the Bering Strait. He did not, however, rule out the possibility that some of the racial stock of the New World had reached it in early times across either the Pacific or Atlantic Oceans.

Much of volume two of *Prehistoric Man* is given over to Wilson's oldest and one of his most absorbing anthropological

interests — physical anthropology. In it is a reworking of an attack on Samuel Morton's *Crania Americana* which he had published in the *Proceedings of the American Association for the Advancement of Science* in 1857 (Wilson, 1859:2, 109-127). Morton, whom Hrdlicka was later to describe as "the father of American anthropology" (Hrdlicka, 1914:x, 508-554) had postulated one head type for all the Indians in America. In his paper Wilson produced a good deal of evidence, some of it from Ontario, to show that there was considerable variation in cranial types among the Indians of the New World.

Even more interesting were Wilson's views on race. Wilson was aware what a delicate topic this was, particularly in the United States where pseudo-scientific writers like J.C. Nott and George Gliddon were attempting scientifically to justify the institution of Negro slavery. One of Wilson's comments in a later popular publication deserves to be quoted in full since it summarizes the position which he took from the first:

> It is not easy to discriminate here between hereditary race differences and those due to particular food and habit of life. A similar difficulty has hitherto stood in the way of any definite classification of the emotional, moral, and intellectual characters of races. Some of the most confident judgments which have been delivered on this subject have been distorted by prejudice or wilful slander, as in the many lamentable cases in which slave-holders or conquerors have excused their ill-treatment of subject and invaded races on the ground of their being creatures of bestial nature in mind and morals (Wilson, 1885: no. 71, 11).

Through his personal association with the Indians of eastern Canada Wilson became convinced more than ever that the triumph of Europeans over other races was due less to "constitution" than "acquired civilization". He pointed out to his readers that while Magyars and Indians were both descended from much the same stock, the Magyars had been able to develop along with the rest of European civilization, whereas the Indian cultures had not been given such an opportunity (Wilson, 1876:2, 243).

Wilson noted that interbreeding between Indians and Europeans was very common and that in more settled areas few of the reserve Indians were pure-blooded. The frontier half-breed was often a vigorous sort and proof that racial interbreeding was not in itself harmful (*ibid*:238-298). Turning to the United

States he branded Nott's theory of the inferiority of hybrids as nonsensical and pointed out that it could be disproved merely by an examination of census data. Wilson, as a prehistorian, noted that the mingling of peoples had been the usual accompaniment of human migrations, and that the British, then at the height of their power, were a particularly mixed population. Wilson felt that prolonged government tutelage of the Indians might ultimately be harmful and looked forward to the day when the Indian and European races would mingle to form a single Canadian population. For Wilson, races were not necessarily fixed entities of great antiquity but groupings which had perished and been created in historic times as a result of mixture and segregation. Races were not only modifiable, but the cultural results of contact and blending were a positive historical force, unlike isolation, discrimination, and repression, all factors which promote "race purity".

Wilson extended the same understanding to the Negro. He pointed out to his readers that evidence of the capabilities of the Negro should not be sought solely among the Negroes in America whom the white population had excluded "from every source of intellectual or moral development" (*ibid*:325), but rather among the free Negroes of Africa. He testified that many of the latter, whom he had met in England, impressed him as being men of vigour and ability. But he further cautioned that cultural attainment was no measure of potential ability. Seemingly unprogressive races have shown considerable capacity for progress; the Arab bedouin were very primitive, yet these same people had built a great civilization after the Islamic conquests (*ibid*:1, 11, 12). Replying to the charge that the free Negroes of Haiti were incapable of self-government he pointed out that the government of France itself was not notable for its stability and that English liberty had taken many centuries to develop. He was not sparing in his advocacy of fair treatment for Negroes and branded the practice of having separate schools for Negroes in Canada and New England as a fraud. These schools were defended on the grounds that the pattern of learning among Negro children differed from that of Whites (*ibid*:2, 325).

A final section dealing with language was slimmer than the rest, but contained the interesting observations that the form of

languages was not racially determined and that New World languages were every bit as complicated as were those of the Old World. Any person was capable of learning any language and there was no evidence that grammar was not historically independent of race, as demonstrated by the variety of grammatical systems within Indo-European (Aryan). Later he observed that "to classify mankind into races on the mere evidence of languages is an intrinsically unsound procedure" (Wilson, 1885: 27).

This book, which dealt with race, language, and culture marked Daniel Wilson's coming of age as an anthropologist whose scope was at least as broad as that of Tylor or Boas. As a result of his interest in prehistory Wilson's anthropology had a decidedly historical and processual cast and in a small pamphlet entitled *Anthropology* Wilson defined the discipline as being "the natural history of mankind" (ibid, 1885:1). Nevertheless in later years Wilson applied himself to many problems that were not historical.

Some of these concerned physical anthropology. In several important papers published in the last decades of his life,[9] Wilson presented evidence to show that cranial capacity did not provide a reliable measurement of the relative ability of different races or individuals. He rejected any significance to Pierre Broca's (Broca, 1862:3, 102-106) evidence that the cranial capacity of Parisians had increased significantly since medieval times. Indeed he pointed out that the cranial capacity of Frenchmen as a whole had declined since the Upper Paleolithic. Later Gustav Retzius (Retzius, 1914: 18, 49-64) was to show that Broca's conclusions were not supported by data from Scandinavia.

A special and highly personal interest of Wilson was in right and left handedness. Wilson was himself left handed, but his training in school had given him much of the "dexterity" of a person who normally uses his right hand. Beginning in 1872 he published a series of articles in the *Canadian Journal* and the *Transactions of the Royal Society of Canada* which formed the basis of a book entitled *The Right Hand: Left Handedness*

[9] The most important is "Brain-weight and Size in Relation to Relative Capacity of Races", Canadian Journal n.s. vol. 15, 1876:177-230.

(1891). The announced aim of the book was practical — to prove "the folly of persistently trying to suppress an innate faculty" (Wilson, 1891:7-8), which he then attempted to demonstrate that left handedness was. After discussing the importance of the hand in human evolution, Wilson presented historical evidence (based on tools, paintings, and handwriting) and ethnographic data to show that while the majority of the members of any human group are right handed, a minority are not. He demonstrated that there was no foundation for the claims that some living tribes were left handed; moreover there was no evidence that such groups had existed in the past. As a result, it could be concluded that right or left handedness was not merely a matter of cultural choice, although he admitted that almost every element of culture tends to reinforce the use of the right hand and to penalize those who are left handed. In a number of experiments carried out on University of Toronto students in cooperation with W.J. Louden, Wilson determined the number of students who preferred or had a tendency to prefer one hand over the other, and with tests using water of various temperatures failed to confirm a commonly held notion that the preferred hand was the more sensitive.

He also produced some genealogical evidence to suggest that left handedness was inherited, hence biological, and concluded that handedness was probably related to the dominance of one hemisphere of the brain over the other. However, he admitted that he had not been able to gather much anatomical evidence in support of his conclusion. He argued that a natural predisposition to left handedness should not be suppressed, but that supplementary training of the right hand could develop a useful ambidexterity.

Perhaps the most disturbing problem Wilson had to deal with was raised by the new theory of human evolution. The heated discussions which surrounded this problem troubled Wilson, who was both a devout Anglican and a man who had a deep respect for scientific findings. In his earliest writings he had expressed pleasure that his materials were not such as to require deciding between the biblical chronology and the new geological one. As the theory of biological evolution itself evolved, Wilson came to see that here was essentially a scientific problem

whose resolution depended upon scientific findings. In *Prehistoric Man* he observed, perhaps half-introspectively, that "our own confused minglings of religious questions with scientific investigations will seem ridiculous" (Wilson, 1876:1, 54).

Wilson was pained not so much by the idea of human descent from animal forebears as by the use that was being made of this concept by men whose plain ambition was to weaken faith in established religion. Thomas Huxley's attempt to show that men and apes were more closely related than apes and monkeys was too overtly an attempt to view man as "nothing but" an animal. In his book *Caliban,* which is primarily a study of Shakespeare's treatment of mythical creatures, Wilson expressed some of his strongest sentiments on the subject. He attempted first of all to cool the ardour of the Huxleyites by pointing out that at that time there was no fossil evidence to back up the claim of man's descent from other primates. Until such evidence was forthcoming, he argued, "all is vain" (Wilson, 1873:7). While it is now apparent that this first argument was to prove his weakest, it is doubtful that Wilson took it too seriously, for elsewhere he grandly observed that "man is the last and best of an order of animated beings whose line sweeps back into the shadows of an unmeasured past" (Wilson, 1865:126). He also accepted the theory of evolution in general, not only praising Darwin as a most competent naturalist (Wilson, 1885: 16) but also noting that the theory itself was "the key to a thousand difficulties" (Wilson, 1873:7). Rather he was opposed to the notion that man was not a creature of a very special, if not a unique, sort within the animal kingdom. For Wilson, man was set apart by his possession of tools and reason in place of instincts and by his ability "to use words in themselves unmeaning, as symbols by which to conduct ... complex intellectual processes (Wilson, 1885:5). Man was "no longer subject to (aimless) change in a changing universe"[10] and change in "mind" was more characteristic of recent human evolution than change in "body". Today it is generally recognized that while man is closely allied to the higher primates and they are all descended from a common ancestor, man's ability to symbolize and his possession

[10] Quoted from A.R. Wallace in D. WILSON, Anthropology, 1885-6.

of culture makes him unique. As anthropologists, we can both appreciate Wilson's agreeing with Wallace that all living men are intellectually highly differentiated from all other animals and also sympathize with his anger at the dogmatism of men like Huxley — anger which at one point led him to exclaim that "infallibility has deserted the chair of St. Peter and finds itself at home on a new throne" (Wilson, 1873:6).

Despite Wilson's early and intense interest in physical anthropology his later work was far from confined to this topic. In 1882 he began to write a series of articles which he finally published as a book, *The Lost Atlantis*. It was on this book that he read proof just before he died in the summer of 1892. In it he developed a number of topics first touched on in *Prehistoric Man*. The first essay, which gives the book its rather strange title, dismisses the then popular notion of the origins of New World culture on a lost continent in the Atlantic as "speculation of unregulated zeal unworthy of serious consideration" (Wilson, 1892:5). The body of the article is a restatement of his belief in the essentially indigenous nature of cultural development in the New World, a development which was little affected even by genuine contacts with the Vikings. Long before this, Wilson had systematically examined and dismissed all of the supposed proofs of Irish, or even Phoenecian contacts which such men as Ezra Stiles, the president of Yale University, had championed.

The chapter on trade and commerce discussed archaeological and ethnographic data (much of it collected personally by Wilson in Northern Ontario), concerning aboriginal trade. He noted the early development and extent of trading among even the simplest societies and recognized that as a result a division of labour beyond that of the household could evolve at an early stage in man's cultural development. Far from sharing Rousseau's view of primitive society, Wilson saw that a man who possessed superior skills and aptitudes could acquire a fair measure of power even among hunters and gatherers.

The chapter "Pre-Aryan Man" contained the key notion of American anthropology in the years following Wilson's death, namely, that the native cultures of North America were quickly changing, if not dying out, and needed to be recorded. Other

articles include studies of the aesthetic faculty in aboriginal races, the Huron-Iroquois, one on hybridity and heredity citing additional information that race mixture was not harmful and providing much information on Indian acculturation in Canada at the time, and finally an article on brain weight. While containing less original material than Wilson's earlier works, the anthology is an indication of the continuing breadth of his interests. It expresses his fundamental belief in the unity of man, in the possession by all peoples of a common humanity, and in the capacity of all peoples for attaining the highest possible state of development. His work is nowhere blemished by the tendency of so many of his contemporaries to try to explain cultural and even linguistic differences in terms of racial variation.

To judge Sir Daniel Wilson's work in proper perspective we must consider the early development of anthropology as a whole. His first major work was published in 1851, the same year as Lewis Henry Morgan's *League of the Iroquois* and almost a decade before the great year 1859 that saw the publication of Darwin's *Origin of Species* and the acceptance of the findings of De Perthes and Pengelly. *Prehistoric Man,* written entirely in Canada, appeared in 1862, only a year after Maine's *Ancient Law,* Bachofen's *Mutterrecht,* and Tylor's first book, *Anahuac.* The latter's *Primitive Culture* was not to appear until 1871 and Morgan's *Ancient Society* not until 1877. To appreciate fully the extent of Wilson's achievement, it must be remembered that circumstances made it impossible for him to devote full time to anthropological research. Even the *Prehistoric Annals of Scotland* was "a work of leisure time from less congenial pursuits" (Wilson, 1863:1, 29). This book was still considered as a standard work on Scottish prehistory as late as 1900 (Wrong and Langton, 1901:202). In Britain it marked an escape from a bankrupt antiquarianism and an advance in the development of scientific prehistory.

His second major work in anthropology, and the one I believe his most important, was published only nine years after he came to Canada. *Prehistoric Man* dealt with all the major fields subsequently recognized as part of anthropology — ethnology, prehistory, physical anthropology, and linguistics. Wilson showed a keen interest not only in the prehistory of the New

World but also in its living peoples and the social and cultural changes that were overtaking them. From an original interest in culture history, Wilson gradually became interested in problems of culture contact and race mixture, as indicated by the increasing number of his publications devoted to physical anthropology and ethnology.

This book's major theme — the comparison of cultural development in the Old and New Worlds-reflects the new environment in which Wilson found himself. Nevertheless it is significant that the book was written within a decade of his arrival in Canada and that, with the exception of some work in physical anthropology, it contains the germs of most of his later ideas.

Thus, while the initial result of his coming to Toronto proved beneficial to his work as an anthropologist, we are bound to inquire whether his life in "the wilderness of Canada" did not ultimately have adverse effects. According to his friend, Robert Chambers, the author of *Vestiges of Creation,* the effects were not as harmful as his friends in Britain had at first feared. Following a visit to Canada, Chambers wrote, "I had been mourning over you as banished, cut off from all congenial pursuits ... but here I find you fit into your own favourite tastes as aptly as though Graeme's slough had marked out the line of your Toronto railway!" (Wilson, 1878:2, 148-149).

But Wilson himself was aware of "the clearly marked line which the broad Atlantic has drawn between those early years and later Canadian experiences" (*ibid:*2, 41). For he was not only being increasingly drawn away from scholarly pursuits into those administrative duties which he came to regard as the great work of his life, but at the same time he was also cut off from the libraries of Europe and the mainstream of scientific development. He did, of course, manage to visit Europe a few times and tells with relish of being able to examine (in 1864) several large collections of palaeolithic implements in France and England (Wilson, 1865:27). He also frequently visited the United States and noted with pleasure the generosity with which private and public collections alike were thrown open to him. "In no country of the world are ... libraries and collections made available to the inquirer with the same unrestricted freedom as in the United

States" (*ibid:*xxiv). But his references to these American visits mention collections and sites rather than the scholars he met. Wilson's intellectual ties were with Europe and he was more often critical than approving of Americans who were interested in anthropology. He contrasted his work with that of the "native school of American ethnologists". Prescott, Schoolcraft, and Morton all came in for their share of criticism, and while he recognized Morgan as "the historian of the Iroquois" and corresponded with him, he manifested with his silence the same disregard for Morgan's theoretical works as did the contemporary ethnologists in Britain[11].

But, while he never developed close ties with his colleagues in America he was aware that his distance from Britain not only cut him off from sources of data and ideas but even made the process of publishing his books more difficult. Much of what he learned about distant peoples was gathered from travellers like Paul Kane and from missionaries, and it is significant that in 1885 it was E.B. Tylor who prepared the appendix on archaeology for his *Anthropology* — and this for a man who formerly had been one of Europe's most promising prehistorians. In Canada, Wilson found men who were interested in various facets of his work, but none who had an interest in anthropology that was at all equal to his own. In a very real sense, Wilson's anthropology was forced to develop in isolation. It is thus understandable that as his other duties increased Wilson's work failed to keep abreast of world anthropology. One instance of this is his almost total lack of interest in studies of kinship and the development of primitive social structure, which played such an important role in contemporary anthropology outside of Canada.

Because of this, it is all the more remarkable not only that his interests were as broad as they were, but also that until the end of his life he should have continued to reach conclusions which have stood the test of time. He had a rare talent for detecting fakes and rejecting fantastic notions. Among the former

[11] A description of the British treatment of Morgan is found in Frederich ENGELS' "The Origin of the Family, Private Property, and the State". (See K. MARX and F. ENGELS, Selected Works, Moscow; Foreign Languages Publishing House, 1962:2, 182.)

were the many false claims put forward in support of Viking and Phoenician visits to the New World, fraudulent Runic translations, and claims of a Mound Builder script. The fantastic notions which he dismissed as wrong or unproved included Atlantis, Basque as an American Indian language, a living Maya civilization lost in the jungles of Central America, that environmental factors would eventually cause the Europeans in North America to resemble Indians, and the existence of lost secrets of Egyptian engineering. Wilson was also an early defender of the intelligence of Neanderthal Man.[12]

Many of his approaches were sophisticated for the time, as for example, his conclusion that the Indians had been living in the New World for a long time which he supported with independent anatomical, linguistic, and cultural data. George Peter Murdock's *Africa* (Murdock, 1959) is a recent example of this kind of an approach. In many cases Wilson reached sound conclusions using limited and often homespun data, drawn as often from his own experience or from history as from anthropological sources. His concept of races as non-permanent entities and his contention that brain size is not a good indication of intelligence are accepted today, although the latter was conclusively demonstrated only much later. Wilson saw more clearly than most anthropologists of his time the necessity of dealing with race, language, and culture separately. If few Americanists are in sympathy with his derivation of a part of the native population of the New World across the Pacific, most would agree with his contention that the New World cultures developed separately. For him cultural development was neither unilineal nor unidirectional. If his occasional doubts about man's biological evolution seem unjustified today, his insistence on the uniqueness of man is accepted by most anthropologists.

Wilson's work is not completely forgotten. Factual material which he collected concerning Scottish archaeology is occasionally quoted, and his reports on the Lake Superior copper sites have recently been described as "valuable (being) those of an individual with some training in both observation and recording of

[12] "Brain Weight and Size in Relation to Relative Capacity of Races" WILSON, 1876 (see note 9).

prehistoric data" (Griffin, 1961:71). On the first page of *The Idea of Prehistory* (Daniel, 1963:13) he is credited as the inventor of the term "prehistory" and *Prehistoric Man* was recently listed alongside such classics as Lyell's *The Geological Evidences of the Antiquity of Man* and Huxley's *Man's Place in Nature* in an editorial in *Antiquity*.[13] A.D. Lacaille has described the *Prehistoric Annals of Scotland* as "the first great classic on Scottish archaeology" (Lacaille, 1954:17) thus making good a curious oversight by V. Gordon Childe who ignored it in his two syntheses of Scottish prehistory (Childe, 1935; 1946). In part the oversight seems to have resulted from Childe's dependence on Joseph Anderson's *Scotland in Pagan Times* (1886), for while Anderson covered much the same material as Wilson, he gave credit to his predecessor only rarely. In a review article written in 1914 Ales Hrdlicka gave cursory notice to Wilson's anthropometric studies, but he does not seem to have paid any attention to his more important general ideas about physical anthropology (Hrdlicka, 1914:532-533). More recently T.D. Stewart and Marshall T. Newman have called favourable attention to Wilson's criticisms of Samuel Morton (Stewart and Newman, 1951:x, 19-36).

Nevertheless, Daniel Wilson is not remembered as an anthropologist who made a significant or lasting contribution to his discipline. If many of his opinions and conclusions are today held to be correct, it is largely if not wholly due to a large number of other men who worked independently of Wilson and may never even have heard of him. Many of his papers were published in obscure Canadian journals and his two most important books, the *Prehistoric Annals of Scotland* and *Prehistoric Man,* were syntheses rather than compilations of data. Like all such syntheses they were bound to become dated. Although Wilson realized that valuable data about the Indians were fast disappearing, he published nothing in the way of systematic ethnography. While this was a trait shared by most important ethnologists of his time,

[13] In the editorial of a recent issue of Antiquity (vol. 37, p. 87) Daniel points out that the terms *historic* and *prehistoric* had already been used in an article published in French by M. Tournal in 1833. It was Wilson's independent invention, however, that introduced the term among English speaking scholars and to archaeology as a whole.

Wilson was in a better position than most to collect and publish such data, and the following comment in a discussion of Indian religion shows that he had at least some appreciation of the problems which fieldwork involved:

> Our own experience with the American savage is that it is only by slow and careful observation of his spontaneous utterances that any conception of his real beliefs can be arrived at. By means of leading questions you may get any answers you like (Wilson, 1873:106).

Much of Wilson's data, especially concerning reservation conditions in the west, were collected by means of questionnaires but only his conclusions were published. If the answers to these questionnaires have survived, they may contain information that would still be of interest. He also enjoyed laboratory work such as anthropometry, the testing of metals, and experiments carried out with students. Nevertheless this work was too erratic to establish either trends in anthropology or a solid reputation for Wilson himself.

But the largely synthetic nature of Wilson's published works cannot explain his lack of fame. Anthropologists like Tylor are remembered while their books remain largely historical curiosities. Rather we must attribute his failure to a lack of interest in his work among his students. Wilson's early work had won him acclaim in Britain, but, however well his later books were received there, he remained a distant figure — isolated from the mainstream of British scholarly life. In Canada his interest in anthropology seems to have aroused more curiosity than genuine interest; on the other hand his skill in practical matters was greatly admired. To his contemporaries he was "essentially a man of affairs", an educational reformer and the champion of the University of Toronto in its many struggles. A short semi-official biography, written soon after he died, dismissed his academic attainments, with a pride more characteristic of the frontier than of a great university, as being "more diffuse than accurate", adding that had he remained primarily a scholar "the real man would have been submerged" (Wrong and Langton, 1901:204). To the still rough society of Upper Canada his scholarship was of little consequence. Since his anthropological interests were relegated to the status of a hobby, he had no students and left no strong impressions. Modern anthropology began in Canada not many years after his death

but it was to grow from new, largely American roots. His books have remained unread in his adopted land for over 70 years, and in the academic community the idea has persisted that since anthropology was Sir Daniel Wilson's hobby his writings must be those of a dilettante.

The last important evaluation of Wilson's anthropology (*ibid*) was made by a person untrained in anthropology and was biased by a too one-sided admiration of Wilson as a man of action. In this paper I have tried to present a short synopsis of his work and to redress the balance of that unfortunate article. Realizing the soundness and worth of his accomplishments we need not consider the time he devoted to the study of anthropology as time wasted. Rather his anthropology can be regarded as another facet of an exceedingly versatile and capable man. Although Sir Daniel Wilson's writings have little to teach us that is new today, it is not too late for the Canadian academic community to accord Wilson the honour he deserves, not only as an educator but as an insightful, if isolated and unheeded, anthropologist of our country's past.

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The Structure of French Canadian Acculturation

1759 to 1800

BY TREVOR DENTON

RÉSUMÉ

Les Franco-Canadiens ne furent pas assimilés dans les années qui suivirent la conquête mais on ne peut nier que leur vie sociale subit tout de même des changements profonds. — L'application par l'Auteur d'un modèle d'acculturation, aux faits connus de cette époque, met en lumière un certain nombre de facteurs (surtout démographiques, écologiques, et administratifs) et de relations conjonctives entre français et anglais qui empêchaient l'assimilation. — La réaction adaptive qui s'en suivi fut marquée par des phénomènes d'acculturation aussi communs que: la rigidité, les mécanismes d'autocorrection et de maintien des limites. Il en est résulté un pluralisme qui prépara en quelque sorte l'orientation actuelle du Canada Français.

INTRODUCTION

The era of the Conquest, 1759 to approximately 1800, was a vital period in the history of Quebec. It was at this time that the pattern of future relations between French and English was determined.

The purpose of this paper is to examine the structure of acculturation in Quebec following the Conquest — the nature of the conjunctive relations between French and English at contact, some of the characteristics of pre-Conquest French Canadian society which affected the course of acculturation, and the reactive adaptations which occurred in the French Canadian community as a result. Acculturation refers to "culture change that is initiated by the conjunction of two or more autonomous cultural systems" (SSRC 1954:974). "Conjunctive relations" refers to the "intercultural role networks that not only establish the framework of

contact but also provide the channels through which the content of one cultural system must be communicated and transmitted to the other" (SSRC 1954:980). The term "reactive adaptation" indicates the accentuation of elements in the French Canadian community in response to the presence of the English.

The attempt to understand the Conquest in terms of structural changes provides valuable insights. Too often scholars have merely stated that there was no assimilation of French Canadians because of the small number of English-speaking immigrants in Quebec, or, because "French Canadian institutions were too deeply imbedded". To adopt such a viewpoint is to lose sight of two important aspects of French-English relations. First, this was an acculturative setting and as such was a complex process involving many interrelated factors. More important, merely to state that there was no assimilation, completely overlooks the very real changes which did occur in French Canada. When two social groups come into contact several results may occur. One group may adopt characteristics of the other group, both groups may do this, neither group may change, one group may be completely assimilated, or one group may change by accentuating previous characteristics which are different from those of the impinging group. Some writers have looked only to assimilation as an all-or-nothing-at-all process. This attitude fails to realize that while a people may not be assimilated, they may still change radically in response to the presence of the other group.

The past twenty-five years have seen a marked increase of interest in French Canadian history. Contemporary historians have sharpened the tools of investigation left them by their predecessors whose interests were sometimes as much literary as historical. Several geographers and demographers have studied the history of French Canada from a fresh new viewpoint. Recently, sociologists have added still another dimension to Quebec historiography. And yet, an examination of this historiography suggests that scholars have concentrated on times when the colony was an active entity. They have written on New France, on broad panoramic interpretive schemes of development, or upon periods such as the nineteenth century political movements, when Quebec was reacting to the British. More attention has been paid to the study of attempts to "escape the yoke of

British domination" than to the study of how and if this domination occurred in the first place. It is not because French Canadian institutions were too deeply imbedded that assimilation failed, for it was not until after the Conquest that many of the institutions took their final form.

This article does not attempt the complete explanation of the phenomenon of French-English acculturation. This is impossible at the current state of knowledge of the period and of social change in general. Rather, a conscious effort is made to build a model, accurate if possible, but largely no more than a means of ordering classes of facts, figures, events and trends within a broad interpretive framework of acculturation theory. This paper relies heavily on the acculturative model suggested by the Social Science Research Council (1954:973).

When an attempt is made to analyse the nature of the contact between French and English and the changes which subsequently occurred, the years following the Conquest, from 1759 to approximately 1800, emerge as an era, the study of which is necessary to understand subsequent trends. This paper will progress from an examination of French-English conjunctive relations to the reactive adaptations which occurred as a result in the French Canadian community.

FRENCH-ENGLISH CONJUNCTIVE RELATIONS

An examination of the conjunctive relations between French and English brings into perspective the intercultural role networks which constituted the framework of contact, or, more simply, the ways in which French and English "came up against one another". Obviously, there would be many facets to any type of contact and the type of contact would be all-important in determining the nature of the acculturation which might occur. A large amount of the contact was of a rather strangely directed variety. Even if the military governors doubted its feasibility, the official policy of the British Government was the ultimate complete assimilation of French Canadians. It is true, of course, that the British Government wanted to retain the loyalty of French Canada in

the face of the growing hostility of the thirteen colonies. Partly for this reason, but largely, it appears, through complete inadvertence, the military government took several steps, which, assuming that assimilation was the goal, had the unintended consequence of largely preventing the very assimilation which was desired.

The seigneurial system of New France was the Canadian counterpart of the feudal system of land holding found in France before the revolution of 1789. Land was held and distributed through a hierarchical pyramid of statuses. The Crown, at the top of the pyramid, empowered seigneurs to distribute units of the land which then were granted to farmers. There existed a system of contractural rights and duties between the seigneur and the Crown, mostly of a military nature. Equally, there was a system of rights and duties between the seigneur and his enfeoffed land holders. The habitant paid various duties and rents, mostly in kind, in return for his land and various other rights, such as the right to use a mill constructed by the seigneur. In actual practice, the seigneurial system of New France was not as rigidly organized as that of France, being an adaptation to New World conditions. Parishes were first instituted in 1727. The Church was intimately involved in the seigneurial system as it held almost two-sevenths of the granted lands in New France (Munro 1907:182). The "fabrique" was the local ecclesiastical parochial political unit commonly found throughout the Catholic Church.

The area of public administration clearly illustrates one example of unintended consequences. An examination of the structure of political power reveals several interesting facts which helped determine the type of acculturation which occurred. To a greater or lesser extent, the British merely took over the existing structure of power. Table 1 is designed to show the changes which occurred from the French to the English regime, and also to show the ethnic origin of persons occupying various statuses in the English administration. While the seigneurial system of land holding was retained, English seigneurs were not appointed. The captains of militia, previously the effective local power of the Crown, were abolished. Because the seigneurs were never very influential, the status of the Church in the administrative

chain of command increased as the military government used it more and more. As the populations became somewhat more concentrated, the fabrique became a more visible method of local administration. It is to be remembered that local governments did not occur in Quebec until the 1840's. The type of administration which thus emerged left administrative relations between French and English extremely diffused. The only English persons in the administration were at the top of the pyramid of power. Administrative policy was diffused through French seigneurs and, more important, through French curés. Because the community level was left almost completely untouched by this administrative structure, there were virtually no face-to-face relations between French and English at the level of the parish or seigneury. This had an obvious effect on the nature of the acculturation which occurred. Assimilation would be impossible without face-to-face relations.

Certain demographic factors also seem to have kept face-to-face relations at a minimum. When the United Empire Loyalists came to Quebec after the American revolution, they preferred the freehold tenure of land which they had previously enjoyed, rather than the seigneurial system which the military government had perpetuated. Consequently, they settled largely in new areas, away from the established population concentrations. Thus, there was a great deal of physical distance between French and English. In addition, the majority of the French Canadian population was strung out in sparse rural conglomerations, as opposed to being concentrated in urban areas. It is difficult to deduce from the available census information reliable French-English rural-urban ratios (Henripin 1965: personal communication) and, indeed, it is difficult accurately to define the terms rural and urban as they relate to this period. It seems safe, however, to suggest that had the French Canadian population been more concentrated in urban areas, a far different form of acculturation would have resulted.

An examination of Table 2 reveals two important points. There were very few English-speaking immigrants in the years immediately following the Conquest. Murray estimated them at 450 in 1765 (Langlois 1935:132). By the time a larger number of immigrants had arrived, the acculturative pattern had already

been largely defined. Secondly, it is apparent that at all times in the relevant period there were many more French than English-speaking persons in Quebec. Any degree of assimilation would therefore be unlikely, as Brunet (1964:196) has pointed out. But this does not in itself explain the phenomenon. This attitude again presumes that acculturation is an all-or-nothing-at-all phenomenon. It was not only the ratio of French to English which was important, but also the numbers of members of the two groups which were in effective contact. For example, there would still have been little assimilation, in all likelihood, during the post-Conquest period to 1800, even if there had been a larger English concentration in the urban areas, since the French were concentrated in rural areas.

The economic structure of the colony also figured importantly in determining the nature of the acculturation which occurred. While there were some early British agricultural interests, many of the British who first came to Quebec were interested in running the flourishing fur trade. French Canadian merchants were kept out of the fur trade and, indeed, from most specialized economic activity which was not of an agricultural nature (Brunet 1964d:49 *et seq.*). This fact resulted in the rather surprising unintended consequence of a boundary-maintaining mechanism imposed by the British on the very people whom they wanted to assimilate. A boundary maintaining mechanism refers to the means by which a "system limits participation in the culture to a well-recognized in-group" (SSRC 1954:976). Usually the term is used to indicate a feature imposed by the group being acculturated. In this case, the opposite was true. In other words, while it might have been the British Government's official policy to encourage assimilation, the actual practice of the British was the subtle exclusion of French Canadians from the lucrative fur trade, again minimizing the face-to-face, and also the indirect, contact between the two groups. Furthermore, there was no practice of relying on seigneurial agricultural produce. This had the effect of stifling seigneurial productivity and of decreasing French-English interaction.

Language played a role intimately connected with these economic factors. Clearly, the nature of the acculturation between two groups will depend upon the breaking down of lin-

guistic barriers, or the failure to do so. For example, linguistic assimilation or at least bilingualism, would be a necessary preliminary step, or at least in a symbiotic relationship with, social assimilation. For several reasons this did not occur. As mentioned above, there was virtually no opportunity for the French to participate in the British-operated fur trade. Moreover, the British had little need to rely on or participate in the French socio-economic world of the seigneury. Consequently, few people of one language knew the other. Few people had the impetus to learn the other language which economic inter-dependence would have supplied.

Thus, largely as a result of the conjunctive relations between French and English, the acculturative process took a definite form. Assimilation did not occur. What did occur was a stabilized pluralism — two cultures existing side by side, but almost completely independent of one another. French Canadian culture did, however, react to the impinging British culture, and it is to the changes in French Canadian society, to the nature of this reactive adaptation, which we now turn.

