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COLLABORATEURS — CONTRIBUTORS

Richard F. Salisbury
René R. Gadacz
Leo G. Waisberg
G. E. Mortimore
Thomas F. Johnston
Trevor Denton

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THE CANADIAN RESEARCH CENTRE FOR ANTHROPOLOGY
LE CENTRE CANADIEN DE RECHERCHES EN ANTHROPOLOGIE
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Non-Equilibrium Models in New Guinea Ecology

Possibilities of Cultural Extrapolation[1]

RICHARD F. SALISBURY
McGill University

RÉSUMÉ

Beaucoup d'analyses anthropologiques qui se disent écologiques, sont vraiment du fonctionalisme pur; elles assument gratuitement une relation équilibrée entre facteurs culturels et milieu physique, se basant sur l'étude d'un seul moment historique, et sans démontrer aucune histoire de feedback ou d'adaptation à long terme. Une analyse des fêtes de porc des hauteurs de Nouvelle Guinée et du développement de la population Tolai de Nouvelle Bretagne depuis sa première colonisation aux environs de 1200 (A.D.) illustre la possibilité de construire une famille de modèles écologiques qui n'assument pas l'équilibre. On assume en premier lieu une population humaine en croissance continuelle, qui n'est pas limitée absolument par le milieu, et qui possède des règles culturelles relativement fixes. Même avec des règles constantes on en observerait diverses réalisations dans le comportement selon la densité de la population, la disponibilité de terre, etc. Parmi les Tolai, aucune réalisation équilibrée "adaptée au milieu" n'est survenue, pendant sept cents ans. On considère qu'il faut un séjour d'au moins trois cents ans dans un milieu stable et limitant, pour produire une "adaptation". On ne trouve cette stabilité que très rarement. L'analyse écologique culturelle ne devrait pas viser à l'analyse d'adaptations culturelles à des milieux fixes, mais plutôt à l'analyse des façons de réaliser des configurations culturelles stables dans des milieux changeants et changeables.

[1] Fieldwork among both the Siane and the Tolai has been conducted at various times with the support of the Australian National University, the University of California, the U.S.P.H.S. grant MH-4912, McGill University, the Canada Council, and H.H., the Administrator of Papua New Guinea. For discussions leading up to the present paper I would like to thank Ann Chowning, Ward Goodenough, Jim Specht, Peter White, John Terrell, Dale To-Pin, Tirupia, ToMange and Titai. The paper itself was first presented at the Smithsonian Conference on Biogeographical Models, May 1974.

The ecosystem models most commonly used to organise the data obtained from traditional subsistence-based societies have been equilibrium models. This has been particularly true for New Guinea, where one of the most sophisticated such analyses — that of Rappaport (1968) has been a paradigm. Unfortunately, despite their sophisticated techniques for the collection of data, such studies have relied on data from only relatively short time periods. The multitude of variables measured for any one society have not been fully demonstrated to be causally related, but most have rather been *assumed* to be functionally related. Much "ecology" has actually been a modern neo-functionalism, explaining why traditional society was unchanging and/or "adapted to its environment".

The present paper begins to develop a family of models, in which equilibrium is *not* assumed, and which seem relevant to New Guinea reality. Human populations are generally assumed to be continuously but slowly expanding, over very long periods, within a physical environment that is not generally restrictive. Technological modifications of productive techniques, and modifications of social relationships within and between populations, are seen as related to progressive increases in population density. But the relationship is not a Malthusian one of negative feedback, where overpopulation causes famine and population reduction, but rather the positive feedback mechanism of new techniques and relationships being seen as more productive with greater population density. The models are time-related phase models, not necessarily valid for infinite extrapolation. The family of models also assumes that a large number of cultural behaviours observed at any one time in a particular society are *not* aimed at adapting the population to the current physical environment. The aim of these behaviours, and much of their content, is derived from a long cultural tradition which current generations try to replicate; *qualitatively* the cultural tradition may change slowly. But the extent to which any population does tangibly recreate the tradition is highly variable, with the availability of resources, technology, or relationships dramatically influencing *quantitatively* the population's success with that culture. The maintenance of cultural continuity is indeed the major parameter for each model.

The effect of the technological change from stone axes to steel (Salisbury 1957, 1962; Sharp 1952) exemplifies this assumption. In about ten years, and without the introduction of pacification or colonial control, most measurable aspects of New Guinea highland societies were drastically altered. The single change in the amount of time spent in subsistence activities altered the balance of political power, the involvement of social groups in religious ceremonial, the balance of relations between the sexes, the distances travelled for trading, and a hundred and one other features. All listable features of "traditional culture" were retained in the repertory, and very few features added, so that studies made five years after technological change appeared entirely "traditional". Yet the "florescence" actually observed was quantitatively extremely unlike the drabness of life ten years earlier. Yet many fieldworkers, without a historical or economic sense, have interpreted their descriptions of steel-using times as though they corresponded with life in stone-using times. This has had no disadvantages for qualitative studies of symbolism, myth, child-training, etc, and the cultural continuity through cargo-cults, colonialism, and copper-mining is striking. But for more quantitative studies of politics, economics or of ecology the assumption of "traditionality" is questionable. At the very least it makes extremely suspect any studies of "long-term balanced adaptation to a physical environment". Not only is "balance" something that must be proved, not assumed, the idea of "adaptation" as "arrival at a relationship, through mutual negative feeback" is something that is extremely unlikely to occur within a ten year period.

RAPPAPORT'S ECOLOGICAL MODEL

Specifically Rappaport's (1968) analysis of Mareng pig feasts as an ecological mechanism, whereby religion automatically regulated crises of over-population by humans and pigs, suffers from all these difficulties. In the first place its figures on productivity were all obtained five years after pacification and twenty years after the first advent of steel axes; they say little about the state of crop production, pig populations and consumption even ten years earlier. Secondly, and this comment will be explicated later, the figures were all collected in villages which were in the last year of preparation for a pig feast. Thirdly, "there is reason

to believe that Mareng occupation of the Simbai valley is relatively recent... within the last 200 years", (Rappaport 1968: 36). With a population of 200 people now occupying 1060 cleared acres in a total area of 2033 acres it would seem unlikely that a "balance" has yet been reached between population and land, but rather that a continuous, if uneven, process of expansion averaging five acres per annum has been in progress over 200 years and still is occuring. Fourthly, the extreme intricacy in detail of the "adjustment" to the local environment of the Mareng analysed by Rappaport raises the twin questions of why other New Guinea Highland groups in different environments also have similar pig feasts, and of how the Mareng, within two hundred years, could have worked out by trial and error such a fine adjustment.

And finally (for the present argument) the literature on other New Guinea Highlands societies where pig feasts are celebrated indicates that the population dynamics of pigs, which Rappaport takes as "natural" or inherent in pigs and something to which humans adapt, are, in fact, the result of deliberate planning by pig breeders. Pig feasts, and the crises which trigger the final slaughters are all matters of deliberate long-range planning. My own Siane data of 1952-53, for example, were collected mainly in a village that had had a pig feast two months before my arrival. During that year many pig owners explained to me their problems in ensuring that the two or three pigs which they had left after the slaughter would multiply for the next planned pig feast; any unexpected ceremonial demand for a pig during the first year can have disastrous consequences for planning. At the same time I was puzzled as to why farrows that I knew to have been of nine or ten piglets at birth were uniformly three young pigs when later censused; the answer is, of course, that although pig population must be made to grow exponentially to provide for a massive slaughter, it is only in the final generation before a killing that the exponential rate can be raised from a rate of doubling each year to quadrupling in the penultimate year; for the final year before a ceremony no piglets at all are bred but in early years the survival of piglets is consciously manipulated to increase slowly (Salisbury 1962: 93). It is this final year that Rappaport witnessed — a year for which the Mareng had deliberately planned years in advance, knowing that the huge pig population would create

huge demands on garden production of sweet-potatoes to keep them fed until slaughter, and knowing what demands and problems to plan for. He would have obtained very different data on planning if he had studied a year later. Thus it may be noted that during my year among the Siane I saw no sweet potatoes at all being fed to pigs in villages which had recently celebrated pig feasts; foraging in fallowing gardens provided sufficient for all pigs. The crises, human and porcine, which Rappaport saw the pig feasts as being a ritual adaptation to, are best seen as the predictable working out of breeding programmes, planned to produce crises. Pig Feasts are causes, not effects, and I am sure Mareng would explain this to any anthropologist who asked, in just the way Siane do. In support of that argument I would cite the fact that the Siane, on religious grounds, gave up Pig Feasts in 1959; in 1966 they decided to reinstitute Pig Feasts "in order to make pigs grow big again" — something pigs had ceased to do when breeding and killing were being planned in terms of maintaining a constant size population. It is the deliberate planning for pig feasts that results in a greater total output of pork than would result from other human planning strategies.

CULTURAL EXTRAPOLATION MODELS

The family of non-equilibrium models that this New Guinea Highland experience directly suggests is that of cultural extrapolation — the analyst assumes that cultural rules as formulated explicitly by informants are followed in practice as far as they can be, and he projects empirical consequences until his projection indicates that constraints emerge. For pig feasts these rules would be those described for pig breeding, and for planning for competitive feasting at intervals. The major data given by Rappaport would then be interpretable as the attempt by Mareng breeders/feast-givers to follow the rules as far as possible, consistent with resource availability.

The long-term ecological model would then be one of Mareng initially moving into a "new" environment of primary forest, at low population density, giving small feasts using largely feral pigs; of gradual change in the human and pig population densities, in importance of domestication, in the frequency and size of pig

feasts and in the associated political activities, as more primary forest was converted into arable bush-fallow land — with the dynamic being the entrepreneurial drive of the New Guinean pig-feaster; such progressive development of pig-feasting might endure for two or three hundred years, before limits to land productivity would necessitate a change in "the culture" away from competitive pig-breeding. Pig-breeding would not be an "adaptation" to a static environment, but the definer of a dynamic self-modifying culture. The culture would, for a long time, adapt the environment so as to meet its own ends, but only after a very long time would the environment become selective *against* the culture, and then only against a critical feature of the culture — pig-breeding.

Historical data are not yet available to specify such a definitive model for New Guinea Highland pig-breeding and feasting. Yet the archaeological picture of underlying continuity of populations, dramatic local changes with changing technologies of drainage and fencing, composting and changing crops, of taro, pueraria, and (two hundred years ago) of sweet potato (and recently of Irish potatoes) suggest that such a model may be constructed. The work of Brookfield (1968), Watson (1965) and White (1970) appears to lead in this direction, confounded, it is true, by arguments about whether changes were "revolutionary" or not. As it is, the present suggestions for Highland models remain speculative.

CULTURAL EXTRAPOLATION BY PHASES — THE TOLAI

By contrast, recent work among the coastal Tolai people of New Britain (Salisbury 1969, 1973; Specht 1966) gives a fairly firm time horizon of between 700 and 1,000 years, for the expansion of a small population of around 100 within a defined geographical area, to a size of about 50,000 people in the same geographical terrain. The exercise of trying to interpret the "ethnographic present" of 1883 (Parkinson 1887) and 1961 (Salisbury 1969) as the working out of cultural rules derived from an earlier less densely populated period (with recent modifications of technology or social relationships), and of then iterating the process to arrive back at the founding 100 — although it may be empirically

erroneous — is instructive about the nature of a particular model of cultural extrapolation.

In the first place there have been many attempts to show how the "traditional ethnographic present" Tolai were in an equilibrium functional relationship with their environment of rich volcanic soils surrounding the harbour of Rabaul (e.g. Salisbury 1969, Epstein 1969, Bürger 1913). These attempts will be discussed as they highlight how the equilibrium is questionable. Since 1870 — the time of first European contact — there has been a rapid population growth from 20,000 to 50,000 in a hundred years, though pressure on land resources first surfaced only in the 1920's and became acute after World War II. Since 1950 land has been acquired in "foreign" areas, although at a rate less than proportionate to population growth. In 1961, the time of my first study, the Tolai still produced enough food locally to support their entire population. They themselves did not eat all they produced as they sold for cash large quantities of root crops and vegetables, for European residents in town, and for the feeding of immigrant workers in town and on European plantations; these sales balanced Tolai purchases of canned proteins (corned beef and fish) and storable carbohydrates (rice, sugar, biscuits). Less than a quarter of Tolai land was used for subsistence crops in 1961, however, the vast bulk being planted to cocoa and coconuts as cash crops. Local pressure on land was pressure to obtain higher cash incomes, aggravated by the alienation of about one third of their 1870 land for use by European plantations. It was not a matter of population density exceeding *subsistence* carrying capacity of the land.

In 1950 Tolais owned about 500 square miles of land, giving an average population density of less than 80 per square mile, but local densities of over 400 per square mile. If as much as a quarter of the land is under subsistence crops, the carrying capacity under subsistence agriculture of the fertile Tolai land would appear to be between 320 and 1,600 per square mile. By contrast, in 1870 the average density was below 30 per square mile, and no local group exceeded 150. Pressure for land can hardly be invoked as a Malthusian cause for a stable situation in 1870.

Nevertheless at this time, late ages of marriage, high bride-prices, small families due to spaced pregnancies, and considerable

bachelorhood among non-wealthy men suggest that population growth was actually very slow, if not zero. An equilibrium theorist might argue that population was then spaced, at a level below one fourth of what would "fill" this ecological niche, by the endemic feuding, with cannibalism between groups of neighbouring hamlets which limited normal travel to a range of four or five miles. Contrasting with this local dispersion were several area-wide activities — ceremonials, held periodically in individual hamlets, to which people came from a wide area, and at which shell money was distributed (Salisbury 1965), local markets, for trade in the specialties of ecologically different but neighbouring villages; and long distance voyaging to obtain shells for manufacture into currency. But the extrapolationist would realise that although the feuding may have *acted* to limit population growth, it can hardly have been *introduced* because people valued low population growth; twenty years of Pax Germanica stopped warfare, lowered bride prices, produced a lower age of marriage, and produced a 3% per annum population growth that was continuous thereafter except for the crises of an eruption in 1937 and occupation by 120,000 Japanese in World War II. A pre-existing cultural valuation for ritual and shell money political entrepreneurship which had previously been checked by feuding and cannibalism, then worked itself out in ways which had marked consequences for population growth and man-land relationships.

But even the 1870 pattern of endemic entrepreneurial expansion constrained by feuding and cannibalism showed evidences of not being stable but of having changed during the preceding hundred years. Richard Parkinson (1887, 1907) suggested that the Tolai themselves were recent arrivals in the area from New Ireland, and his suggestions were widely adopted by other German writers. His strongest evidence, however (Bürger 1913: 12) was of the importation of particular *tubuan* rituals from New Ireland or New Hanover less than a hundred years earlier. Whether this was the first importation of the rituals, or whether it involved the addition of a set of new variants to pre-existing local rituals is uncertain; my own evidence and collections of myths (e.g. Meier 1909) suggest the latter. Another field in which the 1870's evidenced recent change was that of overseas voyaging to find shell money. This appears as an infrequent and highly dangerous

activity in the first ethnographies (Parkinson 1887, Powell 1884) but within twenty years (Kleintitschen 1906) had developed into regular annual exoduses using established over-night villages en route with major portions of villages temporarily absent. My own calculations of the rate of manufacture of new shell money suggest that at a period somewhat before 1780 enough shell could have been produced from Tolai beaches without the need for overseas voyaging, and that production since 1780 accounts for more than is now in circulation. The 1870 situation must be seen as the delayed result of dynamic forces already present in 1780.

Finally there is historical evidence that volcanic activity has produced changes in the environment. In 1878 islands emerged from the sea in Rabaul Harbour after an eruption of the local volcano; in 1937 two cones grew on opposite sides of the harbour, one from nothing to one thousand feet, and the other (Matupit cone) spewed out ash and mud over a wide area. The island of Matupit at the foot of the cone was reported in 1878 to have risen above the water only a few generations previously (Brown 1908) and this report is confirmed by Epstein's geneological inquiries (1969). He reports myths of first settlement going back only a few generations, and a pattern of conflict over land rights almost unique among Tolai, but resembling what was ascribed by Burger (1913: 17-18) to the effects of pioneer individualism on newly cultivated land. 1780 may well have been a volcanic eruption period.

The ardent equilibrium ecologist might then wish to go back beyond 1780 in his search for a stable equilibrium and might try and recreate the preceding, less populous society, without developed *tubuan* rituals or elaborate shell money, and probably without the internal political differentiation that both contribute to. Would it have had cannibalism? Probably not. Inter-village cannibalism would seem likely to have been part of a phenomenon of increasing political differentiation, whereby big men made use of population density and "sold" "troublemaking" supporters to big men of nearby groups in a pattern of elite collusion to buttress their own powers. Any "balance" would have had to exist in a very much simpler society than existed in 1870. And such a "balance" would probably not have had feuding and cannibalism to maintain it; it would have existed at a time of potential popu-

lation expansion with much available land, before the advent of feuding and cannibalism as control mechanisms. In short, having started in a search for an equilibrium, one would have had to go back to before 1780, to a time when populations were much smaller, and when there would have been no apparent ecological constraint on expansion, and when "balance" would have been unlikely. At the same time one would have suggested how phases might have succeeded one another in a growth model.

There is local evidence for relatively static, non-dynamic agricultural populations in the region. In the ethnographic present the neighbouring people to the Melanesian-speaking Tolai are the non-Melanesian Baining. They had, before recent village consolidations, population densities below one per square mile, and cultivated swidden patches in primary forest, rather than the long-fallow rotation of areas of secondary forest and grassland used in the Highlands and to some extent by the Tolai. It is hard to envisage a sharper cultural boundary than that between the Tolai and the Baining, and one which, before recent modifications, was more closely tied to the ecological boundary of the volcanic soils used by the Tolai.[2] Elsewhere in New Britain, in the other islands of the Bismark Archipelago, and in the Solomons, other enclaves of Melanesian speakers dot the coastline, while bordering sparsely populated areas are inhabited by non-Melanesian speakers. On linguistic (Goodenough 1961) and archaeological (White 1970) grounds the dispersal of a Melanesian-speaking stock, and the arrival of people using Lapita-type pottery on many of the islands has been placed about 3,500 years ago. Could it be that the emergence of an equilibrium between a static non-Melanesian agricultural population and a dynamic immigrant Tolai population had still not been established after 3,500 years of boundary interaction, sporadic warfare, and occasional trade for feathers, stone clubs, obsidian etc.?

The time-horizon is actually much shorter in the Tolai case. At a conference at Santa Cruz in 1971 on the Bismarck Archipelago the role of periodic catastrophes or unique events in the cultural

[2] The physical anthropological boundary is not so clear; Tolai and Baining appear phenotypically to be more similar than Tolai are to other Melanesian speakers from Bougainville or Papua; the latter appear more similar to neighbouring non-Melanesian speakers than to other Melanesians.

evolution of small scattered populations such as the Baining was considered. In the Tolai area the periodic volcanic eruptions provide just such catastrophes. Specht's (1966) excavations of the island of Watom, just off the Tolai coast, indicate that the early bringers of Lapita-ware to the area, were in fact overwhelmed by a massive eruption that dumped ten feet of pumice on the island, twenty miles from the centre of the Rabaul crater. This eruption must have been of the same order as that which disrupted Krakatoa, blasting away completely the east side of an earlier crater about eight miles in diameter, and leaving deposits over a hundred feet thick near the crater edge. While a firm date has not been established for this cataclysm, J.M. Specht informs me that it must have been somewhat before 1250 A.D. and could have been several hundred years earlier. I shall use the figure of 1200 for convenience. Tolai occupation of the area now covered by the volcanic soils must date from *after* the eruption, which rendered the whole area sterile. It could have begun as early as 30 years after the eruption, if one is to judge by the first clearing of forest and planting of gardens on "new" land created by the 1937 emergence of Mt. Kalamanagunan. By 1971 these were under way. On the other hand a longer interval may have been necessary in the 13th century as the new cone of 1937 was not too far distant from reservoirs of seeds brought by winds or birds; the 1200 eruption must have desolated an area of up to 1,000 square miles.

With this background I was intrigued during fieldwork in 1971[3] to be insistently told myths of the first peopling of the area by Tolai, which I had not previously heard. Though they do not mention any volcanic eruption, they make no mention of a previous local population being present, and they are uncannily consistent with the picture of a steady intrusion of a new population into a vacant ecological niche created by a volcanic eruption. The places cited as the first and second settlements — the second occurring when the first split over a trivial conflict — were respectively at the east and west ends of the boundary between the pumice deposits and the non-volcanic soils. These would have been the

[3] This fieldwork was as a consultant investigating causes of dispute among the Tolai, one of which was a resettlement project, involving the site listed in the myth. Details of the myth, particularly local place names, are given in Salisbury (1973): the present article explores the significance of the myth and modifies some of the figures hypothesized earlier.

first areas to become habitable after the eruption, offering an ecotone with the neighbouring primary forest (occupied presumably by Baining people) as the pumice weathered and vegetational colonisation occurred. No settlement was made on any intervening coastal site — presumably because that land was still sterile. The second settlement, the home site for all western Tolai, unfortunately proved disease-infested — it is low-lying and virulently mosquito-ridden even today — and the myth narrates sequences of moves by some groups inland along the ecotone of the forest-pumice boundary, and sequences of moves by other groups gradually north into the pumice area. The legendary original home site of the western Tolai was in fact unoccupied at the time of German takeover in 1883. The occupation of Matupit island in central Rabaul Harbour was narrated as a most recent stage of direct movement northward. The movement eastward around the forest boundary was rapidly completed, by a linking up with eastern Tolai groups who maintain no myth of a western origin. For both eastern and western groups subsequent movements are reported as mainly northward, towards the coast, but east-west ties are common and no sharp "border" can be demarcated between east and west.

