
Some Major Problems in the Social Anthropology of Hunter-Gatherers¹

by Alain Testart

What is the relationship between the present-day hunter-gatherers studied by anthropologists and the societies of the Palaeolithic? And how is the articulation between the economy of these societies and their other aspects to be conceived? In attempting to answer these questions, this article takes into account a further problem, that of the uniqueness of Australian Aboriginal social organization.

ALAIN TESTART is Directeur de Recherche, deuxième classe, of the Centre National de la Recherche Scientifique (mailing address: Maison des Sciences de l'Homme, 54 boulevard Raspail, 75270 Paris Cédex 6, France). Born in 1945, he was educated at the Ecole National Supérieure des Mines de Paris (diplôme d'ingénieur, 1968) and at the Université de Paris VII (doctorat de troisième cycle en ethnologie, 1975). His research interests are the social organization of the Australian Aborigines, the social anthropology of hunter-gatherers, and symbolism. His publications include *Des classifications dualistes en Australie: Essai sur l'évolution de l'organisation sociale* (Paris and Lille: Editions de la Maison des Sciences de l'Homme and Lille III, 1978), *Les chasseurs-cueilleurs, ou l'origine des inégalités* (Paris: Société d'Ethnographie [Université de Paris X, Nanterre], 1982), *Essai sur les fondements de la division sexuelle du travail chez les chasseurs-cueilleurs* (Paris: EHESS, Cahiers de l'Homme, 1986), and *Le communisme primitif*, 1, *Economie et idéologie* (Paris: Maison des Sciences de l'Homme, 1985). The present paper was submitted in final form 19 VI 87.

Two major questions present themselves in the social anthropology of hunter-gatherers. These questions do not overtly shape the studies of researchers in the area and probably need not even be explicitly posed.² Rather, they could be seen as problems *constitutive* of the anthropology of hunter-gatherers, problems that necessarily come to be posed in respect of it if only from outside the discipline. I have the feeling that the intellectual value and general interest of research on hunter-gatherer societies lie in our capacity or potential for scientifically answering these questions.

The first question arises from the fact that hunter-gatherers appear to be the most ancient of so-called primitive societies—the impression that they preserve the most archaic way of life known to humanity, that characteristic of the whole of the Palaeolithic. I am not saying that hunter-gatherer societies *are* the most ancient, merely that they *appear to be* so—that they evoke the societies of the Palaeolithic. Every question necessarily arises initially at the level of appearances, and it is the business of science to criticise these appearances. The first question may therefore be formulated thus: Given the appearance of similarity in terms of life-style, technology, etc., between existing³ hunter-gatherer societies and those of the past, how should one conceive of the relation between them?

This question is an evolutionary one, and I know that many of my colleagues will not concern themselves with it, for anti-evolutionist feeling has been intense for most of this century, particularly in France, and to a large extent remains so. Therefore it is necessary here to say a word about evolutionism and *in its favour*. In its minimal form, evolutionism appears to me to consist, once it has been recognized that social forms change in the long term, in an investigation of the general character of that change and of the laws, if any, that govern it. Such an inquiry cannot but be legitimate, and it is astonishing that scholars, and not the least eminent, such as Radcliffe-Brown in certain of his writings (1968[1952]: 115), have supposedly been able to found the scientific standing of social anthropology on the a priori rejection of all evolutionary concerns. I lack the space here to develop this argument (but see Testart 1985c, 1987b); I will say only that one should not confuse the undeniably outdated evolutionist schools of the 19th century with a careful modern inquiry based on the considerable findings of prehistoric archaeology and embarking on what I would call a “reasoned evolutionism.” Among the errors of earlier evolutionism could be cited the particular theses of the different 19th-century schools, most of them untenable; the methods adopted by the evolu-

2. Posed at the Chicago symposium of 1966 and aired in *Man the Hunter* (Lee and DeVore 1968), they seem to have been less prominent in the course of the four international conferences on hunter-gatherers held in Paris, Quebec, Bad Homburg, and London between 1978 and 1986.

3. “Existing” in the sense of the “ethnographic present” or what prehistorians call the “sub-present,” that is, those societies capable of being treated anthropologically or ethnohistorically and broadly observed from the 17th or 18th century up until our times.

tionists, particularly the abuse of the notion of "survival," which was applied willy-nilly to different institutions without asking why and in what context a past institution could survive and be integrated as a living element into a new structure; and the general philosophy governing evolutionary inquiries, including the outdated idea of moral progress and the concept of social evolution on a biological model. Here was an ensemble both odd and obsolete.

For a better understanding of the difference between these old evolutionist positions and those currently possible, we may return to the question: What is the relation between existing and former hunter-gatherer societies? Writing at the turn of the century, Sollas (1911:382 et passim) replies without more ado to this question with the statement that the Tasmanians are a people of the Eolithic, the Australians "Mousterians of the Antipodes," the Bushmen Aurignacians, and the Eskimo Magdalenians. He identifies the one with the other, purely and simply. It is evident, however, that the Eskimo are not Magdalenians any more than other existing hunter-gatherers are prehistoric peoples. There can be no question of repeating such naïve statements today. The anti-evolutionists could well reply that the two series of peoples compared by Sollas are separated by at least 10,000 years. But anti-evolutionism⁴ contents itself with asserting this difference, as much in space as in time—a viewpoint that prevents comparison of peoples with obvious similarities. In other words, between a simplistic evolutionist position that claims to identify past cultures with those of the present and the opposite position that restricts itself to observing difference and rejects even the idea of comparison, there is an intermediate and more subtle position that takes account of both evident differences and apparent similarities. This contention may seem trivial to some, but it is still heresy to others.