REACTIVE ADAPTATION

When two social groups are placed in contact with one another what are the several different forms which acculturation may take? It is possible that neither group may change. One or both groups may adopt characteristics of the other, partly or completely. Also, one or both groups may change in response to the other group by developing new characteristics or by accentuating old ones. Two factors largely determine the type of reaction which occurs: the structure of the conjunctive relations between the two groups, and the characteristics of the groups themselves (SSRC 1954:973). As we have seen, the nature of the conjunctive relations between French and English tended to discourage any assimilation during the period from the Conquest to roughly 1800. What did in fact occur was a reactive adaptation in the French Canadian community caused in part by the prexisting structure of French Canadian society. Again, it is important to emphasize this fact. While there was no assimilation, there were

definite changes which did occur. Largely by varying and accentuating previously existing characteristics, French Canadian culture became something new and different.

This fact is clearly illustrated by the socio-economic classes existing after the Conquest. As mentioned previously, French Canadians were excluded from the major entrepreneurial activity of the time — the fur trade. The nature of the class structure of New France has recently been the subject of a great deal of controversy (Eccles 1961). This controversy need not concern us here. What is important is that while there may or may not have been a trading middle class bourgeoise before the Conquest, there was no such French Canadian class after 1760 (Brunet 1964d: 49 *et seq.*). While there were some coureurs-de-bois fur traders travelling in the interior and some professional men such as doctors, lawyers and priests, by far the outstanding class of the period was the habitant — the farmer tied to the ground.

This fact, which saw the large majority of the French Canadian population engaged in agricultural activity, was one of the major characteristics of the era. The seigneurial system continued to be the method of land distribution for a population whose rate of increase remained at a high 100% roughly every thirty years (Henripin 1965:207). As previously mentioned, the seventy odd thousand of 1765 were over 150,000 by 1800. This rapid rate of increase meant that a slight amount of land pressure was beginning to be felt by 1800. For a while, virtually all the increase was thrown back on the land. Towards the end of the century, however, towns were beginning to appear and, along with them, the first signs of a more complex economy (Roy 1906:182).

The application of Redfield's model of a folk-urban continuum to French Canada, as used initially by Miner (1939) and Hughes (1943), has recently been the subject of critical reappraisal (Garigue 1964; Rioux 1964). The application of this model to this period in particular provides some illuminating insights. Redfield's typology utilized a scale mainly emphasizing four features: isolation, homogeneity, self-sufficiency and size. At one end of the continuum, the tribe is isolated, homogeneous, self-sufficient and small. The larger heterogeneous unisolated

town is dependent on the relatively nearby peasant community. The peasant community is more homogeneous than the town but is dependent on it socially and economically. The controversy involved in interpreting all of French Canadian history in the light of a folk-urban continuum need not concern us here. However, it is useful to apply some of the analytical tools of the scheme to the period under consideration. While the years 1759 to 1800 were marked by a more than doubling of the population in Quebec, the French Canadian community was more homogeneous, in terms of occupations, than at any time after 1800 or even during the thirty or forty years prior to the Conquest. During the period under consideration, the towns — Montreal, Quebec and Trois Rivières — had English entrepreneurs as the dominant commercial class. There was very little social or economic inter-dependence between these towns and what might have been their agrarian hinterland. While seigneurial agriculture was self-sufficient it produced little surplus for sale in the towns.

The relative independence of the rural community or communities and the towns during the forty years following the Conquest raises the problem of communications in French Canada. There were only a couple of newspapers with a fairly small circulation. As noted, there was very little inter-dependency between town and countryside. Relatively speaking, it seems that during the post-Conquest years there was not much communication at all among the various members of the French Canadian community. The Church is the one obvious instrument of dissemination of knowledge. Through its intimate involvement in the socio-political parochial groupings it had an opportunity to express its attitudes as the one centralized French Canadian institution. The various "mandements" (pastoral letters) are overt evidence of this type of dissemination of ideas. It was, in fact, at the level of the Church and the parish that some of the most important aspects of French Canadian reactive adaptation occurred. As previously mentioned, an examination of the administrative chain of command shown in Table 1 shows that the military government relied heavily on the cooperation of the Church to administer policy. In fact, the Church was the effective local conduit pipe of British policy. In this regard, the Church, in its role as a major French Canadian institution, exhibited the characteristics of a

"self-correcting" mechanism. It was able to "shift function and adopt internally, irrespective of its outer protective devices" (SSRC 1954:977). In this way the Church helped to prevent assimilation and, largely unconsciously, to direct the course of acculturation towards the reactive adaptation which occurred. The process was in part moulded by the fact that the Church in Canada had always been more authoritarian than its European counterparts (Fallardeau 1949:359). Thus, we see in the Church another clue as to the reasons for this reactive adaptation. The original French Canadian culture was such that an integral part of it was able to adapt its role to fit the new circumstances. In this way, the culture as a whole was aided in perpetuating itself.

It was at the level of the parish that the Church had its greatest effect. The importance of the parish during the period under consideration has been briefly discussed by Garigue (1965: 130). It was during the post-Conquest years that the parish achieved a large part of its importance. The fabrique was the parochial administrative unit in the agrarian community or communities. Its influence increased immensely in this era when most of the local administration was carried on by the Church. It is to be remembered that there was no local government, aside from the fabrique, until several years after Lord Durham's report.

If one examines the ways in which individuals became integrated into society, two integrating mechanisms come sharply into focus. First, the extended family, using the devices of the seigneurial system, provided the relationship between land and family (Guindon 1965:147). Aside from the extended family, it was the parish which was the social, political, and economic focus of the French Canadian community. Communal gatherings occurred largely as a result of being members of a parish. The fabrique was the realistic local unit of government. Economically, it was the fabrique which often decided local financial matters. In short, through the parish, individuals were integrated into society.

The importance of this concept is illustrated by examining the growth of the power of the Church in post-Conquest years. Table 1 shows how it was the effective local conduit pipe of British policy, as a result of its position in the administrative chain of command. Through the practice of "mandements", ser-

mons, and through the influence of the local curé in the fabrique, the Church was powerful in the sense that it could develop and implement policy. In short, it could get things done. In addition to this, however, the Church developed a far more subtle power in the sense that almost all individuals became members of society through its various integrating devices — schools, church, and fabrique. People commonly referred to themselves not as members of a small village or of seigneury, but as members of a parish. Thus, in this manner, the Church grew powerful in the same sense as such an innocuous institution as the Home and School associations are powerful today. People are integrated into the community through them. Ideas are learned and opinions formed in them.

The foregoing analysis, illustrating how the Church's influence spread into a far greater sphere of activity, points out another reason why assimilation failed and why a reactive adaptation occurred. Before the Conquest, French Canada was a very rigid society. Rigidity refers to a small and well defined number of authoritarian controls, avenues of prestige, ascribed statuses and clearly defined interpersonal relations. Where there are "relatively few key or command values in its hierarchy, a system is likely to be rigid and self-consciously resistant to alteration on contact, since it is already organized defensively probably as a result of external or internal challenges in the past" (SSRC 1954:977). This phenomenon of rigidity is illustrated by examining the relationships between several important structural components of French Canadian society. There were two basic relationships. The family was functionally related to the seigneurial system, since it acted as the distributing agent for land. The Church integrated religion, local government (the fabrique) and the local unit of social identification (the parish). Rigid social systems tend to perpetuate themselves unless the integrating relationships (in this case the Church and family distribution of land) are destroyed. These relationships were not completely pre-Conquest phenomena. However, to a large extent the means existed by which French Canadian society might shift by accentuating old patterns. Finally, in presenting this analysis of the influence of the Church, it is not being suggested that the Church was uninfluential before the Conquest. What is suggested is that

the Church achieved its unique dominant position of power as a result of the type of acculturation which occurred immediately after the Conquest.

CONCLUSION

The pattern of conjunctive relations established between French and English, along with certain characteristics of the French Canadian community itself, resulted in a reactive adaptation and in the complete failure of assimilation. French Canadian society adopted few aspects of the impinging British culture. Rather, it underwent certain changes in response to the British presence. These changes cannot realistically be traced to "how deeply French Canadian institutions were imbedded", or to the "genius of French Canadian society in resisting English domination". Concrete factors were operative, such as demography, administrative policy, economic characteristics, socio-economic classes, and internal structural changes, especially in the sphere of influence of the Church. Such common acculturative phenomena as boundary maintaining devices and self-correcting mechanisms occurred. Before and after 1760 French Canadian society was very rigid. As a result, French and English lived side by side in a stabilized pluralism. The two cultures in contact failed completely to lose their autonomy.

The reactive adaptation which occurred resulted in an equilibrium in the period between the Conquest and approximately 1800. The population was tied to the earth, to the seigneurial system, in more or less a subsistence rural agrarian economy. The sphere of influence of the Church increased dramatically. However, just as Miner (1939:237) has said of the entire panorama of French Canadian social evolution, even more so for this period in particular, the equilibrium which was reached was, by its very nature, a short run adjustment.

Changes were inevitable in French Canadian society by roughly 1800. The population had more than doubled since 1760. The Church, with an effective monopoly on the educational system, was beginning to produce educated people with little oppor-

tunity to take advantage of their educational background. The land available under the seigneurial system of tenure was beginning to be scarce. It is hardly surprising in the face of deprivation of social and economic opportunities that the growing class of educated French Canadians should have aspired to a share in the political control of the community and in its economic activity. This was especially true now that the number and populations of towns were increasing rapidly. What had been a relatively small and homogeneous society was now becoming larger and more heterogeneous in terms of trades, classes, education, and aspirations. Assimilation had failed. By the end of the century the initial structure of relations between French and English had been defined. The changes which had produced a short-term equilibrium were themselves beginning to change. An era had come to an end.

TABLE 1 — POLITICAL ADMINISTRATIONS.

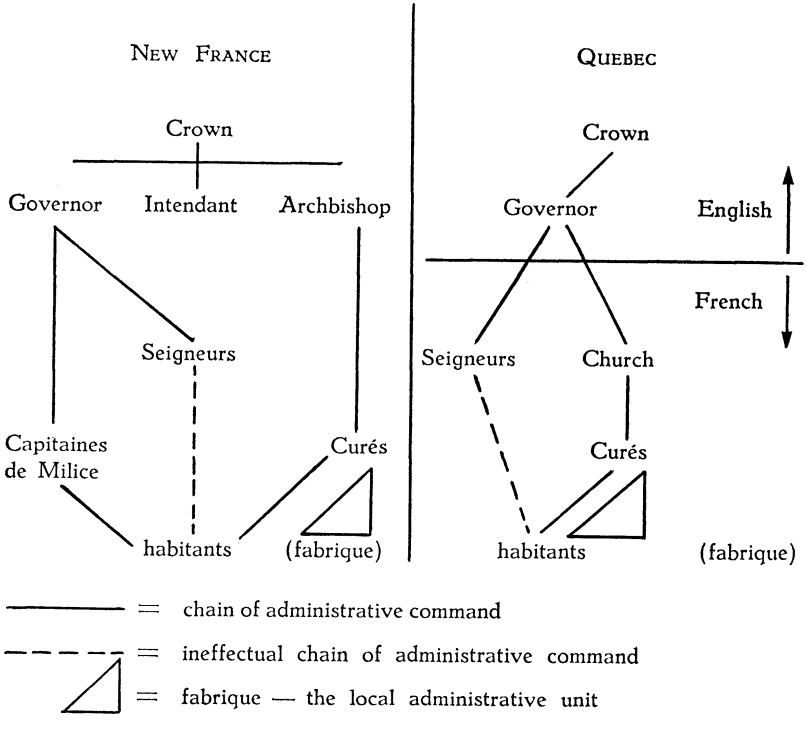

TABLE 2 — POPULATION OF QUEBEC.

	English	French
1765	450[1]	70,000[3]
1790	15,000[2]	146,000[4]

[1] estimated by Murray (Langlois 1935:132)

[2] Caron 1923:143

[3] Census of 1765 (*Census of Canada,* 1871, Vol. IV:65)

[4] Census of 1790 (*Census of Canada,* 1871, Vol. IV:77)

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Diversity in White-Eskimo Leadership Interaction

BY DAVID DAMAS *

RÉSUMÉ

Il y a un contraste marqué dans les interactions de "leadership" observées entre les esquimaux de la région d'Iglulik et ceux de la région de Bathurst. A Bathurst, "leadership" faible, irrégulier, grandement soumis aux Blancs; à Iglulik au contraire, leadership relativement effectif et en grande partie collatéral à celui des Euro-Canadiens. Dans une enquête sur les régions en questions, l'Auteur tente de dégager les facteurs déterminants de cette diversité.

A sharp contrast in leadership interaction could be observed between the two Eskimo regions of Iglulik and Bathurst Inlet during the early 1960's. At Bathurst native leadership was weak and irregular from locality to locality and largely subordinated to white authority. Native leaders at Iglulik were relatively effective in their sphere of authority and that sphere was, for the most part, collateral to that of Euro-Canadian agents.

A review of the history of each area suggests variables that can be regarded as determinants of the present-day diversity in leadership interaction.

One set of variables is associated with differences in the traditional leadership or co-operative structures of the two areas.

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Authority at Iglulik in the aboriginal period was based as much or more on positional criteria as it was on personal qualities (Damas, 1963). The patrilocal extended family which formed the chief residential and economic unit was headed by an *isumataq* (one who thinks) who was almost invariably the oldest male in the male line. Local aggregations of larger scope than the extended family were headed by other *isumatat*. Each of these leaders was usually the head of the largest extended family in the group. The family *isumataq* supervised division of labour, distribution of catch and also the arrangement of marriage and adoption for his kin unit. The local *isumataq* supervised division of food among the family heads and organized winter sealing and walrus hunting. Some *isumatat* also were shamans and had influence in sacred as well as secular life.

The institution of the *isumataq* did not exist at Bathurst and whoever rose to special prominence whether in sacred or secular life did so purely on the basis of personal qualities. The scope and character of secular leadership was diffusely defined.

Positionally defined leadership like that of the Iglulingmiut has certain advantages over the personally defined leadership found among the Bathurst people. Disputes over designation of leaders can be more easily settled and continuity from generation to generation is assured if mechanisms exist in the social structure that outline designation of leaders. Kinship was the chief ingredient in this designation at Iglulik.

Related to the bases for succession to *isumataq-ship* was the ascendancy of kinship factors in determining the course of authority diffusion throughout the local groupings at Iglulik. Each group was a continuous or near continuous aggregation of kin. The dominance-subordinance dyads that make up the bulk of kinship relationships provided a system of command that pervaded the entire group. At Bathurst Inlet dyadic relationships were also important but kinship factors were subordinated to voluntary ones in outlining systems of partnerships in meat sharing, spouse exchange, dancing and joking. The inherently continuous nature of kinship over time as well as the spatial continuity found in the kin-based local groupings provided a firm basis for the persistence

of Iglulingmiut authority systems. The Bathurst social system which was to large extent based on voluntary partnerships did not provide such potential for persistence.

The vertical nature of the dyadic system at Iglulik, based as it was on dominance and subordinance, aided execution of decisions good or bad. The Bathurst system operated on a horizontal or egalitarian basis and must be regarded as a co-operative network rather than a network of authority diffusion. Although the opinions of some persons might have been highly respected there was no unvarying final authority and co-operative action often depended on consensus and probably was often characterized by indecision and inefficiency.

A second set of variables is concerned with differences in resources and in the changing adaptations that leadership structure was faced with in connection with changes in economy after contact. Iglulik possessed a varied resource base with caribou, polar bear, ringed seal, bearded seal, beluga, and most important, large herds of walrus. The Bathurst people had only the ringed seal, fish and caribou, and only the caribou in abundance. The resource advantage, principally in sea mammal life, that the Iglulingmiut possessed over most Eskimo groups was not fully exploited until after introduced artifacts made more efficient hunting possible. The whale-boat in particular, brought into play an approach to walrus hunting that greatly increased meat production (Damas, 1963). Manning of boats in hunts brought about an action group, the boat crew, that had a traditional basis in kinship organization and the system of authority diffusion was probably enhanced by its operation. The *umialiq* or captain usually coincided with the family or local *isumataq*. His authority was probably crystallized to a greater extent in directing (walrus hunting from boats) than it had been in any aboriginal activity. That situation can be contrasted to the one at Bathurst Inlet where introduction of trade goods probably encouraged atomization in economic organization. The chief activities that required co-operative action; breathing hole sealing with harpoon, fishing at weirs, and caribou drives were replaced by sealing hooks, fishing with nets, and rifle hunting of caribou, all being activities that could be carried out by one man. Although all of the latter pursuits

could have benefited from better organization of personnel, resources remained stable enough for a time to not require more unified group effort. Occasions demanding co-operation tended to perpetuate the loose, informal traditional leadership structure. With the removal of the necessity for co-operation it seems likely that emphasis in individualization became accentuated. After the large winter sealing villages were abandoned owing to a shift to winter caribou hunting, local affirmation of the broad ranging partnerships of the area was drastically narrowed. The seal sharing partnerships which had been important to economic life disappeared rather quickly.

Continued abundance of resources at Iglulik can be seen as a chief factor in the continuance of a large measure of autonomy in native leadership. Reduction of caribou herds in the 1950's at Bathurst, and the incomplete readaptation to a mixed economy not only for tools and supplies which were needed at Iglulik as well, but also for food for humans and dogs. The present-day marginal character of current subsistence at Bathurst is not due entirely to depletion of resources. The report of an areal economic survey (Abrahamson, 1964) agrees with my own impressions that local resources could probably support the now reduced population (97 persons remained in the area in 1963) if native leadership and co-operation were better organized to exploit them. Men go off singly or in pairs to nets or after caribou with no overall plan of action being laid out as is the case at Iglulik for the village-wide walrus hunts and also regarding division of the multiple economic tasks within the extended family. Each adult male at Bathurst usually has his own cache of meat and sharing is largely a personal matter with no firmly outlined rules for division of meat existing for the most part. It appears that because of changes in the economic cycle even the loosely organized co-operative networks of the aboriginal period have lost whatever efficacy that they once had and that nothing very substantial has grown up to replace them.

The third set of variables has to do with the intensity and character of white contact. During the early part of the post-contact period point of trade remained distant from the Igluling-miut region. Until 1939-42 and again until 1947 three hundred

mile sledge trips were necessary in order to reach trading posts. These trading trips took place no more often than annually. No continuous pressures were brought to bear on authority in economic matters, and native networks were allowed to flourish and develop largely in their own direction. In the Bathurst area for the same period rival trading concerns competed for furs. Direction of trapping by the natives seems to have been rather well organized with independent white trapper-traders or the employees of the two major firms setting examples of efficient trapping techniques. The population was split in its loyalty to one or the other of the rival concerns though some Eskimos traded alternatively at the various posts. External economic influence was therefore strong but it was not unified. During this same period native co-operative networks were weakening in the face of economic changes noted above. Later when trading was unified at the post of the Hudson's Bay Company the rather intensive direction of trapping and the dependency that had been created with regard to trade goods had built a reliance on outside authority that could easily be centralized. This centralization, though unifying local loyalties to the one white agency, did not provide a climate for the spontaneous development of new native authority systems or for the revival of traditional co-operative systems.

Another sort of influence that could be expected to have some effect upon native leadership patterns is missionization. At Iglulik the Anglican religion was introduced by a native from Pond's Inlet. Later an English missionary made occasional trips through the area but for the most part Anglicanism has been perpetuated by native leaders. The village *isumataq* often performed the role of prayer leader. Shamanism seems to have virtually disappeared. Religious innovation was therefore absorbed into the extant social structure and Anglicanism provided a rallying-point, a bond of common sentiment and belief that was probably greater than any native religion had been able to accomplish. Roman Catholicism was less successful in the area with regard to number of converts although the resident priest probably exerts considerable influence in sacred and perhaps as well in certain secular matters in this minority group. Proselytization was not greatly successful at Bathurst; the dominant Anglican group does not generally hold services and the sabbath is not

observed as is the case at Iglulik. No real unification of the population through religion occurs at Bathurst.

Contacts with the Royal Canadian Mounted Police also showed contrast between the two regions. At Iglulik the annual patrols from Pond's Inlet were concerned principally with census taking, incidents of actual law enforcement being few in the post-contact history of the region. The Bathurst people, on the other hand, refer to the police as "wolves" because of their persistence in bringing to trial Eskimos accused of murder or illegal musk-ox slaughter, the two chief crimes known in the area. Murderers have served sentences on several occasions but almost invariably those brought to trial for musk-ox killings have been exonerated by the magistrate and the natives are consequently left confused regarding the nature of Canadian legal norms. Outside pressure has thus affected both a traditional hunting practice and a traditional means of settling disputes at Bathurst.

In the recent history of the two regions government representatives have joined the contact - traditional white agents: traders, missionaries, and police. Areal administrators moved to Cambridge in 1955 and to Iglulik in 1960. Among the most important duties of the administrators were issuance of social legislation and relief monies and direction of the relocation of sick persons. Administrators were left a certain amount of freedom regarding what they deemed a minimum level of subsistence for the Eskimo but, in general, were expected to forestall famines and extreme hardship.

At Iglulik the areal administrator lived at the point-of-trade and kept in close communication with the trader who actually issued much of the relief goods. Both men agreed on rather a tough philosophy of relief, feeling that the land could support any reasonably ambitious Eskimo and his family. Continued abundance of game kept decisions regarding relief at a minimum.

The post manager at Bathurst found himself in a difficult position with regard to issuing relief. The areal administrator resided 165 miles away at Cambridge Bay and decisions regarding relief were left largely to the judgment of the trader. He felt that over-issuance would tend to destroy native incentive to carry out the trapping and hunting that is the basis for the fur trade. If, on

the other hand, too little relief was given, the resulting hardship conditions would not only be directly his responsibility but the administrator would also be subject to criticism from Ottawa. Difficulties brought on by the spatial separation of the two agents responsible for relief distribution were compounded by occasional inconsistencies in issuance which were the result of difficulty in determining subsistence conditions in the outlying villages. If one adds to these problems the natives' lack of understanding of motives behind issuing relief, it can be seen why this important medium of contact with white authority left the Bathurst Eskimos confused.

Another factor that affected native autonomy at Bathurst was the emigration of a number of young men from the area after 1955. The men were drawn away by employment possibilities on the DEW line and also by the prospect of finding wives. Few of these men returned to Bathurst, and their emigration removed active members from the community, thereby weakening the efficiency of local co-operative networks.

The variables that have been discussed here were generally cumulative in producing divergences from already contrasting aboriginal regional leadership patterns and seldom provided mechanisms which could counterbalance the general trend toward divergence.

There is a possibility that a move toward convergence could take place in the future. If increase of population and/or decline of resources occurs at Iglulik there will be greater reliance on relief and government directed industries. Such reliance would weaken native economic autonomy.

Movement toward increased autonomy on the part of the Bathurst natives is less probable. While current plans for economic recovery through more efficient exploitation of resources could be successful, success or failure of the program will probably be largely dependent on the leadership of Euro-Canadian agents. Lack of tradition-based authority structure, coupled with the emigration of educated youths who might provide a fresh basis for organization, makes it unlikely that internally organized leadership or co-operative structure will appear in the area.

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Eskimo Theories of Mental Illness in the Hudson Bay Region*

BY FRANK G. VALLEE

RÉSUMÉ

Lors d'une enquête (1963) sur les nouvelles formes de groupements sociaux dans les régions est de la Baie d'Hudson, l'Auteur fut amené à prendre soin d'un esquimau atteint de dépression mentale. A cette occasion, il a pu aussi observer d'autres cas semblables. C'est ce qui l'incita à faire enquête sur l'idée que se font les esquimaux de ces dépressions mentales, et comment il les soignent.

INTRODUCTION

Recent studies in social psychiatry and studies of mental illness in different cultures drive home the point that we need to go beyond the conventional clinical ways of describing and explaining, not to mention treating, mental illness because these ways have been developed with reference to medical and other traditions within one kind of society and usually with reference

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* Data for the paper were gathered during the late winter and spring of 1963 while the author was engaged in a study of new forms of Eskimo social organization, under contract with the Northern Co-ordination and Research Centre, Department of Northern Affairs and National Resources, Ottawa. Later in 1963 and early in 1964 several visits were made to Verdun Protestant Hospital; since that time we have interviewed medical and welfare officials who have had experience with Eskimo mental illness and have had access to official case histories of Eskimo patients. We gratefully acknowledge the help of Mr. V.F. Valentine, Director of the Northern Co-ordination and Research Centre, and his staff and of the following key informants in the field: Mr. J.D. Furneaux, D.N.A. Area Administrator, Povungnituk, Nurses Helen Williams, John McGurl, and Miss J. Nuttley, at Povungnituk, Port Harrison, and Great Whale River respectively; Messrs. Eliasie Sadlualu, N. Killupa, J. Sivua, and T. Tukai, of Povungnituk and Port Harrison; and at Ottawa, Mr. J.F. Neville, chief of the Welfare Division, D.N.A., and Miss Betty Marwood, welfare officer, Arctic District Office. Special mention should be made of the help given by Mr. Raymond Gagné, Eskimo language specialist with the DNANR Welfare Division and his assistant, Mr. Elijah Erkloo, in transforming my crude Eskimo language renditions into the new standard orthography.

to the individual taken out of social and cultural context (Opler, 1963). It has been argued convincingly that our understanding of mental illness is superficial unless we describe its incidence in a cultural context, that is, unless we take into account such matters as the ways in which different social groups and categories (ethnic groups, classes, religious denominations, psychiatrists, clergymen, etc.) define, explain, and treat mental disorders (Hollingshead and Redlich 1958; Opler, 1959). Reinforcing the argument for taking such matters into account is the demonstrated effectiveness of various forms of therapy practiced, consciously and unselfconsciously, by people without exposure, let alone training, in the arts of psychology and psychiatry as these have developed in advanced societies with strong scientific traditions (Devereux, 1951 and 1957; Wallace, 1958). There is a growing demand for something like a phenomenological approach, in which the observer describes as well as he can what the people he is studying say and do about what he, the observer, calls mental disorder. It is from such verbalizations and actions that the observer builds up what Devereux calls the cultural 'thought models' from which are derived their explanations of illness (1958:360; see also Bohannan, 1963, esp. page 12f).

Such an approach is all the more called for where mental illness is the subject of study in some group to which is attributed a high level of homogeneity, where it is tempting to overdescribe some single form of illness which then becomes the classical form of illness for that group in the literature: for instance, 'latah' among the Mongolians, hysteria among the Zulu, schizophrenia among the industrial working class, alcoholism among the Irish. Oversimplification in the attribution of prevalence to a group of a specific disorder invites oversimplification in etiology, examples of which are very easy to find in the literature. The people discussed in this paper, the Eskimos, have already had their script written for them as far as mental disorder is concerned and it is one purpose of this paper to invite investigators to rewrite that script for one group of Eskimos by discussing kinds of mental disorder which, as far as we know, have not yet been described in published reports on the Eskimos.

Through the anthropological and psychiatric literature there has been broadcast an idea of a classical form of mental illness

prevalent among the Eskimos, the chief symptom of which is convulsive hysterical seizure, infrequently accompanied by conversion symptoms, such as paralyzed limbs. The best descriptions of this hysterical condition are given by several writers for the Polar Eskimos, where it is termed *pibloktoq*.[1] In a provocative article, Wallace (1961:263) summarizes the "classical course of the (pibloktoq) syndrome, as judged from cases described by various travelers in the north...

1. *Prodrome.* In some cases a period of hours or days is reported during which the victim seems to be mildly irritable or withdrawn.
2. *Excitement.* Suddenly, with little or no warning, the victim becomes wildly excited. He may tear off his clothing, break furniture, shout obscenely, eat feces, or perform other irrational acts. Usually he finally leaves shelter and runs frantically onto tundra, or ice pack, plunges into snowdrifts, climbs onto icebergs, and may actually place himself in considerable danger, from which pursuing persons usually rescue him, however. Excitement may persist for a few minutes up to about half an hour.
3. *Convulsions and Stupor.* The excitement is succeeded by convulsive seizures in at least some cases, by collapse, and finally by stuporous sleep or coma lasting for up to twelve hours.
4. *Recovery.* Following an attack, the victim behaves perfectly normally; there is amnesia for the experience. Some victims have repeated attacks; others are not known to have had more than one.

Wallace does not claim that *pibloktoq* occurs in regions other than the Thule District among the Polar Eskimos, but a more recent reviewer of the Eskimo literature, Seymour Parker (1962) concludes that a hysterical symptom syndrome, involving degrees of convulsive behaviours, such as is described in the literature on *pibloktoq,* is the prevalent pattern among the Eskimos in many parts of the Arctic.[2] He hypothesizes connections between the

[1] For a summary of literature on *pibloktoq,* see Anthony Wallace, "Mental Illness, Biology, and Culture," in Francis L.K. Hsu (Ed) *Psychological Anthropology,* Homewood, Ill., 1961:255-295. In the new standard orthography. *pibloktoq* would be rendered as *pilluktuk,* but we retain the old orthography for this word in the present paper because it is so familiar and wide-spread.

[2] This article presents a good review of the literature on Eskimo mental illness, as well as a plausible analysis of how Eskimo culture and social organization, in its 'ideal typical' form, would predispose its members to hysterical rather than other kinds of breakdown. For data on Eskimo personality see Lantis (1953) and Ferguson (in Honigmann, 1962).

prevalent hysterical pattern and child rearing techniques, the cooperative social organization and communalistic value system of Eskimos, and the provision of sanctioned outlets for hostility and role models for hysterical-like behaviour in their traditional religion.

An alternative hypothesis is suggested by Wallace (1961: 265): that as a result of calcium deficiency, low concentrations of ionized calcium in the blood...

> Produce a neuromuscular syndrome known as tetany which is often complicated by emotional and congnitive disorganization. The neurological symptoms of tetany include characteristic muscular spasms of hands, feet, throat, face, and other musculature, and in severe attacks, major convulsive seizures.

It cannot be decided with the data on hand which of these hypotheses best accounts for the alleged prevalence of hysteria. The chief point we make in this introduction is that forms of mental disorder other than hysteria, such as compulsive withdrawal, paranoid ideation, manic depressive states with self — and other — directed aggression are said to be rare or absent among the Eskimos, the suggestion being that where they do occur, they are the result of intensive contact with Euro-American society (see for example Ehrstrom, 1951). The present writer suggests that a variety of mental disorders, besides the hysterical-type ones, have been common for at least three generations, at least among the Eskimos of Canada's Eastern Arctic and that such non-hysterical symptoms have been manifested both by those who have had and those who have not had intensive contact with Euro-American society. Even the most cursory survey of the incidence of mental disorder in one region, the eastern side of Hudson Bay, gives strong support to this suggestion.

FIELD WORK

The six-week survey carried out by the author in the late winter and early spring of 1963 was only an incidental by-product of an investigation of new forms of social grouping in a few communities on the east side of Hudson Bay. It was while

studying this topic that the writer was thrust unexpectedly into a side study of mental breakdown through having to help guard an Eskimo patient for a prolonged period during which the patient showed most of the symptoms to be found in a manual of psychiatry. Experiences with this patient and other Eskimos impelled the writer to ask questions and to seek out people, Eskimos and Whites, in the region who had had something to do with what would be defined as mental breakdowns outside the Arctic. This is far from being a census-like, systematic appraisal of the incidence and prevalence, cause and treatment of mental illness among the Eskimos. Most of the reliable first hand information is about people in two communities, Povungnituk and Port Harrison, and the camps between them; the remainder is second-or third-hand and is about people in other communities on the eastern side of Hudson Bay: Belcher Islands, Great Whale River, Ivuyivik, and Sugluk. We also have some information on people whose place of origin is the eastern side of Hudson Bay, but who have been relocated to Grise Fjord and Resolute Bay in the High Arctic.

Procedure and Sample

A problem encountered by investigators studying mental illness in a society other than the one in which they were brought up is to decide what is and what is not a mental disorder in the society they are studying. We decided for the purpose of this survey to take as basic criteria of mental breakdown the following: incapacity of the person to perform in some or all of his normal roles accompanied by behavioural oddity, as defined by informants, and where the incapacity and oddity are attributable to the head rather than to some other body organ. So our procedure was to ask informants about happenings in which people were rendered incapable of performing in their everyday capacities and where there was no obvious physical cause for this inability, and where the victims behaved in an unusual, although not necessarily unpatterned, manner. We encouraged informants to give us evidence of anything they had ever heard or experienced about people who had been deflected from their ordinary social routines by other than physical injury or obvious physical sickness. We adopted this procedure because there are no precisely translatable

equivalents in Eskimo for the various terms denoting mental disorders in European languages.