This myth provides a base for extrapolation forwards to meet the extrapolation backward from ethnographic data. We have a picture of a single settlement in the eastern Tolai coast some time around 1200, of two boatloads of settlers (say 50 people). Our first assumption is of constant population expansion. Initially the period of doubling may have been every fifty years. The original settlement probably fissioned into two, of fifty each, about fifty years later, and one half migrated westward about 1250 and settled in vacant newly cultivable land. Disease and periodic moving may then be considered to have slowed the population increase but, assuming for simplicity a constant rate, the southern boundary of Tolai territory would probably have been demarcated, and groups of fifty first settlers located on all Tolai parishes by about 1450. The total population would have been 1600, and the population density about three persons per square mile. Expansion to a population of 12,800 would then have taken until 1600 A.D. and would have involved the internal expansion within their own territories of these small kin groups to yield a density of 20 per

square mile. Slowing population expansion thereafter with the next doubling taking up to 200 years, and stability thereafter would have gone along with progressive increase in political differentiation, in the power of descent groups and the development of feuding and ritual. Such an assumed development accounts for most available ethnographic facts.

Put in more general terms the Tolai "model", is one of a population colonising a vacant niche. In the first phase of expansion by doubling every 50 years, fission and the definition of boundaries and territories was crucial. Internal expansion and occupation of a defined niche then proceeded at probably a slightly reduced rate for a long period depending on local soil fertility and ease of transport. The density-linked control mechanism braking population growth would then seem to have been brought about by increasing internal political differentiation, produced as higher population density gave opportunities for political entrepreneurs.

DISCUSSION

This scenario sounds plausible, but lacks proof. Its strongest support is the way in which it takes features which equilibrium models proved unable to incorporate, and builds them into a consistent picture, through the use of two assumptions: 1) that a human population will double in fifty years, unless constraints operate to prevent this, and 2) that cultural rules, reported in the present, have usually had a long existence, but many form part of very different sets of actualized social behaviours as resource availability varies.

It also suggests particular phenomena that could be studied archaeologically or by comparative ethnology. In the first place it suggests how small populations having sea transportation can establish themselves in small particularly fertile, coastal niches, and within a relatively short space of time can, through simple demographic expansion, appear dominant over a wide area, where nearby sparse, moving, swidden, populations face more severe restraints. The Tolai today form one of the largest single language groups in New Guinea and their educational level has enabled them to become dominant in the national bureaucracy of independent Papua-New Guinea. It is hard to realise from the present

situation that they were only two boatloads of settlers 700 years ago.

The model also suggests a long time-horizon for processes of adaptation and equilibration. The phase of establishment and definition of territorial boundaries may require hundreds of years. Internal expansion within boundaries may also last further centuries before active expansion outside the cultural boundaries takes place. The fertility of Tolai soil and the size of the area they initially marked out are, indeed, exceptional, so that their extremely long internal expansion phase is probably unique. Until the nineteenth century it had possibly involved an abnormally long period of no external conflicts. Nonetheless lack of competition between coastal Melanesian speakers and interior speakers of non-Melanesian languages is reported elsewhere also. Intermittent trading relations for special products in an ample and permissive land environment was the normal rule and the reported Tolai raiding of coastal Baining groups for slaves in the 1870's may well have been a recent development.

None of this is to argue that volcanic eruptions have been the major factor in the establishment of Melanesian speaking groups throughout New Guinea. It is merely to argue that eruptions provide the most dramatic examples of completely new and vacant ecological niches of considerable productivity being created, and permitting local expansion. Changes in coastlines, minor climatic shifts in rain shadow areas, introductions of new plants and many other features could have comparable though less dramatic effects, while the periodic extinction of local sparsely populated groups by stochastic processes would also explain the creation of vacant areas.

A third use of a model such as this is an aid to thought. Although the Tolai example is derived largely from induction, the thought processes that have gone into it are analysable. Most important is the explicit use of the assumption that "cultural rules" (Salisbury 1968; Bailey 1969) obtained in ethnological description from informants' statements are a highly stable aspect of social behaviour. Such "cultural rules" should be contrasted with the realisation of those rules in actual behaviour and the *strategy statments* (Salisbury 1968) [or pragmatic rules (Bailey 1969)] used to summarize appropriate behaviour, which vary according to

the quantitative parameters of the environment. Leach's (1961) demonstration of this contrast for Ceylon is a classic study. Among the environmental parameters, land availability, population density, productivity of technology, and the state of the natural environment are highly important. When the social anthropologist quantitatively records social behaviour at one particular time and in one place, he is not describing a balanced adaptation to a specific environment, but one specific realisation of the cultural rules, by particular people who modify their behaviour to cope with particular parametric constraints of time and place.

Critical in constructing such models is the isolation of what are relatively invariant rules, and what are significant parameters. The intellectual process of modelling and model manipulation enables one to try out many alternative possibilities, but one starts empirically. In ethnographic fieldwork one can deliberately look at, say, a land-rich village and at a land-poor village within a group that asserts its cultural unity (cf. Goldschmidt et al. 1967), to see what "cultural rules" are asserted in both areas, and what specific environmental differences condition the behavioural differences found in the two localities. One can look at different cultural groups with a single cultural rule in common; one can compare sub-groupings of a cultural group, and how they act when a range of environmental parameters are varied.

But the primary parameter that any archaeologist or culture historian should always hypothetically vary as a first step is population density. He should ask how a society with the current cultural rules would have operated with a population half its size, and a quarter its size. Depending on assumptions of population increase rates these scenarios would represent 50, 100, 200 or 400 years ago. Lesser political or economic complexity would be indicated on general grounds, but existing cultural rules would also suggest many specifics such as whether settlements would have been nucleated or dispersed, whether descent groups would have been large and solidary, or constantly fragmenting into local segments.

In the Tolai case, for example, the twin current rules of matrilineal land tenure (Salisbury 1969: 68-71) — that "clan land" is inherited by the sister's son, whereas a man may pass "family land", which he has cleared from forest or otherwise ac-

quired personally, to any person he determines (usually his own children) — clearly have different implications whether "new" land is available or not for conversion into "family land", and these implications further affect the degree of solidarity of matrilineal descent groups, the proliferation of new sub-groupings and their later consolidation into new corporate matrilineages. In fact in the perspective of a society that has land available at its frontier, the rules make better sense than they do now. If one envisages an early period of whole territories of forest, each of three or four square miles, inhabited by single viri-avunculocal matrilineal groups of less than 50 persons it would be expected that these could expand internally several times with little fission over centuries. Even above these population densities, fission of matrilineal groups by male children leaving their natal household, yet clearing new sites for dwelling within the territory owned by their father's clan could have been a common practice, producing internal differentiation. It is widely reported for other matrilineal societies. Eventual institutionalization of such new groupings in their separate clearings as local sub-clans of exogamous major clans is what is stated by modern Tolai beliefs about sub-clan origin sites (*madapai*). The relatively stable period of Tolai internal expansion could be visualised as crystallising an organization of each "village" territory as comprising two or four resident sub-clans, each a section of different non-localized clans. At this point moiety exogamy (a firm current cultural rule) would have modified itself to being consistent with the present predominant village endogamy, where earlier it would have required village exogamy. With further population increase village endogamy could have led to less frequent inter-locality relations, more power in the hands of authoritarian big men who could control local marriages and could prevent further clan fission. The scene would then have been set for the proliferation of shell money exchanges among big men, for inter-village feuding and cannibalism and for the social life of the mid-nineteenth century. The basic cultural rules of matrilineal land tenures can be seen as implying vastly different social arrangements under different quantitative conditions of population density.

This process of projecting backward is, in some ways, an exact opposite of the process of functionalist equilibrium analysis. In the

latter type of analysis one may indeed consider what would happen if parameters were to change (usually by growth) from what they are at present, but only to argue that such change is currently being prevented by some homeostatic mechanism, or else the society would not be the way it now is. Cultural extrapolation tries to see if the present is intelligible as growth from a past, without assuming that any mechanism producing homeostasis has necessarily come into play.

It involves taking a long time perspective. If behaviours exist which do now counteract expansion — limits on the primary forest land available for initial clearing, for example — they must be considered. But not as features that the society *adapted* to in the past. They may constitute the limits beyond which a society cannot in future expand, given existing cultural rules; but the reaching of those limits may take a hundred or more years even with the most rapid expansion. Generally it seems likely that at any one time more societies are in phases of expansion lasting several centuries, than are in states of dynamic equilibrium, mediated by feedback mechanisms, with their environment. Europe, South and Southeast Asia, Africa south of the Sahara, and central America have all been involved in rapid social and technological change for the past fifteen hundred years; during those fifteen hundred years it is the rare exception to find a period lasting over 300 years in which there has been "stability".

Periods of apparent stability, such as the European middle ages, have seen massive depopulations through epidemic disease, but these depopulations, though they may be seen as an epidemiological reaction to greater population density, have not been periods of "balanced feedback", but rather as catastrophic statistical accidents in a growth curve.

A final point, that this use of a growth model for understanding Tolai society brings out, is the need at some point to incorporate cultural innovation into the model. Although many of the rules of present-day culture (such as the land-tenure concepts listed earlier) can be easily visualised in the context of a different stage of population growth, others cannot. Some present-day rules may be innovated strategy statements, that have only recently become accepted as cultural rules. Some of these changes

may be historically documented; others may be indicated by model manipulation.

For example, the current extravagance in the use of shell money among the Tolai, I find incongruent with a population smaller than 10,000 in the area, though complex shell-money systems such as that of Rossel Island (Salisbury 1968) *can* exist with population densities of less than 2 persons per square mile, and using a total of only a few thousand shells. An early simpler system of shell money perhaps dating from original Tolai settlement, could easily be visualized as subject to local elaboration and innovation (with political connotations) as exchanges became more frequent.

This suggestion (and similar suggestions about the relative recency of cannibalism, and of *tubuan* ceremonials), I have earlier derived more directly from historical and oral evidence. But using a cultural extrapolation model as a tool for hypothetical analysis. I maintain, would have thrown up the same elements as anomalous ones, given earlier periods of lower population. To phrase this another way, any extrapolation backwards should always consider successively eliminating *one* of the current cultural rules from each calculation, hypothesizing it as having been an innovation of the time period under consideration. A whole family of alternative reconstructions would thus be possible as different rules were successively considered as innovations; the most probable reconstructions would serve as guides in the search for hard evidence of specific innovations, either archaeologically or through literary or oral history. Using a model, however, permits the search to become explicit and reasoned.

CONCLUSION

As this paper developed, I have steadily realised how close many of its formulations come to those expounded by Kroeber (1943) in his *Configurations of Culture Growth*. It also sees societies with cultural rules, elaborating and playing with those rules, and applying them to varying environmental settings to provide varied realisations of the culture over centuries of time. Like Kroeber it sees the dynamic in this cultural variation as being

individual inventiveness in modifying rules to fit personal circumstances. Culture, in short does not itself adapt to environments but is the means through which *individuals* adapt to their environment which, they as individuals and in the short term, take as "given". Culture develops, elaborates, or stagnates in a process of individual cultural innovation. The vast majority of innovations, like minor genetic mutations, are unrelated to survival, either by individuals or by culture. But the richness in number of innovations occurring (the complexity of a culture and the variability of possible individual behaviours within it) does lead to a greater probability that *some* survival-related behaviours will occur and will be selected for. Selective pressure when it eventually develops, can then most rapidly favour particular mutations if a previous period of affluent cultural expansion has permitted increasing variability.

But the conclusion is also that "adaptation" of whole cultures to environments (either as a static state, or as a feedback process) occurs only rarely. A state of "balance" may have existed among some hunting groups, living in the same slowly changing environments for thousand of years. But the two hundred or more years needed for the selective elimination of maladaptive cultural traits in a constant environment, that would appear needed to produce a state of adaptive balance, is a rare phenomenon in the world of agricultural societies. Even then the elimination of whole societies would seem most common; not a specific selection against the maladaptive feature itself but a collapse of all features of the culture. People rarely learn the lesson of what is maladaptive. The alleged crisis in Rome because of lead-poisoning from the plumbing, would be an example of an unselective elimination of a whole society, which did not solve the problem itself; a better adaptation did not occur as a result of the catastrophe. Crises of over-population may, as Geertz (1963) has shown, unselectively eliminate highly efficient technological systems in favour of a much more intensive "involuted" agriculture which produces a greater total output, but at a much higher cost. Again, the catastrophe is unlikely to produce a balance between people and environment.[4]

[4] Contrary to Boserup's (1965) view that innovation occurs *in response* to population pressure, my position is clearly that innovation occurs much more frequently within cultural systems that have *not* reached a tight limit

Cultural ecology should be looking, not for examples of stable adaptations to stable environments, but for the processes whereby unstable societies with relatively stable cultures cope flexibly with environmental change.

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within a particular environment. It is, however, in affluent societies where multiple alternatives have already become available, that increasing population pressure can then lead to the selective elimination of technologies that are maladaptative under those conditions, and thus to the institutionalisation of changed technologies.

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Montagnais Hunting Dynamics in Historicoecological Perspective

RENÉ R. GADACZ
McGill University

RÉSUMÉ

Cet article décrit la dynamique de la chasse chez les Montagnais avant sa transformation par le commerce des fourrures. Les *Relations des Jésuites,* le récit de Le Jeune en particulier, fourniront la base de l'analyse. Il est supposé que le cycle de la chasse saisonnière correspond à une relation entre la réalisation de la valeur maximale de la nourriture et le principe du moindre effort. En corollaire, la conservation naturelle en est le résultat. Les rapports contemporains indiquent que, sous des conditions idéales, le rendement est quelquefois équivalent à la ration requise, tandis qu'une analyse alimentaire basée sur le récit de Le Jeune démontre l'écart entre la consommation d'énergie et le rendement, dans des conditions adverses. La ration durant cet hiver n'était pas plus qu'un tiers des 4,800 calories requises par personne par jour. Avec l'arrivée du commerce des fourrures, l'épuisement du gibier a réduit les possibilités de chasse et a conduit à l'abandon de la conservation de la nourriture pour l'hiver.

The aim of this paper is to examine the diary of the Jesuit missionary Paul Le Jeune and his account of a winter spent among the Montagnais in 1633-34 (JR VII: 67-211) from an ecological perspective. His account will be appraised in terms of modern anthropological theory to evaluate aboriginal hunting dynamics and to determine the effects of European contact upon these dynamics.

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The analysis is focused on a nutritional study of the winter hunting cycle. I have hypothesized that the seasonal hunting cycle ideally corresponds to an association between maximum food value attainment and the principle of least effort. A corollary to be tested is that if this association holds true, natural conservation is the result. These hypotheses have been derived from a variety of sources, though the concepts have generally been ill-defined and have not been pursued in detail (Cox 1973; Elberg and others 1972; Lee and Devore 1968; Lee 1969). The seventeenth century Montagnais hunting economy will also be evaluated as to its "aboriginality", since it is likely that Montagnais hunting practices and economic life were already in the process of change when first described by the French.

THE AREA AND THE PEOPLE

References to the Montagnais in the early seventeenth century are numerous, although these are exclusive of interior bands whom the French had not yet encountered. They were first known in the area between Tadoussac and Three Rivers along the north shore of the St. Lawrence River. Their hunting grounds at that time extended as far as the Appalachian watershed south of the river. These areas, of a mixed boreal and deciduous forest type, are characterized by severe winters of four to five months duration. Accumulations of ten to fifteen feet of snow are not uncommon, though the amount on the ground at any one time is approximately five feet or less (JR V:123; XLXIX:159; Champlain II:44). Summers are short by contrast, with no more than a 140-day growing season.

The *Jesuit Relations* (VI:271-273) point out that moose, beaver, and the woodland caribou were hunted by the Indians, as were bear, badger, porcupine, fox, hare, marten and three kinds of squirrel. Birds were also taken in large numbers, especially white and grey geese, ducks, teals, ospreys, divers, partridges, and woodcocks. The varieties of fish and water animals included salmon, pike, carp, sturgeon, whitefish, smelt, doré, eels, and turtles. Frogs were not considered fit for human consumption. The Montagnais were gatherers too, and raspberries, strawberries, cherries, wild apples and grapes supplemented the meat and fish diet. All

of these resources were seasonal and none alone could constitute a year-round subsistence base.

Many statements can be found in the *Jesuit Relations* concerning the Montagnais way of life: "they wander through the woods, and climb the summits of mountains of prodigious height, hunting for moose, caribou, and other wild animals" (JR XXXVI: 221). Le Jeune (JR VI:277) stated that "during the months of September and October they live for the most part upon fresh eels; in November and December and often in January, they eat smoked eels, some porcupines, which they take during the lighter snowfall, as also a few beavers. When the heavy snow falls, they eat moose meat... to live upon the rest of the time until September; and with this they take a few birds, bears, and beavers, which they take in the spring and during the summer". Champlain (II:44) noted that eel fishing was an activity lasting from the 15th of September to the 15th of October. From this we may conclude that the subsistence pattern was characterized by summer amalgamations along the shores of the St. Lawrence with band breakup and seasonal movement into the interior commencing in the fall. Le Jeune's account best describes this behaviour in reference to the route chosen (or imposed) and group composition.

Le Jeune's group set out in a northerly direction from Quebec City on November 12, 1633, whereupon they were informed "that there were a great many Montagnais where they wished to pass the winter" (JR VII:107). They were subsequently joined by two other groups, and having learned that hunting up north was not good, decided to pursue a more southerly direction on the south shore of the river. This indicates that the low and late snowfall of that year did not provide the most favourable milieu for the stalking of game, and suggests that the Indians were adopting a strategy of dispersal designed to alleviate the crisis.

In contrast to the band with a membership of 150 to 300, and summertime aggregates to upwards of 1,000 individuals gathering on the banks of the St. Lawrence (Leacock 1969:9-11), the winter hunting party oscillated between a multilodge group of approximately 35 to 75 individuals and a single multi-family lodge group of 10 to 20 individuals. Le Jeune's party initially had a membership of 19, and after other groups joined them, member-

ship rose to 45. A month later, one of those groups decided to leave so they would be spread out over a wider area (JR VII:147). This indicates that group cohesion was based on the amount of game available. The basic socio-economic unit was not the nuclear family, since the nature of the game and dangers involved eliminated single-hunter groups as a viable unit. An efficient hunting pattern would have been difficult to maintain were it not for a strategy of dispersal *and* group membership flux.

While less-skilled kinsmen were certainly supported, hunting skill played an important role as far as matrimonial success was concerned. The Apostate[1] in Le Jeune's account provides an interesting example: "...the Savages jeered at him for being sedentary and not wandering.. the women make fun of him.. he would die if not fed... and gets lost in the woods like a European" (JR VII:173). Not only is a man such as this despised but "such men cannot find wives or retain them — the Apostate has already had four or five."

MONTAGNAIS HUNTING DYNAMICS

Montagnais nomadism was based on hunting "sedentary fauna" of which the beaver was an important component. It was the beaver that the Indians could hunt when the larger game animals were difficult to pursue or were not to be found at all. The Montagnais were not restricted to *one* resource as the bands farther north were. The Naskapi for example, specialized in the mass hunting of caribou in areas where few alternate resources were available. Under such circumstances, the failure of herds to appear or even their delayed appearance had serious consequences. By contrast, the more generalized hunting patterns and broader resource base of the southern forests buffered the Montagnais from the effects of such fluctuations.

For analytical purposes, the writer favours the framework used by Cleland (1966:43-45). Cleland suggests that adaptations,

[1] Taken to France as a child, Pierre Antoine Pastedechouan returned six years later. This experience caused him to forget his way of life. Truly a victim of circumstances, he was to die of starvation alone in the woods some two years after his association with Le Jeune. (Brown 1966, I:533)

or economies, may approximate one of two polar types — focal and diffuse. The former develops only in the presence of a very reliable and readily available resource. Such an adaptation involves specific techniques for maximizing successful exploitation, and the hunting of the migratory caribou by the Naskapi is an example of such an economy. The Montagnais' is a diffuse economy; adaptation is not based on the reliability of one resource, but on an ability to exploit a variety of resources, seasonal or otherwise. Moose, for example, is generally considered erratic and sparsely distributed, and moose hunting itself is regarded as a very unreliable activity (Feit 1973:118). Recourse to the beaver is then in effect a buffer mechanism, permitting a shift of emphasis upon another prey species.

A shift in emphasis may be brought about by certain variables which must be considered if an association between group cohesion and resource availability is to be established. One of these is faunal fluctuation. As Feit (n.d.:27) has pointed out, "the beaver, as well as other game animals, large and small, were periodically subject to severe fluctuations of populations". He adds that beaver interact with comparatively few components of the ecosystem, and neither is affected by nor affects them. As its isolation is generally keynoted by its stability, in contrast to moose, it is clear why "the beaver to them (the Montagnais) is like the bison to the Plains Indians" (Speck 1915:293). Concerning the moose, the *Relations* inform us that (1636) "not since ten years have the Savages killed so many moose" (JR IX:71), and this may be interpreted as a result of population fluctuation, though this is difficult to substantiate given the data available. Another variable, that of snowfall, may also determine resource availability insofar as it is ancillary to successful hunting. The Montagnais "may not be pressed with famine every year, yet they run the risk every winter of not having food, or very little, unless there are heavy snowfalls and a great many moose, which does not happen often" (JR VII:51). The worst, of course, is an adverse combination of both variables, but even one may cause starvation.