The relation between existing and past hunter-gatherer societies is problematic, and it is our job to construct a conception of it. The answer to the question cannot be other than complex, and the two extreme responses I have evoked both err by excess of simplicity, the first because it does not see this relation as problematic and the second because it denies the existence of any such relation. Both evade the problem, which is in what respects existing hunter-gatherer societies are continuous with those of the past and in what respects they are different.

4. Such a position has recently been forcefully reaffirmed by Schrire (1984). When she makes herself the advocate of archaeology and ethnohistory, above all in respect to southern Africa and Australia, two immense regions that have been little investigated from these points of view, I can only applaud, but I am unable to follow her when she appears to oppose history and evolutionism. The question that arises is rather that of knowing whether, on the basis of a better knowledge of *particular histories*, one can discern more general evolutionary tendencies. If the crucial question to be posed to an evolutionism I call reasoned consists in identifying the relations (necessarily historical) between existing and past societies, that evolutionism incontestably counts archaeologists and ethnohistorians among its principal supporters and audience.

The second question arises from our speaking of *hunter-gatherer* societies, that is to say, of an ensemble of societies grouped in the same category because of the similar appearances of their subsistence techniques and their economies. This assumes first of all that peoples as different and widely separated one from another as the Australian Aborigines and the Inuit (Eskimo) can be profitably compared: what is in question is thus initially the possibility of a *social* anthropology notwithstanding *cultural* differences (in the sense of cultural anthropology). Further, these societies are grouped in terms of techno-economic level, and this presupposes that their technological and economic characteristics are relevant for the description and understanding of them. The underlying question is how one is to conceptualize a possible articulation between the economic aspect of a society and its other aspects.

During the past few decades there have been many discussions on how hunter-gatherers should be defined. Here I take it as obvious that hunter-gatherers are *by definition* people who hunt and gather and do other things like hunting and gathering. However, what does it mean to hunt and gather? It could mean to exploit resources the reproduction of which one does not control as one does in agriculture and/or stockkeeping. If the relevant criterion is absence of domestication in respect of subsistence, it would seem necessary to include all who depend for subsistence upon wild resources, whether fishing, collecting, or gathering. Finally, if this technical definition appears a good one it is not because of a materialism that I believe should be a question rather than a doctrine but rather because it allows the *explicit formulation* of one of the questions that give the study of hunter-gatherers its interest: Is there a relation, and, if so, how should it be expressed, between the techno-economic level of a society and the various aspects of its social organization? This definition seems a good one (although it should be emphasized that any definition is inevitably provisional) because it does not evade the problem as would a definition in purely social terms,⁵ which would mix in the very moment of its utterance terms referring to technical activities ("hunter-gatherers") with social forms. In the same way, adherents of "cultural ecology" long used the term "band society" in reference to hunter-gatherers when instead it should have been asked to what extent these societies were in fact organized into bands. This question was no doubt posed, but it would have been better to avoid a terminology that tended to obscure it.

I shall now briefly outline the evolution of what seem to me to have been the major problematics of the anthropology of hunter-gatherers starting with Childe's concept of the "Neolithic Revolution" (1949[1925]:34; 1953[1935]:43; 1964[1936]:65; 1961[1954]:71; etc.). The choice of such a point of departure may seem surprising

5. As proposed by Ingold (1980a:74). Correlatively, an archaeologist specializing in the Middle East, Ducos (1976:148), proposes a socio-economic definition (in terms of property) of the domestication of animals.

in that Childe was not an anthropologist and did not claim to be one. Moreover, his main field of study was the Neolithic; he had little to say about the Palaeolithic and was consistently reticent about the social characteristics to be attributed to hunter-gatherers. Nevertheless, I believe that his concept of the Neolithic Revolution lends strength and relevance to the category of hunter-gatherers, which can be envisaged only *in its opposition* to the category of agro-pastoralists. Two points should be emphasized: (1) Childe conceived of this opposition as both diachronic and synchronic, and it is thus capable of organizing data from both prehistoric archaeology and ethnography. (2) Conceived diachronically, it translates as *a major historic rupture* comparable in importance, according to Childe, to that constituted by the Industrial Revolution in modern times. This, *grosso modo*, is the grand conceptual framework within which the idea of hunter-gatherers has evolved.⁶