There are in Eskimo usage several equivalents to such English words and idioms as simpleton, out of this world, crazy as a loon, and so on, used to describe relatively benign, commonplace behaviour which is not particularly disruptive for the person or group. Because we lacked the time, we did not pursue systematically the study of such usage, concentrating instead on usage which connotes illness. In Eskimo the nearest word translating the English mental illness in general is *niaqureriyuq,* 'he has illness of the head'. But this term does not translate literally as mental illness for it can also be applied to any organic malfunctioning of the head. Equivalent to some of the various diagnostic labels which in English are applied to kinds of mental disorder — schizophrenia, hysteria, manic depression, and so on — the Eskimos on the east side of Hudson Bay use descriptive terms such as *qavarpuq,* 'he is extremely sad', and *quajimaillituq,* 'he does not know what he is doing'. These and other terms will be explained in a later section.

It is worth noting that of all the Eskimos informants on the east side of Hudson Bay who were asked about the occurrence of *pibloktoq,* presented in the literature as the prevalent form of mental illness among some Eskimos, only one had heard of the word, and he had heard of it through a White nurse. Mr. Elijah Erkloo of Baffin Island, who assists Mr. Raymond Gagné, Eskimo language expert for the Department of Northern Affairs, informs me in a private communication that the term, which he renders as *pillukartuk,* means "two things that were supposed to fit together and do not fit together, such as a broken bone where the two ends are side by side." He had never heard the term applied to mental disorder on Baffin Island, although he suggested that it would be an appropriate one to describe a certain schizophrenic state, popularly known as 'split personality'.

Within the limits of time at our disposal and of facilities for travel we were able to build up case histories for thirty-one people, based on our personal observation in three cases and on the reports of Eskimo and White informants in the remaining twenty-eight. Futhermore, visits to the Verdun Protestant Hos-

pital, where most Eastern Eskimo patients diagnosed as mentally ill are treated, and to Department of Northern Affairs headquarters in Ottawa, resulted in our getting 'official' case histories, compiled by hospital and welfare authorities, for eighteen of the thirty-one people in our field sample. This sample includes the patient referred to above and ten others who are personally known to the writer; of this ten, eight were living apparently normal lives when encountered and were either in intervals between attacks or had not manifested signs of disorders for many years. Three who were encountered in the Verdun Protestant Hospital were officially defined as mentally ill at the time. Information on the remainder of the cases is based strictly on hearsay. Six people of this latter group of cases are dead.

Let us emphasize the tentative and exploratory nature of this study. We were unable, in fact we are unqualified, to obtain data in depth for the subjects of study. Our main purpose was to get some notion of the range of kinds of breakdown, to find out what happened to victims, and to find out what the Eskimos had to say and do about such breakdowns. It is hoped that this data will encourage others to probe deeper and more systematically than we were able to do, not only among the Eskimos of Eastern Canada, but in any locations where Eskimos are to be found.

The Region

Our survey area is in New Quebec and stretches from the tree line at Great Whale River northward to Hudson Strait, a distance of about 600 miles. Except for the Belcher Islands people who live on islands opposite Great Whale River, the Eskimo population of about 1300 is divided among five coastal communities at an average distance of about 200 miles from one another.[3]

The trend here as in other Arctic regions is for smaller permanent campsites to be abandoned in moves to one or another of these communities where, since the early 1950's schools,

[3] For community studies in this region, see Balikci (1959); Graburn, (1962); John Honigmann (1952, 1962); John and Irma Honigmann (1959); Willmott (1961).

nursing stations, missions and, at Great Whale River, a defence installation, have been established. Interaction among people of these communities is quite high; kinship and friendship networks extend the length of the coast for many people.

The economy is mixed: the chief source of sustenance and income is from sea-mammal hunting, fishing, trapping, and the production of handicrafts; government transfer payments and relief rank next, with wages the least important, but still significant, source. Contact with whalers and traders extends well back into the 19th century, although it is only since the early 1950's that intensive contact with a variety of White institutions and people has been sustained. It is estimated that only about 10% of the adult Eskimos in this region can read and write in English — although all are literate in Eskimo syllabic script — one indication of the degree of acculturation.

Anglican missionaries converted the entire population of this region to Christianity during the latter part of the 19th and early part of the present century. In the absence of White missionaries in permanent residence at one or another of these communities, in the past the guidance and coordination in religious activities was handled by Eskimo catechists, many of whom were also the headmen of their bands. Today there are three White missionaries resident in the region. In the settlements where they live the erstwhile catechists or their successors have assumed the roles of assistants and elders. Religious belief and ritual are of much significance in this region. In fact, as we shall see, the discussion of much mental illness among Eskimos is cast in a religious idiom.

FINDINGS

An examination of the observed and reported behavior of the people in our sample leads us to distinguish roughly four patterns of symptoms: a) epilepsy, both simple and complicated; b) simple hysteria; c) compulsive withdrawal with acute melancholy; d) manic depression with paranoia. We cannot characterize each person in our sample as showing exclusively one or the other of these patterns, because several persons have manifested more

than one pattern at different times or, in a few instances, during the one period of breakdown.

In this paper we deal mostly with the patterns under c) and d) in the previous paragraphs. We deal with them under their Eskimo labels: the withdrawn, melancholy people are said to be *qissaatuq* and *quvarpuq,* 'he is extremely sad, troubled, and quiet'; the apparently manic depressives with paranoia are said to be *quajimaillituq,* 'he does foolish things and does not know what he does'. It should be stressed that these terms describe *states,* not categories of persons. That is, a person is described as in a process of such and such; after the 'sick' condition has passed, the person is not in that state. This way of describing deviant states should be compared with the tendency in English to use substantive categories, such as, he *is* a manic depressive, he *is* a criminal, and so on. Such categorization enters deeply into the self-definition, even though the person concerned has been so categorized on the basis of only one or a few acts.[4]

Before proceeding with discussion of modes of breakdown, we mention briefly a condition which would be regarded as delusional, and therefore pathological, by psychiatrists but which is regarded credulously as a benign, although not particularly creditable condition by the Eskimos. We refer here to the communications people receive from the spirit world, visible and audible to them but not to others. Now it is possible for any Eskimo to see and hear spirits *(mittilik)* on those occasions when the spirits press for an audience, but some people virtually live with them without being incapacitated in their everyday roles or without interfering unduly with the daily lives of others. For many on the east side of Hudson Bay, the natural and supernatural are not clearly distinguished: visions, signs, omens are commonplace to them. Infrequently these have serious consequences, as in the notable case on the Belcher Islands in 1941 (See John Honigmann, 1962:69) where two Eskimos, one con-

[4] This is not to say that the Eskimos lack ways of classifying, that they have no general words for categories, but only that their tendency to classify and categorize persons in terms of their behaviour is much less pronounced than it is in our society. One way they have of denoting chronicity is to provide an infix meaning 'usually'. For instance, *qiisurtuq* — 'he is having an epileptic fit'; *qiirsuqattartuq* — 'he usually has epileptic fits.'

vinced that he was Jesus Christ, the other that he was God, acquired a following of disciples who killed three persons who were thought to be disciples of the devil. In the same place, in the same year, six people perished after walking naked out toward the ice edge to meet God upon the occasion of His Second Coming, this forecast having been revealed to their leader.

However, as we have noted, most everyday transactions with the invisible world of the supernatural, both sacred and profane, do not have such dire consequences. Some we are tempted to classify as simple wishful thinking. For instance, one unmarried girl of twenty in Povungnituk maintained relations for a few years with a spirit husband and spirit children; an unmarried man of Port Harrison lives with his spirit wife. One witness to the existence of this ghostly spouse is the man's father who has been beaten by his spirit daughter-in-law on visits to the son's residence. Another man, an excellent sculptor, is said to be often in the company of spirits, many of whom can be identified from his descriptions as figures or the distortions of figures from Eskimo folklore. For the purpose of our paper, we do not regard such communication with spirits as symptomatic of mental disorder, except where the person shows other, incapacitating, symptoms which require his being cared for by his fellows.

Qiirsurtuq

The Eskimos have a term, qiirsurtuq, which is the closest equivalent to a diagnostic category in the language, denoting the state we call epilepsy. We have information on six adults who are, or were in the case of two who died recently, chronic epileptics. It could be argued that epilepsy is such an obvious physical condition that one should not label it as mental disorder. However, most of the Eskimos consulted spoke of *qiirsurtuq* as a 'sickness of the head' and linked it with the notion of behavioural oddity and unusual powers. For instance, one man who has regular seizures is said to be a good diviner because of his condition. The spirits use his body to transmit messages to the living. The person in *qiirsurtuk* is 'possessed' only while he is unconscious (*illisimangerpuk,* 'he loses consciousness') whereas in other more awesome and fearsome kinds of possession the

victim remains conscious. The *qiirsurtuq* powers and the unseen forces that generate them are not spoken of in the context of Christian religion, as are the powers and spirits to be discussed later.

Qiirsurtuq does not call for any special kind of therapy. The victim is simply prevented from damaging himself and the things around him during a seizure. He is seldom incapacitated for more than a day or two. However, certain kinds of interaction are curtailed even for the mild epileptic. For instance many men are reluctant to take long hunting or trapping journeys with one who is a chronic victim of *qiirsurtuq*. The condition is not considered to be contagious. No special personal or social characteristics distinguish the six people in our sample from their fellows, except that three of the six have never married, one for the reason that she spent the period between her seventeenth and twenty-second year of age in hospital where she recently died.

The seizures of two of the six epileptic victims in the sample were of the severe type, conventionally called 'Grand Mal', which it is reported so disturbed their fellowmen that the Whites were asked to remove the victims. In one of these cases, the condition of a young man who was diagnosed as "epileptic with psychotic symptoms" in the mental institution to which he was committed, was not regarded as *qiirsurtuq* by at least three of his fellow-Eskimos in the home camp. These regarded him as a person 'possessed' (see *quajimaillituq,* below), expressed much fear of him and did not want him returned to the camp after he was declared cured in the south. We suggest that this rejection, the functional equivalent of killing or banishment in traditional times, drives the victim deeper into the clutches of his disorder, and is conducive to acts of violence against the self and others, but especially against the self in the form of suicidal attempts. Patterns of response to rejection are discussed below. At this point we only want to state that informants regard the *qiirsurtuq* condition as one which can usually be handled within the household or camp without the intervention of Whites.

Conversion Hysteria

Informants described certain patterns of behaviour for which they could not provide one or two Eskimo terms for our con-

venient classification but which seem to fit roughly into the syndrome of simple conversion hysteria. We have information about five girls who, when they were between thirteen and sixteen, were incapacitated for short periods by what informants defined as 'sickness in the head'. Typical of their case histories is the following:

> Anni was walking from her friend's snowhouse to her own. Suddenly she saw something in the sky, like a bright light. She stopped and cried out. Then she tried to run but couldn't move her feet. Her people heard her and came to help. They had to carry her into the house because she still couldn't move. For two days she just sat there asking for this and that and making people get things for her. Sometimes she would make noises nobody would understand, but sometimes she would say things her people would understand, like when she wanted something. They gave her everything she wanted and listened to her all the time. When she was all right again she told her father that she had found out about X (a man who had been killed in a hunting accident a few years earlier) and that he was in hell going around with his rifle looking for Jesus.

Our data on the personal and social characteristics of these girls do not reveal anything especially distinctive about them, nor did informants differentiate them from their agemates as having special traits other than their odd experience of getting messages from the other world. These experiences, the attacks and the messages, are said to be not as significant and serious as those of the people who have been *qissaatuq* and *quajimaillituq*, the conditions which are the chief topic of this paper.

Qissaatuq

Readers are reminded that this is not a diagnostic label; it is a term describing behaviour. It is only for the sake of convenience of reference in discussion that we use such labels. No doubt clinicians would be able to distinguish several separate symptom syndromes among the cases we have grouped together under this heading of *qissaatuq*. In this paper *qissaatuq* denotes that disturbed condition featured chiefly by compulsive passivity, withdrawal, and depression. The condition may be accompanied by brief flurries of manic activity, but mostly the patient broods silently and without moving much for days or even weeks, those around him caring for every need. A typical case history, that of a man twenty-five, follows:

> One day he did not want to move or do anything. He was always going to church but this day he didn't want to do that. He said he was too bad to go, that he was a bad man. For a long time he did not want to speak and told everybody to leave him alone. Once he laughed like a child and grabbed at (his sister-in-law) but most of the time he was very sad. His people would tell him he was a good man, but he would not believe them. One time he fell asleep and twisted around making much noise. Then he woke up and was alright and he told how he had learned from God that he had to help people and tell them what God wanted them to do.

It is very common on the east side of Hudson Bay to discuss such conditions in the context of religion, which has become a kind of generalized theory. One person explained *quissaatuq* to us in this way:

> The devil wants the man to feel bad and to do wrong and so he makes the man think too much of his troubles and what he is doing wrong. If the people tell the man he is no good and if they don't help him, the devil will get what he wants. You don't need to be afraid of a man when he is like this, because he can't give it to you (i.e., the sickness or spell). It is not like the measles. If you can get him to go to church it is all finished. But don't say too much about the church or God. People who go to church too much and pray too much can have this. When you believe too fast, it is like when you put a screw in the wood too much, too deep — the wood will break.

According to the interpretation of one informant, the struggle for the man was begun by the devil but was usually won by God who then used the person under his spell as a vehicle for His Word. One of the leading catechists on the east coast of Hudson Bay was himself directed into his vocation through a vision of Christ revealed to him while in a state of *qissaatuq*.

In some of the accounts by informants the depressed person emerges occasionally from his torpor in brief flurries of activity during which he might harm himself. For instance, here is an excerpt from a description of the sickness of one young woman:

> ...once she was left alone for a little while and she tried to stab her stomach with scissors. She pulled things from the wall and tried to eat broken glass.

Another instance from the case history of a young man mentions his attempts to burn himself and stick a needle in his ear, "because he felt so bad and unworthy to live."

The feeling of unworthiness and guilt which is a feature of so many accounts given by informants is often associated with real happenings in the community, such as epidemics, deaths of relatives, and other misfortunes for which the person perhaps blames himself. In three of the cases for which we have information, persons in *qissaatuq* made serious attempts at suicide; three others threatened or made what appear to have been feeble attempts at suicide. Lack of successful accomplishment of suicide by people in this state could be due, we suggest, to the support provided by relatives and friends during treatment of *qissaatuq*.[5]

Treatment of the condition follows from the theory of its origins. Anyone who is a respected member of the community can be one of the 'therapists'. Relatives and friends pray for the person to emerge serenely from his spell. No specialist is needed, although it is fortunate if a catechist can visit the patient to help him in his struggle. The outstanding feature of treatment is support of the patient in familiar surroundings. In four cases known to us from the east coast of Hudson Bay, people suffering from what has been described in terms suggesting acute depression and withdrawal have been cared for in White institutions with a conspicuous lack of success. The numbers of people involved in this survey are too small to justify confident statements about outcome of different kinds of treatment, but the indications are that the patient, remaining in his household, predictably responds favourably to the network of people and cultural expectations in terms of which his sickness 'makes sense'; he is thus provided with a lever of control and, indeed, provided with opportunities to gain meaningful rewards by responding in expected ways. The person in *qissaatuq* treated in unfamiliar surroundings, by people who do not know what is the matter (in his view), could be driven deeper into his shell and eventually become unreachable.

[5] In his study among the Netsilik of the Central Arctic, a group for which the suicide rate is extremely high, Balikci (1960) reports that many suicides follow upon personal disasters, such as the death of a close relative. He centers his analysis on the "lack of wider relatedness" of the person with the group. He also provides several examples where despondent people who threaten suicide are either not prevented from doing so or actually helped in their design. In our sample people who came closest to accomplishing suicide were those who received least support during their periods of *qissaatuq*.

Of the fourteen people in our sample who have undergone *qissaatuq,* nine are women and five are men. The view is that any person can fall victim to *qissaatuq* and that, of those who do, most experience it only once, unlike *qiirsurtuq,* the epileptic condition, which is chronic and which one cannot contract unless one had attacks as a child. According to older informants, *qissaatuq* is not a recent phenomenon and we have the names of three people, grandparents of living adults, who are said to have experienced this condition fifty and more years ago.

One of the differences between the men and women in the sample of people who had experienced *qissaatuq* is that the men aspired to roles in the religious life of the community, whereas the women were apparently content to report whatever messages they had received from the supernatural before returning to their everyday roles as wives and mothers.

White informants were of the opinion that this condition was to some extent inherited, citing several cases where persons in our sample were descended from people who had themselves manifested signs of breakdown. However, we were not able to confirm or disconfirm this opinion. For three people in our sample of *qissaatuq* cases there was evidence that parents had had visions and supernatural experiences, but then every second person one meets on the east side of Hudson Bay can make that claim. Our procedure of investigation was not sufficiently intensive and comprehensive to justify our making conclusive statements about the background characteristics of the people in our sample. It could be of significance that three of the five men in our sample were not highly regarded as hunters; perhaps they suffered from feelings of general inadequacy as males. But the numbers of people we have good information for is so small that it would be irresponsible to over-describe something like gender or sexual inadequacy as a personality pattern underlying proneness to the condition of *qissaatuq.*

Quajimaillituq

In contrast to the passive kind of disorder just discussed and of special interest because of its similarities to and differences from the much-described *pibloktoq,* is that form of acting out

described as *quajimaillituq,* a term which also denotes rabid dogs during the violent phase of their sickness. A person in this condition is likely to harm others as well as himself.

A feature of this condition is hyperactivity sustained over long periods, punctuated by short periods of quiescence during which the person is frequently lucid and apparently normal. In other phases the person babbles, sometimes incomprehensibly and sometimes using recognizable language in a distorted form, reminding one of the special language used by shamans in traditional times, although informants did not identify it as such.

Such glossolalia is reported for both Eskimo monoglots and others in the sample. Two patients with a limited facility in English are reported to have interspersed Eskimo with English words when in the presence of White persons, indicating some grasp of the nature of immediate reality during high points in a seizure. To illustrate, from one case history[6], that of T. of Port Harrison, the following excerpt is taken:

> ...patient begins in a steady accelerated stream: "Nursing station, (Eskimo word) North America (Eskimo word) — 35, I'm a king. Maybe doctors, A-1, 2-c, mother, Eskimo (Eskimo word) broken, this is my tongue, hit (Eskimo word)... American fighting, (patient snorts) right, old batteries, (Eskimo word) batteries, China, pewie (Eskimo word) export (patient whistles, sniffs, grimaces, makes a sucking noise) ice cream, Westinghouse (Eskimo word) etc., etc.

In the active phase the person careens about, at times spinning rapidly in order to see what is behind him, taking evasive action from the devils and other enemies he claims are chasing him. The person is suspicious of offerings, expressing fear of poison or that the thing offered is a sedative (which it frequently is) which will put him to sleep. Sleep is avoided, for the person must always be on the alert. Extremely compulsive behaviour, such as putting on and taking off of some articles of clothing, is sometimes repeated over and over again. In *quajimaillituq* the person shocks those around him with aggressive and blasphemous acts, normally strongly tabooed. A few excerpts

[6] I am grateful to Professors William E. Willmott, of University of British Columbia and Norman Chance, McGill University for their assistance in preparing six cases histories for this area.

from the entry in a diary recording one day in the career of a person in *quajimaillituq* will serve to illustrate some of these points:

> X had to be removed from his household because he was staggering about, almost upsetting the fire and slaping the face of Y's wife (Y is his closest friend). He was put in the unoccupied cabin of the school janitor... jumped on the table, upset the pan of water, sprang over the stove, burned his hands on the hot plate... When the nurse came he whimpered like a little child, then switched his stance and made as if to fondle her sexually. He would not let her give him an injection. Suddenly lucid, he said, "I don't want magic, I don't want sleep." Abruptly he spun around and punched M, the nurse's interpreter and a friend of his... He insisted on washing his hands and the plastic dishes again and again, each times spattering water about... Occasionnally he would subside, but when his guards relaxed he would leap up and dash for the door... During one quiet period of about two minutes, he whispered that he wanted to marry my eight-year-old-daughter... Gradually his motor control worsened — probably as a result of the drugs which we managed to get into him — he would reach for a cup or a record (for the phonograph) and would miss it or drop it... He kept saying things like "Santans, santans," a distortion of the Eskimo rendering in English of Satan, the devil (Eskimo = torngrak)... He screamed about the black dogs which he could see through the walls of the house — they were devils... he said that people hated him and some wanted to kill him. At about six o'clock he became so violent that the Eskimos persuaded the nurse and other Whites to let them tie him down in a home-made straight-jacket...

The onset of *quajimaillituq* is usually marked by the advent of messages from the devil or some other supernatural agent, telling the person to beware of certain of his fellows who are determined to kill him or do him some harm or that in some manner he has become infected by evil spirits. There is a hint here of something like the traditional belief in witchcraft as a likely source of one's misfortune, but there is also enough evidence of self-blame in accounting for such misfortune to discourage one from regarding the source of *quajimaillituq* in witchcraft beliefs. As far as we could discover, there are usually some empirically existential grounds for the person's fear that he has become infected or that others have something against him. Two excerpts from our notes are given in illustration:

> X, an Eskimo interpreter, had undertaken to shoot dogs in order to curtail an epidemic of rabies. Some Eskimos were against the ruling to shoot loose dogs, while most of the Whites were in favour... X

contracted influenza during the annual epidemic and was recovering from this when he began to hear voices telling him that people were against him because he had shot a few dogs and that these people wanted to harm him. He then began to act in a most unusual manner, dressing in military style, touring the settlement shooting at imaginary dogs. Within days his condition had developed into a full scale *quajimaillituq*.

K, from Port Harrison, had helped take care of X during his attack. When this was over he went hunting. As he was returning to the settlement from this trip, he heard devil voices telling him not to proceed so as to reach the settlement that night, but to stay overnight in a snow-house. The devil voices told him that if he proceeded to the settlement that night, food which would be given him would result in his death through poisoning. The next day when he arrived at the settlement he began to show signs of *quajimaillituq*. Those in his household who knew these signs removed the rounds from his rifle, sought help from the nurse as they feared K had been infected while caring for X. The nurse persuaded K to take sleeping pills and he awoke next morning feeling better, thus narrowly escaping the clutches of *quajimaillituq*.

Attacks of *quajimaillituq* usually last for more than a week and sometimes for as much as three weeks. Futhermore, victims have recurrent attacks on an average of once every two years, usually in the late winter. Since the time when fairly regular air service was established after 1957, the Eskimos have often requested that the Whites evacuate the more violent cases. It is significant that in dealing with this illness the Eskimos call for White help. According to one older informant, until a generation ago the Eskimos would simply kill a person in a state of *quajimaillituq* if he was particularly violent and showed no progress in coming out of that state. When asked for evidence of this practice, the informant told very simply how he himself had killed his own brother after the latter had attacked members of the family. In three recent cases a kind of ritual death and purification of the victim was carried out by stripping him of his personal belongings and burning these, as a last resort.

Eskimo handling of *quajimaillituq* follows from their theory of the origin and meaning of it. As in the case of *qissaatuq*, this theory is set in a religious context by most of the informants consulted. There are minor variations to the theory, but a generalized version is something like the following: the devil takes over

the person who does not live right, he infects the person in the way a germ infects, and tries to spread the sickness through contact with the infected one. He tries to get at Eskimos whose 'head and heart are not strong'. For this reason it is advisable to get only certain people to take care of one who is *qualimaillituq*. Some communities have one or more specialists who are capable of guarding an infected person without themselves succumbing. Whites are impervious to infection, a commentary, incidentally, on the sharpness of social and cultural differentiation between Whites and Eskimos. In any event, because Whites are impervious to infection from an Eskimo source, they can be asked to guard an infected person. However, some informants claim that the Whites tend to be too permissive with the possessed person, listen to him too readily, and thus unwittingly encourage the devil in his design, for it is really the devil and not the person himself who initiates all that happens. Evidence that this is so can be found in the extraordinary powers of the body of the possessed person: he can see behind him, through walls and over great distances, has fantastic strength, and can throw glass things about without their breaking. Again, because it is the devil in command of the victim's body, rather than the victim himself, with whom one has to contend, it is excusable to tie a person down and pummel him when he gets too violent or utters blasphemies. From the writer's observation, this pummeling is the equivalent of shock treatment and is usually effective in the short run.

In the treatment of *quajimaillituq,* the patient is alienated from his everyday surroundings and contacts much more than is the case with *qissaatuq* and is treated in many ways as a nonperson. Again, because the number of cases with which we deal is so small, we cannot be conclusive, but the information we have indicates that extreme alienation, that is, taking the person in an advanced state of *qualimaillituq* completely away from his home community and sending him to a hospital in the south, results in rapid recovery, although it does not guarantee against recurrence. None of the four persons known to us with these symptoms who had been sent to institutions in the south had retained the symptoms for very long. Perhaps the shock for an Eskimo of finding himself in such an exotic and bewildering place as a mental

institution is sufficient to break through the tangle of anxieties and fears in which he had been emmeshed as a victim of this particular form of disorder.

As with the victims of *qissaatuq* we tried to discover what obvious personal and social characteristics, if any, distinguished the victims of *quajimaillituq* from their fellows. Again the sample of six people, representing seventeen separate attacks, was too small to permit statements about genetic predisposition. Nor did we learn of anything obviously distinctive about the physique and dietary habits of the people in question, physiological factors which we thought might be of some importance considering the seasonal occurrence of the illness. It is worth noting that in our sample of six only one is a woman. The pattern for the five men was to first experience this kind of possession in their late teens or early twenties and to experience recurrent attack, frequently — as we have noted — in late winter when physical resistance is low and after themselves suffering a period of some sickness, such as influenza or measles.

With reference to characteristics of social role, it is tempting to stress one possible predisposing factor in the situations of these men: they all have had difficulty in getting a wife or have not yet acquired one. However, it is not possible to conclude with the data at hand whether it is the wifeless state that predisposes the man to this form of breakdown or vulnerability to the form of breakdown which discourages women from espousing these men.

There is one matter which is worth reporting, not so much for the light it throws on etiology and predisposition but rather for the light it throws on shifts in folk-theories about causes: and that is the emergence of a kind of 'culture-conflict' explanation for *quajimaillituq*. To illustrate this let us refer back to the case of X outlined above.

The writer and other Whites who had had little or no experience of mental illness among the Eskimos at first inclined towards the view that the basic source of trouble with X was that he was a 'marginal man' whose White father, a trader who abandoned the mother while X was a baby, was a fantasy model pulling X towards the Whites while the rest of his relatives pulled

him towards the Eskimos. The incident of the dogs, mentioned above, brought this underlying tension to the surface, the conflict overwhelmed X, and *quajimaillituq* resulted. With this premature hypothesis forming in our mind, we sought information on other victims who might be in this special marginal position. Unfortunately for the hypothesis, three of the *quajimaillituq* victims for whom we eventually got information were without this background, in fact were less 'acculturated' than most Eskimos; furthermore, we were reminded that some of the most stable and symptomless people in the community were of ethnically mixed parentage and to some extent marginal with reference to the White and the traditional Eskimo cultures.

The point we make here, however, is that our initial view, our premature hypothesis, was given credence by two Eskimo informants. One of these, an elder whose views are given much weight in the community, pointed out in a sermon that X's sickness would not have happened if his father had been Eskimo. He used X's predicament as an object lesson in the sermons, the gist of which was a warning to the young girls to evade the advances of young White men. His remarks reflected a growing feeling of disquiet in the community over the recent procreation of two babies by unmarried girls who had been impregnated by two young clerks of a trading company. The warning was that this kind of mixing between people of ethnically-different backgrounds would give rise to more *quajimaillituq*. We suggest that this kind of interpretation will gradually supplant the favoured religious interpretation of the causes of breakdown.

SUMMARY AND DISCUSSION

In this exploratory and chiefly descriptive study, we have shown that several different symptom patterns or syndromes of mental disorder occur among the Eskimos on the east side of Hudson Bay and that the occurrence of no pattern dates only from the period of intensive contact with Whites after the 1940's. Because this was not a census-like survey, we are unable to calculate rates of disorder of different types for the population in question, nor can we state firm conclusions about the prevalence

of a given pattern among a given category of the population, although the evidence does suggest that conversion hysteria is most likely to occur among young women and the condition of *quajimaillituq* among young men. Before we can make confident statements about differential rates and trends over the generations, we require much more detailed study of the contemporary population and of past ones, based on the recollections of older people as well as on a thorough search of documents left by early travellers in this region: explorers, whalers, traders, missionaries and police. A few hypotheses which could serve as guide lines for such a study are offered during the course of the discussion which follows.

It is hoped that we did not contribute to a new oversimplification of thinking about Eskimo's mental illness by suggesting that each person in our sample manifested one or another syndrome exclusively, or by giving the impression that Eskimos make neat and widely agreed upon distinctions among the various syndromes discussed. We did not want to clutter our brief sketch of each pattern by relating how various persons in our sample manifested more than one pattern. For instance, one person who was a chronic epileptic, at least according to medical diagnosis, was also described by informants as a victim of *quajimaillituq*. Two who had passed through periods of *qissaatoq,* ultimately manifested symptoms resembling the state of *quajimaillituq*.

Moreover there was lack of concensus in the folk diagnoses in some cases. For example, when we asked people in one household to tell us about any person they knew who had gone through the state of *quajimaillituq,* the name of M was mentioned as such a person. The informants told how they had appealed to the Whites to remove this man who had threatened suicide and homicide. Informants in another household denied that M had been *quajimaillituq*. They claimed he was simply a very bad tempered man who would fly into a rage whenever he did not get his own way. Later we were provided with a case history about M in which the R.C.M. Police reported that they were forced to arrest M and take him away from his camp because of complaints about his wild conduct. In this report, the police opine that M is not insane, but exceptionally bad-tempered. They point

out that M reacted violently only because he was very much jealous of his wife and was enraged at his rejection by the people in his camp. Such lack of concensus over diagnoses can be reported for several cases in our sample, but we do not elaborate on this matter here because our chief interest is in the different syndromes as such; in some of the common explanations for the occurrence of these syndromes; in the community response to breakdowns; and in what happens to victims.

Despite these limitations in the data, the study does provide some themes which are worth exploring if even in only a speculative vein. Of these themes we select for discussion the following: aggression and paranoid ideation; the use of the religious idiom in folk theories of mental illness; incorporation and rejection as means of control.

As we pointed out earlier, aggression — particularly against others — and paranoid ideation in Eskimo mental disorder patterns have been regarded as rare or absent by students of the subject. In our sample all people in a state of *quajimaillituq* and a few in *qissaatoq* struck out at people around them, although serious injury to their targets did not occur. The targets were always first degree relatives, spouses, or close friends. However, in at least three cases under review, verbal aggression is reported against people and institutions other than ones close to the person. The institutions concerned have been the Department of Northern Affairs and National Resources and the R.C.M.P. Perhaps physical aggression against representatives of these institutions is, among this population, unthinkable even when thought processes have been apparently suspended. It is interesting to note that one of these institutions, the R.C.M.P., appears in the paranoid ideation of one person who claimed that a constable was concealed in a clothing trunk, waiting for the right moment to emerge and kill the victim. Another was certain that the Liberal Party was determined to force him and other Eskimos to change their religion to Catholicism, an echo no doubt of the contemporary political issue concerning the assumption by the Province of Quebec of control over Eskimo affairs. Further study of mental disorder must include a systematic search for the content of paranoid ideation, and the targets of verbal aggression as well as the targets of physical aggression.

The tendency to formulate explanations about mental illness in terms of the supernatural, and specifically Christian, order is hardly a novel one: the history of European society provides us with an abundance of illustrations of the attribution of 'insanity' to possession by the devil and a view of the afflicted person as a field of operations for battles between good and evil spirits. We have suggested elsewhere that "the contemporary Eskimo stress on (devotional) observances may be interpreted as continuous with the traditional (pre-Christian) emphasis on conforming to rules laid down by spirits" (Vallee, 1961, Chapter 8). We suggest here that the use of this religious 'thought model' to explain certain kinds of mental illness is most likely to occur, a) in those regions where there was much 'absentee' Christian religious guidance, that is, where professionally trained missionaries contacted the Eskimo only infrequently on their tours, and where there was much dependence on Eskimo catechists; and b) where these catechists were also headmen in their camps.

The reasoning here is that under this delegated guidance system, there would be much syncretism, or adapting of new messages to the old containers, there being scant opportunity for the professional missionaries to check and correct the interpretations of catechists. The second consideration in this reasoning is that, where the catechist was also the headman, there would have been a mutual reinforcement of his supernatural and social influence, making it all the more probable that *his* 'definitions of the situation' would be the ones adopted. The hypothesis that the use of the religious thought model to explain mental disorders would be prevalent under the conditions listed above, can be tested in future research for there has been a variety of religious contact situations in different regions in the Arctic. For instance, in some places there were resident missionaries from the earliest conversion days; in others, there were catechists who were not headmen; in still others, there was only sporadic contact by the missionary with no catechist to carry on between his visits, and so on.

