

# Hope Against Hope

*Michel Panoff*

Poor ethnology, never where it should be! One could almost believe that in the intellectual comedy it is always condemned to play the role of the incorrigible blundering fool.

Take a different view. Thirty years ago it was used for any job going, the indispensable commodity of the cultured milieu of the period. The opinion-shapers perfidiously reduced it to structuralism, which was then running out of steam and scarcely intimidated them any longer. Encouraged by this decline, they affected to believe that 'structures' had an empirical existence in society, indeed absolutely anywhere their limitations had made themselves felt for far too long, and that ethnology finally won its freedom at the same time as the various changes in custom and social practice which took place as a result of the huge emancipation movement of May 1968. At once ethnology, the sacred instrument of access – or a smattering of it – was to be found in the media, in the professional training of nurses, the therapies of Bonneuil, the fashion for water-bath deliveries, the electoral campaigns of informed politicians, the 'spin-offs' of the Second Vatican Council, and so on. Today, on the other hand, it is to be found nowhere at all, not even among ethnologists, who appear with any intellectual label whatever, but above all not as practitioners of a science. They are the new romantics, the new adventurers, the incredible travellers, all having to do no more than produce a strictly personal experience, or at least states of mind. In short, ethnology passed itself off the first time as a kind of panacea, and the next it was declared no use for anything whatsoever, being at best a pretext for narcissistic effusions and at worst a snare for the naïve. The more they would have us believe it can do everything, the more nothing can be done for it.

If ethnology thus happened to be perceived through two such manifestly opposite images, this was surely because each time it was the victim of a misunderstanding effectively maintained by certain ideologues.

It must first be recognized that in these two cases the ethnologists had their share of responsibility. There were too many of them to imagine that their vocation was in all simplicity to contemplate and express society, the world and the spirit, no less like the philosophers of the past. Or media commentators proclaimed it in their name and they allowed themselves to be persuaded. It was this hegemonic temptation which caused their fall. It was this which, having mortified them when the moment was passed, now pushed them into a resigned position. In the field of the natural sciences, what would be said of a botanist who entered upon his or her career with the sole aim of becoming the new Darwin or the new Wallace and who, out of pique, abandoned research after having toiled for several years in the obscurity of a subordinate position? Wallace or nothing: such a catch-phrase would produce general hilarity!

Another worrying sign of the present state of anthropology is the frequent recourse to the *notion* of identity – the lack of rigour in the writings of the users precluding the word ‘concept’. This notion has the same explicative value as the ‘soporific virtue of opium’ in Molière and acts as padding for uninspired versifiers. Too often authors invoke identity or ethnicity in order to be able to proclaim: ‘It is like this’, and thus dismiss a phenomenon which they are supposed to analyse. Besides its underlying idleness, a fairly unscientific characteristic, the process has the serious disadvantage of ignoring the dynamic of history and its shaping effect, since it concentrates attention on one state of things, indeed on an essence. Moreover, by having recourse to this, one acts as if the group studied were homogeneous and as if a perfect harmony reigned there. Put differently, internal conflicts and even divergences of factional interests are skirted round. This is an unscientific attitude the consequences of which are eminently anti-scientific, as is immediately apparent: dissent is supposed not to have existed or it is reduced to anecdote, and as for the possible division of society into classes, the question is not to be asked, as the judges said in the Dreyfus affair!

Happily, outside fashionable movements and stentorian or honeyed proclamations, some intellects escaped the reigning scepticism and endeavoured to have the scientific status of ethnology recognized. They are sometimes found in unexpected places. There are many paths or, more modestly, many opportunities for achieving this result. Among them, the practice of applied research deserves especial interest, first, because many ethnologists are engaged in it at one time or another in their career and, secondly, because it enables verification, if only indirectly, of hypotheses formulated by theoretical anthropology.

Everyone knows this well who has been involved, if only minimally, in the development programmes in Africa (cotton-growing at the bend of the Niger river, the ‘Arachide Plan’ in Tanzania, revitalization of the *fokonolona* in Madagascar, and so on). Whether these programmes were successes or failures, there was experimentation of a kind, even if the ethnologist clearly could not check all the original hypotheses nor follow all the subsequent reactions in the way a laboratory researcher would do. In fact, an ultimate failure of the technicians who wanted to carry on regardless of his or her warnings indirectly validates his or her knowledge. And the same applies if, conversely, the target population responds and makes a success of a social engineering operation which he or she has approved. For all that it is rare, this last case is not unprecedented.