Childe has been much criticised for his notion of a Neolithic Revolution. It has been argued that the process involved was extremely slow and not the sudden rupture suggested by the term "revolution," that his materialism is somewhat mechanistic, etc. But these reproaches from prehistorians have to do with the way of conceiving this Neolithic Revolution as a historical *process*; it is always a matter in these debates of describing or reconstituting this process, its explanation, its causes, etc. I am not competent to discuss these criticisms (which, insofar as I can judge, appear well founded), and I do not see the necessity to do so because in my view Childe's main contribution is hardly that of having attempted to imagine a process but of having thought of a conceptual opposition. What he bequeathed to later researchers was not so much a reflection on the way in which one type of society succeeds another as the very idea of a global and radical opposition between two major kinds of socio-economic organization. He was in fact the first to propose a definition—or at least to see the implications of a definition—of the Neolithic as an epoch characterized by sedentary agriculture; correlatively, the Palaeolithic is defined by a hunter-gathering economy. By so doing, it could be said, he achieved, in the field of prehistory and, more generally, in anthropology, an epistemological rupture or break⁷ in relation to three 19th-century approaches to the difference between these epochs: (1) a naturalistic approach, in terms of which the succession of the two stages of the Stone Age is seen as being like that of two geological eras (glacial/post-glacial) characterized by different environments and different faunas,⁸ (2) a technological approach, in

which Palaeolithic and Neolithic are defined as the Age of Worked Stone and the Age of Polished Stone,⁹ and (3) an alimentary approach that goes far beyond the confines of prehistory which classifies peoples according to what they eat and is expressed in the evolutionary sequence hunting, stockbreeding, agriculture.

This last rupture is more difficult to define than the others, because here I am introducing a distinction not made by Childe himself, but it is decisive for all that. Childe undoubtedly interested himself in peoples' diets, and it was even one of his main preoccupations, but he was less concerned with what men ate than with the way they produced it and the repercussions of this production on society.¹⁰ In his hands the distinction between Palaeolithic and Neolithic becomes an *economic* difference: the Neolithic is defined as a way of producing food following the domestication of plants and animals, implies a certain organization of production, and has certain consequences for demographic structure and mode of life. In the final analysis it matters little whether people eat curds or cereals; although Childe did not use these terms, it is economic structure that is in question.

In thus defining the Palaeolithic and Neolithic in economic terms, he drew attention to the difference between two major kinds of society. For one thing, he discovered the social in the depths of prehistory, a discipline whose immediate data were of a geological or naturalistic order. But, by the same token, he bequeathed a continuing problem to social anthropology: if this opposition between two great categories of society defined by their economies is to have meaning and relevance, how should one think of each of these ensembles in all their social dimensions? What ties, what connections, what causal relations link the economy and other aspects of the social? The problem of the articulation between economy and society has been posed in anthropology since the thirties, when Steward (1936, 1955) attacked it in terms of a problematic that was to endure beyond *Man the Hunter* into the seventies. In adopting the notion of "band" already present in American anthropology, Steward proposed a moderate and rather flexible but convincing enough integration of the known ethnographic facts. The American "band" more or less corresponded to what Radcliffe-Brown (1930–31), referring to Australia, called the "horde": a fundamental group, residential and economic, endowed with a certain capacity for social and political integration. The "band

6. One can see something like a hunting stage in Morgan's *Ancient Society*, but the opposition between hunters and agriculturalists does not figure there as a key opposition as it does for Childe.

7. The expression "epistemological break" (*coupure épistémologique*) is proposed by Althusser (1965:24) to designate one of the key concepts of the epistemology of Gaston Bachelard.

8. On this point Childe did no more than take up in his own way a rupture already entrenched in prehistory, the existence, recognized since the turn of the century, of a period called the "Mesolithic" after the last glaciation and continuing the tradition of worked stone.

9. These definitions are generally attributed to Lubbock in 1865, although he in fact made use of the three criteria, including the economic one, to define and differentiate the two Stone Ages—a fact, moreover, recognized by Childe (1963[1951]:29). Childe's contribution thus consisted essentially in underlining the central importance of a criterion already recognized before him, not of inventing it.

10. It was thus that he wrote (1963[1951]:33) to justify his definition of the Neolithic: "Obviously the cultivation of edible plants, the breeding of animals for food, or the combination of both pursuits in mixed farming, did represent a revolutionary advance in human economy. It permitted a substantial expansion of population. It made possible and even necessary the production of a social surplus. . . ."

level of organization" was the social form held to correspond to the hunter-gathering economy. This is—or was—the key concept of the anthropology of hunter-gatherers.

That Childe said but little, as I have remarked, about the relation between the economy and the rest of society among hunter-gatherers may be because of his unhappy awareness of the inadequacy of his thinking on the subject. One cannot propose a Neolithic Revolution or a rupture that separates two major ensembles unless the phenomena on each side of the divide can at least for the most part be thought of in the same terms, and Childe was too well acquainted with the ethnographic data not to know that no such unification is possible. There are, in particular, societies of the Northwest Coast of North America that constitute an enormous challenge: lacking agriculture or stockbreeding, these societies are nevertheless quasi-sedentary and strongly egalitarian (Childe 1954:41–42). Childe was greatly preoccupied with the archaeological data from Lake Baikal and from certain European sites, where again the grave goods of simple hunter-gatherers show that their societies were egalitarian (1963[1951]:82–86). He nonetheless adhered to what might be called the "surplus argument": that in the absence of food production, the economy of hunter-gatherers is too weak and undeveloped to yield a surplus and therefore economic inequalities cannot emerge. Childe appears to have maintained this line of reasoning until the end of his life (1954:41–48), and yet he was obliged to recognize the existence of inequalities among certain hunter-gatherers. If one cannot even say of these societies that they are always egalitarian (1963[1951]:85–86), what can one say about them that is general?