The use of thought models common among Whites in the Arctic which we noted earlier, specifically the culture conflict — marginal man type of explanation, makes headway among the Eskimos, we suggest, where the social structure of the community emphasizes the distinction between White and Eskimo and

where institutions other than religious ones, for example cooperatives, mines, Department of Transport or Defence installations, are prominent on the scene. In such settlements, and these are larger than the traditional camps, the religious institution is just another part of the large scene, the Eskimo religious spokesman tends to assume the role of philosopher and interpreter of current affairs, much of which are cast in social and economic idioms rather than religious ones.

To return to the subject of coincidence in office of headmen and catechists: as we pointed out earlier, on the east side of Hudson Bay, or at least in most parts of it, the touring missionary-headman catechist pattern was the prevalent one. A number of other hypotheses pertaining to the association between this pattern and aspects of mental disorder occur to the writer. For one, we suggest that, all things equal, there would be a higher degree of camp integration where the catechist was also the headman than where he was not, and that favoured forms of deviance where integration was comparatively high would be those of mental disorder rather than of suicide and murder; and furthermore, that where suicide and murder occurred in such camps the rationale for them would have been a supernatural one.

A final hypothesis pertaining to such camps where the catechist-headman roles coincided is that the aspirants to this double role would be under special strains and would be prone to suffer states of *qissaatoq*. As is well known, leadership among the Eskimos in most regions was based on a balance between ascription and achievement. Typically the senior son of a headman would take over the latter's status, provided he was at least an average traveller, hunter and trapper and measured up to Eskimo standards of masculinity. Persons who did not measure up to such standards would not be acceptable (Vallee, 1961: 192f.). We suggest that considerable anxiety about personal worth would beset eligible aspirants, particularly those who had reason to doubt their own abilities, and that where the living up to the role of religious leader was an added requirement, anxiety about personal worth would be compounded. Another factor conducive to the selection of this syndrome among aspirants for leadership who experience breakdown is the traditional cultural precedent pertaining to the role of *angnakoq*, or shaman. The

qissaatoq acting out pattern, with its highlights of withdrawal, suffering, the experience of visions — reported in most of our cases — is in many ways continuous with the traditional pattern for the aspiring *angnakoq,* or shaman (Vallee, 1961:166f). At this stage of our speculation, we confine the hypothesis under discussion to males. Our data suggests that the *quajimaillituq* syndrome is more likely to occur among males who are not serious aspirants to leadership, their disqualification being mainly on ascriptive grounds, for instance, having had the 'wrong' father. It is suggested that the aggression shown by those in a state of *quajimaillituq* is a kind of dramatic statement that 'this system is all wrong', while the dramatic statement of the person in *qissaatoq* is that 'I am not good enough for this system'. Perhaps the latter orientation will be found to be characteristic of the women afflicted with *qissaatoq* whose husbands occupy those kinds of roles which are highly valued by Whites and which bring them into frequent contact with the people in the new order. We refer here to relatively unacculturated women whose husbands are interpreters, store clerks, special constables, and the like.

There remains to discuss a few aspects of the community response to breakdown, namely patterns of incorporation and rejection. In a majority of breakdowns for which we have information, the afflicted person's incorporation within the local group is reflected in the way support and care are extended from every side. At the same time we have cited examples in which persons have been rejected, typically through getting them into the channel of the official White police or medical apparatus, channels which usually lead to the south. At first we accepted the interpretation of a few informants that such persons would be banished only if they were possessed by the devil in a state of *quajimaillituq.* However, in going over the stories of what happened to the various people in our sample, we found that some who had not been described to us as *quajimaillituq* were handed over to the Whites when their condition became such that one or more persons had to devote most of their time to guarding and caring for them. An example is one young man who was simple-minded (*aqittungajuq*) and harmless, but who kept wandering off and causing search parties to be organized in order to find him. Eventually, the people in his camp virtually forced the Whites to take

him away from the camp because "he was taking up the time men and women needed to fish and trap and carve. The *kalluna* (White man) can take care of him".

We do not mean to imply that any Eskimo delivered to the Whites and despatched to a mental or other institution in the south is automatically written off by his family and friends. The mere calling in of the police, nurse, or some other White with the expectation that the sick person will be taken away from the household does not mean that the people concerned want permanent rid of the sick one. If after many fearsome experiences, they 'give up' on a sick person, they will indicate to the Whites whether or not they want the person back again. Our evidence shows that the Eskimos on the east side of Hudson Bay are chance-givers: that is, they will bear with a person's disruptive behaviour, give him another and yet another chance, until the strain on the small group reaches a certain point. Several informants claimed that in former times when that certain point was reached, the person would either be abandoned or killed. A functional equivalent in modern times, we suggest, is to hand the person over to the Whites with the message that he is not wanted back.

From the limited evidence on hand, it appears that the latter course is most likely to be followed by people who live in small camps of anywhere from fifteen to sixty or so people, for the smaller the group the less likely it is to be able to handle persistent disruption. The evidence we have indicates that in settlements with a substantial population, such as Povungnituk with about 500 or Port Harrison with about 200, rejection is less likely to be as complete and final as in the camps. Where such settlements have been in existence for several years and some integrative social machinery has developed, the resident gets incorporated into networks of kin and friends which are of wider span than the ones in the smaller camps. Through the new forms of organization in such places, for example, congregations, vestries, cooperatives, community councils, energies can be coordinated and applied in a calculated, although personal, way to retain and control the deviant person. For instance, at a meeting of the community council of Great Whale River in 1964 it was agreed that the community was not doing enough for an Eskimo who had

returned to the Arctic after three years confinement in a mental institution. The relatives in his household were concerned because he was showing signs of depression and apathy. It was reported that the people present at the meeting decided to try to "cheer him up and make him feel interested in the world," by visiting him, bringing gramaphone records, magazines, and the like. A similar issue arose and was dealt with in the same way at a meeting of the Povungnituk Cooperative Society with reference to one of its members who had suffered a breakdown.

Future research should make a point of studying in detail the conditions under which incorporation or rejection are likely to occur. For instance, we hypothesize that communal efforts to help will be extended to those mentally ill people who are defined as *belonging* to the settlement and whose families live there; and that the mentally ill stranger will remain alienated.

We conclude by observing that of the practical problems facing people responsible for treating mental illness — difficulties of communicating across linguistic and cultural boundaries, which reduce the diagnostic process to something like inspired guesswork and which virtually excludes the technique of psychotherapy under present conditions — none is more acute than that of rehabilitation in those cases where the people in the home community do not want the convalescent to be returned or where home conditions are inappropriate. For example, an epileptic with associated neurotic symptoms has his epilepsy brought under control with medication and the associated neurotic symptoms disappear in the south. For such a person to return and lead a normal life requires regular medication and surveillance by a trained person, such as a nurse, services which are unavailable in many Arctic localities.

In our sample were several cases, two epileptics with psychotic complications, three described in case histories as schizophrenic and two as manic depressives, in which the persons declared ready for discharge from the mental institution who have wanted to go back to their home communities have been sent elsewhere, because a) the people in their home communities declared they did not want them, or b) medication and professional care to prevent relapse could not be provided in the home

communities. Such people typically suffer rapid relapses in the places to which they are sent — rehabilitation communities and foster homes in the Arctic, or convalescent homes in the south — and complete the circuit by their readmission to the mental institution, where the pattern is for them to once again make what is often described in case histories as a 'good adjustment' to hospital life, the kind of adjustment which perhaps does not augur well for their chances of adjustment in the home community.

To the suggestion that unwanted people who are declared ready for discharge from a mental institution should remain in the south and try to fit into the social scene there, we reply that the strains of adjustment for those few normal Eskimos who do live in the south appear to be so great as to offer a dismal prognosis for the successful adjustment of those with special personality problems, one of those problems being in many cases the compulsive yearning for acceptance in one's own terms, an acceptance which is unlikely to be forthcoming for an Eskimo in the south except in the protective environments provided by hospitals, convalescent homes, and the government offices in which many Eskimos in the south work.[8]

Apart from the interest in making contributions to social psychiatry and to the cross-cultural study of mental illness and other theoretical enterprises, certain practical considerations should inform future research on mental illness among Eskimos, arising from the need to provide:

a) an understanding of the specific predisposing and precipitating factors in various disorders, in the interest of prevention;

b) an understanding of the folk interpretations or thought models used locally in explaining certain disorders and in justifying local responses to the disorder, in the interest of diagnosis, prevention, and therapy;

c) an understanding of how social organization in different community settings operates with respect to handling disorders, in the interest of prevention, therapy, and rehabilitation.

[8] Our remark about these special strains in the lives of some adult Eskimos who live in the urban south is based on impression. As far as we know, there have been no studies published on this, a matter which begs for careful research.

It is hoped that the descriptive material and speculations in this rambling research note are useful in suggesting lines to follow in designing a long term study in which Eastern Arctic patterns would be put in the perspective of the entire culture area.

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Demographic Analysis of a Laboratory Cadaver Population

BY TEUKU JACOB

RÉSUMÉ

Il s'agit ici d'une analyse démographique d'un ensemble de cadavres provenant du Java Central et de la région de Jogjagarta. Cette étude a été faite au département d'anatomie du collège médical de Mada, Université Jogjakarta, en Indonésie. La population cadavérique en question comprenait 390 sujets dont 253 mâles et 137 femelles. L'article nous donne la longévité de ce groupe, les causes de la mort et le statut économique, de leur vivant.

PURPOSE

Since the beginning of 1950 until the end of 1962 the Laboratory of Anatomy, Gadjah Mada University College of Medicine, Jogjakarta, Indonesia, had received 390 cadavers from various institutions in Central Java. This study is an attempt to investigate trends exhibited by the cadaver population in the length of life and diseases suffered at the time of death, and to compare them with other populations.*

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MATERIAL AND METHOD

The total number of the cadavers is 390, and the material was received during a period of twelve years from cities and towns tabulated in Table 1.

TABLE 1

Table 1. — Places of origin of the cadavers according to sex

Place	Female	Male	Total
Jogjakarta	61	113	174
Semarang	20	60	80
Magelang	25	27	52
Kebumen	8	8	16
Klaten	5	10	15
Pusworedjo	4	10	14
Surakarta (Solo)	5	7	12
Purwokerto	1	4	5
Wonosobo	1	3	4
Purwodadi	2	2	4
Muntilan	3	0	3
Banjumas	0	2	2
Gombong	1	1	2
Kudus	0	1	1
Salatiga	0	1	1
Wonosari	0	1	1
Unknown	1	3	4
Total	137	253	390

The population consists of 137 females and 253 males. Breakdown in age groups is shown in Table 2.

TABLE 2

Table 2. — Age groups of the cadavers according to sex

Age in Years	Female	Male	Total
Less than 1	11	18	29
1-10	0	0	0
11-20	21	31	52
21-30	28	54	82
31-40	19	51	70
41-50	25	39	64
51-60	9	19	28
61-70	5	13	18
71-80	8	2	10
81-90	0	1	1
Unknown	11	25	36
Total	137	253	390

Data concerning the cadavers were drawn from the anatomical records obtained from the hospitals, penitentiaries, social institutions and police stations whence the material originated. It

is obvious that such information is never complete or absolutely accurate. Thus, in several cases the age and cause of death were not documented.

Age information was no doubt derived antemortem from the persons concerned, or postmortem from the police, doctors or clerks of the source institutions or from the parents in case of several babies. The younger the person, the more accurate is the information on his age, as for instance the newborns and children. Age information on the adults was based on their memory which was rarely accurate; even if it were correct it was usually calculated in Javanese lunar years. Estimates made by policemen, doctors and clerks leaned generally on facial appearance, skin condition, gait etc., and vary in accuracy with the experience of the particular observer. Ages in the adults, given or estimated, are usually multiples of five (Cobb, 1952:799; Krogman, 1962: 28), so that age data cluster around those figures. It is obvious, that the age recorded may be over or under the real age. Thus, if in this study we use an interval of one year for age groups, errors will certainly accumulate. Consequently, we group the material with intervals of 10 years, so that errors will be reduced considerably, since memorized or estimated age rarely exceeds the real one by 10 years. And besides, the trends we are searching for extend over long periods, such as whether the mortality peaks take place during the infant, juvenile, adolescent, mature or senile period. We think, therefore, this demographic study is valid, despite the foregoing weakness in age records.

Data pertaining to the cause of death are obviously not exact. Occasionally, an individual was found dead on the street or died on arrival at the hospital, so that even clinical examination was lacking or incomplete. Classification of the causes of death accurately by means of international codes is totally impossible, because more often than not laboratory tests or autopsies were not performed. But that is beside the purpose of this paper. We only try to know the disease suffered by a particular subject prior to his death which either caused his demise or served as a base for the direct cause of death; and furthermore, the diseases are classified in a general way according to systems involved, or even according to the embryonic origin of the systems. Therefore, from this point of view this study is also justified. It should be

noted that if the terminal diseases were not known, they were not documented in the anatomical records.

RESULTS

Year of death

During the twelve year period the number of cadavers received each year is tabulated in Table 3 according to sex.

TABLE 3

Table 3. — Years of death of the cadavers according to sex

Year	Female	Male	Total
1950	1	3	4
1951	7	10	17
1952	8	18	26
1953	12	18	30
1954	15	25	40
1955	20	26	46
1956	8	29	37
1957	9	24	33
1958	11	27	38
1959	10	17	27
1960	9	9	18
1961	11	18	29
1962	16	29	45
Total	137	253	390

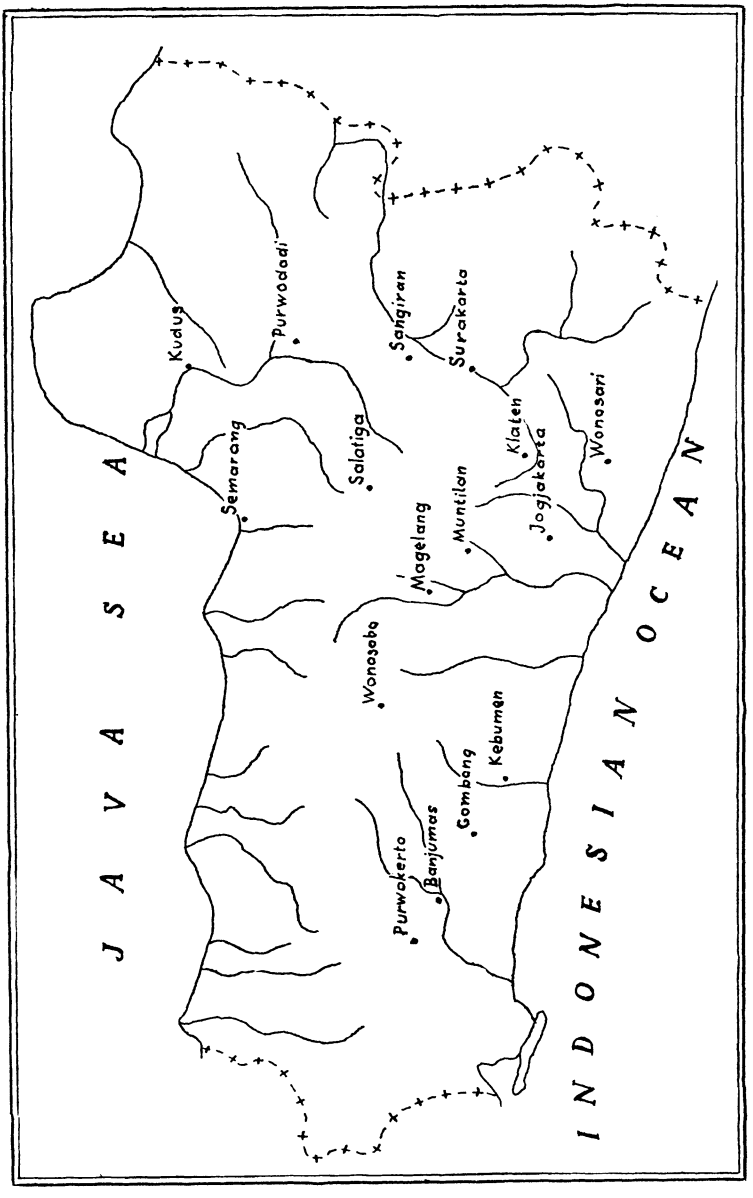

Fig. 1. Map of Central Java, showing the places of origin of the laboratory cadavers studied. The *Pithecanthropus* site of Sangiran, better known to anthropologists, is included for orientation.

It is obvious that the peak occurred around 1955, while in 1950 very few cadavers were obtained. Female cadavers reached their peak in 1955 too, and the male in 1956. The least number of cadavers received for both sexes was in 1950.

Place of origin

As expected (Table 1) most of the cadavers came from Jogjakarta, the seat of the Laboratory, which did not present any transportation problem. Runners-up are the following towns: Semarang and Magelang (Fig. 1). Next come Kebumen, Klaten, Purworedjo and Surakarta with almost equal figures. From other towns the figures are negligible.

FIGURE 1

Female cadavers originated mostly from Jogjakarta, but the second source was Magelang. The great number of male individuals from Semarang made this third largest city in Indonesia the second source of our laboratory cadavers.

Age

Age groupings are shown in Table 2. It is interesting to note that age group 1-10 years is lacking. The mode is in the 21-30 years age group. There are only 11 subjects over 70 years of age. Age data concerning 36 individuals, consisting of 11 females and 25 males, were not recorded. Twenty-nine cadavers are below one year of age.

In the females the peak of mortality occurred in the 21-30 years age group, with a minor peak in the 41-50 years age group. Eleven subjects are below one year and 11 are over 70 years.

The males showed a mortality peak in the 21-30 years age group too, followed by the 31-40 years age group. Eighteen cadavers are under one year and only three over 70 years.

Terminal diseases

Data pertaining to the diseases contracted at the time of death were recorded in 59 males (23.32%) and in 27 females (19.71%), giving a total of 86 individuals or 22.05% of the material. In 9.23% of the cadavers, consisting of 13 females and 23 males the only information documented was "homeless."

The terminal diseases found in the material are summarized in Table 4.

TABLE 4

Table 4. — Terminal diseases suffered by the cadavers according to sex

Disease	Female	Male	Total
Gastrointestinal[1]	11	17	28
TB and respiratory	2	21	23
Perinatal	7	9	16
Accidents[2]	3	4	7
Mental and nervous	0	3	3
Cardiovascular	3	0	3
Malaria	0	2	2
Renal	0	1	1
Tetanus	0	1	1
Diabetes mellitus	0	1	1
Neoplasm	1	0	1
Total	27	59	86

[1] Including undernutrition, hepatic cirrhosis and infection.
[2] Including physical trauma, homicide, suicide and burn.

We can conclude that, as expected, the most frequently encountered ailments are gastrointestinal, followed by respiratory disorders and perinatal death. Among the gastrointestinal disorders undernutrition is prevalent, either below or beyond 50 years of age. Similarly, TB is predominant among respiratory diseases regardless of age. All of the few cases of cardiovascular disorders occurred at age 50 or over.

If the diseases are classified according to the systems involved, we obtain the following table.

TABLE 5

Table 5. — Terminal diseases of the cadavers according to systems involved

System	Female	Male	Total
Gastrointestinal	11	18	29
Respiratory	2	21	23
Urogenital	3	1	4
Nervous	0	4	4
Blood and cardiovascular	3	2	5
General[1]	8	13	21
Total	27	59	86

[1] Including accidents and perinatal death.

Thus, the system most frequently attacked is the gastrointestinal tract, and the second is the respiratory tract; together they contribute to more than 50% of the cases. In other words, systems of entodermal origin are involved in 52 cases, mesodermal systems in 9 cases, whereas ectodermal systems in only 4 cases (Table 6).

TABLE 6

Table 6. — Terminal diseases of different populations according to the embryonic origin of the systems involved

Embryonic Origin	Entoderm	Mesoderm	Ectoderm
Present study			
Female	13	6	0
Male	39	3	4
Total No.	52	9	4
%	80.0	13.8	6.2
Jogjakarta[1] %	53.7	36.2	10.2
Pearl[2] %	57	30-35	8-13

[1] Calculated from Biro Statistik etc. (1961).

[2] Cited from Hooton (1956).

DISCUSSION

Laboratory cadavers in Indonesia originate solely from the lower socioeconomic stratum which is very vulnerable to ups and downs in economic condition and highly exposed to the hazards of life. These factors, however, are not the only ones affecting the number of cadavers received by the laboratory in different years. Other influencing factors to be considered are the activity of the laboratory staff in obtaining cadavers from different towns and the transportation facilities on which this largely depends. And not all hospitals from the places mentioned previously contributed their unclaimed dead since 1950 to the University.

The important sources of the cadavers population are Jogpakarta, Semarang and Magelang. Big cities seem to be more attractive to the economically marginal stratum, since they offer more niches to the homeless, beggars etc. than do the smaller towns, and thus effecting migration to urban areas. In addition, it has to be taken into account that large hospitals and peniten-

tiaries are found in big towns. The only exception in this case is the city of Surakarta from where not many cadavers were derived. The more distant a place is from Jogjakarta the less likely it is that its cadavers will be taken to the laboratory.

It is not surprising that male cadavers exceed the female in number, but compared with other laboratory cadaver populations the female subjects in our population are relatively more numerous (39.7%). Among the cadavers obtained from Jogjakarta we notice that the females constitute more than 50%, while among those from Magelang the male:female ratio is almost 1:1. The average ratio in the whole material is about 3:2, which remains practically constant for every age group, except for age group 31-40, where it is 5:2 and for advanced age groups of which the samples are very small. Cobb (1935) reported 18% females among the Western Reserve University laboratory cadavers of 1911-31 (total population: 2139), giving a male:female ratio of 9:2. Krogman (1962) gives the usual ratio of 15:1 for laboratory cadavers in the U.S. This fact indicates that in the U.S. female dead are less often left unclaimed. It is also most likely that in Indonesia the female has more obstacles in obtaining a job to sustain her life.

The age group 1-10 years is not represented, partly due to the reluctance to receive children for student dissection; infants below one year of age are present in the population, because they were mostly contributed by the University hospital maternity wards.

Only 57 subjects (16.1%) are beyond 50 years, a condition comparable to the state of affairs in Germany of 1910 (de Froe, 1948), where people above 51 years were 18%. The number of old females, although slightly larger (17.5%), does not differ markedly from that of old males (15.4%). It is only in advanced communities that the number of elder females exceeds that of the males significantly (Wrong, 1956:44). There is also no marked sex difference in the number of young people under 30 years of age (female, 47.6%; male, 45.2%).

The death rate reached its peak in the 21-30 years age group, in other words right after the cessation of growth. This means

that only a small portion of the potential length of life could be attained by our population (about 114 years, according to Cobb, 1954). No old age peak is found, indicating that the group did not enjoy public health facilities brought by cultural progress. Vallois (1961:224) commented that "until the beginning of modern times, the average length of life did not exceed thirty to forty years." In the female a minor peak is observed in the 41-50 years age group, but no trends in the pattern of disease contracted in this group could be traced, since 21 of 31 people in the age group did not present the needed data.

Compared with the Western Reserve University cadaver population previously referred to (Cobb, 1935, 1935a), we noticed that the length of life in our material is lower (Table 7).

TABLE 7

Table 7. — Mean age at death in various populations

Population	Age at death
Gadjah Mada U. lab cadaver	21-30
WRU cadaver (Cobb): Negro	37
White	45
Ancient Roman (Mcdonnell, cited by Cobb)	21-30
West African Negro (Todd, cited by Hooton)	30
Tasmanian (same)	< 25
Indian Knoll (Churcher and Kenyon)	30.4
Indian Tabor Hill (same)	30.35
17th century Genève (de Froe)	26
50 B.C. (Dublin, Lotka and Spiegelman, cited by Harrison)	22
Early Iron and Bronze Age (same)	18
Prehistoric: (Harrison)	< 40
(Dahlberg)	25-30
Mesolithic (Vallois)	21-30

We speculate that the Cleveland population enjoyed relatively more advantages brought by public health measures. A peak of mortality between 21-30 years reminds us of certain populations of West Africa, Tasmania (Todd, cited by Hooton, 1956), ancient Romans (McDonnell, cited by Cobb, 1954), a Bronze Age English town (Dublin, Lotka and Spiegelman, cited by Harrison, 1958), American Indians (Churcher and Kenyon, 1956), Genève in the 17th century (de Froe, 1948), and even of prehistoric men (Dahlberg, Vallois, 1961). This again emphasizes the tremendous influence that could be brought about by public health upon human longevity in a relatively short time.

Of the diseases suffered at the time of death most affected the gastrointestinal tract, and then the respiratory tract. Those are diseases of poverty and exposure which reveal the absence of basic medical care. The two systems are responsible for more than 50% of death, which is in conformity with the statistics of Pearl on deaths in the U.S., England and Brazil (Hooton, 1956:273). In Negro and Caucasoid cadavers of Cleveland (Cobb, 1935) the most commonly affected system was the respiratory tract, probably due to the cold climate and the lesser incidence of malnutrition. In Jogjakarta in 1959 the highest mortality in the total population was also due to respiratory diseases (35.9%, calculated from Biro Statistik etc., 1961). In the U.S. of 1900 the leading causes of death were respiratory (TB and pneumonia) and gastrointestinal (diarrhoea and enteritis) diseases (Wrong, 1956:45).

The main terminal diseases in our cadaver population either below or above 50 years of age are gastrointestinal. In this group liver cirrhosis is due to undernutrition and not to alcoholism which is not a problem in a predominantly Moslem country such as Indonesia. TB does not affect any particular age group more often than the others.

The frequent gastrointestinal respiratory disorders make the entoderm the most sensitive system in the cadaver population. This fact is in accordance with Pearl's statistics and with the condition in Jogjakarta in 1959. The second system commonly affected is of mesodermal origin and this is more emphatic in the females owing to the relatively higher rate of urogenital diseases.

However, the mesodermal systems in our material are attacked to a lesser degree compared to Pearl's findings, which are more closely comparable to the condition in the total population of Jogjakarta. The last and the least affected systems derived from the ectoderm, the most differentiated embryonic tissue.

We expect that through socioeconomic improvement in the future the mortality peak of laboratory cadaver populations in Indonesia will shift upward from the 21-30 years age group, and that ailments will more often threaten the mesodermal systems rather than the entodermal. To facilitate the study of these demographic data periodically it is recommended that death records made by the police or hospitals should be more complete and accurate.

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Some Recent Archaeological Discoveries from Prince Edward Island

BY RICHARD J. PEARSON

RÉSUMÉ

Il s'agit ici d'une expédition archéologique dans l'Ile du Prince-Edouard. Organisée par le Musée National du Canada, l'expédition était dirigée par l'Auteur lui-même, assisté du Docteur J. Maloney et de monsieur B. Burns de Charlottetown. On explora et localisa exactement dix-huit sites, collectionnant en surface et creusant de petits puits là où la chose était possible. Les quelques pages de cet article décrivent les sites, la nature des artifaits collectionnés, ainsi que les relations des différents sites avec d'autres sites ou complexes de la région environnante.

One of the areas of Canada most poorly known archaeologically has been Prince Edward Island. Despite the fact that so much of the island's acreage has been cleared and cultivated, archaeological specimens have been rather rare and are only slowly coming to the attention of archaeologists. The only public collection of artifacts in the province is a collection of Nova Scotia artifacts, largely of unknown provenience, loaned to the Montague Museum from the Nova Scotia Museum of Science in Halifax. Recently, interested amateurs have been surface collecting and recording site locations. Extensive excavation has not, however, been attempted.

In 1961 and 1962, a field party which the author directed, from the National Museum of Canada, spent 23 days in Prince Edward Island, and, with the help of Dr. J. Maloney and Mr. B. Burns of Charlottetown, located and explored 18 sites, making surface collections and small test pits where possible. Our strategy was first to pinpoint all the locally known sites, and then to check likely areas near good water sources and rich clam and oyster beds. In this short paper the location of the sites and the

nature of the artifacts are noted briefly, and relations to other sites and complexes in the surrounding area are explored.

All of the sites were relatively small and in most cases badly eroded. Generally we can say that they are shallow, and, in cases where the shell deposits are indisputably aboriginal, they usually consist of very poorly preserved shells. The sites which have been recorded thus far are almost exclusively from the Gulf coast of the island, where the artifacts are usually found washed out of the sites, on the beach, at low tide. The locations are usually quite similar — on salt water, but not on the exposed areas of the immediate coast, and near a reliable supply of fresh water. Bays whose mouths are protected by sand bars are favourable locations. On sites where we have been able to discover undisturbed deposits, these are usually several feet above the high tide mark. From the position of surface finds on the beaches, the sites appear to have been a series of houses spread along the shore. Test pitting has revealed that settlement did not occur more than a few yards back from the high water mark; there was probably a single row of structures along the water front.

In locating and describing the sites, we move from the eastern part of the island to the north-west.

Sites have been located at South Lake, at the very eastern end of Prince Edward Island. South Lake is a brackish salt water lagoon, separated from the sea by extensive sand dunes. At the extreme western end of the lake there is a deep narrow channel to the sea. Behind South Lake, the land rises to almost 100 feet elevation. At intervals along the shore, there are springs — four of which run in the summer, even though all forest cover has been removed from the area immediately behind them. At each one of these springs, a site is located. According to the National Museum site designation system, these sites are coded CcCml, CcCm2, CcCm3, and CcCm4, from east to west along the shore of the lagoon. Of the four sites, only the third, CcCm3, was not completely washed out. In a private collection in Charlottetown, there is a long, thin, finely chipped knife of yellow-brown quartzite from CcCm1. At that site we found many quartz and quartzite flakes. From CcCm2 we found more flakes and the tip of a chert knife, 3.25 cm long. Site CcCm3 yielded flakes of quartz, quart-

zite and schist, and a quartz knife. There was also a rough chopper made of a quartz pebble, 7 cm long, 5 cm wide, and 2 cm thick (see plate 1, top centre), and a small (2.6 cm long) chalcedony corner-notched point. CcCm4 yielded only a few chips. South Lake, with its shallow, warm water, must have provided abundant fowl, fish, and shellfish for the prehistoric inhabitants, and a place of shelter for travel around the eastern end of the island. One would suspect that other lagoon-like bays in eastern Prince Edward Island also supported communities of Indians.

Sites have been reported at the head of St. Peter's Bay. However, the nearby Savage Harbour seems to be one of the areas richest in archaeological materials in the province. It is possible to cross Prince Edward Island with a short canoe portage, into the headwaters of a stream flowing into Savage Harbour, from the Hillsborough River, which is a long arm of Charlottetown Harbour. The major site, on the western shore of Savage Harbour, CcCq1, extends for several hundred yards along the shore. From the eroded bank, about 12 feet above the high water mark, artifacts have been collected for the past several decades. A series of shallow shell deposits, not more than a few inches deep, can be seen at the top of the bank. From this site, adzes, of white limestone-chert (see plate 1, upper right) and schist were found. These are about 13.3 cm long and 5.3 cm wide and the thickness varies from 1.8 cm to 2.7 cm. One axe or wedge of basalt was also found. It is 6.4 cm long, 3.8 cm wide, and 1.8 cm thick.

The predominant material for artifacts at this site and others in Prince Edward Island is brown or yellow quartzite. Erskine (1960:358) mentions that this material also occurs in Nova Scotia. The most common artifacts are thick, disk-like bifaces made from quartzite pebbles which have undergone rough primary flaking (see plate 1, top row, extreme left). They may have been used as all purpose knives — perhaps even as shell-fish-openers. They measure about 6 cm in length, 3.8 cm in width, and 2 cm in thickness. Two types of points were found, although there is only one specimen of each. The first is a thick-cross-sectioned, stemmed point 5.6 cm long, 1.6 cm wide, and 1.2 cm thick, made from a quartzite pebble (see plate 1, bottom row, third from left), and the other is a small, green, flint corner-notched point about 3 cm long (see plate 1, bottom row, fourth from left). Also

found were retouched flake knives with single cutting edges (see plate 1, top row, extreme right). Although the shapes of these are more irregular than that of the usual semi-lunar knife, they would appear to have been hafted in a similar manner. They are thickest in cross-section opposite the cutting edge, and in this sense, have the same appearance as backed blades. Also, a broken quartz knife, 4.3 cm long, was found.

From three points on the western shore of Savage Harbour, similar flakes and artifacts were found, though the sites are smaller and the flakes less abundant (see plate 1, bottom row, extreme left). Two of these sites, CcCr1 and CcCr2 both yielded tapered-stemmed points of quartzite (see plate 1, bottom row, extreme right). From CcCr4, on Savage Island, which is actually a sand spit covered with eel grass but exposed at low tide, the tip of a point was found, and local inhabitants stated that prehistoric burials have been found here many years ago.

Local collectors have material from Winter Bay (site CcCs2); however, we found the site unproductive.