Much more frequent are the instances where pure research receives impetus and substance from applied research without experimental verification at the end of its term. Here is a personal example which did not comprise social engineering. It concerned an enquiry which aimed to clarify land-rights in French Polynesia. The situation which I found there in 1961 and which appeared to warrant an investigation in applied ethnology was as follows:

1. Independence movement threats hung over the Territory despite, or because of, the sentencing of the Polynesian leader, Pouvanaa, and his imprisonment in France. Having made him appear a terrorist, his enemies were afraid that his supporters would succeed in making him a martyr locally.
2. The economic development by which the government expected to wrest the Polynesian population from the attractions of independence was visibly stagnating. In particular, a certain number of businessmen who were also influential members of the local

- Gaullist party were handicapped in carrying out their schemes by the difficulty of purchasing land. The local notaries echoed this and attempted to circumvent the obstacle. Whence the multiplication of court proceedings and bad feelings all round.
3. The Paris government was secretly preparing to establish a base for new nuclear tests at Mururoa and thus wanted to secure a minimal agreement in Polynesia beforehand. Of course, these ulterior motives were only revealed two years later.

In brief, the authorities set themselves the task of bringing the Polynesians round to a more 'rational' attitude towards their land, whilst reducing tension and depriving the independence movement of their best arguments.

The real questions about the land had nothing to do with those which high-ranking officials and local élites asked themselves<sup>1</sup> and which were expressed in these terms:

- What were these poor 'natives' (*indigènes*)<sup>2</sup> lacking before colonization to give themselves a juridical system as good as ours? And why, after conquest by France, were they incapable of adapting to the Napoleonic Code which we had paternalistically brought them and which should have answered their needs?
- Who is bad? Who is stupid? Are the 'natives' bad to refuse the economic development for which the French State has supplied the means; are the European experts stupid to want the good of the colonized?
- The final and most urgent question: how will this misunderstanding affect the next elections in the Territory? Is it not going to give impetus to the 'anti-French' cause?

The real question, which no one in the business world and among the representatives of the French State gave a thought to, was to be expressed in strictly converse fashion, not as a question of fault but as a positive question. Namely, what were the merits of traditional land-tenure which gave it a lasting superiority over French law? Instead of seeking out the gaps in the indigenous system and defining at all costs the claimed intellectual handicaps of Polynesian thought, it was necessary to rediscover beneath the veneer imposed by acculturation<sup>3</sup> rationality and the worth of local practices. And to understand why, despite the efforts in education and the enticing promises of the 'developers', the population at large persisted in preferring the solution of their ancestors. Once this positive perspective had been adopted to initiate the enquiry, a whole tangle of social practices were going to unravel. At first, more-or-less spontaneous acts of solidarity, familial organization and various kinship ties came one after the other within the field of enquiry. Status positions, the ideology of the chiefdom and reference to primogeniture were to follow and prove mutually illuminating. And finally, there came to light in Polynesia a delicate system of counter-balance between political power and economic power, the crowning of the old 'social contract'.<sup>4</sup> From being an apparently very technical matter of the inadequacy of land law implying countless practical disadvantages, it can easily be seen how fundamental research could be stimulated by an enquiry which was supposed only to reach a prompt conclusion and to remain at the service of the rich and powerful. And how they were to profit from it! This is a case where anthropology was to demonstrate itself a science, despite itself.

A second way for anthropology to achieve recognition as a science is comparativism.

For Guille-Escuret,<sup>5</sup> for example, it was the realization of a comparativist programme, in search of cultural variables in the interior of a vast African region, which resulted in

fundamental research on cannibalism, perhaps the first worthy of the name on such a subject. The research was not in the least foreseen at the outset. In revealing fashion, it came up first against the repugnance of the researcher, who did not care to embark upon a study which because of wilful ignorance, prejudices and passions seemed for a long time to have eluded any scientific approach. Afterwards, by contrast, there was the inevitable adjunct of the stimulus provided by militant fervour, when the hope of challenging the various interpretations of cannibalism as well as the traditional position of the West *vis-à-vis* this subject prevailed. One could say almost as much for Mintz's exceptional synthesis,<sup>6</sup> which simultaneously ranges over food and power, the exploitation of labour and colonial imperialism, anthropological ambition and everyday life.