References in the *Jesuit Relations* persist in drawing a close relationship between snowfall and successful hunting, for example: "The snow was deep this year — five or six feet in the woods; consequently moose hunting was excellent" (JR XLIX:159). Le

Jeune was bluntly told "there is not enough snow to kill moose, beaver, or porcupine" (JR VII:145). The depth of the snow alone did not determine the success of the hunt, but rather its condition: "sometimes they chase one of these animals for two or three days, the snow being neither hard nor deep enough; while at other times a child could almost kill them, for, the snow being frozen after a slight thaw or rain, these poor moose are hurt by it and cannot go far" (JR VI:295). Similar observations have been made by Feit (1973:119) for the contemporary Waswanipi Cree. Their preference is to hunt "on moose days, when there is a slight wind that covers low noises made by the hunter, when the temperature is cold so that the wet snow does not stick to their snowshoes and make walking difficult, but not too cold so that the snowshoes do not make excessive noise on the hardened snow and so the branches of trees and shrubs are not brittle and easily cracked."

Successful hunting depends a great deal on specific conditions permitting exploitation with minimum effort. Moose hunting, unreliable as it may be, is not a random business. Contemporary observations indicate that hunters "search the hills for signs" or look for the characteristic packed-snow paths used by the moose (Feit 1973:118-119), and moose behaviour is indeed predictable to a degree. If signs are wanting, the hunter will not waste his time and energy in futile pursuit unless conditions are so bad that even small game is difficult to catch. It would then be to his advantage to hunt for game that provides the most meat. "Scanning" for game is part of strategy that determines how energy is expended (Lee and Devore 1968:307-308): "when our people saw that there was no longer any game within 3 or 4 leagues of us, we broke camp" (JR VII:109). Under ideal conditions it has been observed that "after one and a half or two hours of steady walking on snowshoes a man will have completely exhausted a moose" (Feit 1973:119). By contrast, "it was not easy to approach these animals in the summer" (Jenness 1932:46) — the situation may then be similar to when conditions are bad in winter.

The fact that the Montagnais hunted when it was most productive, when the snow is deep and the conditions right, and fished when it required relatively little effort to fish in the summer (JR VI:309), indicates that the "principle of least effort" was an

important operative concept for them. Paine (1973:303) acknowledged that it was also operative among the Waswanipi, but suggested that the principle ceased to be of consequence when a certain "low" in yields was reached. While Paine was referring to continuous hunting which ultimately reduces the species population (ie: moose) to a level where hunting is no longer profitable in terms of energy expenditure, I would argue that the principle of least effort is invariably subject to environmental conditions. Paine assumed that abundant game as well as good weather and snow conditions were *initial* circumstances permitting continuous hunting, yet the *Relations* demonstrate that such a situation was rare. The hunter will probably exert as much energy in the hunt when there are many moose but a lack of snow as when there are few moose but excellent weather and snow conditions. In order to approach the principle of least effort as closely as possible, if natural conditions do not warrant it, adjustments are made by higher frequencies of moves, maintenance of populations at lower densities, and resource emphasis shift.

Correlation between minimum energy expenditure and the seasonal pursuit of certain fauna is even closer than the preceeding discussion would indicate. Moose are said to be in prime condition as to both venison and robe in the winter season (Cooper 1946: 286). Furthermore, moose meat has a comparatively higher fat content in late winter and early spring, as opposed to the summer season and the rutting season in early fall (Feit, pers. comm.). Feit noted that under ideal conditions, Waswanipi output may be as great as 20 moose for one man per work day. Thus, there is a definite association between maximum food value and minimum energy expenditure.

Few attempts have been made to correlate seasonal subsistence efforts with *seasonal* caloric values of particular resources. Lee's input-output analysis of the !Kung Bushmen (1969:73-94; 1968:30-48) only took into account seasonal conditions, and not the seasonal variations in caloric value of the resources. It was found that "during the lean season of the year, the availability of the staple mongongo nut reaches an annual low, and people have to walk farther and work harder in order to maintain an adequate diet. In other words, a higher energy input yields a relatively lower caloric output".

For the Montagnais, unlike the !Kung, reducing the seasonal input-output gap depended upon prevailing conditions, and not solely on the availability of resources. The !Kung must work harder in the dry season to maintain the same dietary intake as in the wet season. Conversely, the Montagnais, no matter how hard they might have worked during the winter, depended upon environmental conditions of the moment that determined either a high or low food intake. To illustrate the hardships of the Montagnais winter group, Table I has been compiled. It represents the recorded detail for the entire winter of 1633-34. The following analysis (Table II) clearly indicates the wide gap between input and output under adverse conditions, and the slim margin between starvation and satiety.

Le Jeune had quoted a Montagnais to the effect that "to live moderately well and without suffering they had to have a moose every two days" (JR VII:181). This statement was made in reference to the size of the group at that time, that is, twenty individuals. Although the age and sex of the moose is not specified, an average weight of 700 pounds is not unreasonable. Allowing a wasteage factor of 50% (Cleland 1966: Table 6; Feit — pers. comm.), we are left with 350 pounds of edible meat. Consumption can therefore be estimated at approximately 8 pounds per person a day. If an average seasonal value of 600 calories per pound is used (W. Kemp — pers. comm.), the per capita intake is roughly 4,800 calories daily. This figure may be justified if it is considered that the Montagnais were on a 100% meat diet, that "moose meat does not remain long in the stomach" (JR VII:181), and that "in a cold area it makes good sense to eat a lot of meat" (Lee and Devore 1968:93). This figure also compares favourably with Feit's estimate of 4,500 calories for the Waswanipi bush population (1973:121, and pers. comm.). Accordingly, this calculated estimate should be compared with the last column of Table II, and the average caloric intake for all days considered in the sample.

It must be realized however, that waste percentages depend not only on the age and sex of the animal, but on how hungry the consumers are. It is likely that more than 50% of a moose could have been consumed. Additional fauna may also be unreported, and the eating of bark, skins, etc., cannot be analyzed. Caloric

TABLE I: DETAILS ON RESOURCES CAUGHT AND EATEN DURING THE WINTER OF 1633-34

DATE	PEOPLE	RESOURCES	SOURCES
Oct. 18	19	barrel sea biscuits, sack of flour, bread wine corn	JR, VII: 71
Oct. 20	19	"some birds"	: 73
Oct. 21-28	19	"piece of biscuit"	: 81
Oct. 30-Nov. 12	35	"eels, flour, peas"	: 91, 97-99
Nov. 12-15	45	"provisions low"	: 107
Nov. 20-Dec. 6	45	"no more beavers and porcupines"	: 121
		"no food-banquet of smoke"	: 137
		"little frozen water"	: 139
Dec. 20	20	"hunger drove us on - no game or at least very little of it"	: 145,147
Dec. 24-26	20	"a porcupine, a hare"	: 145,157
		"four or more beaver"	: 159
		"tow porcupines"	: 159
Dec. 28-Jan. 16*	20	"moose, two beavers, second moose" (+ recovery of buried meat)	: 161 : 163 : 175,177
Jan. 29-Feb. 9	20	"we always had something to eat... and in so small quanties"	: 179,181
		"we scoured the plain"	: 183
Feb. 14,15	20	"two small moose and a larger one"	: 183
March 6	20	"we shifted our quarters a number of beaver lodges found"	: 185

(*deaths reported January 6)

TABLE II: ANALYSIS OF FOODS CONSUMED ACCORDING TO TABLE I

Compiled from: H.A. Feit - personal communication
W.B. Kemp - personal communication
Cleland 1966 Table 6

SAMPLE DATES FAUNA SAMPLES	DAYS	LIVE WEIGHT	TOTAL WEIGHT	PERCENT USEABLE	TOTAL POUNDS	POUNDS PER PERSON	CALORIES PER POUND	TOTAL CALORIES PER PERSON	CALORIES PER DAY
Dec. 24 - 27	4								
3 porcupines		15	45	70	30	1.5	650	975	244
1 hare		3	3	50	1.5	.07	675	50	12.5
5 beaver (?)		45	225	70	158	8	750	6000	1500
									1756.5
Dec. 28 - Jan. 16	20								
2 moose		700	1400	50	700	35	600	21000	1050
2 beaver		45	90	70	63	3	750	2250	111
									1161
Feb. 14 - Mar. 6	22								
3 moose		700	2100	50	1050	53	600	31800	1445
									1445
							Group membership:		20
							Average caloric intake per total days:		1454

values also vary according to the part of the animal eaten. Moreover, not all days were eating days — December 25th in our 4-day sample "was for us a day of fasting" (JR VII:145), and this not for religious reasons. The span from November 20th to December 23rd seems to have been the most critical time. The derived average intake of 1,454 calories may also be somewhat misleading — the "Eat-all Feast" that took place when the hunt was successful meant a higher intake for a day or so followed by a rapid decline. The Montagnais may thus have been able to approach their required intake, but only for a short time.

It is not unreasonable to assume that the daily caloric intake throughout the winter was never more than one-third of what was actually required. Champlain's (II:53-54) description is vivid enough — "On the twentieth of the month of February they came.. so thin and emaciated that they looked like skeletons, most of them being unable to stand". That the Montagnais suffered from malnutrition is uncertain, but that they were undernourished is beyond doubt.

AN EVALUATION OF THE MONTAGNAIS HUNTING ECONOMY

The discussion will now focus on an evaluation of the Montagnais hunting economy, both in terms of how "aboriginal" it was and the extent to which it was altered by trading incentives. Major sources for the ethnology of the area all derive from a period when the influence of the fur trade was already at least seventy years old (Hoffman 1961:202), and for the southern Montagnais, face-to-face contact began around 1550 AD, more than 200 years earlier than it did for Athabaskan groups of the farthest Northwest (Leacock and Lurie 1971:349).

Rappaport (1969:184) had suggested that ecological anthropology would do well to follow leads indicated in animal ecology. It is well known that most animal populations are in equilibrium with their resources. Similarly, by virtue of their positions within the trophic-dynamic structure, human populations are inevitably bound by the laws controlling the flow of energy, and their long term demands that they do not over-exploit their staple resources. In other words, there is a tendency towards equi-

librium guarding the survival or regenerative capacities of these resources.

This tendency towards equilibrium might well be expressed through the *seasonal* hunting of certain animals. Moose, for example, should be left alone in summer so that the young born in spring may help to maintain the number balance for the winter; a breach of this practice might jeopardize the supply. This is, in effect, natural conservation, which appears in this case not only to coincide with the principle of least effort, but with the maximum food value of the animal as well. Beaver meat, however, is of maximum food value the year-round (Feit — pers. comm.), so that beaver hunting in the winter coincides only with the principle of least effort. The beaver was the object upon which the trade focused, hence any statement regarding conservation will have to be carefully assessed. Consider the following: (1608) "At that time when their eels and other things which they dry are prepared, they go off beaver hunting and remain away until the beginning of January" (Champlain II:45); (1632) "The sixth of August.. one of them approached me and said.. that they were going hunting or fishing for beavers.. that they would return when the leaves fell from the trees" (JR V:59) "In the spring the tent group Le Jeune was with split up.. some members keeping to the highlands to hunt moose and the others following the stream beds where the beavers were to be found" (Leacock 1954:15); and (1634) finally, "Beaver was eaten in all seasons" (JR VI:305).

If the principle of least effort is employed as an analytic tool, Champlain's observation may be interpreted as the one illustrating the aboriginal custom. The termination of beaver hunting would coincide with the time when conditions were at their best for moose hunting, that is, in early January (Feit 1973:118), but beaver could of course be relied upon when other resources are scarce. The other references clearly indicate the influence of the fur trade. It is suggested that beaver was hunted in August in order to trade the pelts to obtain food-stuffs and equipment for the coming winter. The same explanation seems applicable to the spring hunt — supplies may have been needed to tie the bands over until the appearance of valuable schools of fish at the end of the summer. Since beavers give birth to their young in early spring, hunting at that time or shortly thereafter jeopardizes the supply.

Le Jeune (JR VIII:57) stated that "when the savages find a lodge of them (beavers), they kill all, great and small, male and female. There is a danger that they will exterminate the species in this region". Leacock (1954:3) claimed that "aboriginality of conservation" was denied when Le Jeune made this statement. On the contrary, this violation of the natural equilibrium may well have been a reaction to trade incentives. As the Indians themselves remarked, "The beaver does everything well, it makes kettles, hatchets, knives, bread, in short, it makes everything" (JR VI:297). If Montagnais survival depended upon the preservation and regenerative capacities of resources as pointed out earlier, accounts that contradict this indicate how distorted the aboriginal way of life had become.

To correlate Montagnais hunting dynamics further with the propositions of animal ecology, statements claiming that "conservation implies a drive toward settling down" must be rejected (Leacock 1954:3). Limited movement is *not* essential to a system for conserving game, for as Lee and Devore (1968:12) have pointed out, "groups do not ordinarily maintain exclusive rights to resources. Variations in food supply from region to region and from year to year create a fluid situation that can best be met by flexible organizations to allow people to move from one area to another". It would be a misconception to believe that, because of the fur trade, the Montagnais changed their tradition of family or group land holding to individual holdings (Patterson 1972:66). High frequencies of moves, among other things, could not possibly have fostered land holding of any kind.

Another reason why limited movement is not necessary to a system of conserving game is that while the hunter will move on when certain yields have reached a "low", it is unlikely that the hunter's "low" coincides with the "too low" density of the animals (Paine 1973:303). The hunter moves on primarily because of the principle of least effort as was seen. According to Paine, the hunter's yield will merely reduce the species population to somewhat below its ceiling level, and the population subsequently will enter a period of fast growth. In the face of starvation, however, the hunter may linger and any conservationist tendencies arising from moving on when the yields are low may break down. This of course depends on how plentiful the other resources are.

Concerning between-season shifting, Jenness (1932:46) has pointed out that "the seasonal character of the food supply... compelled the various bands to move from one hunting ground to another *as soon as the first began to slacken in its yield*" (italics mine).

Thus, conservation *was* aboriginal and is not merely a questionable inference as Leacock claimed (1954:3). Only later did it develop into individual or family territories and into "farming" or husbandry as Speck and Eiseley (1942:241) consider conservation to be. While Leacock (1954:2-4) rejected Speck's theory of the *aboriginality* of the hunting territory, her rejection was not well substantiated. Suggesting that small game was not of sufficient importance in the first place because Le Jeune's group all but starved on beaver until large game animals were brought down, Leacock failed to realize that the beaver was already depleted by the 1620's (Feit — pers. comm.). Husbandry was necessitated because of this. While Speck and Eiseley's theory was founded on the notion of pre-contact population pressure, Leacock maintained that if such pressure was not observed soon after the invasion of the whites, it is doubful if it was felt before contact. Pressure that might have existed after the arrival of the whites was attributed by Leacock to the Iroquois. A prevailing state of war severely restricted travel for hunting: "The fear that they have of their enemies prevents their going to the hunt so that their lives may be sustained" (JR XX:261). The situation appears to have been serious: "Other resolved.. to reach the woods to the south.. saying that they would sooner die by the fire of the Iroquois as by hunger.." (JR XXV:107). The white population of New France in 1627 was only 107 people, of whom 20 were permanent settlers (Trudel 1973:165); by 1640 this figure had risen to only 270 (Ibid: 257). Effects of population pressure were therefore minimal, and it was the French who attributed the scarcity of game to the Indians, "who, in the pursuit of them, have driven these animals from our settlements" (JR IX:165). There were some, however, who held a different opinion: "Game among river birds is abundant in season.. but it has been so greatly disturbed in the more inhabited localities, it is going farther and farther away" (JR IX:165). Over-kill, practiced by Indians and whites alike, also appears to have contributed much to the misery suffered by all, for in 1638 "they will go 2 or 3 hundred leagues (600-900 miles) into the

woods to find game.." (JR XV:183). The situation was somewhat alleviated in later decades, for there occurred a marked decrease of Indian pressure on resources as Indian populations were decimated. "Formerly, the hunting of them (moose) appeared to our Frenchmen an impossibility, and now it serves them as recreation" (1659; JR XLV:195).

The hunting territory may also have been encouraged to develop because the seasonal cross-over had become a much more critical time due in part to growing dependance upon extra-cultural "aid". Just as the Dogrib Indians of northwestern sub-arctic Canada "can go to the fort and throw themselves at the mercy of government welfare" when their system cannot support them (Lee 1969:78), so the Montagnais "came to our settlement (February 20) so thin and emaciated" (Champlain II:54). The French may have offered aid out of sheer benevolence, or as a means of converting the Indians, or simply keeping them alive to hunt for furs. Yet the situation was made clear when Le Jeune (JR VIII:75-77) was told that "You do not know these people here; all they do is for the belly, they do not care for you, but for your food". Dependance upon French aid may have discouraged extensive preservation of food although the documentary evidence suggests that preparations for the winter included the preservation of food. Le Jeune (JR VI:277, 313) remarked that moose meat, obtained in the winter, was eaten until the month of September, and that smoked eels, prepared in October, were eaten until January. Champlain (II:44) noted that the supply of eels lasted until February, when the snow was two and a half or three feet deep at the most. Only with the advent of the trade did the Montagnais "often fast inspite of themselves, as they did also in the Months of December and January" (JR LXIII:253,255).

These instances demonstrate a former subsistence strategy designed to bridge the so-called critical seasons (fall/spring) and to alleviate stress until circumstances permitted a fuller exploitation of the resources of the season. Champlain's reference to the depth of the snow above suggests that it may not always have been favourable; hence the value of preserved food. That the Indians returned to the settlement on February 20th indicates that circumstances were now beyond their control — their preserved supplies were meant for only a limited period. Thus, the presence

of the French may have encouraged the Indians to abandon preservation altogether. Table I indicates how much preserving was actually done — practically nothing. The fact that the group began to suffer seriously by the end of November can be contrasted with former times, when January and February were months that determined the outcome of the winter. All of this, moreover, contradicts what Leacock (1954:7) had maintained, that "they could not preserve, store, or transport food to any significant extent", and that "occasionally there was surplus meat to be dried and kept, but it merely filled in temporarily when hunting was poor, and could not be depended upon for any length of time".

CONCLUSIONS

In conclusion, one must consider how much aboriginal Montagnais hunting dynamics were influenced by the fur trade. It is suggested that a radical depletion of game was felt as early as the second decade of the 17th century. The moose seems to have been an exception, as reports indicate its abundance throughout the 17th century. Though the famine of 1633-34 came to an end towards the latter part of the winter when conditions warranted successful hunting, the abundance of moose in later decades can be attributed to the decimation of Indian populations and a concomitant reduction of pressure upon the resources by them.

The situation appears to have been most urgent during the early part of the winter, when such animals as the beaver would ordinarily have been sought. This depletion of game, together with the effects of adverse conditions, widened the gap between input and output even more. Since beaver is normally thought of as a stable resource, only its depletion can account for hardships encountered. While it may be argued that snow and weather conditions were less than desirable in the first place, such would not seriously affect the availability of the non-migratory beaver. It is also not possible to determine if depletion and bad conditions also coincided with a general fluctuation of the species population. Perhaps there would have been more fatalities had this been the case.

The struggle for survival was further complicated by the fact that little food was any longer preserved. Had the practice con-

tinued as in aboriginal times, and had there been no depletion of game, starvation may have been unheard of, though undernutrition would probably have prevailed. In consequence, lack of preservation, depletion of game, and unfavourable hunting conditions rendered the principle of least effort relatively inoperative, and with it any proclivity towards conservation. Hunting beaver in the spring may have endangered their regenerative capacities or reduced their numbers, as did hunting when it did not coincide with the principle of least effort used in the pursuit of other game. Thus, these developments completely altered the aboriginal way of life and eventually precipitated the hunting territory. This was one alternative which lessened the danger of complete extinction of the various animals, fur-bearing or otherwise, and perhaps saved the Indian from total cultural annihilation as well.

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Boreal Forest Subsistence and the Windigo: Fluctuation of Animal Populations

LEO G. WAISBERG
McMaster University

RÉSUMÉ

Les psychoses windigo ont été traitées récemment comme des phénomènes d'après contact qui se sont développés à mesure que l'abondance première de gros gibiers a été progressivement diminuée par la traite des fourrures. Cette hypothèse de simple causalité ne tient pas compte de la grande variation des écosystèmes de la forêt boréale. Durant la période précédant le contact aussi bien qu'après le contact, la base de la subsistance des Algonquins du Nord n'était pas constamment abondante ou stable. La fluctuation dans les populations animales a été assez importante pour amener des pénuries de nourriture bien avant le début du dix-neuvième siècle. Le potentiel énergétique de l'écosystème a été tellement sujet à variations dans le temps et dans l'espace que la faim a certainement été une menace durant la période des premiers contacts.

Ideally, mental culture is no longer seen as only superstructure, peripheral to the socio-economic core but connected to it by lines of causation. Practically, there is all too often a tendency for northern cultural ecologists to employ such an assumption, especially when man-environment relationships are seen as direct and one-to-one. One example is a recent analysis of the cause of the Windigo complex among boreal forest Algonkians.

Charles Bishop (1973) relates the increasing incidence of Windigo phenomena through time to an ecological change in the post-contact boreal forest. In this view, an ideological focus on the cannibal spirit is a result of a declining food supply. While

belief in the Windigo cannibal spirit was aboriginal, Windigo psychosis developed because of post-contact food shortages. Starvation occurred as game was depopulated by progressively greater concentrations of people around fur-trading posts. Accelerating frequencies of shortages, and finally starvation, led to greater anxieties, facilitating a heightened concern about famine cannibalism among the Ojibwa.