From the outer dunes at Tracadie, historic period burials with iron grave goods have been exposed by the wind and sea.

Two sites on Rustico Bay are of considerable size. At least one of these sites (CcCt1) was visited by W.J. Wintemberg according to local sources. This site, on Rustico or Robinson's Island, has been well protected by being within the Prince Edward Island National Park and having a dense cover of poison ivy and high-bush cranberries. Here we were able to locate an undisturbed layer of shells consisting of quahogs (*Mercenaria mercenaria*), oyster (*Crassostrea virginica*), and slipper shell (*Crepidula fornicata*). Oyster was the most abundant. In the shell mound, at about 3" below the surface, was the foreleg of a caribou. The deposit was only about 6" deep and yielded no artifacts. From the shore edge of the site we found flakes of chert, quartzite, and quartz, and the lower portion of a stemmed point 3 cm long and 2.7 cm wide. Mr. J.W. Fewkes (1898) found a single copper head from this site, as well as an ivory detachable harpoon point.

At Oyster Bed Bridge, also on Rustico Bay, there is an extensive shell mound, deeper than those mentioned above, but

badly disturbed by a road intersection and an old farmhouse site. A test pit into the disturbed area did, however, yield one very small, undecorated, uniform brown coloured, grit-tempered pot sherd.

From North Rustico, artifacts have been collected from the surface by several amateur archaeologists. Several artifact forms are represented in the collection of W. Gauthier. Of 27 specimens, 5 are laurel leaf-shaped knives, rather thick and crude; 1 is a short basalt adze; another a stone for abrading fine points like those of awls; 4 are ovate blades; 6 are tapered-stemmed points; 3 are parallel sided points or knives; 4 are corner-notched points; and 3 are trianguloid, straight-based knives.

From a bank between 12 and 15 feet high on the shore of New London Bay opposite the village of New London Bridge, the usual quartzite flakes and the butt of an adze were found, as well as broken nodules of yellowish flint. Since such flint is very uncommon, it is difficult to rule out the possibility that it comes from ballast dumped from the early European ships. However, the occurrence of water-washed, ostensibly worked, chunks of similar flint from the peninsula opposite Seal Island in Grand Entry Harbour, Magdalen Islands — a place seldom frequented by ships because it is very shallow, makes the possibility of Indian deposition much more likely.

On Darnley Basin, an arm of the sea, a new bridge provides the southwestern approach to the community of Malpeque. In the construction of this bridge and adjacent buildings, human bones were discovered. Animal bones have also been noted eroding from the bank. The site now appears too scattered for further investigation.

Malpeque Bay, a large salt-water bay with many indentations and a protecting sand spit to shelter it from the Gulf storms appears to be an excellent area for early Indian settlement. While our investigations here did not reveal any deep or stratified sites, some unusual artifacts were collected, particularly from the shores of Grover and Bunbury Islands. From Grover Island, there is a point, perhaps for a harpoon or leister, of slate, 5.7 cm long and 2 cm wide (see plate 1, second row, right). From Bunbury Island, also found on the beach, is a large leaf-shaped knife with

a straight base (see plate 1, bottom row, second from the left). From the northeast shore of this small island, there were found several fragments of adzes. Rough sandstone adzes were recovered from the North Shore of March Water in 2 or 3 feet of water at low tide. Some of them have been chipped all over with only the bit polished, and have a slight concavity midway between the bit and the butt on the underside. From the southwest side of Lennox Island there is a shell mound which is reported to be shallow but undisturbed. It has not yet been thoroughly checked, but at present seems to offer the best prospect in the Malpeque area.

Two areas on the opposite shore of Prince Edward Island deserve mention. The first is around the shore of Peter's Pond at Mimenegash. Here, lithic material has been found in abundance, but the collections have been scattered, and no representative sample is available. The site is of particular interest because there does not seem to be any shell deposit, at least from surface indications and the area which has been extensively plowed. From local reports, projectile points were found, but no mention was made of pottery.

To the immediate south of Fort La Joie or Fort Amherst, on Rocky Point, opposite Charlottetown, shell deposits have been noted on the shore. There are no data on artifacts.

While the above finds are meagre, and the sites in no case fully documented, several trends in settlement, subsistence, and cultural affiliation are evident.

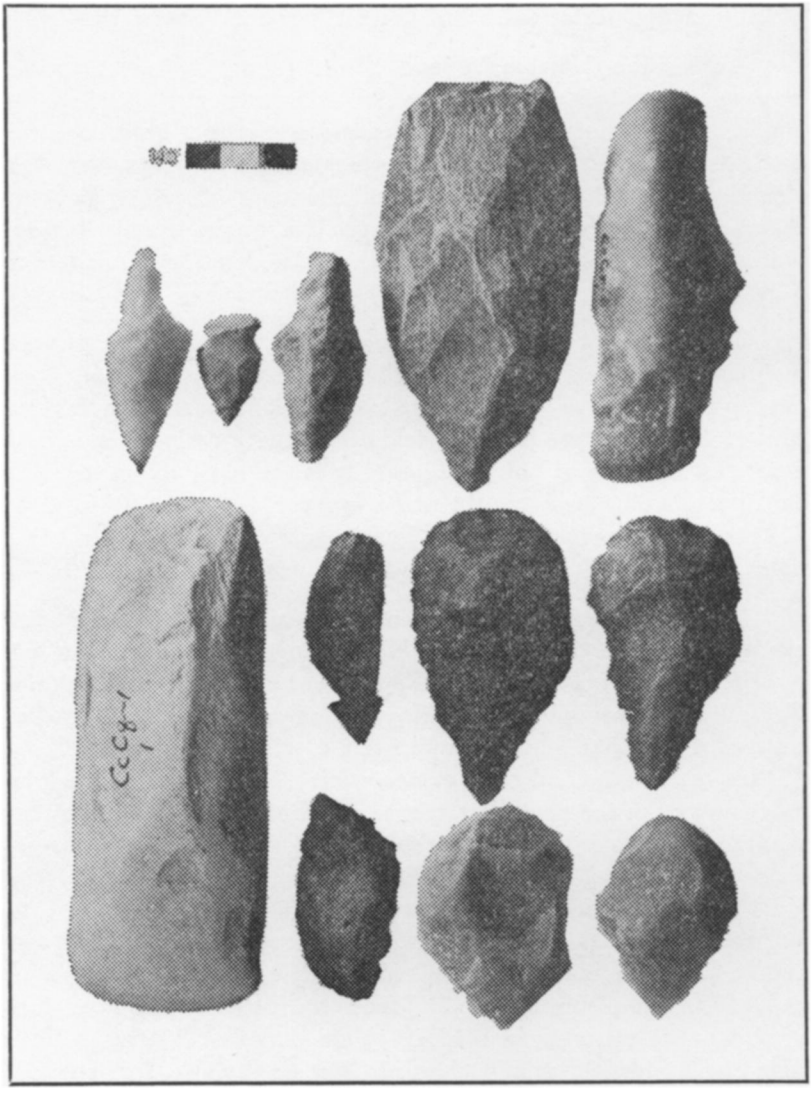
Shore settlements in protected areas, in almost all cases near rich beds of shellfish, are customary. There is no conclusive evidence of inland settlement, nor do we have direct evidence of agriculture. In the latter point, the subsistence would seem to resemble that of the prehistoric Indians of Nova Scotia. The extensive erosion of the sites may be due to the events of the subsequent geologic history of the island, or to the orginal settlement pattern. The sites may originally have been on higher ground, further removed from sea encroachment, or they may have been very hastily constructed right on the shore for summer use only. Local stories say that the Indians came to Prince Edward Island for the summer only. Of the early explorers, we

know that Cartier saw the inhabitants of the island near North Cape, in the summer, on his first voyage, but was not in the vicinity in the winter to record the settlement (Baxter, 1906:99).

Across the Northumberland Strait (about 10 miles to the nearest point in New Brunswick), the settlement pattern is similar, and in many cases the same materials have been used for artifacts — quartzite and grey speckled chert. However, the flaked pebble-knives are much more abundant in Prince Edward Island. There, the variety of material is exceptionally limited; water-worn pebbles, no doubt from glacial deposits, provide the only local stone of suitable quality.

The closest relation to archaeological assemblages on the mainland seems to be with the earlier and middle periods of Nova Scotia prehistory — Bear River and Indian Gardens (Erskine 1960). Sites of these periods yield about twice as many stemmed points as leaf-shaped points. Quartzite is the most common material, but rhyolite and quartz are relatively abundant (ibid.:356, 357). Erskine would date the lower Bear River materials some time before 800 A.D.

Several artifacts relevant to our discussion come from recent excavations near St. Andrews, New Brunswick. In excavations by the National Museum at Pagan Point, St. Andrews (site BgDs1) a brown quartzite knife with primary flaking was discovered (National Museum specimen catalogue number BgDs1-26, see plate 1, second row, extreme left). From BgDs10, a few miles away, on the southeast corner of Minister's Island, near St. Andrews, worked quartzite pebbles were found. Single carbon dates from each of these sites were selected for processing by the Yale Geochronometric Laboratory in 1963. For site BgDs1, a sample from 36"-39" below the surface yielded a date of 1880±80 years before 1950 (Yale sample Y-1291), and from BgDs10, a sample from 18"-21" from the surface yielded a date of 2690±80 years before 1950 (Yale sample Y-1293). Both sites also yielded bifacially chipped, flint or rhyolite, leaf-shaped, round-based knives — much thinner and better finished than the quartzite examples (see plate 1, second row, middle). Could it be that in the absence of better material, the quartzite examples are functional equivalents of the flint of rhyolite specimens known



from New Brunswick and Maine (Moorehead, 1922, Fig. 90-92)? In any case, it would appear that Prince Edward Island was inhabited for at least part of each year at about the same time as the St. Andrews sites were occupied.

This paper has shown how very little is actually known about the archaeology of Prince Edward Island. A search for further collections and richer, less disturbed sites is imperative. Without pottery or artifacts in quantity and settlement patterns from complete excavations, we can do little to supplement the bare bones of a chronology which is very far from being complete.

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La méthode d'analyse dans les sciences de l'homme

PAR RENAUD SANTERRE

"Linguistics is of strategic importance for the methodology of social science."
SAPIR — 1929

RÉSUMÉ

This article does not aim at a critical analysis of the philosophical postulates which serve as a basis to Lévi-Strauss' work. It rather discusses the major points of his methodology, the influence which certain trends in linguistic development exerted on the elaboration of his thought, and finally the profound similarities that exist between his method and that which is commonly used in other sectors of human sciences.

Point n'est besoin de parcourir tous les ouvrages de Claude Lévi-Strauss pour constater l'importance que le langage et la linguistique prennent dans son œuvre. Certains critiques n'ont pas hésité à partir de cette constatation pour lui reprocher d'avoir indûment généralisé un simple modèle linguistique, emprunté, qui plus est, à une école particulière. Cette critique se retrouve, sans être toujours formulée de façon aussi explicite, chez plusieurs auteurs et pose en fait tout le problème de la généralité d'une méthode d'analyse dans les sciences de l'homme.

Il n'entre pas dans le but de cet article de procéder chez Lévi-Strauss à un examen critique des présupposés philosophiques de son entreprise. "Il ne faut pas trop vite passer à la discussion de la philosophie structuraliste, disait Ricœur (1963:629), afin de séjourner longtemps dans la méthode structurale."

Plus utile apparaît pour le moment d'approfondir quelques-uns des traits principaux de sa méthode d'analyse, l'influence qu'ont pu exercer certains courants linguistiques sur son élabora-

tion et les analogies profondes qui existent entre elle et la méthode en vigueur depuis longtemps parfois dans d'autres secteurs des sciences humaines. C'est ainsi, semble-t-il, qu'au problème soulevé par la critique déjà mentionnée on peut le mieux contribuer à apporter une solution.

I — LA MÉTHODE DE LÉVI-STRAUSS ET SES RAPPORTS AVEC CERTAINS COURANTS LINGUISTIQUES

La poursuite d'un tel examen nécessite qu'on tienne compte non seulement des principes énoncés par l'auteur, mais aussi de l'application qu'il en fait en réalité. Pratique et théorie méritent de retenir également l'attention.

A - *Emprunts à la phonologie*

Si Lévi-Strauss n'a pas manqué d'emprunter à l'école de Prague, plus précisément à Troubetzkoy et à Jacobson, certains concepts clés, il ne semble pas que la phonologie ait exercé sur sa pensée toute l'influence qu'il veut bien lui-même ouvertement reconnaître.

Sans doute les notions de permutation, de neutralisation, de variante combinatoire, de trait distinctif, de binarisme[1] ont-elles une origine nettement phonologique. Il n'en est pas de même de certaines autres, d'un usage courant chez les phonologues, mais d'origine ethnologique ou linguistique plus ancienne. C'est J. Baudoin de Courtenay qui a fixé au début du siècle le "phonème" dans son acception actuelle; "système", "opposition distinctive" ou "éléments différentiels", "signification de position" ont un passé saussurien. Quant au concept de structure, bien nombreux sont ceux qui peuvent en revendiquer la paternité.

Même la notion capitale d'inconscient, qui joue un rôle important en phonologie, provient d'ailleurs. Sans parler de l'usage qui en est fait en psychanalyse, on peut lui trouver une origine au moins implicite autant chez Boas que chez Mauss ou de

[1] Encore qu'à cette dernière la cybernétique ne soit pas étrangère.

Saussure. Peut-être est-elle même impliquée, et plus ou moins clairement aperçue, chez les linguistes comparatistes du siècle dernier dans l'idée de lois phonétiques.

Notion capitale parce que l'inconscient devient pour les sciences de l'homme ce facteur de rigoureuse objectivité tant recherché pour écarter l'objection préjudicielle qu'on oppose habituellement à leur valeur scientifique: la liberté individuelle de l'acteur social. Sans nier l'existence de cette dernière, mais hors de ses atteintes, c'est dans l'inconscient que les hommes de science peuvent maintenant envisager de découvrir la base de nécessité qui la fonde, "de réduire des données apparemment arbitraires à un ordre, de rejoindre un niveau où une nécessité se révèle, immanente aux illusions de la liberté" (Lévi-Strauss 1964:18).

Brève caractérologie de la méthode phonologique

C'est à ce niveau de l'inconscient que "la science des sons de la langue"[2], la phonologie, s'efforce de dégager sous la multiplicité des sons prononcés dans un idiome le nombre limité de phonèmes qui le caractérise. Prenant son point de départ au concret, à ces sons, elle remonte à l'abstrait, au phonème, ou plutôt, puisque le phonème isolé n'a pas de sens, au système phonologique de la langue en question. La phonologie est en effet opposée à toute attitude isolatrice et s'efforce de montrer dans chaque cas étudié l'interdépendance rigoureuse qui existe entre les phonèmes, dons les traits distinctifs favorisent le jeu complexe de relations d'opposition de type binaire. La démonstration de cette interdépendance, Jacobson l'a administrée de façon définitive dans ses articles sur le langage enfantin et le problème particulièrement révélateur de l'aphasie.

A partir donc de variations phoniques dans la parole, la phonologie retrouve à un niveau inconscient pour les locuteurs un invariant phonémique, d'ordre systématique, qui en rend compte. Pour que sa recherche s'avère fructueuse, le phonologue doit faire preuve d'une puissance d'analyse minutieuse, qui ne néglige aucun des faits, les distingue nettement et les catégorise en vue de les

[2] Troubetskoy définit ainsi la phonologie par rapport à la phonétique qu'il appelle la "science des sons de la parole".

mieux comparer, et n'hésite pas à recourir à des constructions théoriques pour expliquer les faits qui resteraient incompréhensibles autrement et pour en dégager des lois générales. Le traité de Troubetzkoy, *Principes de phonologie,* constitue le meilleur exemple d'une application rigoureuse de la méthode.

Telle qu'elle, dans le secteur restreint qui est le sien, la méthode phonologique s'est acquis une réputation méritée par ses découvertes, le caractère heuristique de sa démarche, qui lui valent d'être à bon droit citée en modèle à d'autres disciplines, entre autre à l'ethnologie.

Ses limites toutefois sont réelles. Malgré le programme de Troubetzkoy[3] et en dépit des affirmations contraires de Lévi-Strauss (1958:361) concernant "l'œuvre structuraliste en fait de grammaire, de syntaxe et même de vocabulaire", la méthode phonologique n'a pas encore véritablement dépassé les frontières du domaine particulier que constitue "la face sonore du langage".

Jacobson le reconnaît implicitement lui-même en insistant sur l'urgence "d'entreprendre une analyse structurale explicite" des processus grammaticaux (Jacobson 1963:175).

Quoi qu'en pense Ricœur (1963:83), pour qui c'est "surtout" l'orientation proprement phonologique de la linguistique moderne qui se trouve à l'origine du structuralisme, la phonologie n'a pas, malgré sa valeur exemplaire pour les sciences sociales, exercé sur Lévi-Strauss autant d'influence que Ferdinand de Saussure.

B - *L'influence de Ferdinand de Saussure*

C'est en effet l'opposition saussurienne langue-parole qui constitue le fondement même de la problématique de Lévi-Strauss. Les discussions ont beau avoir surtout porté jusqu'à maintenant sur l'autre opposition d'origine saussurienne, aussi célèbre, entre la synchronie et la diachronie, il n'empêche que cette dernière, si l'on prend garde de se laisser abuser, apparaît nettement moins capitale et revêt une importance dérivée par rapport à la pre-

[3] "Outre la *phonologie lexicale* il existe donc une *phonologie morphologique* (ou morphonologie tout court) et une *phonologie syntaxique.*" (1933: 240).

mière, dont en fait elle ne constitue que l'envers. Au plan méthodologique c'est la distinction langue-parole qui est fondamentale et c'est celle-là justement que Lévi-Strauss privilégie.

Toujours il fait distinction dans les phénomènes sociaux de deux aspects complémentaires, l'un tributaire d'une méthode structurale, explicative, l'autre d'une méthode plus descriptive, statistique[4], d'une part les modèles de comportement, entendus au sens le plus général, et les comportements eux-mêmes d'autre part.

Prenant au sérieux l'affirmation du *Cours de linguistique générale* suivant laquelle "la langue est une forme et non une substance" (Saussure 1949:169), il cherche en ethnologie à se tenir du côté de ce qui est l'équivalent de la langue pour les linguistes, mais sans négliger pour autant le côté parole qui y donne accès.

De ce rôle fondamental joué dans sa pratique scientifique par l'opposition langue-parole, partout affirmée ou sous-jacente, il n'est pas de meilleure illustration que les deux ouvrages qui constituent jusqu'ici les pôles extrêmes de sa démonstration: *Les structures élémentaires de la parenté* et *Le cru et le cuit.*

Dernier en date de ces deux ouvrages, *Le cru et le cuit* est très explicite à ce sujet. Le point de départ, affirme l'auteur, de cette analyse mythique, comme le point d'arrivée, reste indifférent. N'importe quel mythe peut servir de référence. Bien plus l'analyse peut porter aussi bien sur une dizaine de mythes que sur plusieurs centaines d'entre eux. La raison en est bien simple. "L'ensemble des mythes d'une population est de l'ordre du discours" (Lévi-Strauss 1964:15). Bref, pour Lévi-Strauss, tout se passe comme si les divers mythes étudiés n'étaient que des réalisations particulières, partielles et momentanées, d'un mythe idéal, global celui-là, d'un schème mythique général constituant la langue des discours multiples représentés par les mythes propres à diverses sociétés. Cette position implique que tous les mythes, ou à peu près, sont interreliés, interconnectés, et qu'ils ne sont qu'une réalisation partielle d'un "méta-système".

[4] En distinguant entre la *langue* et la *parole,* de Saussure a montré que le langage offrait deux aspects complémentaires: l'un structural, l'autre statistique." Lévi-Strauss 1958:230.

C'est à la recherche de ce méta-système, d'une structure mythique dont chaque mythe particulier constitue une simple réalisation concrète, une variante, que l'auteur consacre son livre. Chaque particularité d'une variante suppose "que le mythe qui la contient relève, sous ce rapport, d'un ou de plusieurs autres groupes de transformations dont le système total — et pluridimensionnel — doit être d'abord restitué" (Lévi-Strauss 1964:86).

On ne saurait affirmer plus clairement que dans cet ouvrage une distinction nette entre l'ordre de la langue et celui de la parole ainsi que la volonté de recourir aux manifestations multiples et diverses de la dernière pour découvrir la structure de la première.

Pour être moins clairement affirmée, la distinction des deux ordres n'en est pas moins aussi opérante dans la première œuvre d'envergure de l'auteur: *Les structures.*

Dès le point de départ, il se situe du côté des normes, des règles, et non des conduites. C'est de ces règles hautement valorisées que l'auteur veut partir, comme d'autant de variantes, pour dégager la structure inconsciente de la parenté qui les conditionne. Comme dans *Le cru et le cuit,* Lévi-Strauss s'intéresse à plusieurs réalisations différentes, à plusieurs versions, pourrait-on dire, des systèmes de parenté pour éclairer un aspect ou l'autre de la structure d'ensemble. Rien mieux que le texte suivant ne peut résumer la tentative de l'ouvrage, le point de départ de la démarche au plus concret des règles de parenté particulières à diverses sociétés pour atteindre le niveau le plus abstrait de la structure fondamentale qui régit l'ensemble:

> Les règles de la parenté et du mariage nous sont apparues comme épuisant, dans la diversité de leurs modalités historiques et géographiques, toutes les méthodes possibles pour assurer l'intégration des familles biologiques au sein du groupe social. Nous avons ainsi constaté que des règles, en apparence compliquées et arbitraires, pouvaient être ramenées à un petit nombre: il n'y a que trois structures élémentaires de parenté possibles; ces trois structures se construisent à l'aide de deux formes d'échange; et ces deux formes d'échange dépendent elles mêmes d'un seul caractère différentiel, à savoir le caractère harmonique ou dysharmonique du système considéré. Tout l'appareil imposant des prescriptions et des prohibitions pourrait être, à la limite, reconstruit *a priori* en fonction d'une question, et d'une seule: quel est, dans la société en cause, le rapport entre la règle de résidence et la règle de filiation? Car tout régime disharmonique conduit à l'échange restreint,

comme tout régime harmonique annonce l'échange généralisé." (Lévi-Strauss 1949:611-12).

Comme l'indiquent cette citation trop révélatrice pour être abrégée ainsi que le titre de l'ouvrage lui-même, l'auteur situe son analyse de la parenté du côté de la structure et des modèles de comportements plutôt que des comportements eux-mêmes.[5]

Ces deux ouvrages font donc une distinction nette entre deux ordres, un choix décisif pour la langue au détriment de la parole; cette dernière se trouve réduite au rang de porte d'accès à la langue, seule apte à fournir des manifestations diverses de la parole une justification rigoureuse et complète.

Durkheim et de Saussure

En reprenant à son compte la distinction fondamentale établie par de Saussure entre la langue et la parole et en l'appliquant à un objet nouveau d'étude, autre que le langage, c'est avec toute la tradition durkheimienne que Lévi-Strauss, par l'intermédiaire du linguiste genevois, se trouve à renouer.

Pareille affirmation peut surprendre quand on connaît la critique sévère que Lévi-Strauss fait des théories de Durkheim dans *Les structures,* le rejet de son évolutionnisme et spécialement le refus du totémisme durkheimien qui caractérise *Le totémisme aujourd'hui.*

Le paradoxe n'est qu'apparent. Dorozewski a bien montré ce que l'opposition langue-parole devait au grand débat qui mit aux prises Tarde et Durkheim et ce qu'avait de rigoureusement durkheimien la notion de langue.

En qualifiant la langue de "fait social" au sens durkheimien, de Saussure attribue à la langue au moins quatre traits propres aux faits sociaux: 1° la langue est constituée de "représentations", 2° de représentations extérieures aux consciences individuelles, 3° de représentations douées d'un pouvoir de coercition en vertu duquel elles s'imposent aux individus, 4° de représentations ayant pour substrat et support la conscience collective.

[5] "Le schème conceptuel commande et définit les pratiques." Lévi-Strauss 1962b:173.

La langue, ou son équivalent ethnologique, garde ces traits distinctifs, qui se précisent et se complètent, en passant de de Saussure à Lévi-Strauss. Sous la notion de signe se perpétue l'idée de représentation extérieure aux consciences individuelles[6]; la conscience collective cède la place à ce qu'on pourrait appeler l' "inconscient collectif", n'eût été l'acceptation particulière que possède déjà le terme. Quant à la contrainte chère à Durkheim et que de Saussure admet dans la langue, on la retrouve transformée et singulièrement accusée chez Lévi-Strauss dans les relations nécessaires, qui pour lui, lient l'événement à la structure, dans l'ordre de nécessité qui fonde les illusions de la liberté.

Lévi-Strauss ne se contente pas d'assumer l'héritage saussurien, il le prolonge non seulement en prouvant dans la parenté et la mythologie l'existence de systèmes analogues à celui de la langue, mais en les montrant à l'œuvre grâce à une description détaillée de leur fonctionnement. Il le prolonge également en tentant de mettre sur pied cette science des signes dont le génie précurseur de de Saussure avait entrevu la nécessité. La *Leçon inaugurale* en 1960 affirme le caractère symbolique de tous les faits sociaux et fait de l'anthropologie sociale la nouvelle sémiologie. Déjà dans *Les structures* d'ailleurs le tournant était pris avec cette notion ambiguë de communication.

Voilà cet héritage saussurien que Lévi-Strauss assume en le prolongeant et qui se retrouve au cœur de sa problématique. Reste, une fois élucidé ce point fondamental, à approfondir les caractéristiques de sa méthode d'analyse et son maniement.

C - *Particularités de sa méthode ethnologique*

Caractère symbolique des phénomènes sociaux, nécessité d'une distinction méthodologique entre deux ordres de faits, l'un social et structural, l'autre individuel et statistique, stimulant exemplaire de la phonologie, qui applique avec succès au domaine des sons de la langue une problématique analogue, autant d'éléments caractéristiques, pour peu qu'on se résume, qu'une brève

[6] "Tout est symbole et signe qui se pose comme intermédiaire entre deux sujets." Lévi-Strauss 1960b:17.

analyse critique de deux courants linguistiques a permis de reconnaître chez Lévi-Strauss. Ce ne sont pas les seuls.

Quel lecteur n'a pas senti, en parcourant son œuvre, cette préoccupation majeure chez lui de constituer l'anthropologie sociale comme science rigoureuse et complète? Comme pour Bachelard, il n'est de science, pour lui, que du nécessaire. C'est ce qui lui fait rechercher, ainsi que nous l'avons vu, sous les manifestations apparemment arbitraires de la liberté individuelle, l'ordre de nécessité sociale qui les rend possibles.

L'idéal étant représenté par les sciences physiques, son aspiration profonde sera de retrouver dans la réalité sociale les faits "élémentaires", les éléments irréductibles, ces atomes sociaux[7] à partir desquels il deviendrait possible de dresser des faits sociaux une sorte de tableau périodique à la Mendéléev. Dans un remarquable commentaire à *Tristes tropiques* d'une allure psychanalytique, Roger Bastide (1956) a très bien fait ressortir chez Lévi-Strauss la tendance à la pétrification qui apparaît de façon positive dans les images minéralogiques à travers lesquelles il perçoit ce qu'il affectionne en ethnologie ou, à l'inverse, dans les termes de liquides, de viscosité qui lui servent à manifester une attitude de mépris ou de dégoût.

Sous le flux des réalités changeantes, il s'applique à retrouver les "glaçons immobiles" ou, mieux, le roc caché. Le caché revêt à ses yeux autant de fascination que le nécessaire. D'où sa recherche sous les variantes apparentes d'un invariant nécessaire, au vulgaire imperceptible; d'où également l'importance capitale de sa notion de structure.

La notion de structure

Il la définit lui-même par référence à quatre critères: modèle d'un caractère systématique (1°) tel que la modification de l'un de ses éléments entraîne la modification des autres, la structure fait partie d'un groupe de transformations (2°), réagit de par ses propriétés d'une façon prévisible (3°) et se trouve construite de façon à rendre compte de tous les faits observés (4°).

[7] Ne parle-t-il pas dans *Les structures* d'atome de la parenté?

Il s'agit d'un modèle construit à partir des faits par le chercheur comme hypothèse de travail, mais auquel la vérification confère un statut spécial en fondant son caractère explicatif. C'est grâce à un tel modèle construit logiquement et soumis à la vérification qu'il devient possible de réduire les exceptions, comme ce cas difficile des Katchin, dans *Les structures,* ou de fournir une explication sans reste du mythe de référence dans *Le cru et le cuit.* Le propre d'un pareil modèle est d'être heuristique, de permettre la découverte de lois générales déduites logiquement. Ainsi que le montre le texte des *Structures* cité plus haut, il devient possible de reconstruire a priori un système particulier de parenté à partir de l'examen du seul rapport entre la règle de résidence et celle de filiation.

La prévisibilité s'enracine, non pas dans son application à la totalité des cas existants, mais dans les propriétés logiques du modèle vérifié par un nombre restreint de cas bien choisis. "Quand une loi a été prouvée par une expérience bien faite, aime-t-il à répéter après Durkheim, cette preuve est valable universellement."

La structure constitue en fait à la fois un instrument méthodologique et une propriété du réel. C'est l'instrument logiquement construit qui permet d'accéder au cœur même du réel, de découvrir sa nature profonde. Avancé comme hypothèse au point de départ, le modèle construit, une fois convenablement vérifié, peut être identifié à la structure même de la réalité[8]. Il n'est pas nécessaire de la voir à l'œil nu pour l'admettre. L'existence de l'électron, avancée à l'origine comme hypothèse, a été admise comme réalité indéniable bien avant que des instruments plus perfectionnés n'eussent permis de constater "de visu" la justesse de l'hypothèse. On ne saurait donc partager les craintes de Granger devant ce qu'il appelle le risque "d'ontologiser" la structure. Cette dernière non seulement peut, mais doit être "ontologisée", c'est-à-dire que pour le chercheur elle devient réalité, aussi réelle que le réel apparent, du moment qu'une vérification correcte est faite de son existence.

[8] La structure d'une réalité, pour Lévi-Strauss, ne s'identifie pas à cette réalité et ne prétend pas en épuiser toute la richesse. Mais elle en constitue une propriété essentielle.

Observation et théorie

Pour la construction de ses modèles, opération caractéristique de toute science complète, l'ethnologie lévi-straussienne adopte une double démarche. La préface aux *Structures* est explicite sur ce point et l'ouvrage lui-même l'illustre bien. Dans une première phase descriptive, le chercheur doit se plier à une observation aussi minutieuse que possible du plus grand nombre de faits disponibles. L'analyste des mythes par exemple doit tenir compte à la fois du contexte ethnographique, des diverses variantes ainsi que des contes et des légendes. C'est à cette première phase descriptive que Lévi-Strauss, hostile à toute philosophie de l'histoire, limite habituellement l'apport des historiens et des ethnographes.

La deuxième phase, plus abstraite, proprement théorique et explicative, celle qui donne à l'ethnologie son véritable statut de science, se consacre à la construction, à partir d'un nombre limité de faits significatifs dégagés par l'observation, de modèles logiques aptes à en rendre rigoureusement compte et à se vérifier dans le concret. C'est dans cette phase que la collaboration du mathématicien, spécialiste des modèles, devient souhaitable comme dans la première l'était celle de l'historien.

Parler de phases présente le défaut d'évoquer en termes de mouvement à sens unique ce perpétuel va-et-vient qui mène du concret à l'abstrait pour mieux ramener au concret; il s'agit d'un mouvement à double sens entre le connu et l'inconnu, la variable apparente et l'invariant caché. Toute théorisation n'a de sens qu'élaborée à partir des faits observables et confrontée à eux en fin de course. Le départ et le retour se font toujours au concret. Ainsi se trouve résolue la vaine antinomie du concret et de l'abstrait posée par le formalisme[9]. *Le cru et le cuit* s'emploie justement à "démontrer l'existence d'une logique des qualités sensibles" (Lévi-Strauss 1964:9). Ainsi tombe également l'opposition irréductible que certains s'acharnent à introduire entre la structure et

[9] "A l'inverse du formalisme, le structuralisme refuse d'opposer le concret et l'abstrait, et de reconnaître au second une valeur privilégiée. La *forme* se définit par opposition à une matière qui lui est étrangère; mais la *structure* n'a pas de contenu distinct: elle est le contenu même, appréhendé dans une organisation logique conçue comme propriété du réel" Lévi-Strauss 1960a:3.

l'événement. Tous deux sont aussi réels l'un que l'autre et se trouvent nécessairement liés dans les faits.

L'esprit les distingue pour mieux comprendre et mieux expliquer l'événement par sa structure malgré l'incapacité de cette dernière à épuiser la richesse du premier. Le lien nécessaire qui les unit, si l'on peut parler ainsi d'une même réalité perçue sous ses deux aspects, permet de passer avec assurance de l'un à l'autre grâce à certaines lois que l'analyse a dégagées[10], de remonter de l'événement à la structure.