But to take heart once more, nothing compares with consideration of the cross-fertilization that has taken place between ethnology and the fields long established as the natural sciences. In this encounter the 'scientificity' of our discipline is, moreover, not bestowed as the paradoxical fruit of an activity with an entirely different focus. It is attained after conscious targeting by researchers active in the two disciplines who have two twin research programmes progressing in step, the results obtained by one being of direct advantage to the other and vice versa. The model here is Barrau's article<sup>7</sup> which demonstrates how economic globalization and new consumer lifestyles involve ever-increasing uniformity in food, which in its turn multiplies desires for taste and stimulates an excessive recourse to spices and flavours by way of compensation. Everything is there which an anthropologist wanting to take their discipline seriously could desire: a holistic vision of phenomena, the sense of a historic depth and above all a *concern to explain*. Moreover, it is remarkable that the 'symbolic', to use the jargon, is very important in this work. The same goes for Mintz's initiative<sup>8</sup> which does not remotely sacrifice the non-biological and non-economic aspects of food. Accordingly, he can justifiably condemn the narrowness of Douglas,<sup>9</sup> which is unfortunately confined within this symbolist obsession which was long the distinguishing mark of ethnological dandyism.

In respect of this weakness, the study of food-producing plants appears particularly heuristic, for the object to which it is directed is not only good for eating but also good for thinking, which is something which should attract even the anthropologists who are keenest on systems of classification and representation. Dinh's researches showed this, notably in his article of 1985–6 on the villages north-west of Hanoi: he makes use of the entire anthropological discipline in examining the composition of the flora of gardens and orchards.<sup>10</sup> Likewise, it was by examining the botany of cultivated plants, horticultural techniques, the transmission of knowledge, soil yields and the destination of harvests that F. Panoff was able to base her sound research which examined the division of labour between the sexes, agrarian magic and the position of women in New Britain.<sup>11</sup> The approach was successfully repeated by Kahn for another Melanesian population.<sup>12</sup>

Finally, anthropology has on many occasions been able to demonstrate its own scientific requirements when researchers have decided to 'put in context' such and such a relevant phenomenon or such and such a factual description borrowed from other disciplines (history or economics, for example). It is then obvious that militant engagement can be seen to stimulate or focus research, since the relevance attributed to an isolated phenomenon or the interest in facts described by other experts does not drop out of the sky.

However, it is clear, even if it remains implicit, that this putting-into-context is nothing other than the approach of anthropology itself. In the presentation of a collection of exotic

– a pressing question brought to mind by the creation of a gallery of primitive art in Paris  
– objects it is this too that mainly distinguishes ethnographical museums from museums of fine art. By setting the context, anthropology actually responds simultaneously both to Mauss's concern for the total social fact and to the militant motivations of the intellectual desirous of making sense of data which appears purely technical or juridical, and as such incoherent or pointless. Bureaucratic routine abounds in practices of this kind. But in reality, they deserve more and better than a shrug of the shoulders.

Brass demonstrates this well when he analyses the working conditions of coolies, their contracts and the judicial position of their masters.<sup>13</sup> By reintegrating these arid facts into what is known of the economic history of the nineteenth century, he succeeds in making an anthropological study of the Queensland plantations. Since the author does not hide his convictions with regard to colonial capitalism, there is no doubt that he did not choose his subject at random. He aims to prove, in particular against the Australian revisionist school, that strong coercion was required for the system of plantations in Queensland to function and that manpower was not recruited in a free market. His motivation, which the naïve might call partisan, far from making his work flimsy on the contrary renders it more scientific. First, because it pushes him to gather the maximum amount of information when an uninvolved researcher would perhaps limit him- or herself to the minimum required by academic practice. Secondly, because his conclusions are inevitably submitted to a rigorous scientific debate, as is the rule with biologists and physicists. The result: instead of being accepted with indifference by his peers, his research will instigate further research with a view either to refutation or to a deeper understanding and, whatever may transpire, science is always the winner.