The corollary to this hypothesis is what is most disturbing. There were no Windigo psychoses before the advent of the fur trade because there was no starvation, a view for which evidence on the great magnitude of kills is presented (1973:13). The threat of starvation was non-existent in pre-contact times. According to Bishop, some Algonkian group did not suffer severe food shortages until the collapse of big game in the early nineteenth century; elsewhere Bishop has documented the breakdown at Osnaburgh House (1974). During pre-contact and well into post-contact times, big game and the Ojibwa existed in equilibrium. The subsistence base of boreal forest Indians is seen by Bishop as both stable and rich until disrupted by outside influences. The ultimate determinant of Windigo psychosis was the dwindling game supply caused by the fur trade. Windigo psychosis among Algonkians was extremely uncommon before European disruption because game was uniformly abundant across the boreal forest (Bishop 1973:13). Such an interpretation of northern game populations appears oversimplified.

Bishop's approach is a form of Stewardian cultural ecology. Because Steward's concept of environment included only its physical and natural aspects, it usually appears in his work as a stable entity. Change comes from outside of the environment, either a creative shift in exploitative technology or a chance extraneous factor, such as European contact. The early contact Ojibwa, indeed any boreal forest Amerindian, have been viewed as possessing a stable adaptation to a rich and static environment. Such a position stands directly contrary to our knowledge of the ecology of northern animal populations. Both large and small game experience oscillations in numbers, frequently of great magnitude, over long and short periods of time. There is great variation in a boreal forest and taiga system. A subsistence base, the productivity of game animals, fluctuates in supply.

The core of Bishop's analysis of Windigo psychosis is the notion of environmental stability before effective European contact. I believe that this idea, based upon one interpretation of seventeenth and eighteenth century historical literature, has not been examined ecologically, Bishop's ecogrammar notwithstanding. While such sources may indicate that the environment was rich, they do not show that it was stable before the increase in the numbers of people utilizing it. In light of Bishop's approach, we must ask ourselves if the relevant carrying capacity, the limit at which the environment can support the human population of hunters, is a constant value, to which Indians adjusted, or if instead the carrying capacity is subject to changes. Bishop makes the assumption that it was not subject to fluctuations before European penetration into an area. Hunters and moose or caribou had a "good" predator-prey relationship, with the Indians living off the interest or the expendable capital. The base of subsistence had not been disrupted; it was stable before the fur trade (Bishop 1973:5). In such a view, a system of checks and balances is seen as operating among the populations. There is little relevant fluctuation of food supply because the traditional societies were well below the carrying capacity of their ecosystem. Bishop does not note the reasons why Indian populations had stabilized at this level. A notion of rich stability should have immediately elicited protest; it does no correspond with the oft-noted pattern among Algonkians and Athapaskans of "feast or famine" (Feit 1969; Knight 1965:32).

Is a subsistence base composed of game populations stable? The specific data needed to resolve such an issue lies only partly in the anthropological literature. Feit, Knight, Helm (1961), and Slobodin (1962) have shown an awareness of animal fluctuations. However, their work, while sophisticated in its approach, does not contain primary information. By far the bulk of the necessary material lies in works on wildlife ecology. While organizing my thinking on this matter, I searched for data on populations of arctic and sub-arctic animals. Population was defined as an aggregation of similar individuals in a continuous area which contains no breeding barriers. Research was oriented around these questions. Do populations of game animals vary from region to region and over time? If they do, was this a pattern of extreme fluctuation?

Lastly, is contemporary information relevant to the early contact period?

Specifically, data of various quality were found on the demographic cycles of certain sub-arctic and tundra animals: mouse, vole, rabbit, grouse, ptarmigan, fox, lynx, muskrat, mink, fish, beaver, elk, deer, moose, and caribou. Population fluctuations appear to exist for all of these categories of animals. These occur at both regular and irregular intervals. Successive peaks in numbers are often of different magnitude. Effects were felt more so in some areas of the continent than others. Some variations in number were random, some periodic, some oscillatory. Population cycles of many animals were related directly, a situation most clearly seen in predator-prey inter-actions. There are in the north four and ten year cycles that have been broadly delimited. In addition, successional changes in the boreal forest directly modify game populations. Certain species exist at optimum numbers in different vegetational stages, a statement that is especially true of deer, elk, moose, caribou, and grouse. There is also the possibility of fish fluctuation.

A cycle implies recurring variations of regular timing and of constant amplitude (Kendeigh 1961:238). Adhering to this definition, most "cycles" are actually oscillations, where both amplitude and timing are inconstant (ibid.). While irregular, most population cycles (to use the most common, though technically incorrect, term) are not entirely random. Two cycles have been delineated; one averages 3-4 years, the other 9-10. Both predators and prey are involved. Each oscillation follows a definite pattern. The basic cycle is that of the herbivores, with predator populations responding to an increased food supply by expanding their own numbers (Kendeigh 1961:241).

The four year cycles involve lemmings and voles, microtine rodents, of the tundra and taiga (Lack 1954:204), as well as hazel grouse (Kendeigh 1961:237). Voles and lemmings have distinct cycles which are not always or everywhere in phase with each other. Their predators oscillate in a corresponding manner, some out of phase with the prey by a year. Peak numbers of the predators follow shortly after that of their prey (Lack 1954:210). Since the preferred food supply of the same predator species vary from region to region, often a single species will follow different

cycles over its range. Marten and red fox have a four year cycle in northern Labrador and Hudson Strait, where they prey on microtine rodents, but in more southerly climes where they prey on the varying hare, they fluctuate in number every ten years.

The important oscillation for our purposes is the cycle of 9-10 years, which is most noticeable in certain boreal forest species. The varying hare (snowshoe rabbit), the ruffed and sharp-tailed grouse, and the willow ptarmigan oscillate in numbers, followed shortly by their predators (Kendeigh 1961:237). Using data obtained from Hudson's Bay Co. fur returns, Butler's study of eight sub-arctic species over thirty years showed that the snowshoe rabbit peaks were followed by those of lynx, arctic fox, fisher, coyote, and wolf. Predator population increases began "about the middle of the snowshoe rabbit peak and continued some years longer" (MacFayden 1969:254).

Ptarmigan, and especially grouse, appear to fluctuate with the varying hare (Kendeigh 1961:238). In any one region, the collapse of all three populations will show a definite pattern. The most common is for the grouse decline to begin a year after the hare decline. Grouse and ptarmigan are the common alternate prey for predators which usually feed on varying hare. When rodents decrease, the expanded predator population rapidly decimates the numbers of grouse (Lack 1954:220-221). From our standpoint, which is concerned with the availability of food to boreal forest Amerindians, the indication that grouse and hare populations oscillate together is a significant one. Convergent declines are especially menacing. Small game, particularly hare, have been noted as very important in the absence of moose or caribou (Bishop 1974:265). A simultaneous collapse of small and large game populations during the winter when fish were relatively unavailable, would have dire consequences.

All of these four and ten year cycles show rather marked regional variations. As well, the 3-4 year oscillations are not apparently synchronous with the 9-10 year ones (Lack 1954:205). Generally, cycles are more distinct in the north, a phenomenon which is most noticeable for individual species. Lack documents the wide agreement that peaks are much larger in the north than the south, when one is considering a single species in its continental range (Lack 1954:212). According to Kendeigh (1961:237):

> South of latitude 45°—50° North in North America and 60° North in Europe, variations in population size appear progressively less extreme and cyclic, more irregular or random in character.

This statement is generally true, which means that it applies on the average and in the long run. Local and regional variations are considerable. As Lack emphasized (1954:212), "peaks are not synchronous everywhere throughout the range". Animal catches frequently show a cycle when considered across a wide area, but fluctuate erratically at individual collecting posts (Lack 1954:211).

Attempts to build mechanistically symetrical models of population cycling have proven difficult. The finite failure rate potential, Finagle's Second Law, intervenes. Local and regional areas are frequently out of phase with the main population peak across Canada. Maritime grouse were three years ahead of the general Canadian peak, while Alaskan grouse were three years behind (Kendeigh 1961:238). Some local peaks are occasionally out of phase by five years, which is the largest discrepancy one would expect in a ten year cycle.

To make matters even less regular, regional peaks are not out of phase consistently. Such variations in tempo do not increase with time; different areas tend to come back into phase by the time of the next peak (Lack 1954:211, Kendeigh 1961:238). Considering game animal populations in any one area, fluctuations occur in amplitude of population peak and decline, in phasing, and in synchrony with other game. When we add to this the chance of purely random accident, such as a herd of pronghorn antelope crashing through the winter ice to their deaths (Dasmann 1964), a considerable amount of instability must be added to the anthropological conception of northern ecosystems. Such variation argues against notions of environmental stability. Clearly, small game and predators are not in static equilibrium.

Some fish species oscillate in number. Sockeye salmon of the Pacific coast appear to have a 3-4 year cycle, while Atlantic salmon have a 9-10 year one (Kendeigh 1961:237). While this is to my knowledge the only 'hard' data provided in the ecological literature, anthropologists have occasionally commented upon the possibility that North American lake fish cycle, or at least vary in supply. June Helm, for example, refers to Slavey fish resources as "subject

to seasonal and yearly fluctuations" (1961:32), much the same as these people's access to barren-ground caribou in the winter. When both failed simultaneously, people died from lack of food. Helm quotes Frank Russel (ibid.):

> The Slaveys, who formerly killed large numbers of caribou between the MacKenzie and the Rae are now compelled to live principally upon fish, and when the fish fail, as they did during the winter (1893) that I spent in the country, they were reduced to actual starvation.

Specific information on the various fish species appears to be limited, generally, to references to the yearly abundance at spawning time, and does not usually consider the differences between the catches of particular years. Helm, in her treatment of the annual catch during the autumn, is an exception.

Cycles similar to those of rabbits or voles, except in duration, have been occasionally mentioned for big game. Ecological evidence is much less definite. The cycles, if they do exist, are of much longer duration than those previously discussed. One sometimes hears mention or 35-70 year cycles for caribou or moose. Direct data are as yet sparse as rabbits in a bad year. Oscillations in big game populations are, needless to say, extremely important to northern Indians.

It is generally conceeded that the availability of plant food interacts with rodent herbivores to produce oscillations in numbers (Lack 1954:213, Kendeigh 1961:241); predation and disease are seen as secondary factors, although this is by no means unanimous. A similar factor is sometimes cited as operating on the moose, an observation made on Isle Royale in Lake Superior. Population increased up to the food limit; thereafter quantity and quality of the vegetation declined. Lack notes (1954:172):

> in 1912 or 1913 a few moose crossed over the ice on Lake Superior to Isle Royale, which is about 45 miles long and 8 to 9 miles wide. By 1930 their numbers had risen to between 1,000 and 3,000 and there were signs of serious overbrowsing. There followed a big decrease, to only 200 by 1935. The numbers then rose again to 500 in 1945 and to 800 in 1948, but they then feel once more, to about 500 in 1950... There have, therefore, been two increases and two declines in numbers, the peaks being about 18 years apart. The declines were accompanied by extensive damage to the vegetation and by starvation of the moose, and it is reasonable to suppose that the moose and their vegetable food have interacted in predator-prey oscillations.

Such irruptions, however, are marginal phenomena, a result of herbivore immigration or humanity's misplaced protection. As in the case of the deer of the Kaibab Plateau, cyclical fluctuations of this sort appear in the absence, or relative absence, of natural predators, such as the wolf, coyote, lion, or man. Leopold has suggested that predators formerly kept large herbivores below the food limit, but evidence is somewhat unclear (Lack 1954:171). However, it is noteworthy that, since wolves were introduced to Isle Royale in 1949, the moose population has stabilized and the browse recovered (Mech 1970:274). Estimates of annual calf production, 225, are remarkably close to annual kill, 140 calves and 83 adults (Mech 1970:273). There is also evidence that wolves control the deer population in Algonquin Park in central Ontario (Mech 1970:274).

Regular moose and deer cycles, when they occur, seem to follow either immigration or protection, phenomena less than common in the long run of contact history. Such oscillations do not correspond with the seemingly stable pattern presented by the Isle Royale study. However, while there appears to be no regular cycles, there is still considerable fluctuation of these animals in the long run. Stability is a relative event. Mech's information was gathered from the period 1959-1966, while his production/kill estimates were taken in 1966. In contrast, vegetational cycles on the order of decades and centuries are also affecting regional game populations.

Different forms of wildlife are related to various stages in the biotic succession of boreal forests (Dasmann 1964:81, Feit 1969): grass, sedge, herbs → brush → deciduous forest → coniferous forest → coniferous climax. For any species living in an area undergoing succession, the amount of preferred food that can be taken is gradually reduced. Different animal populations will eventually die or migrate out of their area as their food resource base is transmuted into a different one through vegetational succession. As deciduous mid-succession merges inexorably with coniferous late succession, most deer, for example, will find their shelter and their preferred food diminishing. Optimum numbers of animals for each species are usually associated with a stage in the vegetational cycle. Stage refers to the frequency of tree species in any given region. Various areas are at different stages of suc-

cession, since "regeneration is conditioned by a large number of variables and is highly uneven" (Feit 1969:113). Stream margins, to cite a case, are usually occupied by alder and willow (Feit 1969:105); periodic spring flooding prevents the growth of larger species or the attainment of climax coniferous. Beyond this, local soil conditions may also affect the attainment of certain stages. Thus, any large region of northern forest will be heterogenous, a noticeably patched quilt of local areas in somewhat different stages of succession, many of them steadily approaching a coniferous climax of black spruce and balsam fir. Among the patches will be water margins and areas already burned out and beginning the cycle once again. A forest fire, or more recently lumbering, over any size area will begin the stages once more.

Small mammals, such as the varying hare, appear in greatest numbers in early successional stages, least frequently in coniferous stages (Feit 1969:112). Moose, elk, and especially white-tailed and mule deer are mid-succession animals (Dasmann 1964:85), depending on leafy browse; ruffed grouse also prefer this stage. Woodland caribou inhabit the climax coniferous forest. Barren ground caribou rely for winter range on a mixture of climax lichens that are intermixed with undisturbed taiga, the 'parkland' between the full boreal forest and the tundra (Dasmann 1964:81).

Mid-succession animals, such as elk, moose, various species of grouse and deer, cannot be permanently maintained in any one area unless that area's vegetation is frequently disturbed (Dasmann 1964:85), which it was not in aboriginal or early contact times. Territorial studies of deer species indicate that most animals of a population will orient themselves to individual ranges and will not leave the area even if they are dying of malnutrition and adequate food is outside (Dasmann 1964:118). Similarly, starving moose are not noted as fleeing Isle Royale. Speaking of elk, moose, white-tailed and mule deer, Dasmann states (1964:85):

> Since usually they will not abandon a home area and move elsewhere, their populations tend to be impermanent. Only a few individuals will move out and colonize newly created habitats. Most will remain on the declining habitat and in time die out... As such, when they are to be used for hunting, liberal regulations governing the take are advisable. There is no point in trying to maintain a permanently high population in a temporary habitat.

Any one territory does not have a stable productivity over time. As the home range advances steadily towards climax, food conditions become less satisfactory through time. Such a situation helps to explain the tendency of unacculturated Indians both to move about and to kill all the food animals they could take, including pregnant ones. After the local area was 'cropped out', it would be left fallow until immigration restocked it. Clearly the pattern Helm found at 'Lynx Point', where newly sedentary Indians still tried to hunt in this fashion but found their yields decreasing, should be instructive.

Moose, however, are in a peculiar situation as there are various disagreements in the ecological literature concerning its mobility and its feeding habits. In studying this, Feit noted that, with the exception of Peterson, all of his sources agreed that moose moved back into an area early in the brush stage and "declined significantly" as the vegetation began to approach coniferous stages (1969:107-108). Summer browse is taken at water margins and in other areas supporting low to mid-succession vegetation. Feit noted that nearly all of the studies were conducted in the northwestern sections of the continent. Beyond this, since they were based on 'pellet' counts, they were probably conducted in the summer. However, Peterson, who seems to have conducted most of his work in the east, believes that moose browse in winter on balsam fir, a tree characteristic of coniferous stages, and not found in significant numbers on burned-over areas (1955, in Feit 1969). Which stage of a burned-over area is unclear, however, since coniferous climax itself is but a stage of a burned-over area. Possibly there are differences between moose populations in various parts of North America, as suggested by Peterson and Feit. If we accept the model, winter → climax/spring, summer → low, mid-succession, then our condition of unreliability of food supply in any one local area is still met, however the ecological controversy is finally resolved. Whether moose are migrating seasonally or after decades or both, or slowly dwindling in any one area or region, the amount of moose that can be captured in one band's territory is not a constant. Conditions and take will vary, seasonally and over a generation.

Woodland caribou undoubtedly inhabit the mature coniferous forests (Feit 1969:109). Beaver migrate into an area during early

stages of succession and remain there until the end of the climax period. Its numbers do not oscillate, but there is a population crash in areas which have suffered recent burns (Ray 1974:120). Because of forest fires which destroy the coniferous vegetation and begin the cycle again, the climax species in any one area are eventually decimated by fire and lack of food. Over a long span of time (Feit estimates about two centuries), the late succession or climax communities in a region are themselves impermanent.

Overall, the amount of variability in most animal populations is significant. If, as Feit argues is suggested by the early contact literature, forest fires were large enough to significantly alter the vegetation of even the most extensive areas, then the various animal populations will be suddenly collapsing. Even if this is not the case, because of the inexorable successional changes, species will be constantly shifting in areal concentration. In any one locale, low and mid-succession species will first proliferate, and then decline as their food resources become scarce. Annual production of certain game will be at first high but will taper off. As those animals that remain are killed, fewer new ones will migrate into the area since it is no longer, for that species, of optimum quality. Some individuals may emigrate, further affecting the local decline in game. It should be remembered that these areas are as variable in size as the forest fires which originally created them; they range from extremely small to very large (Feit 1969:83).

Declines similar to those that affect the mid-succession species, reduced production per area, will also make late climax areas unproductive. The chief difference is that the change from climax, effected by wildfire, will be sudden and catastrophic in its effect on game. With some exceptions, this matter of the role of forest fire in boreal forest ecosystems was competently described by Feit. Migrating barren-ground caribou will shift to a different winter range. while woodland caribou, beaver, and moose will attempt to migrate to a different area. Significant numbers of these animals will starve.

Another factor which adds great complexity and variability to our picture is weather, short term climatic fluctuations of snowfall, temperature, and rain. Sudden, brief thaws that convert the upper layers of snow to hard ice can deprive large game of food

to such an extent that the animals die; such a situation has affected a herd of musk oxen (C.J. Wheeler: per. comm.) as well as barren ground caribou (J.G.E. Smith: per. comm.). Warm winters of little snowfall hamper hunting efforts. With little snow, moose have greater mobility and can more effectively evade pursuit. Barren ground caribou remain longer on the tundra before seeking their winter pasture in the taiga. Excessive rain during spring runoff can flood the burrows of muskrat and drown the young. Conversely, low water levels in the autumn, followed by cold weather, hamper insulation of burrows by mud (Kormondy 1969:91).

Let us picture, then, a population of Amerindians utilizing a region which includes areas of late climax, late middle succession, and low succession water margins. A wildfire, sufficiently strong to leap streams and ponds, burns out a significant part of the late climax forest as well as the alder and willow at water margins. The food supply for both beaver and woodland caribou is destroyed, as is the winter range of moose. Meanwhile, deer yields in the late mid-succession forest areas will have been dropping for several years. This leaves the alternate game: grouse, hare, and fish, all animals whose numbers fluctuate. Grouse and hare reach nadir almost simultaneously. If these lows correspond with a winter absence of larger game, starvation will result, *if the Indians remain in the area.* This is the pattern that affected the Slave in historic times emphases.

Conditions are serious even if we do not postulate a large burn. A relatively small burn outside the band's territory may be sufficient enough to deflect migration routes of wintering barren ground caribou. For any variety of reasons, other animals may not yet have migrated into the region and built up the populations. If even one large species of game fails in the winter when fish are difficult, if not impossible, to obtain, and if alternate prey, hare and grouse populations, have recently collapsed, then I do not find it difficult to conceive of starvation as a serious threat, aboriginally and in early contact times.

Dispersing farther in the food quest, perhaps to relatives in neighbouring bands, may only temporarily alleviate the situation; their condition may be no better than those primarily affected. Even if it is, extra mouths may decrease their own supply. Citing Os-

naburgh material, Bishop documents that hunting groups of large size were sometimes starving in the late eighteenth century (1974:265).

There has been no evidence to prove that such variations, oscillations, successional changes, large fires, and random accident did not operate before European social environments, such as the fur trade, intruded significantly into a region. Simple magnitude of supply in any one year does not provide evidence for continuation of supply at similar levels. This is best illustrated by reference to the situation on Manitoulin Island in the early 1670s, during and after the winter when Nicholas Perrot reported that an Amikwa band had killed 2400 moose there (Bishop 1973:13; Blair 1911:221).

Such a large kill certainly shows that the region was rich in game resources. The *Jesuit Relations* provide interesting additions to this statement of Perrot's. Father André, travelling and preaching across the northern Georgian Bay region from September 1670 to late spring 1671, wrote of severe famine and scarcity of meat among the Ottawa and the Nipissing. Several nations of Ottawa, who had just left Chequamegon, were also living on Manitoulin Island during this winter of 1670-71, a particularly difficult one for them. The hardship of that late fall and early winter, the necessary utilization of a wide variety of plant food, the boiling of old moose skins, the eventual scattering of the Ottawa out of this area to search for game, all noted by Father André, must leave us to wonder at the variation in this rich regional environment. While the Amikwa may have killed 2400 moose on the island that winter, the words of André's journal leave no doubt that game was insufficient to support the Manitoulin Ottawa. The "Outaouacs" were "starving", having "poor success in hunting and fishing". They were subsisting primarily upon roots, acorns, rock tripe, old skins, and very little meat (JR 55:143). Such an account, especially the boiling of skins, is something much more than an inexperienced Jesuit grumbling that Indians do not eat three meals a day like Europeans. Two months spent with the Ottawa exhausted André. Finally, "the continuance and increasing severity of the famine dispersing all the people and closing this mission", André concluded his report next spring with these words (JR 55:155):

> Missionaries to this country of the Outaouacs must know with Saint Paul what it is to experience scarcity much oftener than plenty.