Childe was too great a thinker to have recourse to expedients to rid himself of this embarrassing problem. It was in fact crucial for his thinking, because one cannot affirm the validity of an opposition between two terms when one of them is itself a problem. One could recognize the diversity of Neolithic societies once they had emerged from their common source, but what was one to make of these hunter-gatherers, themselves so diverse? This is why it can be said that, despite the modest place of hunter-gatherers in Childe's writings, the question posed by such exceptional hunter-gatherer societies as those of the Northwest Coast haunts his entire work.

It equally haunts the anthropology of hunter-gatherers. In this respect it is extremely interesting—at an epistemological level—to see how this unresolved question provoked a regression to an earlier problematic:

1. In American anthropology informed by the theory of the "band," the question takes the following form: How is it possible that the societies of the Northwest Coast are organized not at the band level but at that of the chiefdom? Two answers were proposed during the fifties and sixties. According to one, it is exceptional ecological conditions that account for the exceptional character of these societies (Steward 1955:175; Service 1962:47; et al.). According to the other, it is a matter of

fishers' constituting a category distinct from that of hunter-gatherers (Murdock 1968a:15). Neither of these solutions includes the idea of a specific articulation between economic form and social form, because at precisely this point recourse is had to a criterion external to the economic structure, either *ecological* or *alimentary*.¹¹

2. Vis-à-vis Soviet anthropology and prehistory, the presence in Siberia of egalitarian hunters and fisher-folk poses a problem similar to that of the Northwest Coast. This problem has been obscured for a long time by a purely *technological* conception of the Neolithic, principally defined by the use of pottery and polished stone: hence all the Siberian hunter-gatherers are excluded from the Neolithic category (Mongait 1959:83–87; Okladnikov 1962:273–74; et al.; but note some different approaches since the seventies, such as that of Khlobystin, cited by Howe 1976).

The question is thus a double one. On the one hand, one has to decide whether to recognize, within the large class of hunter-gatherers (defined as peoples not practising domestication), different *categories*; eventually this question implies another on the periodization of prehistoric time. On the other hand, the question is that of the idea of an *articulation* between economy and society: how to understand the fact that hunter-gatherers held to be exceptional, if indeed they possess the same economy, have been able to construct such different societies. It is to answer this double question that I have put forward the concept of "storing hunter-gatherers,"¹² a category arising from recognition of an economic structure characterized by (1) an economic cycle based on seasonal and massive storage of staples and (2) some annual planning of the economy, implying a certain rigidity of behaviours and strategies. As the broad outlines of this thesis have already been laid out in the pages of this journal (Testart 1982b), I return to it here only to show how it relates to the problematic just outlined and to indicate how it may be improved upon.

Conceived in economic terms, this proposed solution shatters the supposed unity of hunter-gatherers in that these terms do not lead to domestication; the solution consists in redefining the economic categories and dis-

11. Some discordant voices made themselves heard from the sixties in relation to California, which posed a problem similar to that of the Northwest Coast. These critics contested, with reason, the universality of the "band" model and established that the Indians of California, the most numerous of the hunter-gatherers and among the most important for their cultural achievements, could in no way be simply set aside as "exceptional" (Bean and Saubel 1961:237; Kunkel 1974:8; Bean and Blackburn 1976; et al.).

12. Although the idea appeared in two preliminary articles (Testart 1979a:101–3; 1979b), it was not fully developed until my book (1982a). Since the publication of this work, numerous studies of "exceptional" hunter-gatherers have appeared, among which could be cited a collective Japanese publication (Koyama and Thomas 1981) with the significant title *Affluent Foragers* that traces a very interesting parallel between aboriginal California and the Japan of the Jōmon epoch; some works on the Northwest Coast, emphasizing such exceptional aspects as slavery (Mitchell and Donald 1985) or the potlatch (Mauzé 1986); some studies of the Eskimo of northwestern Alaska (Burch 1986); etc.

tinguishing others that are more precise and relevant. But this economic solution also appeals to the other approaches (ecological, technological, alimentary) and permits a restoration of the factors implicated by these approaches to their proper place as *secondary* ones, not stripped of relevance but drawing their meaning from their interaction with economic structures. Thus the economic structure based on storage presupposes four conditions for its realization, two ecological and two technological: resources have to be (1) abundant and (2) seasonal, and there must be techniques for (1) the acquisition of large amounts of resources and (2) their preservation and long-term storage. Thus not only environmental and technological factors but also the alimentary one become subordinated to and integrated with the economic structure. From simple considerations of the time required to preserve resources it is apparent that it was relatively short when it came to vegetables (the products of gathering) and fish and very great for meat; it follows that this storage economy could come into being only for those who were principally gatherers and/or fishers rather than hunters.