Le maniement de cette méthode essentiellement scientifique par son caractère heuristique, qui fait passer du connu à l'inconnu et explique les faits par leur nature profonde, n'est pas nécessairement facile et suppose chez celui qui la pratique une grande maîtrise et beaucoup d'intuition. A ces traits se reconnaissent les grands théoriciens des chercheurs ordinaires. Le flair de Lévi-Strauss le guide vers les faits révélateurs. Très souvent sa démonstration procède par analogie. Le bricolage n'est qu'un exemple parmi tant d'autres. L'introduction de *Le cru et le cuit* fourmille d'images, de métaphores souvent "filées" qu'il emprunte à des sciences comme la psychanalyse, l'astrologie, la géométrie, l'optique, la linguistique et l'anatomie, sans parler de la musique qui sert de thème. N'est-ce pas lui quelque part qui déplore le discrédit où se trouve tombée la psychologie associationniste et souhaite sa revalorisation? Condamnée comme méthode générale, il l'utilise comme technique: la couverture de *La pensée sauvage* en constitue la meilleure illustration. Qui eût songé à symboliser le titre (nom et adjectif) par une fleur et un carcajou?

Certains critiques réticents ne soupçonneront là qu'artifice pour circonvenir le lecteur, poudre jetée aux yeux. Ce serait se méprendre et méconnaître, sous le recours à l'intuition, à l'analogie et à l'associationnisme presque érigé en système, un procédé subtil mais efficace de comparaison systématique. Toute méthode scientifique est naturellement comparative: elle suppose une comparaison rigoureuse des faits significatifs à expliquer afin d'y décou-

[10] Lois d'inversion, de permutation, d'homologie ou d'isomorphisme, etc. Elles ne sont pas sans analogies avec les lois d'interprétation du rêve ou les lois phonétiques, comme on le verra plus loin.

vrir les éléments communs susceptibles de servir de base à la construction du modèle théorique destiné à en rendre compte.

L'analyse méticuleuse de Lévi-Strauss, qui n'hésite devant aucun découpage ni aucune distinction, fait étrangement songer à la minutie du Troubetzkoy des *Principes* et met on ne peut mieux en pratique la célèbre recommandation de Descartes: bien distinguer pour résoudre. Son découpage de la réalité sociale en phénomènes relativement autonomes comme la parenté, la mythologie, le langage, etc., à analyser séparément, apparaît comme le refus d'une tentative brouillonne qui, sautant les étapes, tente dès le début de faire la théorie des sociétés globales et se retrouve à la fin dans les plates-bandes de l'histoire sans avoir rien expliqué.

La généralisation

Pareille attitude n'exclut pas la généralisation. Cette dernière reste possible, mais en vertu du même processus qui a permis de trouver les structures particulières à partir de divers événements. En considérant les structures mises à jour comme autant d'événements, il devient possible de dégager d'autres structures de plus en plus générales à l'intérieur desquelles les premières viennent s'intégrer comme éléments d'un ensemble.

La tendance marquée de Lévi-Strauss à généraliser ainsi, de l'événement à la structure, de la structure à la culture, de la culture à l'esprit humain et de ce dernier à la nature est trop connue pour qu'on y insiste. On peut critiquer les aspects discutables de ces généralisations au 2^{e} et 3^{e} degré, qui semblent parfois bien hâtives et dont l'absence de rigueur laisse souvent transparaître les présupposés philosophiques de l'auteur, en particulier son postulat antiévolutionniste de l'unité de l'esprit humain. Pour prématurée qu'elle soit, la tentative n'en indique pas moins la voie à suivre.

L'œuvre de Lévi-Strauss manifeste donc, si l'on synthétise un peu les résultats acquis au cours du présent examen, l'application d'une méthode d'analyse scientifique véritablement complète dans sa double démarche, alliant l'observation la plus minutieuse des faits à une élaboration théorique rigoureuse, qui en dévoile la nature profonde et les secrets ressorts, prenant son

point de départ au plus concret pour y revenir dans la vérification après le passage par l'explication abstraite de la théorie. Méthode d'analyse qui tire sans doute profit de la valeur exemplaire de la phonologie dans un autre secteur, qui utilise en particulier et avec un rare bonheur pour ses fondements les aperceptions fécondes de de Saussure, mais qui avant tout, en dépit des apports étrangers, sait trouver en elle-même et par elle-même une cohérence propre dans son application fructueuse à un champ d'étude nouveau et vaste, celui de l'anthropologie.

II — LA MÉTHODE D'ANALYSE EN D'AUTRES SCIENCES HUMAINES

Les acquisitions du rapide examen épistémologique auquel on vient de procéder incitent, comme complément, à un bref tour d'horizon dans d'autres secteurs des sciences humaines. Peut-être n'est-il pas inutile d'analyser là aussi le mécanisme intime de la méthode qu'on y pratique et d'y déceler de possibles analogies. Dans cette mise en rapport, il ne s'agit pas de montrer l'impact d'influences supplémentaires sur Lévi-Strauss — pareille influence, réelle dans le cas de la psychanalyse freudienne, s'avère inexistante dans ceux de la grammaire comparée et de la psychomécanique —, mais plutôt d'avancer arguments à l'appui, l'hypothèse d'une unité réelle de méthode d'analyse, sous des modalités diverses d'application, dans les sciences de l'homme à l'heure actuelle vraiment constituées. Outre la phonologie et l'ethnologie, qui ont déjà retenu brièvement l'attention, l'examen se limitera pour le moment à trois autres disciplines.

A - *La grammaire comparée*[11]

Datant de Frantz Bopp (1816) et du *Grundriss* de Karl Brugmann au 19e siècle, la grammaire comparée possède en sciences humaines ses titres de séniorité. C'est elle qui a mis au point

[11] L'examen de la méthode d'analyse dans cette discipline s'inspire largement de la communication faite en 1962 par Roch Valin au congrès de l'ACFAS et reprise de façon plus élaborée dans une brochure (cf. Valin 1964).

la méthode comparative, souvent critiquée depuis le début du siècle, notamment par Boas, Jacobson et même Lévi-Strauss. Justifiée, quand il s'agit d'un comparatisme primaire à la Tylor ou des idéologies historicisantes qui sont nées du succès de la grammaire comparée et ont trop souvent servi à masquer sa valeur, cette critique méconnaît ses mérites, réels malgré ses limites, et n'effleure même pas, bien loin d'en donner une juste appréciation, le mécanisme heuristique de la méthode comparative en linguistique indo-européenne.

Ce mécanisme relativement simple repose sur le postulat de la parenté et de l'origine unique de langues entre lesquelles se découvrent des concordances de forme, de vocabulaire, assez nombreuses pour justifier la comparaison.

Du fait qu'il existe par exemple une correspondance telle que "nuit" en français, "noche" en espagnol et "notte" en italien, le comparatiste peut postuler l'existence d'une forme unique en latin, langue mère des langues romanes, et retrouver, par une application précise de sa méthode, la forme populaire "nocte" dans le latin du 5e siècle P.C. C'est cette forme du latin vulgaire qui, par différenciation progressive, a donné naissance à la forme correspondante du français, de l'espagnol et de l'italien.

La démarche de la grammaire comparée implique la distinction de deux ordres de faits, le premier conditionnant[12] et le second conditionné, et la double antériorité historique et logique du conditionnant par rapport au conditionné. Pour obtenir les trois formes conditionnées, il a fallu qu'existât d'abord[13] "nocte" et qu'il s'écoulât un espace plus ou moins long de temps[14] entre le moment où l'on prononçait "nocte" et celui de l'apparition des trois formes conditionnées.

Dans sa recherche, le comparatiste part des faits conditionnés et, par un cheminement nécessaire, remonte au conditionnant qui

[12] Le conditionnant contient en lui virtuellement tous les conditionnés. Dans l'exemple ci-dessus, "nocte" est le conditionnant, tandis que "nuit", "noche" et "notte" sont les conditionnés. Conditionnant et conditionnés correspondent à ce que nous avons appelé jusqu'ici invariant et variables ou variations.

[13] C'est l'antériorité logique. Pas de conséquences sans condition.

[14] C'est l'antériorité proprement historique.

peut être indifféremment déjà connu ou encore ignoré. L'absence de forme attestée n'est nullement un obstacle et certaines langues tel l'indo-européen, aujourd'hui connues grâce à la méthode comparative ne sont ou n'étaient nulle part attestées. On passe ainsi de l'apparent au caché, des variantes à l'invariant. L'essentiel de la méthode repose sur une remontée de nécessité en nécessité jusqu'au fait conditionnant, seul capable de fournir une explication valable de la similitude des faits conditionnés.

On remarquera que cette histoire de la transformation du conditionnant en conditionnés n'est pas une histoire anectodique, qui relaterait toutes les sinuosités de la route empruntée, mais une histoire nécessaire qui se borne à retracer les étapes sans lesquelles la transformation eût été impossible. C'est de l'histoire à rebours[15], si l'on veut, — le comparatiste remonte dans le temps au lieu de suivre la marche des événements comme le fait habituellement l'historien — mais parfaitement justifiée, comme en témoignent les résultats.

L'analyse, en grammaire comparée, se limite aux signifiants, entendus au sens saussurien du terme; elle se situe donc complètement du côté de la parole. Sous la multiplicité des formes, par exemple "nuit", "noche, "notte", subsiste l'unicité relative des signifiés. La date d'apparition des formes a en soi peu d'importance. Il existe naturellement un écart de temps entre conditionnant et conditionnés, mais cet écart peut varier énormément sans problème.

Une autre caractéristique réside dans le recours continuel à la comparaison — c'est pour ce motif qu'on parle de méthode comparative —, qui permet de déceler dans les conditionnés les éléments communs dont l'esprit va se servir pour postuler en hypothèse un conditionnant de tel type, en d'autres termes pour cons-

[15] Par une méprise curieuse chez un sociologue et un linguiste, Haudricourt et Granai (1955:116ss) s'en prennent à ce qu'ils appellent l'anachronisme de cette méthode qui consiste, disent-ils, à caractériser l'état originaire de la langue "à partir de formes appartenant à des états postérieurs". Poursuivant dans la même veine, les deux auteurs découvrent le grand mérite de de Saussure dans le fait qu'il se situe, d'après eux, "à un point de vue de véritable historien, c'est-à-dire à un point de vue anti-anachronique". C'est méconnaître étrangement le véritable apport de de Saussure et surtout la nature exacte de la grammaire comparée dont la valeur scientifique ne peut plus être mise en doute.

truire l'équivalent d'un modèle chez Lévi-Strauss. Une fois trouvé le conditionnant, le même processus peut se répéter en faisant de ce conditionnant le conditionné d'un conditionnant antérieur. C'est ainsi que de formes restituées en latin ancien, en grec pré-homéric et en proto-germanique, on est remonté jusqu'à un type commun, l'indo-européen.

La recherche permet là aussi de dégager des lois de transformation, les célèbres lois phonétiques, qui expliquent le passage *inconscient* au cours des générations successives d'une forme unique de conditionnant à la variété des conditionnés et qui permettent au comparatiste de remonter de ces derniers à la découverte du premier.

Le caractère heuristique de la méthode en grammaire comparée apparaît évident quand on considère le nombre de mots ou même de langues restituées dans leur intégrité bien avant que des découvertes ultérieures d'ordre archéologique ou paléographique ne soient venues confirmer la justesse des reconstructions. Ces découvertes ont joué un rôle patent de vérification, entraînant ainsi l'adhésion d'esprits jusqu'alors réticents. Quoiqu'il s'agisse d'une prévision un peu curieuse, à cause de son orientation vers le passé — les faits d'un passé lointain et inconnu sont l'objet de la prévision —, voilà une science humaine qui, au même titre que les sciences dites exactes, admet la prévisibilité.

Jacobson a discerné à juste titre les limites de la grammaire comparée dans son caractère "atomistique", "individualiste". Elle ne s'intéresse qu'aux mots, aux extériorités de la langue. Bien plus son succès incontesté est resté cantonné presque exclusivement aux langues indo-européennes. De Saussure lui reproche en outre de ne s'être pas "préoccupée de dégager la nature de son objet d'étude", de ne s'être "jamais demandé à quoi rimaient les rapprochements qu'elle faisait, ce que signifiaient les rapports qu'elle découvrait" (1949:16). En d'autres termes, de n'avoir pas approfondi les fondements de sa méthode et d'avoir limité son application, faute d'une vue claire de l'entier de la tâche à accomplir, à un petit secteur seulement de la linguistique.

Son mérite n'en est pas moins grand, en particulier dans la restauration de la face externe de langues inconnues, et réelle sa valeur exemplaire. Nombreux sont les domaines où l'on a tenté,

avec plus ou moins de bonheur, d'adopter sa méthode. Encore de nos jours l'œuvre d'un Dumézil par exemple s'en inspire largement.

A un niveau profond, et sans qu'on puisse parler d'influence directe, il existe une identité frappante entre la méthode d'analyse de Lévi-Strauss et celle de la grammaire comparée. Certes le premier s'intéresse avant tout en synchronie à ce qui relève de la langue ou de son équivalent ethnologique, à la seule antériorité logique de la structure par rapport à l'événement, tandis que la dernière, sise en diachronie du côté du discours, paraît ne se préoccuper que des événements, les unités lexicales, où elle s'applique à relever apparemment les antériorités autant historiques que logiques.

Mais les apparences ne doivent pas abuser sur une recherche, identique dans les deux cas, d'un invariant ou conditionnant à partir de variantes, de conditionnés soumis à la comparaison, qu'elle soit explicite ou sous-jacente à une technique associative. Dans les deux cas, il s'avère nécessaire de compléter l'observation par le recours à l'hypothèse, à la construction d'un modèle, structure de parenté d'une part et structure du mot postulé de l'autre. Ici et là se dégagent des lois de transformation et apparaissent des possibilités de vérification ainsi que de prévision. Bref, il s'agit foncièrement, adaptée à des objets différents, d'une seule et même méthode d'analyse.

B - *La psychomécanique*[16]

L'étonnement est prévisible devant cette discipline apparemment nouvelle dont certains peut-être entendent parler pour la première fois. Son initiateur, Gustave Guillaume, mort en 1960 à l'âge de 77 ans, a pourtant enseigné pendant une vingtaine d'années à l'Ecole pratique des hautes études et publié, outre de nombreux articles, trois ouvrages (1919, 1929, 1945)[17] dont les deux premiers, couronnés par l'Institut, se sont mérité le prix

[16] Cette section s'inspire également de la communication de Roch Valin mentionnée plus haut (cf., note 11).

[17] Les deux derniers ouvrages viennent d'être réédités en un seul volume par Roch Valin, de même qu'avaient été regroupés dans une seule réédition tous les articles publiés de son vivant par Guillaume. On en trouvera la référence exacte en bibliographie.

Volney. Malgré l'intérêt de ces publications, qui ne représentent, avec ses nombreux articles, qu'une partie de son œuvre, l'autre demeurant encore inédite, le fondateur de la psychomécanique est resté jusqu'ici, à cause de circonstances défavorables, peu connu en dehors du cercle étroit de ses amis et disciples[18].

Initié à la linguistique par Havet et Meillet, maniant avec aisance la méthode de la grammaire comparée, il entrevit dès la publication du *Cours* tout le parti qu'on pouvait tirer de l'héritage de de Saussure et résolut d'apporter une preuve détaillée aux aperceptions du maître de Genève en mettant à jour le système de la langue. Son article capital: "La langue est-elle ou n'est-elle pas un système?", où il synthétise, en 1952, plus de trente ans de recherches et de découvertes, résume bien ce que représenta pour lui l'œuvre de de Saussure et la force motrice qu'elle imprima à son entreprise.

Sa problématique prend pour base, comme celles des phonologues et de Lévi-Strauss, l'opposition saussurienne langue-parole[19] et le fait s'orienter résolument en synchronie du côté du système de la langue, seul apte à fournir une explication véritable des faits de discours[20]. Si le fondement est identique dans les deux cas, la psychomécanique toutefois se distingue nettement de la phonologie. Alors que cette dernière fait porter son attention sur le système des sons de la langue, c'est à la recherche de ce que Guillaume appelle le psychosystème, en gros le système morpho-syntaxique de la langue, que la psychomécanique s'applique.

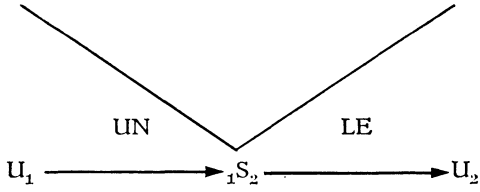
[18] On se reportera avec profit à la *Petite introduction* (Québec, P.U.L.) que Roch Valin, héritier des papiers de Guillaume et continuateur de son œuvre, a consacrée en 1954 à la psychomécanique du langage, ainsi qu'à sa brochure, déjà mentionnée (Valin 1954).

[19] Signe chez Guillaume désigne la notion saussurienne de signifiant, tandis que ce dernier concept, chez lui, recouvre l'ensemble signifié-signe, donc le signe de de Saussure. De même a-t-il troqué la notion de parole pour celle de discours qu'il trouvait plus précise, pouvant désigner aussi bien l'expression orale qu'écrite.

[20] Parlant de l'*Essai de grammaire française* de Damourette et Pichon, remarquable sous bien des rapports mais foncièrement inadéquat, Guillaume (1964:106) soumet que "ces auteurs ont voulu catégoriser les emplois de discours à partir d'eux-mêmes; ce qu'ils n'ont pas vu, c'est qu'il n'est, en l'espèce, de catégorisation autre que celle consistant à découvrir sous les emplois, conséquences de discours, la condition de langue invariante et non transgressée qui les permet, si divers et opposés soient-ils".

Pour découvrir le psychosystème sous la multiplicité des emplois d'une forme, elle procède comme l'ethnologie, la phonologie et la grammaire comparée, c'est-à-dire par une remontée nécessaire des valeurs d'emplois caractéristiques, qui constituent les variantes, à un invariant, le psychosystème lui-même, qui les a rendues possibles et peut donc seul en rendre raison. Comme dans les autres disciplines, il a fallu procéder, dans la morphologie des langues, à des découpages judicieux de phénomènes relativement autonomes dont l'ensemble intégré forme la langue. C'est surtout au psychosystème du verbe en divers idiomes et à celui de l'article que les recherches de Guillaume ont été consacrées et c'est à les dégager qu'elles ont connu le plus de succès. Deux exemples rapidement esquissés vont servir d'illustration.

On peut schématiser de la façon suivante le mécanisme extrêmement simple de l'article français que Guillaume avait entrevu très tôt, soit dès son premier ouvrage en 1919, mais qu'il ne put figurer clairement, avec une rare économie de moyens, qu'assez tardivement:



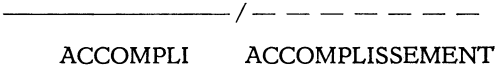
L'article est en français la forme de l'*extensité* nominale: il sert à déterminer l'extension du nom. Ce mécanisme peut par suspension plus ou moins précoce ou tardive du mouvement qu'il implique produire toutes les valeurs imaginables d'article. En U_1 par arrêt prématuré du cinétisme, on a par exemple "*Un* homme est toujours un homme", c'est-à-dire un universel entaché de singulier, produit à grande distance du singulier par un mouvement qui y conduit.

"*Un* homme entra". On obtient le singulier en S_1 au terme d'un mouvement qui y conduit, donc un singulier homogène. "*L'*homme déposa son chapeau". L'article ici, par arrêt précoce au voisinage du singulier, en S_2, prend une valeur de singulier

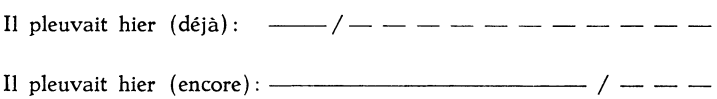
dans un mouvement orienté vers l'universel. Atteint en U_2 dans la phrase: "*L'*homme est mortel", l'universel est un universel homogène, pris à extrême distance de S et non entaché, comme U_1, de singulier. Toutes les valeurs d'articles peuvent être ainsi obtenues à des positions diverses entre les extrêmes.

Pour dégager ce mécanisme d'une remarquable simplicité, Guillaume est parti du discours, des valeurs caractéristiques d'articles dans certaines phrases, comme d'autant de conditionnés, de variantes, d'un conditionnant ou invariant, situé en langue et logiquement antérieur à ces "effets de sens" dont il est responsables.

Même démarche pour dégager le système de l'imparfait. Partant d'effets de sens divers, pouvant parfois aller jusqu'à la contradiction, — comme dans l'exemple "Un instant plus tard, il tombait", habile à signifier soit qu'il est réellement tombé, soit qu'il aurait pu (irréel) tomber, — Guillaume parvient à construire théoriquement le modèle susceptible de rendre compte de tous les emplois:



Le mécanisme de l'imparfait opère en lui la discussion du rapport variable de l'accompli et de l'accomplissement. Suivant que ce rapport varie au profit de l'un ou l'autre des termes, on obtient un effet de sens différent:



A la limite, l'accompli peut être nul et l'on obtient alors un entier d'accomplissement équivalant à l'irréel de l'exemple cité plus haut; inversement, l'accomplissement peut s'annuler au profit de l'accompli: l'effet de sens équivaut alors au passé simple:

"le 10 juillet 1909, le roi mourait" (=mourut). Toutes les valeurs d'imparfait sont justiciables d'un pareil schème explicatif.

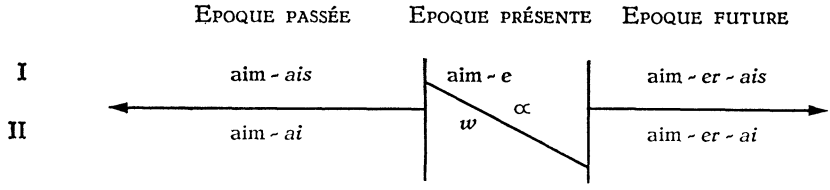
Il s'agit là du système particulier à un seul temps qui s'intègre dans celui plus général du mode, puis du verbe, ensuite des parties du discours, enfin dans le système global de la langue. Cette dernière apparaît alors comme un système de systèmes; pour réussir à le discerner correctement, il faut, comme l'ethnologie entreprend de le faire pour la société globale, éviter les tentatives trop brouillonnes et procéder méthodiquement par l'élucidation progressive des systèmes particuliers plus accessibles avant de s'attaquer d'emblée au système global.

Héritier de de Saussure, dont il emprunte, comme Lévi-Strauss, le fondement de sa problématique, soit la distinction langue-discours, Guillaume prétend également à l'héritage de la grammaire comparée sur laquelle il porte un jugement nuancé et dont il réussira à adapter la méthode diachronique en synchronie. Des deux antériorités logique et historique entre conditionnant et conditionnés dans la grammaire comparée, il avait saisi que seule la première était essentielle à la méthode, l'autre, l'historique, ne constituant qu'une modalité d'application à un objet d'étude particulier. Cette antériorité logique, en d'autres termes le lien nécessaire entre conditionnant et conditionnés, qui permet de passer des uns à l'autre, lui fournira le moyen de retracer le système ou les sous-systèmes de la langue en partant des emplois du discours et en les soumettant à une rigoureuse comparaison. L'exigence de comparaison en effet apparaît ici aussi vitale que dans les autres disciplines.

Autre certitude chez lui, qu'il manifestait en théorie et dans sa pratique linguistique quotidienne, c'est celle de la double démarche nécessaire d'une science véritablement complète, comme nous avons déjà eu l'occasion de l'observer chez Lévi-Strauss. La part importante qu'il consacrait à la réflexion épistomologique transparaît dans cet article, difficile sans doute et peut-être pour ce motif passé inaperçu, que les *Etudes philosophiques* ont publié sous le titre: "Observation et explication dans la science du langage". Ce qu'il entend par "syndèse autoptique" et "syndèse cryptologique" correspond exactement aux deux modes complémentaires de compréhension, l'observation concrète et la théo-

risation abstraite, la phase descriptive des faits et la phase constructrice de modèles pour les expliquer, entre lesquelles, pour lui, doit régner un perpétuel "itus et reditus". Nul plus que lui ne tint à rester esclave des faits à observer, mais nul aussi n'accepta plus résolument de paraître s'en éloigner momentanément pour prendre plus de champ par la construction d'un modèle logique et réussir ainsi à les mieux expliquer par la théorie. S'il fut la cible de nombreuses attaques au *Français moderne,* ce fut de la part d'historicistes hostiles à la théorie, à son pouvoir explicatif et définitivement enfermés dans la seule syndèse autoptique.

Le psychosystème mis à jour par le double mouvement syndésique apparaît comme un mécanisme abstrait d'une grande simplicité, qui en rend la représentation graphique possible. Il n'est pas à confondre[21], comme certains critiques de Guillaume l'ont fait, avec la psychosémiologie, qui n'en constitue que la traduction, comme le signe, entendu au sens guillaumien, n'est que la matérialisation du signifié. La loi régnante dans le psychosystème est une loi de cohérence, tandis que la psychosémiologie se contente d'une loi de suffisante convenance expressive pour signifier le psychosystème. C'est ainsi que la psychosémiologie, calque du psychosystème dans les meilleurs cas, souffre parfois de legs historiques qui en altèrent la cohérence. Le schème suivant, qui représente le mode indicatif français, illustrera ce qui précède:



Tel quel, l'ensemble est cohérent, la psychosémiologie, représentée ici par les désinences verbales et la particule de virtualisation -er- à l'époque future calque exactement le psychosystème

[21] De Saussure ne distingue pas les deux.

figuré par le schème mécanique constitué de deux vecteurs orientés en sens contraire et séparés à l'époque présente par un espace bidimentionnel en α et w. Le singulier ne pose pas de problème; seul le pluriel de niveau II à l'époque passée fait difficulté puisque aim - *âmes* et aim - *âtes* ne renvoient plus à aim - *er* - *ons*, aim - *er* - *ez* du futur, les terminaisons aim - *ons*, aim - *ez* étant monopolisées par le présent. La psychosémiologie doit se contenter de terminaisons moins adéquates, mais qui ne remettent pas en cause la cohérence du psychosystème lui-même, dont le propre, à l'indicatif, est d'opposer nettement une époque passée à une époque future. Il n'est pas question ici d'expliquer toutes les particularités de cette construction dont on trouvera la justification complète dans les œuvres mêmes de Guillaume.

Cette double loi, de cohérence en psychosystématique et de suffisante convenance expressive en psychosémiologie, sont impliquées dans la loi d'analogie repérée depuis longtemps par les linguistes pour expliquer dans certains cas l'absence de généralité des lois phonétiques. Tout ce qui n'est pas phonétique dans l'évolution des signes est analogique, a-t-on pu dire. L'analogie trahit l'action sous-jacente du système qui se reconstruit sous l'effet apparemment corrupteur de l'évolution phonétique. Alors que de Saussure, sensible au jeu des lois phonétiques, ne parle que de la destruction du système, Guillaume ne se fie pas à ces apparences superficielles et porte son attention sur sa construction progressive et ses réaménagements continuels sous le couvert de l'évolution linguistique: pour lui la langue se fait, bien loin de se corrompre. Ces trois lois dont il vient d'être question sont autant de lois de transformation qui guident le chercheur dans sa quête d'un système cohérent à partir d'emplois divers et sous une sémiologie parfois défectueuse.

Comment s'opère la vérification des hypothèses postulées pour rendre compte de cette diversité d'emplois, des constructions théoriques élaborées en vue de parvenir au psychosystème? Comme chez Lévi-Strauss, la vérification interne du modèle et son aptitude à fournir une explication sans reste constituent des arguments de poids. Si une hypothèse permet de réduire toutes les exceptions et les anomalies apparentes que signale en grand nombre la grammaire traditionnelle, l'hypothèse se trouve vérifiée et l'aptitude à prévoir apparaît. Quand on cherche l'explication du côté du

mécanisme producteur d'effets plutôt que du côté des effets eux-mêmes, tous les faits observés ou même possibles doivent pouvoir trouver leur place dans un schème explicateur d'une grande généralité. C'est pourquoi Guillaume refuse de catégoriser les différents emplois d'imparfait, pour ne citer qu'un exemple limité, à partir d'une valeur ou de quelques valeurs dominantes, qui admettent toujours des exceptions; il s'attache plutôt à les catégoriser par le mécanisme simple qui rend compte de tous les emplois possibles.

Outre cette vérification du modèle construit par sa cohérence interne et son aptitude à une explication sans reste, Guillaume recourt régulièrement à un procédé qui pourrait bien revêtir un statut particulier dans la vérification: le schéma. Un explication réussie doit pouvoir se schématiser.

> "Là où l'explication ne se laisse pas schématiser", écrit-il, "le devoir du linguiste, devoir de probité, est de reconnaître qu'il discerne en gros, assez mal, vaguement. Une faute serait, en l'absence d'une meilleure lumière, de vouloir par interprétation expliquer le dehors des choses. Le mérite des schémas réussis, c'est d'éliminer l'interprétation"[22].

Ce bref examen de la psychomécanique permet de réaliser l'identité foncière de la méthode d'analyse qu'emploient Guillaume et Lévi-Strauss. Adoptant une même problématique de base d'inspiration saussurienne, tous deux cherchent à remonter du contingent au nécessaire, du connu au caché, des emplois au système qui les rend possibles. Tous deux aussi minutieux dans l'observation, ils ne reculent pas pour expliquer la réalité ainsi observée, devant la construction de modèles logiques, devant la théorie qu'ils confrontent aux faits, notamment aux cas aberrants et aux exceptions traditionnellement reconnus comme tels. Dans chaque cas, la comparaison implicite ou explicite joue un grand rôle. Elle est explicite chez Guillaume, formé à la méthode de la grammaire comparée, qui en outre n'hésite jamais à recourir comme Lévi-Strauss à l'intuition et à ce qu'il appelle "la méthode des similitudes", en fait à une sorte de recherche intuitive des analogies. Tous deux s'inquiètent des modes de vérification de leurs hypothèses et s'intéressent particulièrement aux exceptions dans leur

[22] Cité par Santerre, s.d.: 115.

tentative de fournir une explication sans reste. Le souci de traduire graphiquement les modèles explicatifs, systématique chez Guillaume, n'est pas absent de l'œuvre de Lévi-Strauss, où l'on constate certaines tentatives en ce sens. L'un et l'autre enfin admettent la possibilité d'une généralisation, d'une théorisation générale de la société globale ou de la langue, mais à partir des systèmes ou structures limités d'abord dégagés et conçus comme nouveau point de départ dans la découverte de systèmes plus vastes, plus compréhensifs, qui les intègrent.

La seule différence, considérable, qui sépare ces deux chercheurs réside dans leur philosophie profonde: foncièrement évolutionniste, Guillaume a consacré une bonne part de ses énergies à la mise au point d'une typologie évolutive des langues, tandis que Lévi-Strauss réaffirme constamment sa conviction antiévolutionniste de l'unité de l'esprit humain. On peut y voir une différence d'expérience scientifique personnelle, l'un linguistique indo-européanisant, l'autre ethnologue américaniste, ou une différence, au point de départ, d'options philosophiques; mais chose certaine, l'unicité de la méthode d'analyse, commune aux deux, n'est pas en cause. Ce qui prouve l'aptitude de cette méthode à servir dans n'importe quel cadre philosophique.

C - *La psychanalyse*

Multiples sont les analogies intéressantes qu'on peut à prime abord déceler entre la psychanalyse freudienne et l'anthropologie sociale de Lévi-Strauss et qui manifestent la valeur d'exemple que, à l'instar de la phonologie, la psychanalyse freudienne a revêtue pour l'anthropologue français. Mais rester à ce niveau superficiel des analogies faciles, c'est se condamner à négliger l'essentiel: qu'en est-il de la méthode profonde d'analyse, de cette "recherche du caché" dont parle Bastide?

La problématique freudienne repose sur la distinction dans la vie psychique de l'individu d'un conscient et d'un inconscient. La latence habituelle de ce dernier, qui le distingue du caractère manifeste du conscient, ne l'empêche pas d'être dynamique, "turbulent" même, et de tenter diverses incursions dans la vie consciente sous le voile des symptômes dans les maladies mentales, des rêves, des lapsus et oublis dans l'état normal de santé.

La maladie mentale résulte du refoulement à l'étage de la vie inconsciente d' "événements" d'une grande densité psychique, de traumas, et ce sous la contrainte de facteurs divers, moraux, religieux ou sociaux. Ces facteurs conscients et contraignants cherchent à éliminer jusqu'au souvenir des traumas, mais s'avèrent incapables, malgré un succès momentané, d'empêcher totalement ces derniers de réapparaître sous une forme ou l'autre, sous un masque, à la conscience. Cet état de tension violente entre conscient et inconscient est générateur de malaise psychique et entraîne dans les cas les plus graves l'apparition de symptômes psychopathologiques caractérisés, qui ne sont rien d'autres que les manifestations voilées de l'inconscient, de l'événement refoulé, empêché de se montrer au grand jour.