This was in an area which Bishop cites as little affected by European contact. With little game and few fish resources to fall back on, and little nourishing vegetable foods, Ottawa starved. Comparison of Ottawa population estimates over the decade 1665-1675, one based at Chequamegon (JR 50:301) and the later one at Mackinac (JR 1:34), shows a drop in numbers from 2000 to 1300. While war and disease certainly played roles in this, André's report indicates that starvation cannot be dismissed.

From the reports of Jesuits in the northern Great Lakes area during the early contact era, it appears that band territories were not rigidly delineated, especially when neighbouring or allied bands were seeking food or trade. The autumn whitefish runs at Chequamegon served as a focal point for Ottawa and French trade during the late 1660s; hundreds of warriors from many different nations gathered there (JR 50:273-279; JR 54:151). Perrot narrates that some, but not all, Amikwa, Saulteaux, Nipissing, and Ottawa were gathering at the Sault to take advantage of whitefish runs (Blair 1911:179). In light of this as yet limited evidence, it would perhaps be more factual to speak of band ranges rather than sharply defined perceptions of territory that is defended against incursion by 'outsiders'. The exact position of the various Ojibwa nations varied from year to year. This movement might be a response to native estimates as to where an adequate supply of game might be found, taking into consideration which areas had suffered fires over the summer or which had been previously hunted out in recent years. That such estimates as to the best place to winter were occasionally incorrect is one of the vagaries of existence in the northern forests.

Periodic cycles and larger variations in faunal distribution promote wide kin affiliations, an absence of rigidly fixed rules of social organization, and "swidden" hunting. Conservation, the preservation of a proportion of game to ensure local supply, was not generally practised in former times; for example, the Montagnais-Naskapi (Leacock 1954). Slave Indians formerly took all the available moose, even feeding it to their dogs (Helm 1961:32); pregnant cows were not spared. The total moose population of Manitoulin Island appears to have been captured in one winter (J.G.E. Smith: per. comm.). Cree hunters of Rupert House told

Knight "when they trapped, they tried to clean an area out" (1965:32). When this was done, the area was usually left 'fallow' until either immigration restocked it or different animal populations moved in. With such shifting areas of utilization, and with all the variations in game density from place to place, it should seem difficult to hypothesize a lack of starvation, or uniform abundance, before and just after effective contact. That the Ottawa or Nipissing made in the early 1670s a poor ecological judgement as to game supply in their region indicates the characteristically human inability to ever be perfectly in synchrony with an environment. A notion of stability in boreal forest ecosystems is Hiawathan romance.

The variation in animal communities appears considerable. Statements that aboriginal populations utilize between 20% — 50% of the environmental carrying capacity are meaningless in the north, because such calculations are not specific to area, nor are they constant. If we further define the carrying capacity as the accessible food/area, it is always fluctuating through time, circumstance, and region, governed along each parameter by random variations (accident, runoff, temperature, snowfall) and those less random (population and successional cycles).

Human populations, however, do not fluctuate in such a marked manner. If, as Solomon believes, they tend to increase in numbers up to the capacity of the environment, such as "the limit of supply of some essential resource such as food" (1969:53), and this resource frequently varies in supply, then food shortages become inevitable. Such shortages were occuring before the 1820s, when Bishop places the collapse of big game across the boreal forest (1974; 1973). There were Ojibwa suffering starvation in the 1670s, long before Bishop postulates his collapse of big game. Similar inferences can be drawn from the journal of Alexander Henry in the 1760s, who reported that one of several groups of the dispersed Ojibwa he was familiar with had starved over a winter (Henry 1901).

CONCLUSION

The subsistence base, the productivity of game animals, was not uniformly abundant or stable. Variation enters the boreal forest

system in any number of ways: animal cycles, vegetational changes, climax-destroying fires, high and low water levels, variations in temperature and snowfall, immigration and irruption, accidents, hunting out, lack of immigration, and so on. There is little evidence to indicate that this shifting nature has developed solely through European intrusion. While Windigo psychosis may be a post-intrusive phenomenon, as Bishop asserts, starvation was not. A hypothesis of a post-contact Windigo should not rely upon such a theoretical base. A simple causal relationship between mental culture and the socio-economic core is invalid because the 'core' is not the simple stable system it is often pictured to be. Ecological conditions were not static across the full boreal forest or its southern and northern margins. Regions differed, as did local areas, in game density at any particular point in time. Hunting behaviour of Algonkian societies was adjusted to fluctuating conditions before the white man arrived, and after.

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Colonial Transfer: Abandonment or Disguised Domination?

A Canadian Indian Reserve Case

G. E. MORTIMORE
University of Guelph

RÉSUMÉ

Cet article étudie une séquence d'événements sur une réserve indienne du Canada où le transfert de pouvoir et de ressources à la bande par le gouvernement a conduit de fait à une perte de ressources et à la fragmentation du groupe indien. L'action de processus internes de règlements de conflits et d'adoption de certains buts ont préservé la bande de fragmentation et de pertes continues. L'auteur recommande une formule pour le transfert du pouvoir. En l'absence de techniques effectives pour trouver ce que les gens veulent et pour les aider à l'obtenir, le transfert du pouvoir à des groupes dominés restera nominal et peut très bien être regardé comme un abandon ou une domination voilée.

The transfer of power to colonial territories and submerged ethnic groups has been the announced goal of several governments since World War 2. However, there is a gap between goal and performance. I propose to analyze the case of a Canadian Ojibwa Indian band in which some of the policies of governments, both federal and provincial, created fragmentation, internal conflict and loss of resources. These effects ran directly counter to the increased economic and political power which governments were outwardly trying to convey. In fact, some government policies did achieve partial success, but negative and conflict-creating policies came close to cancelling out all that had been achieved.

In the case of Eagle band, only the working of internal conflict-resolving processes has saved the band from continued fragmentation and loss. Eagle band is a fictitious label for an

Ojibwa community where I did field work for two years in several intermittent periods of residence between 1969 and 1974. I use the assumed name to protect confidences. Attention is focussed on Eagle band in the hope that it will provide a microcosm of colonial transfer and a set of principles that can be tested elsewhere.

Colonial transfer, as the term is used here, means that the dominant government hands over a substantial measure of decision-making authority, power and control of resources to the subordinate group. The subordinate group may be an internal colony, an internal interest group or an overseas territory. I include Canadian Indian reserves among internal colonies. Canadian Indians are a legally defined ethnic population category. Indian bands are units of this legally defined ethnic population: units which have been administered by a special branch of the federal government. Bands usually, but not always, hold rights to tracts of Crown land reserved for them. Their reserves and their status may be viewed as the consequences of the bargains which representatives of the British Crown and the Canadian government made with the ancestors of the present-day Indians (Hawthorn, 1966:396). These bargains involved the ceding of hundreds of thousands of square miles in return for relatively small tracts of reserve lands, continuing cash payments (typically, $5 per head per year in perpetuity) and other rights and payments.

The pattern of European-Amerindian transactions in North America can be divided into three policy phases: 1. Courtship and conciliation. 2. Domination, control, custody and forced or managed acculturation. 3. The image of transfer of power; the image of development and of increased economic and political self-determination for Indian groups. The three policy phases were not sharply separated. They did not march across the continent in neatly distinguished sequence. They were *tendencies* in attitude and policy. The title of each phase does broadly describe what happened; but the characteristics and timing of each phase vary a great deal according to regional and local circumstances. In addition, the practices of an earlier phase do not abruptly cease when the tide of a new policy begins to flow; the old policy continues as a slower and weaker undercurrent. Governments went on soothing Indians with gifts long after the military pressure

for conciliation had gone; even in the 1970's they seek Indian support. Domination and control continues to be exercised by government agencies in some situations where the avowed aim is to stimulate self-rule. The pressures of forced acculturation are still applied, though in subtler forms.

This paper deals with Phases Two and Three. Phase Two is discussed mainly for the light it sheds on Phase Three.[1] In subsequent sections I will give a condensed history of Eagle band, analyze the processes that led to the cohesiveness and relative independence of the band, and summarize the effects of the policies of governments during the period of colonial transfer. Assuming that the aim is to transfer some degree of authority, power and control of resources to the subordinate group, I will catalogue the actions that seem most likely to make the process of colonial transfer effective. Finally I will offer some general remarks about three contrasting views, three alternative action frameworks for colonial transfer and community development.

HISTORY OF EAGLE BAND

Seven economic choices were open to Ojibwa and other tribes of the eastern and central Mid-North of the continent during Policy Phase One and the early part of Phase Two: 1. Subsistence hunting and gathering. 2. Subsistence horticulture. 3. Barter trade. 4. Intermittent wage-labor, mainly in the fur trade. 5. Market hunting and gathering — fur-trapping and the sale of wild meat. 6. Market horticulture. 7. Market trade: the activity of middlemen or merchant entrepreneurs.

Eagle band had followed the first three options in pre-contact times. After the European invasion, members of the band followed all seven choices; but their central activity was that of merchant entrepreneurs. They were a small trading band. Under the guid-

[1] The three policy phases can be clearly documented from primary and secondary sources. Scott explicitly drew attention to the transition from Phase One to Phase Two (in Shortt and Doughty, 1914:4:695, 724). Various letters from government officials: i.e., Colborne, 1820) forecast and advise the policy change. Hawthorn and associates, as expert policy advisors, recommended the change to Phase Three (1958:495-492; 1966:13-20). However, even before the first Hawthorn report of 1958, there had been scattered efforts by change-oriented officials and native leaders to convey more power to Indian bands.

ance of a strong and shrewd chief, they maintained a canoe trading network that spanned hundreds of miles.

About 120 years ago, Eagle band and a number of other bands signed a treaty with the British Crown, by which Eagle band received 40,000 acres of land astride what had been the fur-trading waterway. Much of the land was heavily timbered. After treaty, and even into the age of roads, railways and settlement, Chief Eagle and his band continued to operate a trading network — not from their own reserve, but from a base granted them by Mallard band on the opposite shore of Lake Mallard.

The government's aim at that time was to maintain a policy of control, custody and managed acculturation at minimum expense. In the interests of thrift, heavy pressure was placed on Indian bands to sell land and timber so that the money would reduce the cost of Indian administration.[2] Mallard band succumbed to the pressure. Eagle band successfully resisted until a time of thicker settlement and higher prices; and even then it sold only its timber. It has never sold or leased land. The political-economic histories of the two bands have been much different. In 1868 Mallard band surrendered its timber for sale at 32 cents a sawlog, a price which in retrospect seems meagre. In 1904 and 1907 it surrendered for sale nearly 75,000 of its 100,000 acres. The land surrenders are remembered with bitterness. Disagreements over the land sales led to animosities that remain alive today.

Eagle band members lived by trading, subsistence hunting and subsistence horticulture, while leaving their timber untouched during the period of low prices and sparse settlement. When the building of roads and railways made waterbourne trading unprofitable, Chief Eagle continued his enterprise until he had to admit that it was a losing venture. Members of the band moved from their Mallard post to their isolated reserve across the lake and down the river. The band tided itself over lean times by subsistence farming, hunting and gathering, until 1910, when the

[2] Letters from Indian superintendents contained in Public Archives of Canada, Record Group 10, and annual reports of agents and superintendents-general of Indian Affairs, contained in the sessional papers of the Parliament of Canada, clearly illustrate this pressure and these goals. Some government officials were paid partly on commission from the sales of Indian lands and timber, as an incentive to stimulate sales.

band at last agreed to sell one of the several varieties of timber that grew on the reserve — the white pine. When all returns were in, the white pine alone fetched a price of $1,100,000. The band at that time had 80 members. By 1972 there were 232.

The timber sale marked the beginning of a new period in the life of Eagle band. For the first 60 years after treaty, band members lived by hunting, trapping, horticulture and commerce, and kept their assets of land, timber and strategic waterfrontage in holiday country intact. For the second 60 years the band has been converting its assets into cash, short-term consumer goods, long-term consumer goods such as houses, public service facilities such as roads, band community hall and street lights; public service positions such as that of a salaried chief; publicly owned business enterprises such as tourist cabins and a marina; and privately-owned capital plant and equipment such as horses, bulldozers and trucks for logging and hauling, privately owned tourist cabins, taxidermist's shop and general store.

There were two stages in the conversion of Eagle band's assets into cash and capital goods and income. In the first stage of logging, 1910 to about 1945, Eagle band people merely sold their timber to outside operators, but got no share in the profits from cutting, processing and marketing the wood, except as poorly-paid cutters — and much of the cutting was done by an imported labor force. In the second phase, Eagle band people began getting a share of profits by doing their own logging. In the first phase of tourism, up to the 1960's, Eagle band people worked for tourists, cottagers and lodgekeepers as guides and shore employees. In the second phase, they entered business for themselves as operators of facilities for fishing and hunting tourists. The band spent $170,000. of its own money to build a hydro-electric power line and a road connecting the isolated reserve with the outside world. After that, tourists could readily travel in, and logs could move out in trucks belonging to band members.

Eagle band has undergone several transformations: 1. Trading band. 2. Group of opposite numbers to the Crown in negotiation of a treaty. 3. Possessors of legal rights and disabilities as persons of Indian status and sharers in a joint estate. 4. Legally defined ethnic population unit partaking of the nature of a unit

of local government, internal colony and holder of corporate property. 5. With the creation of band-owned tourist enterprises, Eagle band became an expanded version of the original population unit: a trading band. It took on each new role without losing earlier roles. It stands now as an enlarged and altered version of its former self, having lost most elements of its original Ojibwa Indian identity. The Ojibwa language is rapidly fading. The band's political forms, including the election of a chief and two councillors every two years, were imposed from the outside. Many other sociocultural patterns were voluntarily adopted. Yet the band retains some degree of cultural "Indianness." It retains an identity and a microculture of its own.

Demographically, Eagle band has gone through the following processes: fivefold expansion in membership from treaty to timber sale; threefold expansion from timber sale to 1972; scattering of population from trading-post base to isolated homesteads on the reserve; gathering in of the reserve population to a compact village at Eagle Bay; migration of half the population to live elsewhere.

Throughout Phase Two, Eagle band was able to retain substantial resources and accumulate information about ways of profiting from those resources. The band was prepared to a limited extent for the changes that took place in Phase Three. In some ways, agents of the larger society (specifically, the federal government) exerted pressure toward fragmentation and loss of resources — pressure which Eagle band resisted. In other ways, the government acted to conserve the band's resources. It observed the letter of the law which required the band's formal consent for any sale or lease; and if the band withheld that consent, it exercised stringent trusteeship over the band's land and money. The band's access to its own money was restricted, and permission to draw on capital funds was only sparingly granted.

ANALYSIS OF SOCIAL PROCESS IN EAGLE BAY

Even in the period of domination, Eagle band achieved a relatively high degree of independence, by the social-ecological mechanisms which the band developed for maintaining cohesive-

ness, and by the deliberate decisions of leaders operating with the support of band members[3] inside this social-ecological framework. Among these decisions were a decision to engage in trade as middlemen, and a decision to avoid forced acculturation. An elderly informant told me: "Chief Eagle said he didn't want schools, because schools are where they learn to lie and steal."

In Mallard band, some people accepted schools quite early. It is my hypothesis that the acculturative pressure of schooling played a part in creating a cultural split between progressives and conservatives which was compounded by economic and geographical fragmentation to produce a high degree of disunity. In these conditions it was easy for outsiders to separate the people of Mallard band from their resources.[4]

Members of Eagle band maintained a united front on acculturation. The group accepted the idea of acculturation, which was a major goal held by the main decision-makers in the larger society on behalf of the Indians. However, the band insisted upon becoming acculturated at its own speed. When acculturation had reached a fairly advanced stage, Eagle band accepted schools with enthusiasm. Today the residents of Eagle Bay are uniformly in favor of acculturation and economic development. Eagle band might have chosen, as did the Fox (Joffe, 1962), to bargain for advantage with the outside society while maintaining a high degree of cultural separateness. Eagle band members decided that cultural separateness did not pay off, in the social and material rewards offered by the larger society. However, they continued to defend their special legal status, which carried clear-cut benefits without perceptible disadvantage. The band is collectively wealthy,

[3] Letters from Hudson's Bay Company factors who saw Chief Eagle and his band as possibly dangerous rivals contain several references to the practice of Chief Eagle of consulting members of his family before making decisions. Personal observation and interviews with Eagle Bay people in recent times give evidence of the obligation on a chief to gather consensus in private conversations before making decisions or launching public discussion.

[4] In talking to former pupils of Indian residential schools across Canada, I was told many stories about harsh punishments inflicted on pupils for speaking their own language. Many have admitted feelings of hostility, emotional confusion and inadequacy which they attribute to forced acculturation in the schools. Other persons spoke in praise of residential schools but expressed ambivalent or hostile feelings about Indian identity. More intensive investigation may well show that schools were a powerful influence for cultural fragmentation. (Mortimore and Yesno, 1966).

although its cash reserves are dwindling. Individual band members are not rich, but they are well housed, well-fed and well-clothed. There is no economic or geographical fragmentation.

Experience gave Eagle band members some preparation for taking on increased authority and control of resources. A number of men acquired some skill in bargaining. Examples of benefits from bargaining, planning and postponed gratification were in plain view. Band members had prestige — in the eyes of tourists because of knowledge of wildlife, in the eyes of many people in the surrounding country, because of their wealth. Per capita payments from the band fund, once more than $20 a month, now $6, increased the people's mobility. With this financial cushion to fall back on, they could choose between the city and the reserve, and adjust to the larger society on their own terms. They were receptive to new ideas. Development suggestions came from tourists and others.

The band evolved effective devices for resolving and preventing conflict. These devices still operate. Among them are joking and withdrawal behavior and a cooling and sifting process that filters out dissidents, troublemakers and victims of trouble; who tend to leave the community. These processes, together with some examples of benefits obtained, have led to a consensus among Eagle Bay residents in favor of economic development. However, the sorting-out process produces conflict at another level. It deposits at Eagle Bay two basic kinds of people: planning or efficiency-minded persons, and the "happy-go-luckies" who seek immediate satisfactions. Some persons are wedged near one of these ideal-type poles; others are strung in between. Observation at Eagle Bay points toward a concept quite close to Belshaw's Type C values:

> ...a conflict of possible objectives, reflecting the presence of inconsistent values, each with a different valence and potentiality for being translated into action. The actual choice in the light of behavior would be a reflection of the dominant value subscribed to by the actor, given the specific circumstances surrouding the action (1959: 557).

Four major politically relevant beliefs are held and followed by the people of Eagle Bay. They are an anti-conflict ethic, an egalitarian ethic, an intense interest in neighbors and a belief in

economic development. "Bold" and "cross" are common terms of reproach for persons who violate the anti-conflict ethic by undue frankness of speech and displays of bad temper. Disapproval descends on a person who is "bossy" or "tries to make a big guy of himself." However, this foundation of agreement is overlaid by a level of conflict between two opposing clusters of principles, which I call the efficiency code and the personal code. Those who tend to the efficiency code hold the following general beliefs: 1. Although tact is necessary, blunt speech is also needed at times. 2. A chief or manager must sometimes be a managerial and technical expert rather than merely a co-ordinator. He must sample opinions and gather consensus, but he must also make decisions and give orders. 3. Band membership and market efficiency often take precedence over kinship and personal loyalty. 4. Indian legal rights, and other benefits, must be fought for and defended; and Indian legal rights have definite limits. 5. Long-term goals must sometimes take precedence over short-term goals. Personal-code adherents, by contrast, believe strongly in the anti-conflict ethic and the egalitarian ethic as inviolable principles; they believe in the chief as consensus-gatherer, not as boss; in kinship and personal loyalties as of paramount importance; they take the unlimited view of Indian rights and other benefits, and the short-term view of spending, planning and investment. They tend to think of Indian rights as automatic and unbounded — connected, in a sense, with a bottomless pool of obligations.

Some Eagle Bay persons can be identified as tending strongly to the efficiency pole or the personal pole; but no one person follows all beliefs to the letter. The code is implicit at both ends of the scale; but sometimes it finds partial expression in words. There is a back-and-forth surge of support between preference for instrumental, efficiency values and socio-emotional, personal values. This social-ecological process is a device for solving the problem of apportionment of increased resources. Eagle Band people had sizeable resources at their disposal during much of Phase Two. Even then they had some decisions to make about apportionment. It seems likely that the back-and-forth surge developed some time ago. In any event, its operations can be noted during the regimes of the three men who have been chief in the last 20 years.

Joe Eagle, the first of these three chiefs, held office for 17 years. It was under his regime that the band gained control of its logging and tourism, and built the road that ended its isolation. Chief Joe knew how to respond to public demand and how to integrate conflicting or divergent goals. When he came to office, post-World War 2 inflation had reduced the purchasing power of per capita distributions; people were becoming increasingly aware of the reserve's isolation. Only five families lived all year round at Eagle Bay. Among the remainder, some stayed away in winter, some left permanently and some resigned their Indian status and claimed their $5,000-per-head share of the band fund. Eagle band's money was leaking away at more than $40,000 a year. Band members had two important goals — to enjoy the comforts of modern home technology and access to the city, and also to enjoy the fishing and hunting and the tranquility and close emotional support of life in a small bush village. Eagle Bay people could only attain these goals serially. Chief Joe integrated them by building the road, and brought both within simultaneous reach. The outflow of people was checked. Families began moving back to Eagle Bay.

