# Non-Equilibrium Models in New Guinea Ecology

## Possibilities of Cultural Extrapolation[1]

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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.

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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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