This positive judgement is confirmed by observation of the great strides made by the most 'naturalist' of the authors cited above. What a path taken by Barrau from his article in 1965 to that of 1979!<sup>14</sup> The first, concerned with the cultivation of tubers, contains only surreptitious references to the Melanesians and their society, whilst the second accords as much space to socio-cultural realities as to botanical and agronomical themes. Quite obviously, the critique of levelling capitalism is responsible for the importance played by anthropology in his study. Similarly, it is solidarity with his people that pushes Dinh to denounce, despite the risks incurred, the ecological disasters caused by the policies of the Vietnamese régime and gradually to give priority to anthropology in his research: witness especially his text of 1990.<sup>15</sup>

After this general survey, it will be seen that there are good reasons for hoping that the scientific status of anthropology will be accorded more recognition in the future, although on condition that the debate once more finds a place in our discipline considered the equal of other intellectual undertakings. The overriding necessity of doing so is self-evident when it is a case of encounters between anthropology and the natural sciences or the relations between applied research and pure research. But it is no less true of militant intervention. We should not allow ourselves to be misled here by the appearances which bad practices, the practices of pusillanimous respectability, have allowed to spread across the scientific field like a sickly sweet whitewash. In fact, worst of all would be the persistence of the limp consensus which anthropologists believe it smart to seek and preserve in the interest of their precious studies, as well as for the good name of their professional milieu. Underneath it all we detect the idea that controversy is the pet sin of philosophers and politicians, that it is the legacy of a pre-scientific level of intellectual activity. Only

minds incapable of rigour and ready to be swayed by the passions would be so weak as to quarrel. To be taken seriously, and to be worthy of admission into the community of scientists, ethnologists must, it seems, put an end to this infantile malady. This idea is wrong and reveals either ignorance or scientific hubris: all science, however 'hard', necessarily progresses through numerous debates, and thanks to them. To confine ourselves to well-known recent examples, we might cite the battles over the recognition of quantum mechanics, the debates among geologists concerning plate tectonics, between astronomers and cosmologists over supernovae or among biologists about the origin of asthma. And even mathematics, most erroneously considered as the paragon of scientific disciplines, has not been unaffected by specialist debates, as was notably the case over the years with Fermat's celebrated theorem.

We should make up our minds to the fact that anthropology is no sinecure, even if it is not the most dangerous sport the saints might dream up.

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[translated from the French by Juliet Vale]

## Notes

1. Michel Panoff (1966). Un demi-siècle de contorsions juridiques: le régime foncier de Tahiti de 1842 à 1892. *Journal of Pacific History*, **1**, 115–128.
2. The use of the word was current at the time.
3. At the time this word began to spread among people who were well read and who practised a kind of 'political correctness' before its time.
4. Michel Panoff (1970). *La terre et l'organisation sociale en Polynésie*. Paris: Payot.
5. Georges Guille-Escuret (1998). Le corps du délit et l'exotisme artificiel: à propos de l'anthropologie guerrière en forêt centrafricaine. In M. Godelier and M. Panoff, *Le corps humain: supplicié, possédé, cannabalisé*. Amsterdam: Éditions des Archives Contemporaines.
6. Sydney Mintz (1991). *Sucre blanc, misère noire: le goût et le pouvoir*. Paris: Nathan.
7. J. Barrau (1979). Essai d'écologie des métamorphoses de l'alimentation et des fantasmes du goût. *Information sur les Sciences Sociales*, **18** (3), 421–435.
8. Mintz *op. cit.* (1991).
9. M. Douglas (1972). Deciphering a meal, *Daedalus*.
10. Trong Hiêu Dinh (1985–6). Habitations vietnamiennes, *Cahiers d'Études Vietnamiennes*, special issue, 90–136.
11. F. Panoff (1972). Maenge gardens: a study of Melanesian relationships to domesticates, Ph.D. dissertation. Australian National University: Canberra.
12. M. Kahn (1986). *Always hungry, never greedy*. Cambridge: Cambridge University Press.
13. T. Brass (1994). Contextualizing sugar production in nineteenth-century Queensland, *Slavery and Abolition*, **15** (1), 100–117.
14. J. Barrau (1965). L'humide et le sec: an essay on ethnobiological adaptation to contrasted environments in the Indo-Pacific area. *Journal of the Polynesian Society*, **74**, 329–346.
15. Trong Hiêu Dinh (1990). Environnement, homme, société: impacts du modèle socialiste au Vietnam. In *Ho Chi Minh, l'homme et son héritage*. Paris: Voie Nouvelle, pp. 230–278.