During Chief Joe's regime there were several oscillations between the efficiency pole and the personal pole. Local control of logging was followed by wasteful high-grading in which many logs of lower value were left to rot. Chief Joe (with support from the members) increased timber dues, started tree-planting and began imposing quotas on cutting. When returns from logging declined and resentments grew against restrictions and orders, he obtained development grants, launched the band into the tourist industry and provided short-range satisfactions in the form of jobs and income. To the critics who objected to the large-scale use of band funds to supplement the grants, and the hiring of excessive numbers of employees in the tourist enterprises, he countered by securing further grants. He had a broad-spectrum style. Like a bicycle rider, he turned the way he was falling. From the band's viewpoint, the grants the band received were only a partial repayment of the $180,000 plus labor which Eagle band had spent to build a road and a hydro-electric line which benefitted Indians and non-Indians alike. When Chief Joe took office in 1953, Phase

Three in government policy was taking shape — and he hastened it by pressing for increased control of money and timber.

When Chief Joe died in office in 1971, the principles of Phase Three had become official dogma. He was succeded by his nephew and adopted brother, Leslie Eagle, who as a councillor had been Joe's hatchet man, his scapegoat deputy charged with the unpleasant jobs. During Leslie's term of office, his policy swung quite far in the direction of efficient, instrumental leadership; and in pushing for long-term goals Leslie ran afoul of deeply-held socio-emotional values; he issued blunt orders and commands, he tried to make people pay their gas and oil debts to the band-owned marina, and he stringently enforced logging quotas. He pressed tactfully for stricter control of hunting by band members, and hinted that it might be necessary to cut off the per capita distribution. He rebuked the band administrator, Betty Brunelle, for not spending enough time in the office, for delay in paying bills and answering letters, and for issuing herself cheques in advance. He was defeated in the election by the administrator's husband, Ronald Brunelle, who was better attuned to the values of the community, and had the support of close relatives and affines.

As soon as Ronald took office, he began to show his weakness in tasks that involved planning and universalistic efficiency values. He took criticism as a personal affront. For about four months he held no band meetings — breaking a long-standing band tradition. By the end of his term, band business enterprises had drawn $84,000 from band funds to meet heavy deficits, without any clear accounting. Resentment built up against Ronald. In the 1974 election he was sharply defeated by Leslie Eagle.

THE CONFLICTING EFFECTS OF GOVERNMENT POLICIES

Government policies produced conflicting effects in Phase Three, as in Phase Two. There were considerable transfers of authority, information and resources. In many bands, the transfer of resources was inadequate to produce any increase in economic self-sufficiency[5]. In the case of Eagle band the group already held

[5] This inadequacy is pointed out by Hawthorn and associates (1966:163-166).

substantial resources in its own right. The transfer process took two forms: conveying to the band a greater degree of control over its own money and timber, and bestowing grants for economic development. Increased authority to make decisions locally was conveyed to the band in several areas of policy and administration.

It is my hypothesis that some degree of conflict is inevitable when a group's decision-making authority or the group's resources are increased, decreased or converted into new forms. The conflict concerns the ways in which the increased or remaining authority or resources are to be used or shared out. Warnings might have been circulated in advance to allow co-ordinative techniques to be devised. This process is not immediately apparent; there is no intention of passing moral judgments when I note that governments failed to take such ideal measures. However, the faults of policy were more serious than this. Not only did governments fail to take conflict-soothing measures; it is now obvious (although it was not easy to see at the time) that governments acted in such a way to create and increase conflicts.

In the case of Eagle band, transfer of authority and resources was not accompanied by the transfer of an equivalent amount of skill and knowledge in administration, record-keeping, business management and resource conservation. As a result, substantial amounts of money and timber were lost.

Skill and information were transferred in disproportionate amounts to the band administrator, who thereby attained an information monopoly and became a power rival to the chief and council. The resulting conflicts contributed to the loss of resources. The same pattern of events occurred in several other bands. Training courses of a few days' or weeks' duration were designed for councillors; but only a slender volume of information was transmitted. A more serious training effort was focussed on band administrators.

In Eagle band, a field worker came at intervals to train Betty Brunelle, the band administrator. An atmosphere of secrecy surrounded the training sessions. If an outsider chanced to knock on the door when training was in progress, instruction abruptly halted; there were long embarrassed silences. Secrecy was part of the ethos of this federal agency.

At the suggestion that administrative and bookkeeping skills should be given to a number of persons in the band, so that there would be no information monopoly, an Indian Affairs official offered this opinion: "There is never enough money allotted in the budget for an integrated and wide-scale training programme of any sort. Experts in accounting and administration should be hired to teach people these skills. But just try to get the money for this kind of programme.... You're told the money isn't available.... The job of teaching bookkeeping and administration is entrusted to one general-purpose fieldworker who is so busy that he or she never gets more than a few hours each month with each administrator...."

Specialized, concentrated administrative training programmes were later made available. However, in the case of Eagle band and many other bands, the administrator was the only person chosen to take such training. In any event, such programmes involve removal to a distant training centre, which diminishes their effectiveness. Information transmitted to administrators remained skimpy; and yet, in the case of Eagle band and many other bands, it was sufficient to confer an information monopoly. So complete was the information monopoly at Eagle Bay that the administrator was able to defy or dodge a number of requests for information from band members, from government officials with a voice in awarding grants, and from a consultant preparing a development plan. Band members, officials and the consultant could not get a clear profit-and-loss statement about band business enterprises. Routine audits were unable to disentangle the knotted threads of unorthodox record-keeping. It was not until a provincial development officer began questioning (as a preliminary to consideration for further grants) and sent in a special auditor, that he was able to find out some of the business information that had never been available to members of the band[6].

In the process of transferring authority, certain areas of authority were left unclear or ambiguous. For example, Eagle band

[6] In Mallard band, which was fragmented, a business corporation had the effect of bringing together scattered advocates of economic development into an interest group with the capacity to act. It might be argued that the corporation would ultimately be beneficial to Eagle Bay, for this reason: Building of the road somewhat reduced the interdependence of Eagle Bay people, and reduced the cohesiveness of the community. The corporation might later act as a consolidating influence. So far, however, its influence has been divisive.

members could now get quite easy access to their capital money for housing loans of up to $5,000, but responsibility for collecting repayment of the loans was not effectively transferred to the band and was not effectively carried out by the federal government. Therefore the bulk of the loans were not repaid, although most borrowers could afford to repay them. The band fund suffered serious losses as a result.

The policy of budgetary thrift clashed with the policy of transfer. Because of the small size of appropriations for communication, training and fact-finding, the flow of information in both directions was meagre. Lack of information flowing into Eagle band resulted in loss of resources. Lack of information flowing out raised difficulties for policy-makers and development workers. Officials and development workers, bearing responsibility for liaison with many communities, lacked the intimate contact which is needed for an understanding of social structure and process. These difficulties also affected the provincial government, which entered into closer relations with Indian bands in Phase Three.

In the case of Eagle band, officials and development workers lacked full information about the band's cohesive and unitary structure, and its rythmic swing between two sets of values and two kinds of leadership behavior. Important decisions were made by votes of the entire band at regular band meetings. When the band was compelled to form a specialized band business corporation as the price of cutting rights in provincial Crown timber, the results were damaging. A swing from an "efficient" to a "personal" chief had just taken place. The efficient chief was voted out — mainly because he violated the egalitarian ethic and the anti-conflict ethic of the band, by behavior that was judged to be too brusque and authoritarian. The new chief was able to control the corporation, transfer from open band meetings to closed corporation meetings most of the transactions of publicly-owned business enterprises, and suspend open meetings. In this way, the chief muffled conflict, avoided confrontation by hostile critics, and concealed information about the serious drain on band funds that had resulted from inefficient management of the band's tourist and logging enterprises.

CONCLUSIONS

In the Eagle Bay case, federal and provincial governments took some actions to increase the authority, power and resources of the subordinate group. At the same time they took some actions that made effective transfer difficult. From observation of positive and negative results, it is possible to say that the following conditions among others, are likely to make transfer effective:

1. Even in the period of domination, control and custody, the group should have access to substantial resources.

2. The group should have a pool of information about ways of profiting from these resources.

3. The group should have a social structure and ideological climate which encourages leaders who have skill at bargaining for advantage with outside agencies, and sensitivity to short-range demands within the group.

4. Cohesiveness and homogeneity or harmony of goals within the group should be reinforced.

5. The transfer of authority and resources must be accompanied by an equivalent transfer of information and skills.

6. Information and skills should be transferred to as many persons as possible.

7. Transfer is likely to lead to conflict over the sharing out of the authority and resources. Measures should be taken to prevent or resolve the conflict.

8. Grafting of alien political-economic forms on indigenous structures may have a distorting or fragmenting effect.

9. Policy-makers in the dominant society should examine their goals to see how closely those goals complement or coincide with one another and with the prevailing goals and values in the client society.

10. When authority is transferred, the division and assignment of authority should be clearly stated.

There are three possible policy directions that a dominant government can take toward colonial transfer. The same alterna-

tives hold true for the transfer of power to economically distressed groups at home or abroad. I suggest these alternatives may be labelled abandonment, disguised domination and genuine transfer. When conflicting policies are followed, causing fragmentation, loss of resources and at least partial failure of transfer, the general trend of policy is likely to fall into one of the first two categories. The dominant government may relinquish its authority and confer resources in such a way that the client population remains dependent on the dominant society while exercising the forms of independence. The dominant government may behave in such a way that the client population has no effective resources or skills. It will therefore be at the mercy of all outside agencies that operate in the political-economic market. This kind of transfer may be labelled abandonment. Genuine transfer is an active process. It involves willingness on the part of the patron government to supply resources, information and skills in large quantities. The task may involve enabling the client population to state its goals clearly and vigorously. The task may involve sponsoring a dialogue between "people" and "experts" in which goals are increasingly refined and ordered, and an enlarged range of alternatives placed before the people for their consideration. In short, effective transfer and effective development means finding out what the people want and helping them get it.

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Co-Residential Groups[1]

TREVOR DENTON
Brock University

RÉSUMÉ

L'étude des groupes co-résidentiels est refaite autour de la question de proximité, c'est-à-dire du fait que les membres du même ménage vivent sous le même toit. Se servant des hypothèses de congruence et la théorie de l'échange, l'auteur soutient que, toutes choses égales par ailleurs, les gens vont vivre dans le même ménage seulement quand ils forment un groupe minimal équilibré (ordinairement, une famille nucléaire composée des parents et des enfants). Les ensembles plus étendus vont résider en commun seulement quand des avantages additionnels viennent contrebalancer les difficultés cognitives potentielles qui en découlent. Plusieurs hypothèses sont déduites de ce principe et mises à l'épreuve à l'aide de données sur une réserve indienne du Canada.

The aim of this paper is to reconceptualize the study of co-residential groups. Traditionally, co-residential groups and residence patterns generally have been approached from three main perspectives. First, in the typological tradition the problem has been formulated in terms of specifying the range of types of (usually post-marital) residence found in cultures around the world (eg. Murdock 1949). Few today adhere to this approach, perhaps because of the seemingly endless variety of types found and difficulties of fitting vague definitions to concrete data. The second approach to residence pattern analysis has been that of decision theory, in which (usually post-marital) residence has been conceptualized in terms of couple choosing a residence in such a

[1] The data in this paper were gathered during research in 1967-1968 jointly financed by Central Mortgage and Housing Corporation and the Department of Indian Affairs and Northern Development (see Denton 1970). This is a revision of a paper read at the 1973 meetings of the American Anthropological Association. Michael P. Carroll and Melvin L. Perlman made helpful comments.

way as to optimize gains in the light of the constraints which they face (eg. Barth 1966; Goodenough 1956). The third approach has been that of the domestic cycle, in which the diversity of households present in a given society at a given time is seen as the product of different norms appropriate to the different life cycle statuses present (Goody 1958).

All these approaches have missed a key dimension of household composition, which is — proximity, the fact that all members of the same household live together under the same roof. The aim of this paper is to reformulate the study of co-residential groups around this central issue.

CO-RESIDENTIAL GROUPS

In any explanation of concrete behaviour, such as a residence pattern, it is important to generalize, to conceptualize a problem in a theoretical framework of the widest possible range of application. Heider's balance hypotheses (Heider 1958) and Homans' exchange theory (Homans 1974) together provide such a conceptual framework for the study of co-residential groups. Other things being equal, people will live in the same household only when they form a cognitively consistent balanced minimal group, usually a nuclear family of parents plus children. Larger aggregates will tend to be cognitively inconsistent and unbalanced, and will live in the same household only when "other things" are not equal, namely, when there are additional social rewards for such co-residence which outweigh the potential strain involved. This argument can be formulated as follows.

While many theories of cognitive consistency have been advanced (cf. Abelson *et al.* 1968), Heider (1958) sets out a framework in which proximity plays an important role.[2] Briefly, he suggests that a cognition for a person P involving another person

[2] The field of cognitive consistency is somewhat chaotic, being characterized by many different competing theoretical formulations (cf. Abelson *et al.* 1968). Heider's formulation, while ambiguous about such issues as strength of relations among elements of a cognition and degree of balance, is in the writer's opinion among the best of the theoretical formulations which exist at the moment, and one of the few to incorporate the concept of proximity. See Carroll (1973) for an interesting cross-cultural application of Heider's work using the Harary *et al.* (1965) formulation.

O (or an object X) involves two kinds of relations — liking (+ 1 for liking, — 1 for disliking) and unit forming (+ u for unit forming, — u for not unit forming). Unit forming relations are those of, for example, causality, ownership, similarity, membership, and in particular proximity and interaction (which flows from proximity). A cognition is balanced if both signs are negative or both are positive (ie. + 1 + u, or — 1 — u). Heider suggests that unbalanced cognitions are stressful and tend to change to a state of balance. Now, co-residence implies positive unit formation (+ u). Thus, a cognition in which P lived in the same household as O (+ u) but disliked O (— 1) would be stressful to P and therefore likely to change to liking (+ u + 1) or fission of the household (— u — 1). Restoration of balance by ignoring and not thinking about such a cognition is also theoretically possible, but highly unlikely for any lengthy period of time when the unit forming relation is proximity or frequent interaction.[3]

Now, Sherif and Sherif (1953:2) define a group as "a social unit (1) which consists of a number of individuals who, at a given time, stand in more or less definite interdependent status and role relationships to one another and (2) which explicitly or implicitly possesses a set of values or norms of its own regulating the behaviour of individual members, at least in matters of consequence to the group." Members of a group experience motives, aspirations and frustrations in common. If two such in-groups are "brought into functional relationship under conditions of competition and group frustration, attitudes and appropriate hostile actions in relation to the out-group and its members will arise" (p. 237). The same result would occur in the case of an in-group in hostile relations with individual outsider(s) not constituting an out-group but rather simply hostile individuals outside the in-group.

It can be argued that, if two in-groups or one in-group plus outsider(s) live in the same household, there will exist the potential

[3] It is also possible to have a tripartite cognition involving P, O and X, or P, O and Q, where X is an object and Q is another person. Balance exists here when all three of the relations are positive or when one of the relations is positive and two are negative. Theoretically, an unbalanced P O relation (+ u — 1) could be balanced by adding a third person to the cognition (eg. P (— 1) O, P (+ 1) Q, Q (—1)O). However, where proximity or frequent interaction are involved, dyadic cognitions are inevitable and will be unbalanced if the sentiment relation is negative (ie. + u — 1).

for mutual hostility and therefore cognitive strain; each in-group or outsider, having its own set of norms important to it, would mutually frustrate one another in varying degrees. The simplest co-residential unit is thus a minimal group sharing a single set of common norms as with a mother plus children, plus usually a father.[4] Such minimal groups are balanced and cognitively consistent (+ u + 1). All other groups are "abnormal" from the perspective elaborated above and require further explanation.

However, it is well established that in many societies around the world, co-residential groups larger than minimal groups do exist. The question then becomes, under what conditions will this arise?

Exchange theory provides an explanation. One of Homans' exchange theory propositions (the value proposition) states that, "the more valuable to a person is the result of his action, the more likely he is to perform the action." (Homans 1974:25)[5] Thus, minimal co-residential groups will exist unless there are additional social rewards which make it profitable to endure the strain of a potentially unbalanced household. "Reward" is defined as "profit — cost", and refers to the value of an activity. Thus, we may predict that potentially unbalanced co-residential groups will occur when the profits of co-residence outweigh the cost of the strain toward cognitive inconsistency.

Consider a household in which P is co-resident with outsider O, but dislikes O (+ u — 1). There are four possible outcomes (although several may partially occur together):

1. — u — 1 —— break off the unit forming relations
2. ignore the cognition (unlikely as a permanent solution where the unit forming relation is proximity)
3. + u + 1 —— change the cognition to liking and ignore undesirable behaviour
4. + u — 1 —— retain the unbalanced cognition and endure the strain

[4] While parents plus children seem to be a typical minimal group, other varieties of balanced minimal groups have been reported in the literature (cf. Stephens 1964).

[5] See Homans (1974) for the other propositions of exchange theory.

Other things being equal, the simplest solution is number 1, because, other things being equal, solution 1 means an end to any potential cognitive strain whereas solutions 2, 3 and 4 will all require continued psychic energy (ignoring the cognition in solution 2, ignoring or reinterpreting unpleasant behaviour as pleasant in solution 3, or enduring the strain in solution 4). To maintain co-residence where group norms will inevitably be frustrated to some extent, ie. to opt for any of solutions 2, 3 or 4 or some mixture of them, means that additional social rewards will be present to make these a preferred solution over number 1 — fission of the household.[6]

The argument developed above can be summarized as follows. 1) Proximity creates a positive unit forming relation (+ u). 2) The sentiment relation between any given minimal group member and outsider(s) is likely to be negative. 3) Therefore, there is a high probability that the relation between any given minimal group member and the outsider(s) is likely to contain one positive bond and one negative bond (+ u — 1). 4) But, any such pair linked by a positive and negative bond is imbalanced according to Heider and a strain toward balance will occur. 5) Fission of the household to achieve balance will occur except where there are additional social benefits to outweigh the costs of the potential cognitive strain involved in continued co-residence.

CO-RESIDENTIAL GROUPS AT A CANADIAN INDIAN RESERVE

To test these ideas let us examine co-residential groups at a Canadian Indian reserve.[7] The reserve is an acculturated one in the settled southern region of Canada. While older villagers are bilingual and younger ones speak only English, the kinship terminology in both English and the native Indian language follows the North American pattern in which mother's and father's siblings are equated as uncles and aunts, and the latter's children are equated as cousins.

[6] See Homans (1974: 59 *et seq.)* for a discussion of the relation between exchange theory and the balance hypotheses.

[7] See Denton (1970) for a detailed study of the reserve. By agreement with the Band Council the reserve is left unnamed to preserve anonymity.

While there are no residence "rules" at the reserve, there are certainly residence preferences. Ideally, a married couple should maintain their own separate residence, as should unmarried adults whether single, separated or widowed. It is quite proper for a married son or daughter plus spouse to live temporarily with parents if they are saving money to purchase their own house within a year. It is also appropriate for households to care for close relatives who are very elderly or infirm and who cannot take care of themselves.

Other forms of residence do exist but are held in lower repute. For example, unwed mothers plus offspring normally reside with the mother's parents. Unmarried adult sons sometimes live with their parents. In some households a brother, sister, brother-in-law or nephew is allowed to stay, and in a few households a married indigent son or daughter plus spouse and children live on a semi-permanent basis.

Here is the actual composition of households at the reserve as of June 1, 1967. The village population was 411 persons spread among 84 households.[8] Of these 84 households, 20 were single person dwellings, and 38 were balanced households of a husband and wife or parent(s) plus children (of which four households also had a child plus spouse temporarily living with them to save money to purchase a house within a year). It should be noted that, because of intense norms of familism at the reserve, it is safe to assume that these nuclear family households are indeed reasonably balanced minimal groups. A further three two-person households consisted of non-nuclear family groups, with a woman plus her brother, or nephew or grandchild, who provided company for the woman and help around the household. All in all, 23 of the

[8] All census terms used in this paper follow the definitions utilized by the Census of Canada. Thus, the term "household" is defined not in the *de facto* sense of persons at a house on a given day, but rather in the *de jure* sense of persons who themselves defined a particular house as their customary place of residence.

In large houses (eg. Iroquois longhouses etc.) it might be possible for household members to minimize proximity and interaction by turning the house into a virtual "hotel" wherein different segments belong to different people who interact minimally among themselves. This is not possible at the community considered in this paper because the houses are quite small. However, the concept of degree of proximity might be a useful one for refining cross-cultural applications of cognitive consistency theory to co-residential groups.

84 households were unbalanced in the sense that they consisted of parent(s) plus children plus someone(s) outside this group.

It has been suggested that single person households or balanced minimal groups are the norm, and that larger aggregates will occur only when there are rewards for enduring the strain of potentially unbalanced relations. Thus, only the 23 unbalanced households at the reserve need be accounted for.

Recalling that "reward = profit — cost", a number of hypotheses can be derived from Homans' value proposition. In each of the 23 households there exists a main minimal group consisting of the household head plus (usually) a spouse and/or children. It can be predicted that co-resident persons not in this main group[9] will:

1. have first or second degree kinship links with the household head rather than third or fourth (high cost to household main group of ignoring close kinsmen and low cost of ignoring distant kinsmen); this hypothesis includes both adults and children not in the main group;[10]

2. have no other household of closer kinship ties on the reserve where they might live, nor a house of their own on the reserve (high cost to household main group of

[9] Marginal persons who are members of both a main group and another group are counted as non-main group persons because they share non-main group norms. Thus a daughter living with her husband and children in her parents' household would count as a non-main group person.