This solution evidently leads to rejection of the idea of a "Neolithic Revolution" and the related idea of a radical separation between hunter-gatherers and agro-pastoralists. Or rather it results, *within Childe's own problematic*, in displacing the locus of the problem: it replaces the opposition between hunter-gatherers and agro-pastoralists with another that is more relevant but still conceived in terms of economic structure. Not only is the previous category of hunter-gatherers irremediably split into two irreducible categories but also the hunter-gatherers I have called "storers" are seen to have the same economic structure as cultivators of cereals, the former doing with wild resources (products of gathering, fishing, etc.) *exactly* what the latter do with domesticated ones. Here again the new model allows us to integrate the criterion of domestication as a secondary factor. Where wild resources are not both abundant and seasonal, the introduction of adequate resources *domesticated* by man and *transplanted* into the environment is an indispensable precondition for the realization of the economic structure of storage. The model thus allows us to view agriculture not as an economic factor of radical or universal importance but as a technological factor that becomes decisive only under certain environmental conditions. Agriculture becomes one of the technological preconditions of the economic structure. Envisaged in historical perspective, the invention of agriculture loses the radical importance that it had for Childe and has to be resituated among the ensemble of inventions marking the final Palaeolithic and Mesolithic. Agriculture thus occupies a much more modest position in relation to the *origin* of the economic structure based on storage, but once this has been established, it reassumes its historical importance as the sole factor capable of developing to the limit certain tendencies that had appeared earlier, in particular with the storage-based structure: it allows the intensification of production in a way not realizable by simple hunter-gatherers

and provides the economic basis on which the first statelike societies can develop. The "Neolithic Revolution" conceived by Childe as the ensemble of the radical changes introduced by the adoption of agriculture is thus exploded in space and time along two axes, geographical and historical. Rather than a refutation of Childe's views, the proposed model consists, in a sense, of its generalization.

The main interest of the new model is that it allows us to paint a picture of social evolution that is much more satisfying than a simple succession from Palaeolithic to Neolithic. This picture is more complex not only in that it reveals a rupture that had not been apparent before and in distinguishing two ruptures where only one had been seen but also, while recognizing the existence of a major line of development, in drawing attention to regional modalities of evolution. To me it appears neither rigorously unilinear nor, properly speaking, multilinear.

That said, we now come to the second major question posed at the beginning of this article, that of the relations between economy and society. How does the proposed model allow us to respond to this question? It could be said to have been especially designed to take account of certain social characteristics so surprising for hunter-gatherers that they had been called "exceptional": their sedentary character and the profoundly egalitarian nature of their society.¹³ Conceived in economic terms, it must appeal to a certain concept of the relation between economy and society. Correlations become evident between economic structure and storage, sedentariness, and socio-economic inequality. These correlations are satisfying and allow us to envisage the category of storing hunter-gatherers as an autonomous and properly constructed one solidly anchored in empirical fact and quite distinct from the other category of hunter-gatherers characterized more classically by nomadism (a mobile life-style) and the egalitarian nature of their society. The idea of eventual causal links between the correlated elements is, however, at least in an important part, a problem. It is understandable that groups should practice intensive storage of their main food resources to provide against the season of scarcity and also that they should be sedentary or nearly so throughout this season, since the accumulated reserves render unnecessary any migration in search of food. This connection between economic form and "residence pattern" is simple and obvious.¹⁴ It enables us to deduce

13. I leave to one side a third characteristic of these societies, their high demographic density, which is much less pertinent to the discussion here.

14. This does not mean that intensive food storage is the only means for hunter-gatherers to become sedentary or that it is the only factor that can explain the condition of those that are sedentary. I have always acknowledged at least three other factors (Testart 1981:184–87; 1982a:28–30) that combine in different degrees in each case: (1) the development of effective means of transport, such as sledges and draught and pack animals; (2) the possibility, mainly realized in northern Eurasia (a point rightly emphasized by Watanabe 1983), for the group to split into sub-groups, one of which remains stable while others go on periodic expeditions; (3) the existence of *non-seasonal*, abundant wild food resources that are concentrated in a limited area. Intensive storage (as I have

certain major characteristics of what Durkheim's followers would have called "social morphology": stability and concentration of the population in villages during the season of scarcity, which is also the time of leisure, the season when ceremonies occur, etc. But the other connection, between what I have been calling the "economic base" and the development of socio-economic inequalities, is far from clear. The central idea (far from the only one, but I simplify) is that the massive stockpiling of staples constitutes the material base for a possible development of socio-economic inequalities. The key to the problem and the whole ambiguity of the formulation turn on the use of the adjective "possible." It has two senses. According to the first, the transition from an economy of nomadic hunter-gathering to an economy based on storage permits (renders possible) the *development* of socio-economic inequalities to the extent that the bulk of the production is thenceforward transformed (by techniques appropriated and accumulated differentially by individuals or by groups). Let it be understood that there may be socio-economic inequalities outside the storage-based economy, but these can only become significant on the basis of such an economy: in concrete terms, whereas a man socialized among nomadic hunter-gatherers could at the most accumulate some stone axes, feathers, furs, and other items valued by the culture, enjoy great prestige, accumulate wives, and dispose of the best portions of game that he was obliged to redistribute in the absence of the practice of preservation, the same man could, among storing hunter-gatherers, control a considerable mass of foodstuffs either as a private owner or as the head of a group. In the second sense of the word "possible," the development of inequalities is not ineluctable: it is *only* a possibility inherent in what I have called the economic base, and I was somewhat surprised to discover in each case study of storing hunter-gatherers the presence of marked inequalities. I have even asserted that this possibility is subordinated to certain *social* conditions, that it cannot be realized as long as food sharing is the rule and requires the appearance of a kind of private property in foodstuffs or control by the collectivity through a privileged individual who is socially invested with a pre-eminent right over the management of the stores. (These two divergent possibilities seemed to me to have been realized *grosso modo*, the one in California, the other on the Northwest Coast.)