Le traitement, bien simple en son principe, se résume pour le psychanalyste, si l'on schématise un peu, à faciliter à son patient la prise de connaissance de ces événements refoulés et à les ramener en pleine lumière, de l'inconscient au conscient. C'est une cure par la connaissance, cure "intellectuelle", pourrait-on dire, ou mieux cure "symbolique", puisqu'on recourt aux images et aux symboles et que ces derniers affectent la totalité de la vie psychologique, le niveau affectif autant que l'intellectuel.

La méthode d'analyse ici consiste à retrouver l'inconscient, qui tient lieu d'invariant, de conditionnant, sous ses différentes manifestations: rêves, symptômes, lapsus, en partant de ces dernières. Puisqu'il existe un lien obligé entre ces manifestations conscientes, contingentes, et l'inconscient qui les provoque[23], le processus est encore dans ce cas une remontée du contingent apparent au nécessaire caché.

Comme dans la grammaire comparée, cette remontée tient compte d'une double antériorité: antériorité logique d'abord du trauma par rapport au symptôme, qui en est une résultante possible parmi d'autres; antériorité historique aussi puisque le psychanalyste doit souvent retourner jusqu'à la prime enfance du patient pour y retracer le trauma originel. Mais cette antériorité historique, comme dans le cas de la grammaire comparée, n'est pas essentielle à la

[23] "Le résultat le plus précieux auquel nous avait conduits l'observation de Brewer était la découverte de la relation des symptômes avec les événements pathologiques ou traumas psychiques" (Freud 1962:147).

méthode elle-même et ne représente en fait qu'une modalité d'application d'une méthode générale d'analyse aux particularités d'un objet singulier. L'essentiel, c'est l'antériorité logique, le lien nécessaire entre conditionnant et conditionnés sans lequel tout espoir d'explication scientifique s'évanouit.

Dans cette quête de l'inconscient s'offre au psychanalyste la possibilité de points de départ multiples: erreurs, oublis, lapsus, symptômes et surtout rêves. "L'interprétation des rêves est, en réalité, la voie royale de la connaissance de l'inconscient" (p. 147). Comme dans l'analyse mythique de Lévi-Strauss, n'importe quel rêve peut conduire à l'invariant, à l'inconscient; la même structure transparaît d'un rêve à l'autre; certains sont simplement plus explicites, plus faciles d'accès que d'autres ou s'avèrent plus révélateurs à un psychanalyste qu'à un autre. On retrouve ici la même exigence d'intuition qui caractérise les bons chercheurs, "les trouveurs heureux", dirait Guillaume. La lecture de Freud dégage l'impression qu'il s'agit d'une interprétation géniale, mais toute intuitive, basée sur des analogies et des associations d'idées ou de mots. Comme chez Lévi-Strauss, il ne faut toutefois pas s'y laisser prendre: la comparaison que Guillaume juge essentielle à toute méthode scientifique est sous-jacente en psychanalyse freudienne comme ailleurs à toute technique d'analyse qui fait grand cas de l'analogie et de l'association.

Aussi nettement que chez Lévi-Strauss apparaît dans les œuvres de Freud le souci du concret, de l'observation méticuleuse des détails révélateurs. L'histoire la plus minutieuse et la plus concrète de l'individu, jusqu'à ses phantasmes les plus intimes, est refaite et revécue par lui dans l'entretien psychanalytique. C'est de là que part le psychanalyste pour élaborer une hypothèse sur l'origine et la nature du trauma responsable de la maladie de son client. La comparaison des éléments communs aux différents symptômes et rêves lui donne une idée de ce qui est refoulé[24] et lui sert d'indices pour reconstituer logiquement la structure de l'inconscient. Le recours à la théorie est évident en psychanalyse. Avant de songer au bout de plusieurs mois de traitement à poser une pomme sur son sein pour la faire manger

[24] "A côté des signes de défiguration, le symptôme offre un reste de ressemblance avec l'idée refoulée" (Freud 1962:140).

à sa jeune patiente et déclencher ainsi le processus de sa guérison, Mme Séchehaye (1950) dut émettre l'hypothèse, basée sur son observation précédente, mais hypothèse théorique quand même, que la jeune schizophrène souffrait d'un traumatisme de sevrage. Ainsi apparaît également en science psychanalytique la nécessité d'une double démarche alliant l'observation minutieuse du concret à l'abstraction théorique.

Le passage de la structure latente de l'inconscient à ses manifestations diverses, en particulier le rêve, s'opère suivant des lois particulières que Freud a dégagées: lois de condensation, de déplacement, de concrétisation, de revision ou remaniement. Si bien qu'il est plus facile maintenant qu'aux débuts de la psychanalyse, une fois ces lois repérées, de redécouvrir l'inconscient à partir du conscient.

Quel rôle peut bien jouer la cure? Un rôle capital non pas seulement pour le patient qui cherche la guérison, mais aussi au plan théorique, pour justifier les prétentions scientifiques de la méthode psychanalytique.

Réticent à reconnaître une analogie trop profonde de méthode entre l'ethnologie et la psychanalyse, Lévi-Strauss écarte un peu rapidement la théorie de la cure[25] pour ne retenir dans la psychanalyse que la théorie de l'esprit humain. Du moins est-ce ce qui ressort de sa réponse (1963:648) à une question sur l'identité des deux entreprises, la sienne et celle de Freud.

[25] On peut se demander si la cure n'existe pas également dans l'"ethnoanalyse" que fait Lévi-Strauss. En quittant le plan individuel pour le collectif, il ne faut pas s'étonner de voir la cure s'opérer suivant des modalités différentes, voire inversées. L'ethnologue joue, Lévi-Strauss le montre bien, un rôle ambigu par rapport à la société qu'il étudie et à la sienne propre. Il est à la fois analyste et analysé. Il représente sa société, l'Occident; c'est lui qui vient chez le "primitif", contrairement au psychanalysé qui se rend chez le psychanalyste quérir son aide. A l'opposé de ce qui se passe en psychanalyse, le "primitif" n'a rien à recevoir de l'ethnologue et sa rencontre avec ce dernier ne le change en rien. Il en va autrement de l'ethnologue et de sa propre société, l'Occident, dont il est le mandataire et à qui s'adressent exclusivement les publications ethnologiques. L'ethnologie exerce une fonction curative, à travers l'ethnologue dans sa position ambigüe, non sur la société "primitive", mais sur la société occidentale qu'elle exorcise de ses complexes, en particulier de son complexe de supériorité. Même le phénomène du transfert ne manque pas de se manifester clairement, mais à l'égard de l'ethnologue qui fascine et exaspère sa société. Lévi-Strauss, depuis sa polémique avec Caillois, est bien placé pour s'en rendre compte. Il n'y a peut-être là qu'analogies troublantes, mais qui mériteraient d'être creusées.

C'est malheureusement négliger la fonction de vérification que remplit la cure en théorie freudienne. Si la maladie mentale dépend du refoulement d'un trauma psychique dans l'inconscient, la découverte de ce trauma et la conscience qu'en prend le patient doivent entraîner sa guérison. Que cette dernière se fasse attendre et l'on peut se convaincre que le véritable trauma n'a pas été découvert, l'hypothèse théorique suggérée par le psychanalyste s'avère fausse; il ne lui reste plus qu'à reprendre ses recherches et poursuivre le traitement. L'hypothèse théorique d'un traumatisme de sevrage aurait été reléguée par M^me^ Séchehaye aux oubliettes, en compagnie d'un certain nombre d'hypothèses antérieures peut-être, tout aussi infructueuses, si la jeune schizophrène ne s'en était trouvée guérie.

Il n'en manquera peut-être pas pour critiquer le peu d'intérêt accordé ici au phénomène du transfert, dont l'utilité semble plus grande pour la guérison du trauma que pour sa découverte. Son importance n'est pas négligeable certes, mais pratique et non heuristique. Son rôle est de concrétiser la connaissance à réactualiser. Le trauma s'étant produit au niveau du vécu, c'est également au niveau du vécu, dans la relation analysé-psychanalyste, que doit s'effectuer l'exorcisme.

Ce développement sur la méthode psychanalytique manifeste certaines différences comparativement à la méthode ethnologique. La première s'intéresse à un invariant individuel, se situe en diachronie et possède un effet curatif qui opère comme moyen de vérification. Dans le deuxième cas, il s'agit d'un invariant collectif, recherché en synchronie et dont la découverte ne présente aucun effet curatif, du moins pour le "primitif" étudié.

Ces différences ne mettent nullement en cause la remarquable identité qui les caractérise fondamentalement. Même schéma de base, même rôle capital dévolu au symbole, même caractère dynamique reconnu à l'inconscient, même processus du connu à l'inconnu, de la variante à l'invariant, même recours à la théorie couplée avec une observation minutieuse du concret, basée sur l'intuition des similitudes et leur mise en rapport. Il n'est besoin que de lire de suite et peu importe l'ordre adopté *Le rêve et son interprétation* ainsi que *Le cru et le cuit* pour ressentir fortement cette impresion d'identité de perspective et de procédé analytique.

On dirait de ces deux ouvrages qu'ils représentent les deux volets d'une même entreprise.

L' "ethno-analyse" de Lévi-Strauss constitue en fait au niveau collectif un remarquable prolongement de ce qu'est la psychanalyse freudienne au plan de l'individu. C'est en fait la psychologie collective dont Freud avait rêvé dans *Totem et tabou* et qu'un vice de procédure au point de départ, une erreur dans l'application d'une même méthode à un objet nouveau, l'avait empêché de réussir.

CONCLUSION

Le problème de la vérification

Malgré le caractère schématique, incomplet, parfois même très sommaire de la démonstration, l'hypothèse que développe le présent article d'une identité de méthode d'analyse dans cinq secteurs constitués des sciences humaines apparaît fondée. La phonologie, l'anthropologie sociale ou ethnologie, la grammaire comparée, la psychomécanique du langage et la psychanalyse se sont donné comme point de départ une problématique similaire et poursuivent avec le gage d'objectivité que confère l'inconscient une même quête du caché et du nécessaire que la hardiesse de leurs théorisations, basées sur une observation minutieuse, leur permet d'atteindre pour expliquer des faits apparents et contingents. Mais pour compléter ce qui n'est ici qu'ébauché, pour fournir une démonstration plus complète de l'hypothèse avancée, il s'avère nécessaire d'approfondir encore mieux cette identité, de pousser plus avant l'examen des similitudes tout en tenant sérieusement compte des différences ainsi que de toutes leurs répercussions sur le fonctionnement de la méthode d'analyse et son adaptation à des objets particuliers et irréductibles.

Cet approfondissement devrait prendre pour objectif premier la solution du problème de la vérification. On sait toute l'importance de la vérification, ses liens avec l'expérimentation et la prédictibilité, pour conférer un statut indiscutablement scientifique à une discipline.

Ce qui a été établi jusqu'ici, avec plus ou moins de facilité et de clarté suivant le cas, c'est le caractère heuristique de la méthode, sa valeur explicative, son va-et-vient du concret à l'abstrait, la nécessité de la comparaison et de la théorie.

La difficulté surgit quand il faut expliciter le mode de vérification dans chaque cas. Si l'on entrevoit assez bien comment elle s'opère dans la grammaire comparée et la psychanalyse, on la saisit encore mal en phonologie, en psychomécanique et en ethnologie.

L'idée d'une vérification préalable, comme dans la *Leçon inaugurale* Lévi-Strauss le suggère pour l'ethnologie, doit dès l'abord être rejetée. On ne comprend pas comment pourrait être soumise à l'expérimentation une théorie encore à naître. Pas plus que dans les sciences physiques, la vérification en sciences humaines ne peut précéder l'observation. Le doute ethnologique, l'expérience du terrain, — constituant pour l'ethnologue l'équivalent de l'analyse préalable pour le psychanalyste —, l'éloignement culturel de sa propre société représentent en ethnologie autant de techniques de contrôle de l'observation, de gages d'objectivité, mais nul ne peut les substituer à une véritable vérification postérieure à l'observation et à l'élaboration théorique. Ce sont les verres, si l'on permet une comparaison grossière, qui corrigent la myopie de l'observateur en sciences physiques et lui permettent une lecture correcte des instruments à mesurer les phénomènes! La myopie de l'observateur étant plus prononcée dans les sciences humaines, à cause de son insertion partiale dans une société donnée, il lui faut, pour valider ses observations, une correction préalable d'autant mieux réussie.

La vérification interne des modèles construits pour éprouver leur cohérence logique n'est pas négligeable. L'explication sans reste, qui réduit toutes les exceptions connues, et celle qui met en cause une économie de moyens se rapprochent de l'idéal de la vérification. Même la vérification sur soi dont Lévi-Strauss fait un grand cas peut n'être pas inutile à l'ethnologue malgré le risque de subjectivisme. Tout ceci présente certes beaucoup d'intérêt, mais manque d'une évidence frappante, pourrait-on dire, pour emporter d'un coup l'adhésion d'esprits exigeants.

La recherche mérite d'être poussée du côté des schémas dont Guillaume et même Lévi-Strauss font un courant usage, le premier avec la certitude qu'ils représentent un mode particulier de vérification. Peut-être sont-ils en réalité plus que des commodités de représentation, seule valeur que semble leur reconnaître à l'heure actuelle Lévi-Strauss.

Une fois poursuivi cet approfondissement de l'hypothèse soumise dans le présent article et résolu ce problème de la vérification en sciences humaines, il sera possible d'établir une comparaison entre la méthode d'analyse telle qu'elle se pratique dans les sciences sociales et celle qui prévaut dans les sicences physiques. Peut-être arivera-t-on alors à la conclusion que la même méthode d'analyse vaut partout. D'ores et déjà toutefois apparaît nettement l'inanité du reproche adressé à Lévi-Strauss d'avoir indûment généralisé en ethnologie un modèle linguistique particulier.

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On the Classification of Bifurcate Merging Systems[1]

BY WARREN SHAPIRO

RÉSUMÉ

On croit généralement qu'en terminalogie de parenté les trois types reconnus (exprimés dans l'équation: FZD = MBD = ZD) sont déterminés selon des critères analogues. L'Auteur s'en prend à cette assomption générale, lui opposant une équation terminalogique pourtant assez connue, mais à laquelle on a prêté que très peu d'attention. L'A. conclut à la nécessité d'introduire dans l'équation courante quelques nouveaux termes.

It is generally assumed by students of kinship terminologies that the three recognized types of bifurcate merging nomenclature — Iroquois, Crow, and Omaha[2] — are "on the same level of abstraction," i.e., that they are based upon analogous criteria. In this paper, I shall take issue with this assumption, bringing to bear upon it a fairly common terminological equation which has heretofore received virtually no theoretical attention. From the conclusions thus reached, I shall suggest the adoption of some new terms.

The equation in question is: FZD = MBD = ZD, where the single term applied to these kin-types is used for no other

[1] This paper has grown out of a larger comparative study of avuncular marriage. I should like to express my thanks to those who have read and commented upon this study in its various manuscript forms: Ann Chowning, Melvin Ember, Robert F. Murphy, and W.E.H. Stanner.

[2] "Bifurcate merging is here used not in its more limited sense as referring to a kind of avuncular terminology, but in the broader sense of pertaining to a certain consistent pattern throughout Ego's and the adjacent generations. For a definition of this pattern, see Murdock (1949:141-142). "Iroquois," "Crow," and "Omaha," though usually thought of as applying to cousin terminology, are at this point used rather freely; this usage will be refined shortly.

consanguineal kin-type which is not in the relation of "brother" to one or more of these. It occurs in the kinship systems of at least the following societies: the Barama River Caribs of British Guiana (Gillin 1936), the Bhumia and some Gond groups in central India (Fuchs 1960; Karve 1953:240-247), the Gururumba of the New Guinea highlands (Newman 1965), the Kanarese of South India (Beals 1962, Karve 1953:231-235; McCormack 1958), the Nama of Southwest Africa (Hoernlé 1925; Schapera 1930), the Tukuna of northwestern Brazil (Nimuendajú 1952), and the now-extinct Tupinambá of Brazil's Atlantic Coast (Fernandes 1963). As might be expected, it is generally associated with a kind of marriage system in which cross-cousins and the sister's daughter are possible mates and unions between close parallel relatives are prohibited. (The sources mentioned are silent on the relation between kinship and marriage among the Gururumba and Tukuna; for the Bhumia, cross-cousin marriage is reported but there is no mention of avuncular marriage. The Nama allow parallel (as well as cross-) cousins to marry, though this seems to be a rather recent development).

The cousin terminologies of the above-mentioned societies are of the Iroquois type, according to Murdock's definition: "FaSiDa and MoBrDa called by the same terms but terminologically differentiated from parallel cousins as well as from sisters ..." (1949:223). In so classifying them, it should be noted, we are considering the terminological pattern for a single (Ego's) generation in isolation; we are ignoring the equations and inequations that relate the kin-types of this generation to those of other generations. This is, of course, conceptually legitimate: if we have two kinship systems — one equating FZD with MBD but separating these kin-types from all others, the other merging these two with ZD — we may apply the rubric "Iroquois" to both of these systems on the basis of certain common characteristics, though for other purposes we may want to distinguish the two as being instances of different "Iroquois subtypes." To do this, however, we must go beyond the terminological pattern for a single generation.

Now suppose we have a system in which FZD is not equated with MBD but is equated instead with ZD, while MBD is terminologically merged with M or MZ. To such systems we con-

ventionally apply the term "Omaha." But here, in defining our isolate, we are considering more than the terminological pattern for Ego's generation; we are "on a level of abstraction" different from the one which is used to define "Iroquois." The concepts denoted by the labels "Omaha" and "Crow" are analogous, not to that denoted by "Iroquois," but to, say, the "Iroquois subtype" in which ZD is equated with cross-cousins.

New terms should be coined to represent newly-recognized concepts. To this "Iroquois subtype" we may apply the label "Amazonian," on the basis of its occurrence in at least three societies in tropical South America. The Amazonian pattern of kinship terminology is thus one in which a single term is used for FZD, MBD, and ZD, but for no other kin-type which is not in the relation of "brother" to one or more of these.

Similarly, if we accept "Iroquois" for the more inclusive category defined by Murdock, another term is required for the more common subtype characterized by: FZD = MBD = ZD. I would suggest "Australian," since the conceptual separation of ZD from cross-cousins is — so to speak — "guaranteed" in most Australian systems by the so-called "section" and "subsection" arrangements (cf. Radcliffe-Brown 1930:53-58).[3] The Australian pattern of kinship terminology is thus one in which a single term is used for FZD and MBD, but for no other kin-type which is not in the relation of "brother" to one of both of these.

Still another term is needed to denote the kind of cousin terminology of which Crow and Omaha are variants. This term would be taxonomically homogeneous with "Iroquois," and it would refer simply to a pattern of own-generation terminology in which patrilateral and matrilateral cross-cousins are separated from siblings, parallel cousins, and each other. The common label "Crow-Omaha" is inappropriate, since there are many systems which fit this definition but do not conform to either the Crow

[3] "Kariera," since it pertains to the classic Australian section system, is also appropriate, but this term is frequently used to refer to the general four-section pattern of terminology, wherever found. "Australian" was used in the same way by Radcliffe-Brown (1927:344), but this usage has not gained general acceptance.

or the Omaha pattern.[4] I would propose instead the adoption of the term "Pomo" for this purpose, since both Crow and Omaha terminologies are found in the kinship systems of the various Pomo groups of California (Gifford 1922:104-115). The Pomo pattern of kinship terminology is thus one in which FZD and MBD are separated from siblings, parallel cousins, and each other, though they may or may not be equated with aunts, nieces, etc. It occurs in such diverse systems as Cherokee (classic Crow), Kalmuk (classic Omaha), Mundurucú, and Murngin (Aberle 1953; Gilbert 1937; Murphy 1960; Warner 1937).

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VEUILLEZ NOTER -- PLEASE NOTE

CORRECTION: Anthropologica Vol. 8, no. 1 (1966)

Page 147, 7^{e} ligne

Au lieu de	FZD = MBD = ZD
veuillez lire	FZD = MBD ≠ ZD

Page 147, line 7

Instead of	FZD = MBD = ZD
please read	FZD = MBD ≠ ZD

MERCI

THANK YOU

Recensions — Book Reviews

An Introduction to Prehistoric Archaeology. By Frank HOLE and Robert F. HEIZER. Holt, Rinehard and Winston, Inc., New York; 1965. x, 306 pp., 28 Figs. $7.70.

This remarkable and estimable book is sorely needed and one of its kind is long overdue. In no other single volume can laymen, students, and practising archaeologists find such a wealth of guides to, and considered comment on, the technical literature of prehistoric archaeology. It is not encyclopedic for the inevitably doctrinaire layout of a text book of method and technique precludes its being so. However, in spite of a few deficiencies, its scope, organization and content are catholic to the extent that the catechumen and the science writer alike can refer to it with confidence and one may hope that his colleagues in geology, biology and the social sciences would do so as well. The book should do much to dispell the apparently widespread notion that archaeological tenets come to archaeologists as revelations rather than as results of disciplined research. Anyone who plans to own a half dozen or more books on archaeology should consider this one seriously, whatever his interest in the field may be.

The book has already been reviewed by two distinguished archaeologists, Stuart Piggott in the British Journal *Antiquity* and J.B. Griffin in the American journal *Science*. Both men gave it the praise it deserves. The volume has seventeen chapters grouped in four parts; One, Introducing the Study of Prehistory; Two, Acquiring the Facts of Prehistory; Three, Dating the Events in Prehistory; and Four, Describing and Interpreting Prehistory. A Preface briefly states what the authors perhaps would like to have seen on the dust jacket; they commend it to the intelligent layman and beginning and advanced students. The dust jacket itself bears a short, colorful paraphrase of the Preface. There is an impressive forty-page bibliography and a very good index of thirteen pages. In general the book leans heavily on a spate of recent collected works on technical aspects of archaeology, most of them edited by British archaeologists. It also draws on a number of well-known volumes edited by Heizer but the mature archaeologist will find in the text references to important papers in journals seldom read by North Americans.

The book surely will go to a second edition very quickly. With this welcome prospect in store it behooves a reviewer to draw attention to those sections of the book that might readily be improved. The discussion of culture on pages eight and nine apparently presupposes that the reader has taken an introductory course in anthropology, in which the authors' concept of culture

has been explored fully. This will not always be the case and because proper understanding of some of the authors' views requires this knowledge, I think it would be desirable to devote considerably more space to their definition of culture and to an examination of the relation of archaeological evidence to cultures. I particularly take exception to the unsupported, unexplained statement "the artifact is the smallest cultural unit of concern to archaeologists." This implies that artifacts have the elegant integrity and simplicity of phonemes or atoms, which is certainly not the case. If any class of archaeological objects is to be ascribed this virtue, it should not be that of artifacts; perhaps modes or attributes but not artifacts.

Parts of this and other sections of the book seem to have been styled for the instruction of an eager, malleable, obedient fourteen year old, with more mechanical aptitude than ability to think in abstract terms. For example on page 68, near the beginning of a chapter on Preservation, we find a paragraph beginning with the statement "Man's world is animal, vegetable, and mineral." Such defects of writing style (there are others) are further compounded by lapses in consistency which might have been eliminated by more careful editorial review. On page 87 we find a statement "most sites are found when someone walks over them," then on page 89 a further statement "aerial (photographic) survey is one of the most useful means of finding sites." We must wonder if it is good didactic practice to state, as is done on page 103 in the section on Excavations, "Rectangular pits may be dug into the site wherever an archaeologist thinks they will give him useful information." Granted that an archaeologist must depend on his judgement in the field but why must pits be rectangular? I quite agree that they should be rectangular because flat, vertical faces gives much the best picture of stratigraphy but we are not told this anywhere in the text.

Thus it is my impression that the style and the organization of many of the chapters are not compatible with their scope and intensive coverage: the authors tend to depreciate their generally excellent product both by talking down to the reader and by neglecting to insure that arbitrary statements are more or less congruent with one another. But lest I give the erroneous impression that the entire book is painfully pedantic or gauche, let me quote a refreshing statement from page 122, "Most archaeologists assume that they are doing a good job and that other archaeologists understand what they are doing. Unfortunately, this is not always the case. A person who wants to sum up the results of many excavations must himself handle the material or at least be sufficiently familiar with the excavators and their techniques to make judgements about what they report. Too often synthesizers overlook the extremely variable quality of reporting; this in addition to the very variable quality of excavation." Such elementary truths are all too rarely recognized in the professional literature and I do not recall ever having run across this one stated elsewhere in anything resembling a text book.

Not all readers will agree, but I would like to see the distinctions between archaeology and history in the first chapter honed a little finer. It seems to me that prehistoric archaeology shares with history only interests

in humanity and in the passage of time. Prehistorians are akin to geologists in their concern with time beyond memory or record and with natural environments of the past. In their concern with groups of people rather than with personalities, they resemble biologists and sociologists. One hesitates to suggest an affiliation for them as evolutionists but their bravura, at least, is occasionally matched by astronomers. In part the paucity of the archaeological record forces them to adopt techniques and generalizing habit of science, but at the same time they are led by their fellow anthropologists and their academic training to take a holistic view in which they ignore neither the sciences nor the ephemeral realities of art historians, nor for that matter, the actuarial niceties of archivists. At least, we may wish that this were so.

In chapter 9, "Technical Analyses", some sections are disproportionately short. In the Analysis of Pottery, two pages give some of the variables of paste and surface finish and refer us to other authors, primarily Shephard, 1956. There are but 9 other references in this chapter, and no comment on the special utility of or the restrictions imposed by this plastic subject, nor mention of the fact that in much of the New World it was made only by women. In contrast, 4 pages on Analysis of Plant and Animal remains give a broad cross-section of studies (31 references, most to works on mammalian fossils but also some to the study of copralites from which "surprisingly rich results have been obtained" (page 138). Plants are not covered adequately; there is no direct reference to agriculture in this section or to works primarily concerned with agriculture nor are there any but indirect references (via copralites, Huaca Prieta and Tehuacan) to wild or possibly wild plants used as food.[1] The "analysis of metal and stone" gets but little more than half a page on which seven authors are cited. Although the chapter on Classification and Description treats this subject and pottery as well, nowhere is there discussion of the problems of recognizing or of describing the attributes of manufactured articles. The reference to Binford's 1963 paper on Attributes of Projectile Points comes as close as any in the book to meeting this need but the utility of Binford's approach is questioned by the authors and students are encouraged to consult it only as an object lesson in over-elaboration.

Most of the foregoing deficiencies, if deficiencies they be, probably are inevitable concomitants to the tremendous amount of organization that has gone into this pioneering work. The fact that I am able to find fault with some aspects of the book and therefore urge that it soon be revised, in no sense reflects on the abilities or diligence of the authors. Rather it is a measure of the enormity and importance of a task which they have carried to one stage of completion with signal success.

[1] Three pages are given to palynology in Section 12 on Methods for Dating, using plant and animal remains. An intriguing reference to Kurten Vasari, 1960, identification of pollen from Choukoutien, Locality 1, as indicative of a cold climate and hence the second glacial date is not documented in the bibliography. Although dendrochronology is discussed in the same section, no explicit reference is made to the use of this technique outside the southwest United States.

This book is attractively turned out and adequately illustrated with photographs and line drawings of artifacts and archeological sites. The price is reasonable. The authors may legitimately feel that they have contributed to attainment of their objective as stated on the final page: "In a sense this book is a plea for a long-range approach to archaeology, even though its major emphasis has been on what archaeology is and what some of the techniques for doing it are."

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Sculpture Sénoufo. B. HOLAS. Abidjan, Côte d'Ivoire, Centre des Sciences Humaines, 1964, 24 pp., 46 pl.

Dans ce volume l'Auteur étudie la sculpture sur bois, à caractère sacré, des Sénoufos. En raison de la complexité du sujet l'Auteur se borne à analyser les prototypes les plus significatifs. L'étude d'une vingtaine de pages est suivie d'un "Index des termes vernaculaires", d'une "Orientation bibliographique" et d'une série de 46 planches de très belle qualité.

J.T.

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Knowing the Gururumba. Philip L. NEWMAN, Holt, Rinehart and Winston, 1965. x-110, illustrations. $1.50.

This is another of the Case Studies in Cultural Anthropology produced under the general editorship of George and Louise Spindler. In reviewing this type of monograph, then, it is always necessary to consider the audience for which it is intended. While I am in favor of the series in general I still feel that there are certain implicit dangers. Foremost among these is the fear that many undergraduates may acquire a false perspective of the task of the ethnographer and his productions. For example, I can conceive of generations of undergraduates acquiring only a superficial acquaintance with the Kapauku Papuans and no realization or appreciation of the fact that the study is backed up by two very thorough and substantial contributions. Secondly, there is a danger for the professional anthropologist as well who by proffering a short monograph in this manner delivers to his colleagues what may well be his last word on the subject. This criticism points to the fact that the professionnal anthropologist can utilize these compact ethnographies only in a very limited fashion, but he must do so since he has no assurance that the same topics will be more fully treated by the same author elsewhere. What I am intending is that the professional anthropologist must not let a contribution to this series become, in his own mind, a substitute for a more thorough treatment of the same topic. But then, of course, the series was not designed for this use which makes the criticism a bit awkward.

Gururumba is a phratry name for a group of indigenes of the Eastern Highlands District of the Territory of New Guinea. The field work was conducted during the period 1959-1960. Newman's monograph is more than simply an introduction to these people. Its deceptive simplicity masks the fact that this is an excellent statement of one man's resolution of the problem most basic to any ethnographic undertaking, how does one come to 'do' an ethnography. Field work is shown to be a dramatic exercise in unveiling one's own ignorance. Undergraduates with unbounded assurance that field work is some sort of prolonged vacation in exotic-land will do well to read between the lines for there are countless anguished moments of epistomological inquiry imbedded in Newman's prose. The reader is effortlessly led from the beginning of the job to its temporal conclusion, only realizing as he closes the book that he too has been led gently to 'know' the Gururumba as the author knows them. Newman is undoubtedly a sensitive ethnographer, but more important to the reader is his ability to communicate this in an articulate manner and in this he has been quite successful.

In his analysis of the named units of the social structure, Newman commences with the largest group claiming a common identity, the people "of one leaf" of the Upper Asaro Valley, and progresses down the oppositional scale to the individual actors. A Gururumba is shown to be a member of a tribe (dual named units of political alliance), a phratry (a territorially based unit), a sib (the putative patrilineal descent group), a lineage (the maximal unit in which descent is traced), a clan (in which membership is determinable by both descent and residence), and a ward (those individuals affiliated with a single men's house). Each societal unit is seen to have quite distinct functions.

Newman's presentation of the composition of societal units and the delineation of the social mechanisms whereby they are integrated into functioning elements represents the sort of analysis necessary to deal the death blow to vague notions of 'flexibility' and 'structural looseness' expressed by other authors over similar material. But how much of the rigor achieved is an artifact of the structure of the monograph series will always be a nagging question. Certainly the information on the Gururumba does fill a gap in the ethnographic coverage of Highland New Guinea but, as Newman himself notes in reference to his abbreviated list of kin terms, it "is not, in itself, very informative". (p. 25)

Having identified the individual in his formal network of kin and residence ties, Newman then proceeds to a thoughtful analysis of segments of individual behaviour (roles). The opposition and complementarity of giving and taking in economic exchange, of men and women in the flute cult and other activities, and of man and his environment, are some of the organizational themes to which the fabric of Gururumba society is attached. He has integrated all of these varying topics through consideration of the connections between ritual and social structure, and man and the supernatural. By progressively narrowing the scope of inquiry he has produced a remarkably clear picture of the significance of individual behavioral variation.

In conclusion, the following typographical errors should be noted; *illiances* for *alliances* (p. 66), *take* for *takes* (p. 69), *palmiforlia* for *palmifolia* (p. 77), and *FaFlBrSon* for *FaElBrSo* (p. 24). It also appears to me that there might be an additional error in the listing of the kinship denotata. After the kin term *arunE* (p. 24), Newman lists MoSiDaDa. If the children of an *arunE* are *noho* (e.g. SiSo, SiDa), then MoSiDaDa would be *noho*. Perhaps Newman intends this listing to be MoSiSoDa rather than MoSiDaDa. But if this is so then he has not listed the kin terms for the children of either the MoSiDa or the FaBrDa and we can only assume that the proper term is *noho*. Obviously, the kinship terminological system of the Gururumba is more complex than this short list depicts.

Aside from some of my possibly misplaced criticisms, this monograph on the Gururumba should be a 'must' for all undergraduates and it won't hurt graduate students to study it as well.