[10] Degree of relationship of kinship links between ego and any other kinsman is computed by counting the number of potential kinship categories between ego (the household head in this case) and the kinsman. Thus, in the reserve kinship system, ego's parents, siblings, children and spouse are all one link away; ego's uncles, aunts, parents-in-law, siblings-in-law, grandparents, grandchildren, etc. are two links away since in each case one kinship category intervenes between them and ego.

The community is endogamous and any given individual will have a much larger number of third and fourth (or more distant) degree kinsmen in the community than first or second. Thus, the probability of the results obtained in Hypothesis 1 is really much smaller than the .001 indicated in Table 1 using the binomial test with its assumption that the probability of either kind of kinsman (first and second versus third and fourth) is .5.

The derivation of Hypothesis 1 is made in terms of cost to the main group of ignoring close kinsmen. One could also argue that close kinsmen would be more similar in values to the main group than distant kinsmen, thereby creating less imbalance and constituting less of a cost. However, the same operative hypothesis (more first and second than third and fourth degree kinsmen) would hold for either formulation.

ignoring close kinsmen and hight profit to non-main group persons of place to live on reserve); this hypothesis will be tested for non-main group adults only since children ordinarily accompany parents;

3. be likely to be accepted by poorer households needing rent money and/or ones where they can provide needed services, eg. providing needed help with children (high profit to household);
4. be of lower social prestige in the community (low cost to lower status persons of accepting lower status form of residence);[11] this hypothesis will be tested for non-main group adults only (ie. not children) and will exclude elderly and infirm adults unable to care for themselves;
5. be less assertive rather than more assertive, ie. children rather than adult women and adult women rather than adult men (this scale represents reserve norms of assertiveness) (low cost to main group in household of non-assertive people).

TABLE I — TESTS OF HYPOTHESES

Hypothesis 1 — Degree of kinship links to household head.

persons of links of 1 or 2 degrees = 69	$p < .001$ (one tailed binomial test)[10]
persons of links of 3 or more degrees = 5	

Hypothesis 2 — Households in which non-main group persons have possible alternative residence on reserve of their own or of closer degree of kinship relatedness.

alternative residence = 4	$p < .001$ (one tailed binomial test)
no alternative residence = 19	

[11] There is a three tier social stratification system on the reserve with an elite (based on income and steady off-reserve factory jobs plus respected personal deportment), a lower group (based on chronic unemployment and disrespected personal deportment) and a residual middle group consisting of all others (cf. Denton 1970: 53-55).

Hypothesis 3 — Wealth and/or need of special services of households.

poorer households or needing special services = 17	$p < .02$ (one tailed binomial test)
better off households and not needing special services = 6	

Hypothesis 4 — Social prestige of non-grain group adults (excluding elderly or infirm).

lower = 19	$p < .02$ (one tailed binomial test)
middle or high = 7	

Hypothesis 5 — Assertiveness of non-main group persons.

number of children = 40	$p < .001$ (x^2 test)
number of adult females = 16	
number of adult males = 11	

All five hypotheses were supported (Table 1). It is not suggested that these hypotheses (and the rewards they represent) are the only determinants of co-residential groups at the reserve, but they are important determinants and their tests do support the line of theoretical reasoning developed in this paper.

DISCUSSION

This paper has suggested that co-residential aggregates larger than minimal groups will occur when the social profits of such co-residence outweigh the potential strain involved, and used the balance hypotheses and exchange theory to arrive at this formulation.

While a number of hypotheses were derived and tested with data from a Canadian Indian reserve, many additional hypotheses can be derived for future tests. For example, social profits important to the survival of groups of people include personal safety (as in societies with substantial incidence of violence), social identity (as with lineage and other groups), prevention of theft of production resources, and access to (and especially ownership of) production resources. Where large co-residential aggregates

are equated with any of these social profits, we may anticipate co-residential aggregates larger than minimal groups. Obviously many other hypotheses can also be derived.

The approach to co-residential groups outlined in this paper is to be preferred to previous approaches. It elaborates the decision theory and domestic cycle approaches. For, all members of a society, be they newly married couples or others, tend to maintain balanced co-residential groups on the basis of profits and costs involved, which may change through the life cycle as different norms become associated with different life cycle stages. The framework advanced in this paper can explain everything that past approaches to co-residential groups have explained, and more, and is therefore a preferred mode of explanation.

While the theory of co-residential groups advanced in this paper has greater explanatory potential than previous formulations, it is a start in a profitable direction rather than a polished final product. The theoretical conceptualization needs to be sharpened. For example, the concept of degree of proximity may be a useful one to pursue in cross-cultural refinements (cf. footnote 8). Moreover, in this paper minimal co-residential groups tended to be equated with nuclear families. At the reserve in question this as a reasonable assumption; however, throughout the world not all co-resident nuclear families are groups, and not all co-residential groups are families. This does not alter the theoretical formulation of this paper because the key concept is minimal co-residential group, be it familial or otherwise. Future work in this area may describe the range of co-residential groups other than familial groups to be found throughout the world. The ideas in this paper should be regarded as a start toward reconceptualizing the study of co-residential groups, not the end, and if they spark further thinking in the field they will have served their purpose.

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Eskimo Music: A Comparative Survey

THOMAS F. JOHNSTON
University of Alaska

RÉSUMÉ

Il s'agit d'une revue des études faites sur la musique esquimaude entre 1911 et 1975, accompagnée d'une évaluation de leurs principaux résultats. Les théories de la diffusion et d'autres approches courantes sont critiquées à la lumière de données anthropologiques récentes. L'auteur recommande une approche plus anthropologique à l'ethnomusicologie dans le domaine de la musique esquimaude, la conduite des études socio-musicales dans leur contexte culturel et la coordination méthodologique de la recherche actuelle sur la musique esquimaude.

Since Thuren and Thalbitzer's pioneer work in 1911, numerous substantial analyses of Eskimo music bear testimony to the continued search for comparative data on the subject. Leden followed Thalbitzer into Greenland during the years 1910-26, and subjected his collection to substantial analysis in 1952. The collections of Roberts and Jenness during the period 1913-16 were analyzed by Roberts in 1925, and again by Béclard-d'Harcourt in 1928. Estreicher, utilizing the field work of others, made an extensive analysis of Caribou Eskimo music in particular (1948) and of Eskimo music in general (1950). Ingstad's collection from the Alaskan interior during the period 1949-50 was analyzed by Groven in 1956, though it should be borne in mind that the Nunamiut population of the interior, 97, is by no means representative of the large Eskimo population of the southwest regions, 17,000. Svensson (1956) elaborated upon Groven's findings. Olsen collected Greenlandic material in 1961 and subjected it to melodic and structural analysis (1963, 1967, 1972). Collaer (1967) attempted a summary of Estreicher's and Olsen's analyses. Koranda collected Alaskan material 1964-72, publishing a limited

analysis (1972). Binnington carried out a limited analysis of north Alaskan Eskimo music in 1973. Several substantial studies are at present in progress, including those of Walcott (University of California) on Alaska's Nelson Island, Ager (Ohio State University) in Alaska, Binnington and Liang (University of British Columbia) at Coppermine, Charron, Nattiez, Harvey, and Beaudry (University of Montreal) on Baffin Island, Hauser (University of Copenhagen) in Greenland, and Paquet (Sorbonne), on the recordings of Hauser.

The objective findings of these various studies are urgently awaited, for too often the musical reports on which ethnomusicological analyses have been based have been mere appendages or afterthoughts to ethnographic investigations, and too often there has been lack of a comprenhensive design. Such a design would surely have included, for instance, Alaskan Eskimo music. Fortunately, liaison work and the scholarly exchange of information within the framework of the Society for Ethnomusicology promises to ensure coordination of future studies.

The search for comparative data on Eskimo music parallels that for, and ideally should be related to, comparative data on Eskimo social and economic systems. Until recently the latter has to some extent been lacking. For a long period, the ethnographic literature characterized the various circumpolar Eskimo societies as ones in which the members were almost universally nomadic, egalitarian, ate raw meat, built igloos, rubbed noses, swapped wives, and indulged in female infanticide. For instance, Mead's textbook on the anarchistic individualism of the Angmagssalik Eskimo (1937) — a description which has been reprinted several times in recent decades — is now widely considered to have been grossly overdrawn.

In fact, while Alaskan Eskimos possessed the permanent ceremonial house, with its integrative, multiple musical function and involvement with the social and prestige system, Eskimo societies to the east did not. While Alaskan Eskimos observed a ceremonial cycle requiring a considerable outlay of goods and much musical formalism, many others did not. While Alaskan Eskimos featured the cohesive whale-hunting crew drawn from the male sibling group, and their musical lodge with its own songs and dances, many others did not.

Many Eskimo cultural differences are due to regional ecological variation. The Iglulik, for instance, had no major rivers emptying timber into the sea, and hence were deficient in wood with which to construct the large boats necessary for hunting walrus in the open water during summer, and hence did not focus on boat-crew organization and its potential for hunting-lodge music.

Eskimos of the Barren Lands hunted caribou, while those to their north, the Netsilik, hunted seal. Eskimo hunters at Port Harrison in eastern Quebec Province traditionally have never been involved with either caribou or walrus (those of them who were resettled on Ellesmere Island had to be taught the relevant skills).

Other regional variation is due to the disparate social effects of Moravian, Catholic, or Anglican missionization, and to differential economic change, such as that incurred in some regions when the fur trade radically enhanced hunter autonomy and weakened the communal basis of traditional musical performance.

Regional differences in the ecology affect social structure, which in turn partly determines musical behavior. For example, a present-day whaling lodge in a rich maritime environment, possessing six drummers and a rehearsed dance team of fifteen to thirty dancers at one time, calls for sociomusical prescriptions, formalized musical routines, and tighter musical organization than that needed by a small, nomadic hunting band migrating seasonally between coast and interior, possessing but a single drum and emphasizing a solo drum-dance tradition (Johnston 1974, 1975).

Regional variation in kinship relations carries important implications in the realm of musical performance. The unilineality in Eskimo populations around the Bering Sea region in Alaska and Siberia is almost nonexistent in Canada, where bilaterality prevails, and this has bearing on song inheritance, dance partnerships, the namesake song tradition, and musical lodge membership.

The search for comparative data on *change* in Eskimo music parallels the search for reliable data concerning sociocultural change. The latter is inevitably followed by musical change, but not necessarily acculturative musical change. The coercive national

policy of assimilation in Siberia discourages the use of overt symbols of ethnicity, of which traditional music is perhaps the most obvious. The permissive, bicultural national policy prevailing today within the United States not only permits ethnicity, but, by delineating cultural boundaries such as the Native and the non-Native right to Land Claims, fosters separate group identity and the flowering of a cultural renaissance in traditional music.

Inevitably, the currents of sociocultural change and the new values of a changing world are permeating the Eskimo lifeway. The present emphasis upon carving takes away from hunting and increases dependence upon imported foods, which in turn makes the hunter's song and the thanking dance redundant. Earnings from carving are being pooled by the Netsilik, in order to purchase motorized whale-boats, which in turn become the focus of a modern *"umiak"* group unknown in the days when the Netsilik were mainly individual seal-hunters. Following some future cultural and musical renaissance, the Netsilik boat-crew might well function as the basis for a community hall musical unit.

The Iglukik, formerly without boats, now purchase wooden boats with earnings from trapping, and harvest the walrus herds. This new integrative group activity is not without its effects upon the pattern of musical performance, which generally tend to adapt to subsistence roles and seasonal occupations. Greenlandic male hunters, traditionally disdainful of shark-hunting, have been persuaded into this occupation by artificially raised cod liver oil prices and by the local use of shark liver as coinage, which in turn speeds up local economic development and hastens the demise of the ancient musical traditions connected with seal-hunting. In fact, the overall emphasis throughout Greenland, for retraining the Eskimo population for a commercial fishing economy, is bringing about a de-emphasis of those musical forms associated with older hunting pursuits.

Of the various aspects of sociocultural change which affect traditional Eskimo musical performance in Canada, one of the most prominent is the government's policy of relocation and resettlement. While some studies indicate that social cohesion and such communal activity as musicmaking may survive and even flourish increasingly, other studies indicate that numerous social problems are incurred.

On Southampton Island and at Pelly Bay, where not only Eskimo inter-tribal confrontation but religious sectarian confrontation is a latent source of social conflict, role confusion exists not only among the mixed Eskimo inhabitants but also among the White residents, and the resultant urban polyglot possesses no common musical reference.

While, in the small hunting bands of former times, social interation with any known individual was possible, today modern township residents must perforce be selective in their interpersonal relationships — the old musical mechanisms for integrating strangers into the system via dance has largely been lost. In place of the social and behavioral parameters circumscribed by the extended kin group with its proprietory songs, many communities are now characterized solely by coresidentiality, endogamy on a religious basis, and the independence of the nuclear family, emphasizing a musical repertory culled from northern radio broadcasts and from hymnals.

New Eskimo leaders are emerging, their prominence based upon language facility and Western vocational training. With leadership and prestige now hinging upon such contact skills, traditional pursuits such as song composer and dance leader are falling into disrepute, being considered emblems of cultural backwardness. In Siberia, a recent Soviet publication emphasizes that the Soviet system ensures the betterment of the material condition of Eskimo life and the "liquidation of cultural backwardness;" on the other hand, a British anthropologist writes of the Siberian Eskimos that, if they have gained materially, they have lost spiritually.

Hughes (1965) has considered a list of the situational forces generating and/or constituting sociocultural change, and hence productive of musical change in the north. Among them he includes changing Eskimo demographic features, declining animal populations, conjunction of differing cultures, new reference cultures, new pattern of selective valuation, new types of rewards, discriminatory practices, unemployment. He also lists psychosocial features such as confused self-identity, self-disparagement, alienative reference group behavior, skills inappropriate to new world, role patterns emulating White culture. Hughes also lists positive elements such as constructive adaptation, peer acceptance, new

economic opportunities, and innovative institutions such as educational and medical facilities.

Ethnomusicologically, positive elements are to be perceived in the creation of new musical performing situations such as the pan-Canadian Eskimo Northern Games, the Alaskan Eskimo Olympics, university Native arts festivals, the Fairbanks Annual Potlatch, the Alaska Native Brotherhood dances, paid performances for tourists, ethnic radio broadcasts, television appearances, World's Fair appearances, command performances before the U.S. President, performance for professional film-makers, the Easter, July 4th, Labor Day, Thanksgiving, Christmas, and New Year's Eve dances, performance for the Native Oral Literature Preservation programs, demonstration dances in rural schools, and performances for anthropologists and ethnomusicologists.

It is noteworthy that, in many cases, the work of the latter has stimulated Eskimo interest in their own musical product. Tapes, cassettes, and records often find their way back into the Eskimo communities originally responsible for the music, there to be used as mementos of the recorded singers now deceased, as funeral music for the singers' relatives who subsequently die, as the basis of newly established local folklore archives on the library shelves of the council meeting house, as program material for local broadcasts from school transmitters, and as source material for Native Arts and Crafts aides who teach dance and song in rural schools. There are thus multiple instances of unchanged musical form (musical form is a conservative element) within changed function (the social function of music is highly adaptive).

Regional variation in Eskimo musical behavior and in the social function of Eskimo music is apparent from the wide range of situational descriptions given for the various regions covered in the present study. Considerable regional variation exists in the *sound* of Eskimo music. In Greenland and Eastern Canada, the refrain-plus-verse form engenders a bipartite structure which lends itself to various call-and-response formats. In Alaska and Siberia, the refrain-plus-verse form is less common, the most popular form being a thirty- or forty-measure compact song which is sung first with vocables, and then repeated exactly using the real songwords. Within the song, sectionalized repetition is the structural base.

Some observers consider that, of all central Canadian Eskimo musical styles, that of the Copper Eskimo exhibits the most use of repetition and the highest level of predictability. In Greenlandic and Canadian Eskimo music, songs often consist of sequences of rhythmic values which defy organization at a level lower than sectional, while in Alaska and Siberia, a symmetrical rhythm, and hence the symmetrical placement of bar-divisions, is unerringly indicated by the regular beat of the large drumming ensemble. This beat usually consists of $\frac{5}{8}$, $\frac{7}{8}$, or similar interesting rhythm stressed in combinations of strokes valued at either two or three syllabic pulse equivalents. Leden considers East Greenlandic Eskimo rhythm to be more complex than that of the Smith Sound Eskimos on the northwest coast of Greenland.

If Greenlandic and Canadian Eskimo musical material is divided into meter-like units on the basis of melodic contour, these units consistently exhibit varying length within any given song. In Alaska and Siberia, the stable strophe and exact repetition is common. The multi-strophed narrative song containing much recitative is common in the east and rare in the west. Everywhere, songs-within-stories are shorter than dance songs, and juggling game songs are longer than both.

The basis of dance song classification varies greatly across the circumpolar regions, from the *pisik/aton* dichotomy based on dance style (Copper Eskimo), to the *sayuun/atuutipiaq* dichotomy based on whether fixed motions have been assigned (northwest Alaska), to the *arula/pualla* dichotomy based upon dance style (southwest Alaska).

Greenlandic and Canadian Eskimo song range is generally about a fifth or sixth, an exception being the Copper Eskimo who, according to Roberts' transcriptions, utilize a range of an octave or even a tenth. In Alaska and Siberia the range is commonly a twelfth, with the exception of the Lower Yukon, where it is frequently limited to a fifth or sixth. This is shown by the studies so far available.

Pentatonic and tetratonic scale use is common among all Greelandic and Canadian Eskimos except the Copper, where hexatonism, heptatonism, and even considerable chromaticism appear. In Alaska and Siberia pentatonism prevails, with some use

of two further scale members as auxiliary and passing notes. Everywhere there is additional use of accented grace-notes, glissandi, and elaborate microtonal pitches occurring within specific musical and verbal contexts, and affecting musical meaning. From the Copper Eskimo westward, songs occasionally exhibit a form of modulation or abrupt change of tonal center.

Everywhere, dance songs frequently commence with a short incipit of vocables on the reference tone. Melody is often arch-shaped, terminating in gradual descent and note-prolongation. In Alaska a common ending is prolonged repetition of the second lowest tone employed in the song. Large leaps of an octave or more may occur in the west, of a fourth or fifth in the east. Ascending and descending fourths appear as an integral part of melodic contour.

Everywhere, singing tone is nasal, strident, shrill, and in dance songs is accompanied by throat restriction, glottal pulsation, and diaphragmatic pulsation. In Alaska and Siberia, the latter two are matched to the pattern of the drumbeat of the drumming ensemble. Tempo is around MM 90-110 in Alaska and Siberia, slightly slower in the east. Nowhere is speech-tone significant as a factor in musical composition, and everywhere speech-rhythm is treated with a remarkable degree of musical unconcern. Everywhere songwords are concerned primarily with Eskimo interpersonal relations, and secondarily with the environment (the two are necessarily linked in the singers' perception of subsistence roles).

In most Eskimo dance songs, game songs, and songs-within-stories, the songwords function as an outlet for creative fantasy, commonly revolving around a hunter's success story. Use of the rhythmic vocables *a-ya-yanga* is circumpolar in distribution.

Extensive song diffusion is common in all regions: Jenness tracked and documented the diffusion of one Point Hope song all across northwest and north Alaska within the space of one year, and Nielson has found songs from East Greenland in West Greenland. Today there is much exchange of songs across even language barriers, such as that between the Yupik-speakers and the Inupiaq-speakers of Alaska. This song diffusion does not necessarily result in stylistic synthesis; the borrowers possess skills of mimicry, and

revel in demonstrating song-styles other than their own. Where genetic intermingling has occurred, stylistic synthesis is present, and Fredericksen reports the presence of Danish elements in certain Greenlandic Eskimo song material. Olsen maintains that musical compartmentalization is the more usual reaction to prolonged and extensive contact between Eskimos and Whites.

Estreicher's substantial comparative analysis of Eskimo music suffers from the weakness that only six Alaskan examples are given. All are atypical, none show drum accompaniment, and none possess the 5/8 rythm so common in Alaska. Too many investigators rely solely upon scale patterns and ignore difficult-to-transcribe rhythmic backgrounds. In many non-Western musics, the rhythmic accompaniment is a prime element without which the melody is meaningless. Estreicher postulates that Padlermiut Eskimo music is the purest style, being the 'simplest', and uses this as a basis for comparison with other Caribou Eskimo musics and with all other Eskimo musics.

Estreicher considers that while Caribou Eskimo music and, to some extent, East Greenlandic Eskimo music has been little influenced by the various waves of cultural diffusion of recent centuries, West and North Greenlandic Eskimo music together with Alaskan Eskimo music represent a second stage, one in which Paleosiberian and American Indian elements are found. A final developmental stage commenced with the Copper Eskimo and diffused to Alaska. Estreicher considers that, melodically, Copper Eskimo music exhibits some features of the first stage, but that rhythm and meter are more developed. The music of the Central and Smith Sound Eskimos exhibits a mélange of stylistic elements. In the case of both West Greenland and Alaska, Estreicher additionally perceives European influence. This assumption is probably justified in the case of the former region, where interbreeding has occurred for several hundreds of years. In the case of Alaska the assumption is unsubstantiated; all present evidence points to strict musical compartmentalization.

Estreicher postulates a Mongolian origin for some elements in Alaskan Eskimo music, though he is by no means specific. Here he may be on firm ground, for, as Larsen and Rainey have pointed out, the centers of advanced cultural development in the Amur River region, Manchuria, and even North China lie closer

geographically to Alaska than do the ancient centers of high culture in North America. Swadesh (1962) argues convincingly for a relationship between Eskimo-Aleut and the Chukotan language in Asia.