This is a complex argument that has not always been understood,¹⁵ and it conceals a fundamental ambiguity

in that it does not allow a choice between two converse conceptions of the causal link. According to one of these, it is the storage-based economy that, by generating material riches within the society, gives rise to inequalities; the technological and economic organization is the ultimate cause of social forms. According to the other, it is, on the contrary, the social forms that provoke the transformation in the material basis of the society; thus one could imagine, for example, that individuals or groups socially dominant by virtue of a hierarchy (which would then be envisaged as the initial cause) determined a certain intensification of production and favoured the production of durable goods of which they were the principal beneficiaries through their dominant position and that would allow them to ensure (or reinforce) their domination of an enlarged material base. Seeing no way of deciding in favour of one or the other of these arguments equally compatible with my approach,¹⁶ I have avoided choosing between them. Without being able to identify precisely the causal connection between economic form and social form, the theory inevitably leaves unanswered the question that has every right to be posed. It identifies a certain articulation between economy and society, but it is a weak articulation. I leave my criticism there, because there is another that is much more decisive.

In the process of this general reorganization, the locus of the problem has been displaced: it is not the agricultural revolution that represents the major break among societies but the adoption of an economic structure of which the central feature is storage. It accounts much better than Childe's conceptualization for the distinction between egalitarian and inegalitarian societies: the appearance of inequalities is in large part tied to storage.¹⁷ This reorganization, which I have already indicated as lying entirely within the framework of Childe's problematic, thus takes account of a very important aspect of society, but it has nothing to say on its other aspects—kinship, social organization, and the symbolic dimension, all matters that are the peculiar concerns of social anthropology.

Nomadic hunter-gatherers, although they can be globally characterized as egalitarian, exhibit enormous differences in terms of social organization. Nothing is more striking in this respect than the peculiar position amidst this great class of nomadic hunter-gatherers occupied by Australia. Here are what I see as the three major distinctive traits of Australian societies:

1. Unilinearity. With two or three doubtful excep-

conceived it, integrated into a cyclical economic structure) entails sedentariness, but sedentariness does not entail storage. The question here is only storage as I have defined it (i.e., simultaneously intensive, seasonal, and providing the group with essential nutrition during the whole season of scarcity) and not the more limited storage, less systematic and *directed to other ends*, that is practised, more or less, by all hunter-gatherers (Testart 1982a:149-73; 1985a; Bahuchet and Thomas 1985; Ingold 1985; et al.).

15. For example, the otherwise moderate criticism of me by Cauvin (1985:17 n. 8), according to which I suppose a private appropriation of stocks, can only be the result of a superficial reading of my book.

16. The original attempt by Legros (1982) to develop a theory of the origin of inequalities on a social base sprang from an argument of the second type; as he now sees it, his approach is not incompatible with mine (personal communication).

17. I have never claimed that this was the *only* factor. It would not apply to cultivators of cereals and hunter-gatherers in regions favourable for the establishment of a storage-based economy, i.e., regions in which the two environmental conditions for this economy are realized. I have indicated the limits of this approach in showing that the storage factor probably played no part for cultivators of root crops (Testart 1982c).

tions, all the more or less well-known Australian tribes (among the 500 on the continent at the time of colonization) possess a clan organization and may be matrilineal, patrilineal, or both.¹⁸

2. Binarity, a dualist schema in both social organization and the system of representations. It suffices here to recall the very complex game played out in Australia between moieties that may be patrilineal, matrilineal, or, again, endogamic (*generation levels*). These oppositions combine in the diverse systems of sections and sub-sections and in the semi-moieties, systems exclusive to Australia. Other types of dualist organization include those connected with the division of moieties into phratries in the north and other oppositions not reducible to the preceding ones in the southeast.¹⁹

3. A classificatory mode of thought that classes together men and things. The social frameworks that divide members of society by clan and class, moiety, section, etc., also serve as classificatory schemas for the whole of Nature, with the result that each social segment (clan or class) corresponds to one or several animal species or some natural phenomenon. This is totemism, nowhere so well developed (and so multiform) as in Australia.

Unilinearity is associated with kinship terminological systems such as the "bifurcate merging" ("Iroquois," in Murdock's terminology), unless there is terminological differentiation between the two cross-cousins. Moreover, Australian social organization determines classes that have sometimes been called "matrimonial"; exogamy is expressed by a prohibition against marrying into the same class or a prescription of marriage into another.

The phenomena I have just enumerated are solidly characteristic of Australia and virtually absent among other nomadic hunter-gatherers. Let it be emphasized that I have excluded the stors from the comparison because they belong to an altogether different economic category. For the same reason, I exclude "mounted" hunter-gatherers such as the Plains Indians, because they are as much stockbreeders as hunters, and the tropical peoples of Africa and Asia, because they subsist as hunter-gatherers only through their ties with neighbouring agro-pastoralists in what seems to me a veritable inter-ethnic division of labour.²⁰ The remaining hunter-gatherers are divided between several major culture regions of which the most important are, in addition to Australia, southern Africa, southern South America, the Great Basin, and the interior of Canada. None of the features observed in Australia is found in the first three

cases. The situation in the Canadian interior is more complex: unilinearity combined in varying measures with totemism and with organization into two or three phratries is found among the southern Algonkin and among the western Athapaskans, but these two cases occur on the margins of the region and in contact with cultivating and/or storing peoples, as if the nomadic hunter-gatherers were reproducing in their society the salient features of their powerful neighbours. In brief, unilinearity, dualism, and totemism are the general characteristics of Australian societies; conversely, in the other hunter-gatherer societies *which, however, seem to possess the same type of economy*, these characteristics are rare.²¹ How can one account for such a radical difference?