Edwin A. Cook
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Frameworks for Dating Fossil Man. Kenneth P. Oakley. Chicago, Aldine Publishing Company, 1964. x-355 pp., illus. $8.75.

In an earlier review in this journal (*Anthropologica,* vol. VI, no. 2, 1964, pp. 253-255) I deplored the lack of a reliable and comprehensive textbook on Old World prehistory for use in advanced undergraduate courses. Oakley's book does not satisfy the need completely in this respect, nor did the author have this aim in mind, but it does go part of the way,

Oakley's intention originally, he tells us, had been to write a much longer book, but ill health and the persuasion of his superiors at the British Museum (Natural History) caused him to publish it as it stood in 1963.

The title may be somewhat deceptive, for it deals with prehistoric cultures of the Palaeolithic and Mesolithic periods as well as with fossil skeletal materials. Oakley is primarily a Pleistocene geologist with a particular interest in stratigraphy and in methods of dating hominid remains. He has himself been responsible for developing new means of establishing the ages of archaeological materials, especially the fluorine process which he used so successfully in the Piltdown case. The first half of his book is therefore devoted to descriptions of the principal ways of establishing absolute (or, as he now prefers to call it, chronometric) and relative dates. This part consists of 16 brief chapters on glacial and post-glacial chronology, river terraces, faunal correlations, palynology, varves, sea and land level changes and descriptions

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of the stratigraphic sequences in a number of areas of the Old World. Part 2 concerns itself with archaeological dating and describes in broad outline the main Palaeolithic and Mesolithic cultures of Europe, the Near East, Africa and Asia. Each half of the book is followed by its own bibliography and its own series of notes. The latter are especially useful in enlarging on or clarifying points which could not be treated in the text. There is a valuable set of tables (in 44 pages) listing virtually all the Palaeolithic and Mesolithic skeletal remains known in the Old and New Worlds, giving for each the date of discovery, the name or type, the stratigraphic position, the cultural affinities and, if known, the age in years. Maps, charts and illustrations are liberally scattered through the volume.

This is essentially a handbook, for reference and information purposes, rather than a text. The emphasis is on description rather than analysis, especially in the archaeological sections. This is not meant as a pejorative criticism; the book was not intended to be a definitive treatment, Oakley does most of what he tried to do very well and the original sources on which he bases his descriptions are usually well chosen and up-to-date.

As a rule a reviewer deals most harshly with an author when he impinges on the reviewer's own field or speciality. In this case it is the reverse. Oakley's accounts of the Palaeolithic and pre-Neolithic cultures are good and show a commendable objectivity in treating Africa and Asia in as much detail as Europe and the Near East. Some of his suggestions show considerable insight, e.g. his remark (p. 271) that a number of industries in northern Asia might be viewed provisionally as belonging to a "Far Eastern Late Palaeolithic complex". One could quibble over his ambiguous use of such terms as Levalloisian (culture? technique?) and Mesolithic (cultural grouping? period?). Frequent references to "hybrid" industries could cause confusion. The West European Upper Palaeolithic sequence is given in detail but Central and Eastern Europe are left in the shadows. More space might have been allowed to the Late Palaeolithic in the Middle East. An archaeologist might be forgiven for feeling less confident than Oakley that major geological/climatological events can be shown not to be equivalent from one continent to another "by using the spread of Palaeolithic traditions as time lines" (p. 137).

The weakness of the book, in my opinion, lies in a certain lack of proportion in Part 1. For example, although much space is given to the principles of dating by river terraces, and the Somme Valley gets a complete chapter, that other vital European river, the Thames, where Oakley himself helped work out a magnificent chronology some years ago, is hardly mentioned. The eight pages devoted to fauna are not enough; the important question of "warm" vs. "cold" faunas is hardly touched. Palynology is disposed of in an even shorter chapter. The Alpine glacial sequence has been overemphasized in describing the European Pleistocene chronology. I wish more space had been devoted to loess, and that a separate section had been allowed for climatology. The scheme of Pleistocene subdivisions employed is essentially that proposed by Wolstedt in 1958; a fuller discussion of the criteria used and the superiority of one scheme over another would have been helpful for students who are

not acquainted with the geological and palaeontological backgrounds of these controversies. The terse, fact-filled writing makes this book difficult reading at times for the uninitiated; in fact, even specialists can be puzzled by certain statements.

These relatively minor criticisms should not disguise the fact that this is an important and valuable book. Nevertheless, one can feel a certain regret that Oakley abandoned his original plan of a book four times the length of this one, and especially his decision to publish separately his excellent essay "The Problem of Man's Antiquity. An Historical Survey" (*Bulletin of the British Museum, Natural History, Geological Memoirs,* vol. 9, no. 5, 1964).

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Introduction to Applied Statistics. John G. PEATMAN. New York, Evanston, and London, Harper & Row, 1963. xv-458 pp., illustrations. $7.95.

Peatman does not claim that his book presents any important new developments in statistical theory or technique; it is intended rather as an introductory text in applied statistics, primarily for psychologists, but of use also for sociologists and cultural anthropologists. It contains a good deal of solid substance, and more detail than many introductory texts offer. A diligent student who for some reason had no access to any other statistics book could learn many useful procedures from Peatman's, including uses for Chi Square and for the t Test, and multiple correlation and regression and analysis of variance methods. However, many other introductory books on statistics are available, addressed to roughly the same group of readers, and one must judge Peatman's book in comparison with these others.

My reaction is unenthusiastic. I can think of no potential reader who would not find alternative texts as good or better. In particular, I think it unsuitable for a one-semester introductory course because, judging by my experiences in several years of teaching such a course, most anthropologists lack the mathematical skills to be able to cover, with any useful comprehension, more than about half the material in that much time. For a one-year course, or for more mathematically inclined readers, I would still prefer other books; above all because this one has a general infelicity of style that too often becomes downright murky and obscure. A few examples can be mentioned. On pages 21 to 39 the discussion implies that variables which are countable and orderable differ in some important way from those called rankable, and that variables which are countable and scalable differ from "measurables" (that is, those for which an interval scale can be established). If there are important differences, I do not understand them.

On pages 28-29 methods for getting the mean of a set of ranked observations are given without any warning that this is quite a different thing from getting the mean of a set of interval scale measurements. In the same passage, a concept as important as the median is introduced abruptly and given a cursory definition which could lead the reader to think that it refers to the middle *interval* of an ordered distribution, rather than to the middle *score*. A reader who looks with care at the example with which Peatman immediately follows his definition can deduce what he actually means by the median, and he will eventually find a better (though still not ideal) definition of the median on page 41 (which is not listed under "median" in the index), but I think that the understanding of this concept is made needlessly difficult. The mode is introduced on page 42 as the "point or interval of most frequencies", and as far as I can see the only further reference to the mode is on page 46, where the word "unimodal" is used without definition. At any rate, this is the only reference to "mode" in the index, and "unimodal" is not listed at all. Often formulae are simply stated without any derivation when it would have been fairly easy, using no more than high school algebra, to outline the essential steps. An example is the formula for the number of permutations of n things taken r at a time, given on page 157. On page 64 nj is used to stand for the number of observations within the kth sample of a set of samples, at the same time that j is used as a summation index for deviation scores within any sample, while it is stated that "j signifies the number of measures or observations *within* a sample or group" (italics Peatman's). This is certainly putting a heavy load on j, and it could be cleared up by leaving j as the summation index, replacing nj by nk, and saying that "nk signifies the number of scores or observations *within* the kth sample or group".

Mostly things are not presented wrongly, but just less plainly than in some other books. However, on pages 271-273, where Chi Square and Fisher's Exact Test are being compared as tests for significance of association in small samples, significance levels for Chi Square are given which assume that no direction of association has been predicted. There is then a switch to a significance level for the Fisher test which implies that direction of association *has* been predicted. Assuming direction of association was not predicted, the significance level given should be doubled, leading to a value quite close to that given by the Chi Square approximation with Yates' correction for continuity. One would conclude from Peatman's treatment that Chi Square is badly in error here, when in fact for this example it is quite accurate, even though all cells of the table have expected frequencies of only 2 or 3.

I am also unhappy with Peatman's one-sided treatment of probability. On page 7, in chapter 7, and elsewhere, he gives the reader to understand that the theorems of probability are only applicable to events which are repeatable many times, under well-defined conditions. One would never guess that many contemporary statisticians take the "personalistic" view that probability theory can also be applied to beliefs about specific events. Bayes' Theorem is never mentioned. Many introductory books simply gloss over this controversy altogether, but it seems really better to do that than to imply that

the things to which probability theory does or does not apply are agreed upon, when in fact they are subject to debate.

An unfortunate feature of reviews is that one tends to give most space to a book's faults. I should reiterate that most of what Peatman says about most things is sound. There are worse books as well as better ones.

George L. COWGILL
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The W.B. Nickerson Survey and Excavations, 1912-15, of the Southern Manitoba Mounds Region. Katherine H. CAPES. Anthropology Papers of the National Museum of Canada, Number 4, Department of Northern Affairs and National Resources, Ottawa, 1963, 178 pp., 2 tables, 19 plates, 33 figures, 6½" by 9½".

Mr. William Baker Nickerson was a nonprofessional archaeologist and just as Alfred Hill, Theodore Lewis and later Ellison Orr, his penetrating interest in the antiquities of man led him to explore and record archaeological remains in a sophisticated manner. Although Nickerson had worked earlier in the middle Mississippi Valley, this article is based upon his field notes and manuscripts compiled while an employee of the National Museum of Canada. The investigations pertain to sites in the valleys of the Souris, Pembina, Assiniboine, White Mud and Red rivers of southern Manitoba. The site data are organized into the above regional areas and each area is introduced with a brief geographical description. Some of the sites visited by Nickerson had been investigated previously by Henry Montgomery, George Bryce and Alfred Gould, and Nickerson discusses their observations. Many other sites had been disturbed by relic hunters and agricultural operations and a few were found undisturbed. As reflected in the title, most of this article is relevant to burial mounds, however, a tabulation of the various archaeological manifestations shows a much broader scope. The sites include conical, elliptical, effigy and linear mounds, linear mounds with expanded ends, earthen enclosures, circular and rectangular depressions, campsites, villages and a historic Indian grave site.

The task of organizing these data into a publishable form, searching out artifacts and photographic collections, arranging for restorations and specimen identifications, and analyses by various specialists, was undertaken by Katherine Capes. Through the use of Nickerson's notes and the introduction of more recent literature, the author supplies us with sections relative to other mound excavations in the region, comparative materials, detailed artifact descriptions, and a summary and conclusion.

It is in the last two latter sections that I find some questionable statements and inferences. For example, the author believes that there is little evidence for any extensive time period of mound building and that all of the tumuli

exhibit features indicative of the Late Prehistoric Period. From the data presented, I do not follow this reasoning. Her ceramic descriptions show Manitoba Cordmarked Ware as being found at the Elliot Village, in the Arden "area" and as a pot from Mound R. Regina Ware was not recovered from a mound. The Mandan Ware was retrieved from an old sod line level under Mound R and the Winnipeg Fabric-Impressed was associated with a premound feature at Mound B (p. 43). Suffice it to say that the majority of the tumuli did not yield pottery and ceramic evidence of a late period of mound construction is very dim. Nor is the presence of grooved axes diagnostic of a late period. Their archaeological and artifactual associations are quite different in Mound G, Moore C and Calf Mound, and none of these three contained Plains Side-notched projectile points which supposedly mark the Late Prehistoric Period. Although the Southern Cult is considered to be protohistoric in Manitoba, only two tumuli, the Calf and Lone mounds, have rather niggardly collections that may be Southern Cult. Nevertheless, marine shell pendants, gorgets and beads, even when associated with native copper, do not necessarily connote Southern Cult. The author also relies upon physical anthropological evidence identifying the skeletal remains as Lakotid and thus protohistoric. We are not told how many or which bones were measured; presumably skulls. But from which tumuli? Nor am I sure that Hrdlicka's measurements of 63 adult, Dakota, male crania, upon which the Lakotid Variety was based, will stand the test of time.

I am in agreement with the author when she states that southern Manitoba mounds show influences from various areas in Minnesota and the Dakotas, but unlike her I see a succession of influences that began 1,500 years ago and extended up into the early historic period.

The maps, charts and plates are clear and the publication will be useful. The price of the book is not stated.

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The Fern and the Tiki. An American View of New Zealand National Character, Social Attitudes, and Race Relations. David P. AUSUBEL. New York, Toronto, Holt, Rinehart and Winston, Inc., 1965. 232 pp. $1.95 paper.

The author of this work, which was first published in Australia in 1960, has several purposes, some explicit, some implicit.

The most serious claim, and the one which is least well performed, is that this is a systematic study of New Zealand national character, based on the view that national character is formed during the whole of the lifetime of individuals and through the operation of national institutions, rather than

through mechanisms such as those of toilet training which are stressed in some psycho-analytic approaches. Good studies along these lines are few and far between; we can think of Benedict on Japan, the classical de Tocqueville, Lowie on Germany, and in recent years de Rougemont on Switzerland and Barzini on Italy. Each one of these works is highly controversial, and succeeds in stimulating through the disciplined yet intuitive juxtaposition of contemporary observation (except in the case of Benedict) with historical analysis and a sense of historic direction, a dissection of the most influential institutions such as those of politics and religion, and a humanistic assessment of national mythology, national heroes, the strengths and weaknesses of cultural achievement, and the kinds of individuals who have come to the forefront of the nation's life.

Ausubel states in the preface to his American edition that the work which is closest to his in spirit is Barzini on Italy. There are some parallels. In particular, both are critical of the national character they are describing, and both make comparative reference to the United States (though Barzini shows no evidence of preferring the United States to Italy, despite his criticisms, and is in fact full of affection for his countrymen).

There the parallel ends. Ausubel is highly selective in his subject matter; he is best and most extensive when he deals with the educational system and with race relations for these were the areas of his primary interests when he was in New Zealand. He has almost nothing to say about New Zealand history, hero figures, literature, outdoor recreation, religion, gambling, travel, emigration and immigration, all of which are extremely illuminative of regularities of national character in New Zealand, and which are the kind of material used extensively by Barzini. Except for a somewhat one-sided treatment of the secondary school, and a justified side-swipe at the New Zealand press, there is not a single detailed study of the workings of any organized institution (Barzini, who is not a social scientist, did better than this with the Mafia.)

It is very soon evident that this is not a detached and systematic analysis of national character. What then is it? Ausubel provides part of the answer. It is *criticism*, a criticism frankly based upon a clearly declared American view. As such it may be taken as a case study in the relations between a foreign guest and a national host, but even here in many respects not a typical one. New Zealand shocked Ausubel profoundly, and as a result of this shock he writes many things bluntly, bitingly, but often perceptively and usually, to this reader at least, with justification. There can, from a certain viewpoint, be no more irritating country than New Zealand; so smug, it seems, so self-satisfied, so certain that everything done in God's own country is the best in the world, so resentful when foreigners hint that this is not so, so blunt, downright and earthy in language.

Ausubel's criticism is well written and clear. It is spoiled by failing to conceal the emotion which lies behind it, by over-statement and inaccuracy. Epithets such as Victorian (in the perjurative sense), incongruous, insidious,

and archaic abound. But New Zealand is a living, energetic, on-going society which satisfies a high proportion of its inhabitants a high proportion of the time. If criticism is to be telling, it should be based on understanding rather than frustration at the incongruity, and occasionally one should be able to laugh or find cause for praise. Only in race relations is there modified praise, and even, as on p. 15, when New Zealanders are obviously enjoying themselves Ausubel fails to see that they are laughing at his expense.

Even when considering the study as criticism, and perhaps accepting looser canons of argument than would apply in a scientific study, judgements are too frequently undisciplined (and by discipline, Mr. Ausubel, I do *not* mean authoritarian). He often writes, as on p. 58, as if the same people think disparate or opposing things, making this a conflict within them, and under-glossing the spread of public opinion. He cites the frequent and often misinformed deprecatory view that New Zealanders hold of American culture and education; but no New Zealander with any pretensions to culture himself would deny the greatest achievements of the United States in music, literature, the universities, science. Ausubel's statement is one-sided and prevents him from understanding the criticism when it does come. Other statements are made which may be perfectly correct in themselves, but for which no evidence whatsoever is cited (a common failing throughout the book). Thus, on page 64, the quality of medical and surgical care is said to be not as high as in the United States. Probably true on balance, but is there evidence? And what kinds of medicine, in specialties, in hospitals, in the home? Sometimes Ausubel lets his fancy run away with him, as with the New Zealand woman turned loose in an American department store; the experience of New Zealanders coming *to live* in the United States is not one based on the premise of a spending orgy. Too frequently, Ausubel turns arguments to suit his purposes. An example, by no means unique, is on p. 97. Ausubel complains of New Zealand interpretations of American blue jeans as school dress, since New Zealanders "fail to appreciate ... that national fashions in clothing are purely arbitrary", and on the page before he characterizes the short-pants uniform of the New-Zealand schoolboy as "children's clothing" indicating an almost deliberately stretched out infantilism. A footnote in this case shows that Ausubel is aware of a possible inconsistency; but he completely fails to put short pants into the context of the New Zealand myth of the great out-doors. (Dear me, what *would* he say of Australian bank clerks in short pants?)

A very high proportion of the book is taken up with summary statements of the American national character or of conditions in the United States (many of which are highly simplified and would not command universal acceptance in the United States). These are used partly to indicate the critical standards against which New Zealand is to be judged, but more significantly to correct the malinformation which is circulating in New Zealand, and to defend the United States against such unjustifiable criticism. (In doing so, at one point, and without the slightest evidence of humour, he criticizes New Zealand exchange teachers who come back from the United States spreading distorted information.)

More than this, Ausubel is itching to reform. He would dearly love to put the New Zealand school system to rights; he regards it as out-moded, authoritarian, and dreadfully out of line with the ideals of American education and modern educational theory. He was chagrined when his suggestions were rebuffed, apparently somewhat bluntly, by the New Zealand public (and press?). But he was sufficiently aware of his responsibilities as a visiting scholar to observe the "strict avoidance of patronizing attitudes" (p. 5) (no comment).

I have been very critical, and could be much more so. But if we read the book in another light, and if it had been presented to the reader in another way, I could have warmly recommended it. I like the book for its clear writing, its honestly held opinions, and because it says many things that many New Zealanders say about themselves. In the earnestness with which Ausubel reacts to the cultural shock which hit him with such a whammy there is a refreshing forthrightness which reveals the writer as very human and sincere, and New Zealanders should be the last people in the world to resent bluntness. (Don't mistake protest in equally blunt terms for resentment, Mr. Ausubel.) In England, Australia, Canada, the United States I have felt some of the things Mr. Ausubel has felt, and would feel them again were I to return to New Zealand. Read not as a statement of New Zealand national character, but as a description of a shock to the value system of one kind of visiting scholar, this is a very good book indeed.

Cyril S. BELSHAW
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The most ancient eskimos. Lawrence OSCHINSKY, Ottawa, The Canadian Research Centre for Anthropology, 1964, Paperback, 112 pages, 6 plates, 12 figures, 17 tables, $3.00.

One of the major problems still remaining in the study of Eskimo prehistory is the racial affinities of the people who created the Dorset Culture in the Canadian Arctic. Were they Indians who intruded into the Canadian Arctic from the south or were they the result of direct, close contact involving racial mixture between pre-Dorset and Indian? After a short discussion (there are only twelve pages of text in the book) Oschinsky states that the "Dorset people were morphologically Eskimo" (p. 32).

Facing this difficult problem of trying to determine the racial affinities of the Dorset people Oschinsky has attempted to analyse not only the metrical data from the skeletons but more predominantly what are known as non-metric morphological traits.

The cranial series have been examined for the following seven morphological characters; narrowness of the nasal bone, verticality of malar bone,

thickness of tympanic plate, presence of a sagittal keel, degree of gonial eversion, size of palatine torus, and size of the mandibular torus. These are scored on a five point scale and all observations were made by Oschinsky. Notes on scoring and figures demonstrating both measurements and morphological observation help greatly to objectify this type of non-metric data.

The report is well documented (there are 160 references). Printing is good and the basic anthropometric measurements and scores on the morphological traits are clearly printed in 17 tables which take up 32 pages.

It is encouraging to see a revival of interest in non-metric morphological traits.

William M. Bass
University of Kansas.

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Organizational Analysis: Political, Sociological and Administrative Processes of Local Government. Charles A. Joiner. East Lansing, Michigan State University, Institute for Community Development and Services, 1964, 61 pp.

An editorial in the *New York Times* of December 12th, 1965 began:

> Planning is the vital, broad-range, long-term process that links needs to solutions, that tells a city where it is going and how best to get there. In New York, planners don't plan. They advise, and are often ignored and overidden. They work hard, they mean well, and they have a lot of answers. But officially, nobody needs to listen.

The planning literature is full of breast-beating about the plight of planners whose views are ignored or distorted as the modern dragon, the city, rampages out of control. But why don't people listen to planners? Why do planning agencies appear to be so ineffectual? Charles A. Joiner, Associate Professor of Political Science at Temple University focuses his study of organizational analysis on the role of planning agencies in the governmental structure, and provides a number of answers to these questions.

The author begins by reviewing the literature on organizations. He discusses the various definitions of the word, and the dimensions and typologies of organizational variability, and then examines the peculiarities of government organizations. He sees governmental organizations as processes of converting inputs to outputs — the inputs being both demands and supports, and the outputs taking the form of policies and political decisions.

The second chapter analyses the structure of organizations. Chapters three and four deal with planning organizations, and with governmental administrative organizations and multiple structure universes. A fifth chapter summarizes the discussion, and notes that all government organizations and their accompanying processes of inducement-contribution exchange are political,

sociological and administrative phenomena. Much planning has gone awry in the past because of an ignorance of the delicate balance and interplay of these phenomena.

Reading what the author modestly calls "his essay" is an experience akin to eating a high vitamin biscuit. This publication is really a condensed book, rich in ideas. Professor Joiner has compressed an immense amount of material into a few pages; there are no less than 212 footnotes as well as an excellent bibliography. The discussion is on a high theoretical level, in clear lucid prose. Professor Joiner knows the right questions to ask to determine the effectiveness of a planning agency, and he lists those that must be answered in any analysis of such agencies.

This publication is a valuable addition to the social science literature, and should provide a guide to planners seeking to understand the structure of their own organization and the relationships between it and other bodies involved in the planning process.

The price of the publication is not mentioned on the cover or elsewhere in the book, nor is there any indication whether it is distributed free of charge.

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Saints and Fireworks: Religion and Politics in Rural Malta. Jeremy BOISSEVAIN. London, London School of Economics Monographs on Social Anthropology No. 30, The Athlone Press, 1965, 154 pp., $5.95.

This book is a sober and detailed description of contemporary life on the island of Malta. The author shows a clear grasp of the political power structure of Maltese institutions, and is concerned with the way these institutions influence village life.

In this small island the Catholic Church has had a long tradition of dominance, and in fact, until a generation or two ago appears to have been *the* dominant force in Maltese culture. There was, and still is, a highly ramified church structure, which in one of its many subdivisions includes every person in every village — often a person is a member of several subdivisions. The daily life of the people revolves around church observances. Village celebrations are primarily religious ones. The priest is the person of highest status in the village. Attendance at church is expected of every person in the village, and until very recently almost no one has dared to risk social ostracism by remaining at home on Sunday.

This book describes in elaborate detail the petty concerns that occupy the attention and energy of Maltese villagers as they go about their daily life.

It analyses the bitter factional splits that have riven most of the villages — factions aligned either with the patron saint of a village, or instead, with its secondary saint. The elaborate celebrations on saints' days, involving collecting of funds, decorating church and village, buying fireworks, all provide opportunities for intense group participation and make for group cohesion. Alas, they likewise make for intense rivalries between the opposing factions. In recent years the rivalries have spread to political affairs, as well. The author has shown that there is a tendency for the elite in the villages to align themselves with the party of the patron saint and with the conservative element politically. And there is a similar tendency toward an alignment of lower status people with the party of the secondary saint and with the labor party politically.

The cultural focus then that emerges from this study of Malta is a powerful Catholic Church organization, formerly the dominant force in the culture, but now losing some of its influence. The members of the labor party have recently been resisting the power of the church, and through this successful resistance have themselves become a powerful force. Caught in the struggles, the villagers have found it increasingly difficult to maintain a united front to the outside world — there is too much division among them now, arising not only from religious club affiliations, but more and more, from political affiliations as well. However, it is this very division that is maintaining the uneasy balance of the Maltese society, the author points out in his conclusion. For the people who are involved in the conflicts are trying to keep them from becoming more divisive, by maintaining their contacts with the discordant elements. How long will they be able to keep these conflicts under control?

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La République malgache. Roger PASCAL, Paris, Berger-Levrault, 1965, 202 pp., ill. 21 F.

Madagascar fait constamment l'objet de publications de valeur fort inégale malgré l'abondance et la richesse des illustrations de certaines d'entre elles.

Parmi les plus récents, les meilleurs et les plus importants par leur contenu, citons un livre qui ne comporte que 11 illustrations dont 3 graphiques et d'austères photographies de moments historiques récemment vécus par la grande Ile. C'est celui de Roger Pascal, "La République malgache Pacifique indépendance".

Comme son titre l'indique, l'ouvrage est d'actualité et manifeste en même temps un grand courage pour traiter d'un sujet aussi délicat, une grande lucidité et un souci délibéré d'éviter toute complaisance. C'est l'œuvre d'un juriste qui est aussi un historien engagé. Il a lui-même vécu les années de

décolonisation qu'il décrit, années de la naissance et des premiers pas de la jeune république et R. Pascal présente les faits avec rigueur et un appareil critique qui font de son travail (thèse de doctorat) une œuvre d'importance et comme l'écrit Flavien Ranaivo, Directeur du Service de l'Information, son ancien condisciple à l'Ecole coloniale de Paris, dans la préface du livre: "Nul doute, cet ouvrage fera parler de lui."

Regrettons, ici, pour n'y plus revenir, certaines erreurs, de date: le débarquement anglais eut lieu en 1942 et non en 1943, de noms: le traité de 1841 fut conclu avec Tsimiaro, roi, non des Antaisaka mais des Antankarana, l'auteur de "Fokonolona..." est G. Gondominas, etc, qui sont toutes gênantes dans un travail d'historien ou de juriste.

Pour R. Pascal "la décolonisation de Madagascar doit se comprendre comme l'évolution des rapports franco-malgaches qui ont conduit de la colonisation à l'indépendance, et se poursuivent encore de nos jours" (p. 12). Il en étudie successivement les causes, les moyens et les effets.

Les causes, d'après leur analyse, remontent aux conditions historiques locales qui avaient rendu Madagascar "colonisable", puis les hésitations de la politique coloniale française tiraillée entre le désir d'assimiler la personnalité malgache et le souci de la respecter. D'autre part, il y avait des nationalismes, toujours vivaces, de formes et d'inspirations fort diverses qui provoquèrent l'échec de la politique d'assimilation. Relevons au passage que les pages sur le colon européen de brousse, sur les administrateurs chefs de circonscription, ont un accent d'authenticité que tous les anciens coloniaux sauront reconnaître malgré la sécheresse des traits: "Il ne pouvait être question d'administrer et de respecter le code du travail mais bien de commander et de prendre ses responsabilités pour obtenir tout avec rien" (p. 32).

Parmi les causes de la décolonisation, l'auteur mentionne à juste titre les anticolonialismes, qui paraissent maintenant bien paradoxaux, de l'Amérique et de la Russie, les nécessités internes et externes: "Il fallait que cessent les caprices des sujets mécontents et que naissent des citoyens responsables" (p. 50) et d'autre part, les conditions étaient devenues telles que "si la décolonisation n'était pas intervenue, la métropole aurait eu à supposer des charges vite intolérables" (p. 51). Tout cela est fort pertinent.

En juriste, Roger Pascal ouvre son titre II. "Les moyens" par une chronologie de textes législatifs ou réglementaires qui ont jalonné le processus de décolonisation censé commencer, car il faut bien un terminus *ad quem,* au 28 août 1941 avec la charte de l'Atlantique, bien que la France n'y fût pas partie, et interrompu et non terminé, avec la loi du 27 décembre 1962, portant la troisième modification à la constitution de 1959 de la République Malgache. Ces moyens furent les différentes formules juridiques qui réhabituèrent les Malgaches à l'usage d'une certaine démocratie et à l'exercice des pouvoirs, en particulier la loi Deferre ou "loi-cadre" du 23 juin 1956, puis le vote positif au référendum français du 28 septembre 1958. Un chapitre important inventorie "l'héritage français" consistant en partis politiques dont seuls subsistent réel-

lement maintenant le PSD (Parti social démocrate) qui est actuellement du pouvoir et l'AKFM, (Parti du Congrès de l'Indépendance), la fonction publique qui fut malgachisée très progressivement, puis la dévolution amiable du domaine public et de toutes les réalisations étatiques françaises faites, pour les plus récentes, à l'aide du Fonds d'investissement pour le développement économique et social (FIDES), relayé depuis par le FAC, Fonds d'aide et de coopération. A ce propos, l'A. prend résolument parti pour les accords bilatéraux: "Entre Madagascar et la France, il y a dans une certaine mesure "échanges". Des biens et des services sont échangés contre le culte de souvenirs communs, contre une certaine influence (...) Au nom de la dignité, on exige l'aide internationale, mais cette dernière ne pouvant rien demander en contre-partie fera des pays sous-développés des assistés totaux, leur dignité n'y gagnera rien" (p. 96). Et cet argument est de poids.

R. Pascal envisage ensuite les effets principaux de la décolonisation qui résultent de la liberté du choix face aux responsabilités nouvelles. Ces effets portant principalement sur la nationalité des habitants de Madagascar et des Iles proches, dont Ste Marie de Madagascar qui posait un problème très délicat, sur l'appareil juridique de l'administration, enfin, les options de politique extérieure quant à l'armée, les alliances internationales et les solidarités assumées conjointement avec les anciens ou les nouveaux états. Pour la refonte du droit, il convient de se référer à un autre ouvrage récent publié par le Ministère de la justice, mais nous pouvons dire ici que la réforme fut faite dans le sens d'une sévérité accrue quant au droit pénal, de certains tatonnements pour le droit public et d'une volonté de progrès et d'unification des diverses coutumes de l'île pour le droit privé car: "le gouvernement malgache a pris pleinement conscience que le droit est un moyen pour provoquer et hâter la révolution d'un peuple" (p. 112).

Dans ses choix économiques, l'Etat malgache devait tenir compte d'un "tableau assez peu réjouissant de l'économie malgache à qui font, tout à la fois, défaut les matières premières, les capitaux, les entrepreneurs et les acheteurs" (p. 115). La solution acceptée est actuellement celle proposée par "le nouveau capitalisme de France", qui, affirme l'auteur, mais nous n'en sommes pas si sûr "n'est pas celui de l'époque coloniale, il a conscience d'accomplir un service"! (p. 129). "Le problème à Madagascar, pour le moment, ne consiste donc pas tant à accroître la production pour la vendre à l'extérieur (à part les plantes industrielles, sucre, sisal, etc.) qu'à provoquer une révolution technique pour Madagascar, considéré à la fois comme producteur et comme consommateur" (p. 131). Et l'A. ose même affirmer "pour obtenir cette mutation, il faut que le Malgache soit désespéré" (p. 132) et il ne cache pas son souhait de voir l'annihilation de l'opposition, à laquelle il adresse de graves griefs, au profit du parti gouvernemental auquel "reviendrait alors le rôle ingrat de provoquer la révolution économique qui exigera de la sueur et des larmes" (p. 113). Dans ce désir de voir instaurer un parti unique tout puissant, nous voyons une contradiction avec un trait de la mentalité malgache, relevé p. 72-79 à propos de la description des partis, que l'unanimité doit toujours tenir compte de la contradiction et être obtenue par des conces-

sions réciproques. Et le jugement porté contre l'AKFM dans la conclusion (p. 159) manque par trop de sérénité pour être accepté sans discussion. Preuve en soit l'affirmation que nous a faite, devant Richard Andriamanjato, chef de l'opposition, le Président Tsiranana lui-même, le 21 juin 1965, que tant qu'il serait là, il y aurait la possibilité d'un parti d'opposition. C'est la sagesse de ce grand Président qui est la chance principale de la jeune république.

Cet ouvrage, œuvre d'un homme engagé, est donc un livre important pour connaître la république malgache et sa lecture est indispensable pour comprendre le déroulement d'une expérience unique de décolonisation qui, par suite de la qualité des hommes qui l'ont faite ou qui la vivent, est exemplaire à bien des titres. Notre désaccord avec l'auteur sur l'opportunité d'instaurer un parti unique, dans ce pays de la discussion courtoise, n'enlève rien de la valeur du livre que complètent vingt pages d'annexes fournissant des textes bien choisis pour en illustrer les principales thèses.

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AUX COLLABORATEURS DE LA REVUE

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