In all, Estreicher's analysis appears handicapped by a lack of representative sampling and by his lack of personal experience in the field. Rather than emerging out of musical evidence gathered in social context, his diffusion theory appears to rely heavily upon a projection of Eskimo cultural origins made by Steensby (1917) and later modified by Birket-Smith (1929:219-233). The latter hypothesizes that the Caribou Eskimo is the modern representative of the Proto-Eskimo who, at the time of historic contact, had combined the essential features of the lake-ice hunters with recent borrowings from coastal peoples to become the Eschato-Eskimo. The Proto-Eskimo migrated north to the coast, adapted interior hunting techniques to hunting on frozen polar seas, and gave rise to the Paleo-Eskimo, who expanded east and west. Neo-Eskimo culture developed from Paleo-Eskimo culture and is found today in Alaska and Greenland. This hypothesis takes little account of the widespread Eurasian distribution of typical historic Eskimo characteristics such as skinboats, sleds, lamps, and ground slate. On the basis of these latter, Collins postulates a Kara Sea and Bering Strait movement of peoples from interior rivers to arctic seas (Collins 1937:361-83). Rudenko points out that since neither toggle harpoon heads nor Eskimo cultural remains have been found to the west of the Kolyma River mouth, and since the shallow coastal waters to the west are uninhabitable for sea mammals, Eskimo culture could not have originated in northern Siberia. Its origin should be sought to the south of extreme eastern Asia (1961:176).

Looking in another direction, Borden points out that, prior to Eskimo times, there were important ties between the interiors of western Asia and the area now occupied by Northwest Coast Indian culture. Siberian Neolithic slate forms around 5000 B.C. are similar to those recovered in the Fraser River area 1000-100 B.C. Eskimo ground slate tools become dominant around 100 A.D. (Borden 1962:9-19).

All of these and other archaeological and anthropological data must enter into any search for Eskimo musical diffusion across the

circumpolar regions. Music never travels by itself as an isolated tonal phenomenon, but follows the paths of cultural diffusion. The completion of several major regional studies of Eskimo music now under way may serve to support or weaken anthropological diffusion theories. This is ethnomusicology's chief *raison d'être* — it is a unique tool for social science.

In a comparative study of Eskimo musics, Roberts finds that Alaskan Eskimo music is of richer melodic construction, much faster, and more complex than that to the east. She states that Point Hope songs are short, bear a resemblance to Mackenzie Delta Eskimo music, and possess none of the features of the Copper Eskimo *pisiks*. Mackenzie Delta songs are seen as being characterized by rich melody, rapidity, brevity, the prolific use of small note values, and the lack of verses, refrains, and connectives. They feature a fine balance of five or six melodic phrases.

Roberts finds that Baffin Island Eskimo melody is of freer construction than in Greenland, principally because of the recitative character of the latter. In her opinion, Copper Eskimo music resembles that collected by Thalbitzer in East Greenland, and subjected to analysis by Thuren. Copper Eskimo music contains chromatics for nearly every diatonic tone; in fact, a form of 'modulation' plays a great part in the Eskimo music of Alaska and the Delta. Roberts states that, in the Coronation Gulf Copper Eskimo region, certain scale effects are found which occur in no other Eskimo region.

In a comparison with American Indian musics, Roberts comments that the feeling for a tonic does not appear to be as well established in Eskimo music as in that of the former. Copper Eskimo melody is instead characterized by interplay between *two* tonal centers, the main one of which appears as E when all of the songs are transposed into a treble staff without sharps and flats. Concerning Copper Eskimo intonation, frequent interplay between 'off-pitches' and 'true pitches' occurs. Roberts' assumption that pitch variance is due to the indigenous lack of musical instruments possessing stable tuning, is ethnocentric and probably incorrect. It is more often the case in non-Western musical instrument use, that instrument tunings are modelled after the established principle of communal vocal music, which, because of its social and integra-

tive function and importance, furnishes the pitch norms for other musical media.

Nettl places Eskimo music within a broad musical classification labelled the Eskimo-Northwest Coast area, characterized by the use of rythmic complexity and the use of recitative-like singing. The latter, however, occurs mainly in Greenland and Canada. In Alaskan Eskimo music, recitative occurs mainly in connection with songs-within-stories, and only briefly.

For the last several centuries, Alaskan Eskimos and those of the Canadian Mackenzie Delta have been in the unique position of being able to draw upon rich musical heritages from the south, west, and east. This is facilitated by their proximity to Northwest Coast Indian culture, with its rich subsistence resources and hence highly developed art forms in sculpture, weaving, and ceremonial music. It is facilitated by the receding of the winter ice during the spring break-up, permitting accessibility to the Siberian coastline and the rich conglomerate of ancient Asiatic cultural influences which abound there (it should be remembered that discrete Eskimo morphological traits such as the mandibular torus and the keel-shaped vault place these peoples within the major Mongoloid group). It is facilitated by the unique role of the Alaskan and Delta Eskimos as the aboriginal transmitters and subsequent periodic receivers of great cultural movements such as that of the Birnick culture, which spread eastward across the circumpolar regions shortly after 1000 A.D., carried by the Thule people, later to return to Alaska by the same route. The culture was characterized by permanent houses of stone and whalebone, summer conical tents, soapstone dishes, and, probably, a distinctive musical system.

The Siberian Eskimo benefitted from the same ancient cultural developments and exchanges. That there is little present-day musical evidence of this fruition is due partly to the construction of the trans-Siberian railroad, which brought the east within administrative distance of European Russia. Furthermore, the Soviet administration, anxious to achieve national solidarity in the face of international hostility to its regime, introduced an assimilative policy which ensured the decline of distinctive cultural elements such as Eskimo traditional music, which underline separate ethni-

city. The surprising exception to this policy of elimination of ethnicity was, for a long time, the Siberian Yupik language.

By way of contrast, the relative geographic isolation, late contact date, and benign governmental neglect in the United States facilitated the survival of many Alaskan Eskimo musical forms. White settlement in Alaska during the twentieth century, with its new social, religious, and economic values, wrought havoc with the activities of the shaman-drummer and with other aspects of the musical system. During the 1970's, and following a long period of self-searching, Native society in Alaska made a dramatic reversal of its social and political goals, from one of assimilation to one of biculturality, represented most overtly by the enactment of legislation for territorial autonomy and for bilingual education, both of which engendered a renewed emphasis upon traditional musical performance.

The extensive cultural and genetic merging witnessed in West Greenland has given rise to new musical forms from which, in the opinion of many, the best qualities of Eskimo music — rhythmic complexity and tonal nuance — have been filtered out in favor of a rather bland copying of European folkmusic forms. The indigenous music of the region lives on only in the revivalism and re-creations of groups of interested folkmusic aficionados, in whose performances the most essential aboriginal element — that of functional and seasonal appropriateness — is absent. Distance and political considerations prevent the traditional hunters of Thule and of Angmagssalik from emulating the vital, exciting musical developments occuring in Alaska today.

A probable future stage in Canadian Eskimo music may be predictable from the Alaskan experience. Early manifestations are visible in the popularity and spread of such emblematic, unifying musical rallies as the pan-Canadian Eskimo Northern Games. For the Siberian Eskimo, knowledge of the very existence of such events is totally denied; the walrus hunters, reindeer herders, and fishermen all labor under the illusion that other Eskimo populations are undergoing either Soviet-type cultural amalgation or various forms of imperialistic oppression. Soviet policy in the past has often reversed itself as the winds of reform sweep around the global village. The Soviet's involvement with the

Third World may stimulate afterthoughts concerning the validity of small but highly unique and illuminating cultures such as that of the Eskimo.

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Quelques légendes montagnaises

SUMMARY

Legends are a good means of understanding a people's mind and beliefs. The Canadian Indians have many interesting legends and it seemed that some contained in the *Diary* of the Catholic mission of Reindeer Lake deserved to be known. Following are two narrations concerning deluge and one on the origin of mosquitoes.

Une façon de pénétrer dans l'esprit d'un peuple et de saisir ses croyances est de considérer ses légendes. Les Indiens ont de nombreuses légendes qui sont d'un intérêt particulier et il nous a paru que quelques récits contenus dans le *Codex historicus* de la mission catholique du Lac Caribou méritaient d'être connus.

Ces légendes, copiées à la fin du *Codex* qui couvre la période 1891-1902 semblent bien l'avoir été par le père Alphonse Gasté o.m.i., missionnaire au Lac Caribou durant quarante ans (1861-1901). Il ne peut s'agir de son successeur, le père Marius Rossignol qui n'arriva à la mission qu'en 1900, alors qu'on le verra le copiste affirme explicitement qu'il s'agit de légendes recueillies depuis plusieurs années. La calligraphie semble aussi indiquer qu'il s'agit du père Gasté. Quoi qu'il en soit, les légendes ont un intérêt par elles-mêmes.

Le père indique d'abord au haut de la première page "D'après Jany", sans doute un indien. Puis il continue:

> Ces récits se rapportent à une époque antérieure à l'homme. Les animaux seuls entrent en scène. On les fait parler, agir, se concerter tous ensemble tout comme des hommes.
>
> D'autres légendes disent comment, dans la suite, l'homme a su arracher aux animaux tous leurs secrets, les réduire en esclavage. Apparaît ensuite quelque sauveur extraordinaire. Maître des éléments et de toute la nature, rien ne lui résiste, il a un pouvoir surhumain, et c'est lui qui sauve la nation à maintes reprises d'une destruction complète. C'est la fable pure et simple.

Sont-ce là de pures inventions de poètes primitifs? Point de trace de poésie chez ce peuple.

Ou bien sont-ce des traditions nationales dont le fond repose sur q.q. antique croyance et les détails vont toujours changeant de générations en générations. Les érudits devront décider. Certains contes semblent évoquer l'idée de quelque croyance antique, de qq. événement extraordinaire des temps primitifs. La tradition orale les a altérés comme il arrive chez tous les peuples. On y retrouve partout des traces de la vie actuelle des Montagnais — la chasse aux caribous, la pêche, la vie nomade sous la tente en peaux de caribous, la neige, etc.

Un fait curieux est que la plupart de ces contes tendent à expliquer tout — le froid excessif de ces pays du N[ord] à l'exclusion des autres contrées, la g[ran]de neige, l'abondance du caribou, des maringuouins, etc., etc.

Q.q. contes entendus lors de mon 1er voyage m'avaient fait presque croire à une période d'ancienne civilisation en ces pays. Mais je ne comprenais pas assez la langue & espérant visiter les lieux indiqués, ce que malheureusement je n'ai pu faire, je n'observerai rien encore à ce sujet.

Venons maintenant à l'une des plus intéressantes traditions (ce conte me paraît en effet être plutôt une tradition). Il s'agit de la tradition orale du déluge.

LE DÉLUGE

C'était[1] au temps antérieur à l'homme. L'eau peu à peu inondait la terre [et] montait, montait toujours. La mort menaçait de tout atteindre. "Sauvons-nous sur les montagnes, criaient les uns, nous mourront certainement ici, & les autres incrédules ne voulaient rien entendre. Cependant l'eau montait toujours. Elle atteignit les plus hautes montagnes. Tout être vivant allait alors périr. Mais voici qu'une île de moyenne grandeur flotte[2]. Des animaux, des oiseaux, & tout ce qui se meut sur la terre (un mâle et une femelle de chaque espèce différente) s'y réfugièrent et là attendirent la fin des eaux.

Le salut leur vint d'un canard appelé "an aulik"[3]. An aulik plonge donc à la recherche de la terre. Les huards et tous les au-

[1] Le père écrit entre parenthèses "comme je l'ai dit".

[2] "On ne sait ni d'où, ni comment ni pourquoi elle flottait de ci de là comme pour recueillir les malheureux." (note du père dans le texte).

[3] "Ce nom est purement imitatif, exprime bien le cri de cet oiseau et n'a point d'autre signification par ailleurs. Ce fameux canard de miclon (Hareddas glacialis)."

tres plongeurs l'imitent, puis reparaissent bientôt. Ils n'ont rien trouvé. An aulik cependant ne paraît point. On l'attend. Que lui est-il donc arrivé?[4]. Enfin voici notre an aulik. Il est à bout de souffle. Il est presque sur le dos. Les 2 pattes en l'air, car il a plongé bien longtemps, ses pieds sont pleins de terre. Ayant repris haleine, il plonge encore, mais alors son absence est de moins longue durée et cependant ses pieds sont encore pleins de terre. Tous plongent une 3e fois avec lui et bientôt il semble que l'île flottante repose sur un sol ferme. Tout autour apparaît le continent. An Aulik avait soulevé la terre au dessus des eaux. Le monde animal était sauvé[5].

LE DÉLUGE

Un jour l'ours et l'écureuil étaient en dispute. D'après l'écureuil les roches devaient aller au fond de l'eau et le bouleau surnager.

L'ours lui, au contraire, sachant que le bouleau servirait d'embarcation à l'homme futur, voulait envoyer tous les bouleaux au fond de l'eau & faire surnager les rochers.

La discussion s'aigrit. Peu à peu l'écureuil trouve nombre de partisans, de quoi l'ours fâché s'écrie:

Eh bien soit. Je suis contrarié sans cesse, alors je vais faire la nuit; il n'y aura plus de jour.

L'écureuil — Imbécile, mais tu seras toi-même le premier attrapé. Comment feras-tu pour chercher ta vie?

L'ours — J'irai à tâtons et palpant toutes choses avec mes mains je connaîtrai quoi manger.

L'écureuil — Tu te perceras les mains dans les buissons.

L'ours — Je puis flairer du nez.

[4] "Un (?) ne manquerait pas de dire que les minutes étaient pareilles à ce moment solennel d'où dépendait le salut du genre animal".

[5] Le père ajoute dans le texte: "Eh bien laissons le fond de côté, l'inondation générale, le déluge en un mot dont il put y avoir trace de souvenir ici, il est certain que cet an aulik nous fait l'effet d'un fameux canard. Et bien voici plus fort encore. Il s'agit d'un autre déluge moins terrible que celui-ci et par suite plus compliqué et plus fabuleux." Il donne ensuite un deuxième conte sur le déluge.

L'écureuil — Tu t'écorcheras le nez.

L'ours — Je me roulerai sur la terre et mon nez sentira.

L'écureuil — Tu te crèveras le ventre sur les bouts de troncs.

Alors tous d'applaudir l'écureuil. L'ours se tait. Immédiatement les roches pleuvent sur le lac et plongent jusqu'au fond.

L'ours au comble de la colère: Voyez ce lac devant vous; quand je serai rendu au bout, vous connaîtrez à qui vous avez affaire. Et il s'en va par les endroits déboisés. L'écureuil se lance du côté du bois fort sautant d'arbre en arbre arrive le premier et chante pouille au malheureux ours. Celui-ci crie et jure comme un démon et continue sa marche. Bien des jours s'écoulèrent.

La chaleur n'existait plus. Un brouillard épais qui se transformait en neige enveloppait toute la terre et la neige augmentait toujours. Tous les animaux, seul l'our manquait, commençaient à geler. Ils allument un grand feu et les voilà tous en rond se réchauffant pieds et mains engourdis par le froid. L'écureuil, lui, couché tout près du feu, était proche de brûler. Déjà il avait le dos roussi. On le pique, mais il est insensible. On le pousse un peu plus loin. Enfin, il se réveille de lui-même, prend la parole et dit:

"J'ai rêvé de l'ours. C'est lui qui a pris la chaleur et la garde. J'ai vu sa demeure, partons." Et l'écureuil au dos roussi (c'est depuis lors que tous les écureuils ont le dos couleur rousse) quitte la troupe. On alla loin, bien loin du côté de l'ouest, on quitta ce pays-ci pour entrer dans un autre. On arriva enfin à une place magnifiquement aménagée pour un affut d'été à la chasse au caribou en canot. C'est là que c'était réfugié l'ours. On se concerte. "Toi lynx, appelle les caribous et commande leur de venir de suite, et toi, souris si petite qui sais si bien te fourrer partout à l'insu de tous, va au bord du lac, ronge l'aviron là où commence la palette, qu'il casse au moindre effort. Ainsi fut fait. Le lynx d'appeler les caribous, et la souris de ronger l'aviron de l'ours chasseur. Voici bientôt les caribous traversant à la nage. L'ours les a flairés, va à son canot, le met à l'eau, prend l'aviron et rame droit au gibier. L'aviron résiste. "Ah! ah! dis donc vilaine souris, je te casse la tête, tu n'as pas travaillé comme on t'avait dit; l'aviron ne casse pas." Et la malheureuse a si peur de mourir que ses yeux tout

bleus lui sortaient de la tête. (C'est depuis lors que ses petits yeux lui donnent toujours un air si effaré).

L'ours, cependant, ramait toujours et approchait du caribou. Il rame à tour de bras. Soudain l'aviron casse, le canot verse et voilà mon gaillard qui se baigne à contrecœur. Pendant ce temps, chacun avait voulu voir la demeure de l'ennemi. C'était comme une grosse boule énorme et mystérieuse. On n'avait jamais vu chose pareille. Que peut bien être ceci? Les oursons trahirent le secret. "C'est la chaleur que notre père a ramassée là." Et eux se nourrissaient évidemment de la cendre qui en tombait. Aussitôt on vole la chaleur au malheureux ours qui nage toujours vers la terre. On l'emporte, on se la transmet de l'un à l'autre et on revient en son pays. Les deux derniers qui la transportent furent la loche qui se traine depuis longtemps et son confrère le brochet. Mais celui-ci, les dents trop pointues, la creva de suite (et c'est pourquoi on le regarde depuis comme le dernier des poissons). L'ours qui, de sa demeure, suivait toutes les péripéties des voleurs s'écria alors: "Désormais, jusqu'à la fin des temps, la chaleur et le froid se succéderont tour à tour." Et voilà comment depuis l'hiver alterne toujours avec l'été et c'est merveilleux que l'ours s'endort tout l'hiver sans remuer, ni manger, ni boire, et quand il veut sortir de son antre la chaleur revient sur la terre avec le printemps.

Le brochet donc avait déchiré la boule de chaleur. Aussitôt l'atmosphère devint embrasé toute la neige amoncelée depuis si longtemps se fondit. De là un déluge. La terre paraissait encore par place pourtant. Mais une telle quantité d'eau était fort gênante. Or voici qu'un immense oiseau le "Tulkkuzhi"[6]. Il but toute cette eau qui couvrait la terre et voici un nouveau péril. Tous mourraient de soif.

Le Tulkkuzhi était là cuvant son eau et ne remuant pas plus qu'un mort. Chacun l'entoure et voudrait bien lui ouvrir le ventre. Mais on n'ose le faire. Si on allait manquer! On le flatte, on le remercie, on le flatte pour détourner son attention. Pendant ce temps on appelle le lynx. "Rien ne résiste à tes griffes pointues, va et ouvre-lui le ventre que nous ne mourrions pas de soif. C'est pour toi comme pour nous."

[6] "Son nom n'a point de signification en montagnais et on ne sait ce qu'il était, d'où il venait."

Le lynx s'approche, fait patte de velour, écarte les plumes, et caresse toujours. "Oh que les mains de mon petit fils le lynx sont douces," roucoule Tulkkuzhi riant. Il n'avait pas achevé ces mots que l'eau coulait en abondance. Le lynx lui avait percé le ventre. Mais cette fois, l'eau n'inonde pas la terre. Elle forme des rivières, des lacs comme ils sont aujourd'hui. Et depuis il n'y a plus eu de déluge[7].

LES MARINGOUINS

Il y avait une fois un géant qui s'appelait Ozhin'tezhe[8]. Cet homme était sorti de terre, on ne sait trop comment les hommes le tuèrent. Ils hachèrent son corps en mille petits morceaux qu'ils envoyèrent dans l'eau, car ceci se passait au bord du lac. Les poissons avides se saisissaient de ses chairs. La peau du ventre fut jetée la dernière et ce fut la loche qui s'en empara. Le crâne cependant était resté sur le rivage, et voilà que d'eux-mêmes ses ossements se remuent. Le crâne roule, roule toujours. Les hommes impatientés le jettent finalement à l'eau, mais il revient à terre. On le jette encore à l'eau, et il revient encore. Alors quelqu'un se met en tête de l'écraser. Aussitôt il en sort des milliers de maringouins. Le ciel en est couvert et le jour obscurci. Ils se répandent dans tout le pays et tourmentent depuis tous les Montagnais. Or celui qui avait broyé le crâne a succombé aux morsures des maringouins. Il ne put jamais les éloigner de sa personne ni par la boucane, ni par aucun autre moyen, et ce fut ainsi qu'il mourut.

Et depuis lors, il y a des maringouins dans les pays du Nord et les peuples des autres contrées n'en sont point tourmentés.

[7] "Et moi je pense que c'est heureux, car si la 2ème histoire ne parle pas de canard, elle me paraît à elle même un fameux canard Montagnais, comparable tout au moins au fameux "An Kaulik de la 1ère."

Dans une lettre à à sa mère, le 4 janvier 1851, le père Alexandre Taché, o.m.i., missionnaire à l'Ile-à-la-Crosse, parle en quelques mots des croyances des Montagnais: "Dans l'histoire de leur déluge, ils remplacent l'arche par une petite île flottante, sur laquelle quatre personnes, des animaux et des oiseaux trouvèrent leur salut et échappèrent à la ruine générale. Une pareille tradition, trouvée chez un peuple infidèle au dix-neuvième siècle, étonnerait, je suppose, l'ignorante incrédulité des philosophes du dix-huitième" (*Rapport sur les Missions du Diocèse de Québec,* 10 (mars 1953), p. 10).

[8] "Nom dont on ne connaît pas la signification."

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