To my way of thinking, this problem is the major one facing the anthropology of hunter-gatherers. It can be formulated as follows: what purpose does it serve to retain a category of hunter-gatherers, defined as they are by a similarity in way of life or in elementary economic behaviour, if this category remains powerless to resolve the major problems of social anthropology? What is the point of an anthropology of hunter-gatherers if it has to remain aloof from all the questions that have concerned social anthropology from its beginnings? All the subjects I have touched on in connection with the comparison between the Australians and other hunter-gatherers are *par excellence* the classical topics of the discipline as they have emerged through works that are historic landmarks: that of Morgan, whose major interest was clan organization and kinship systems, that of Durkheim, who was more interested in totemic phenomena and modes of classificatory thought, and finally that of Lévi-Strauss, in connection with exogamy and matrimonial systems.

How is this problem posed in the anthropology of hunter-gatherers as it has developed since Steward, through the works of Service, Leacock, Damas, Lee, and others, and found its classical expression in the sixties with the publication of *Man the Hunter*? The key concept by means of which the social organization of hunter-gatherers is approached and characterized is that of the "band." It is an ambiguous concept: in the first place it has a residential sense, designating a group of persons who live together, share a camp, and perform more or less the same economic tasks, but it also refers to a minimal political unit. By reason of this very ambiguity, it has seemed capable of providing a link between economic base and social forms and consequently has given rise to impassioned debates. Service (1962, 1966), generalizing the already old ideas of Radcliffe-Brown on the Australian "horde," maintained that the local group had to be patrilocal for purely material reasons, roughly, let us say, because of the organization of labour; therefore it was not hard to see in patrilineality the social and ideological translation of the patrilocal composition of the band. While this view was in no way absurd but

18. There are also clans formed on a local or conceptual basis, without a strictly unilineal composition, which it is out of the question to examine within the framework of this article.

19. This last point is important because it shows that the principle of binarity is not limited to the classical and recognized forms of social organization, such as sections, sub-sections, etc. (Testart 1980).

20. Hunter-gatherers of this category present a number of distinctive features that I have sought to enumerate elsewhere (Testart 1981:188–203).

21. One arrives at the same sort of result by taking the kinship systems into consideration (Testart 1985b:248–53).

rather astute, it had to be admitted that it was incompatible with the ethnographic data. Numerous researchers (Turnbull, Lee, et al.) in effect criticised Service's model by showing that the local group was by no means patrilineal but, on the contrary, fluid and flexible in composition.

Paradoxically, the most powerful criticism was to come from Australia, where there developed from the beginning of the fifties a great controversy about local organization: A. P. Elkin, R. Berndt, L. Hiatt, and numerous other researchers rejected²² Radcliffe-Brown's model of the horde and showed that, despite a relatively rigid social organization into patrilineal clans, Australian local groups were extremely variable in composition and the principles of their recruitment corresponded with criteria other than clan affiliation. After that, hope was abandoned of finding a simple correspondence between the organization of bands and of clans: the social edifice seemed more than ever irremediably split between two levels ordered by different principles and oriented towards different ends. But from another perspective Australian studies legitimated a unitary view of hunter-gatherers, since, apart from the fact that some possessed clans and others did not, all possessed the same local organization into bands of flexible and changing composition. Such seems to me to have been the state of the question as it emerged from a reading of *Man the Hunter*.

But such a view of things cannot satisfy us, for at least two reasons. The first derives from its setting aside of some of the major problems of social anthropology. To the question "Why, alone among hunter-gatherers, do the Australians have clans?" it has no answer except perhaps that this clan organization is a superficial phenomenon, something like an epiphenomenon, relating to a hard-to-explain superstructure while the band organization represents the profound reality of the social organization.²³ The second reason this view is unaccept-

able requires more lengthy discussion. Let us say for the sake of brevity that the characterization of a form of society as organized into bands seems to me a characterization by an extremely weak social form, for the band as it has been redefined following the critique of Service's suggestion is not strictly speaking anything more than a rather diffuse local grouping and, at most, a form of organization of labour. It was possible to maintain the band as the general form of hunter-gatherer societies only because it had been emptied of its content,²⁴ i.e., of the sense it had for Steward or Service, who envisaged it as a principle capable of explaining other aspects of society. Finally,²⁵ it is extremely doubtful if one can characterize a type of society through considerations drawn solely from residential and work groups; it is a little as if one were to characterize capitalist societies by the composition of towns or the size of factories. Such an approach is bound to leave out what is most typical of the society, i.e., its characteristic social relations. Thus, to return to our problem, one cannot help wondering, in relation to Australia, why kinship relations are so important there. This is really a question about social relations, namely, Why do these assume in Australia the principal or predominant form of kinship?

In formulating these criticisms of the anthropology of hunter-gatherers and of the cultural ecology within the parameters of which it is situated, I am conscious of their extreme severity. I offer them only because I know how much we owe to the works just cited: the recognition of the determining role of gathering, largely due to the work of Lee; the concomitant abandonment of the old idea that hunters lived on the verge of famine and the slightly provocative generalization of this idea by

22. This very important controversy, which I do not hesitate to say has shaken the foundations of Australian anthropology, has not perhaps received all the attention it deserves outside Australia. In effect it implies the general conclusion that a group, in this case the Australian patrilian, may be *linked* (sentimentally, religiously, etc.), and even in a certain way economically, as I have maintained on the basis of certain facts rarely taken account of [Testart 1978:148-50] with the land even without its members' *residing* on this land.

23. A historical point, with epistemological implications, would not seem out of place. Radcliffe-Brown's concept of the "horde," generally attributed to his 1930-31 article, was in fact much earlier; the idea that the local group is patrilineal appears in turn-of-the-century works such as Howitt (1904), and Durkheim's whole explanation of sections rests on Howitt's idea just as later Lévi-Strauss's was to rest on Radcliffe-Brown's. But Howitt accorded only minor importance to local organization; in the tradition of Morgan, his whole interest lay with moiety organization and matrilineality, which he considered antecedent to patrilineality. Radcliffe-Brown, in taking up the idea of patrilineal group, changed its meaning entirely: this group is presented as the basis of all Australian social anthropology, it becomes the universal foundation, a stable grouping present everywhere in Australia, a reality that is simultaneously economic, political, and social (Radcliffe-Brown thought it exogamic), around which could thenceforth be situated

as secondary phenomena the innumerable variations of social organization. In proposing to see in the horde *the* principal phenomenon, Radcliffe-Brown set himself against the whole tradition of the first period of Australian anthropology, which gave primary attention to the social variations on the theme of moieties, sections, or matrilineality. The same desire to discover the universal beneath the social variations is found with Steward when he proposes the "family" and the "band" as the two primary levels of social integration. In Radcliffe-Brown and in Steward—and I have emphasized the historical parallelism between their concepts of "horde" and "band"—is concealed the same desire to minimize the importance of social organization, in all its specificity and in all its variations, in favour of concepts supposedly more universal, such as the family and the band. And I would add: *concepts as vacuous* as the family and the band. It therefore does not strike me as surprising that after 50 years of thinking about the band this approach still has nothing to tell us about social organization—because the notion of band was put forward precisely to legitimate this deficiency. It goes without saying that I consider such an approach an *epistemological error*, our principal task being to take account of the specificity of different forms of social organization and not of the claimed, ghostly universality of band or family.

24. The term "band" has also tended to fall into disuse. Leacock and Lee (1982:7-9) still employ the term *band societies* as a synonym for "societies of hunter-gatherers" in order to connote collective property, etc., but there is hardly any more question of a specific organization in bands as the key organizational form of hunter-gatherers. Significantly (see n. 23), Burch (1986) proposes replacing the word "band" with "local family."

25. This last aspect of the critique is developed elsewhere (Testart 1985b: chap. 1).

Sahlins with his notion of the "affluent society"; the reevaluation of the role of women; the idea that we are dealing with hunter-gatherers and that environmental factors (in particular the latitude) are primarily responsible for the relative importance of the two branches of a hunter-gatherer economy; etc. All this is apparent in *Man the Hunter* and several subsequent works (among them Lee 1979) and marks the high point of a school of thought that turns away from kinship, symbolism, and other subjects of classical anthropology but represents a theoretical advance of the first importance in the domain of the economy, precisely the domain that classical anthropology had neglected. This observation brings us back to our problem of the considerable social differences that exist between hunter-gatherers. This is of course the question of a possible articulation between economy and society, posed anew, and also a matter of bringing together two areas of study (or, as I would rather say, two theoretical approaches) that are unaware of each other. Once again, we find that on similar economic bases there arise complex and very different constructions having to do with kinship and symbolism. There can be no question of doubting the reality of these differences or of minimizing their importance: they are related to the domains most studied by anthropology since its beginnings. Suspicion falls rather on the premises of the problem, on the very idea of a similarity between the economy of the Australians and those of other nomadic hunter-gatherers, because the study of the economy has always been the poor relation of our discipline. It is therefore the other line of research, that of cultural ecology and *Man the Hunter* and, in more remote fashion, that of Childe, that should be followed despite my criticisms, which are intended less to limit their scope than to deepen their concerns.

To explicate the Australian case, let us begin by setting aside the idea of environmental determinism, because the range of variation in Australia is very wide (desert, tropical, Mediterranean, etc.) and corresponds in part to what we find in southern Africa. On the other hand, we find significant differences between Australian hunter-gatherers and others with regard to two orders of phenomena:

1. Hunting techniques (I summarize Testart 1985b: 115–29). Australia is the only region peopled solely by hunter-gatherers that does not possess the bow and arrow. Furthermore, it is not merely the instrument that is unknown but the very principle of using the elastic energy stored in a curved piece of wood: thus we do not find the musical bow or the bow trap or spring trap in Australia. Other lacunae in the Australian technological ensemble are the absence of instruments using kinetic energy, such as the bola and the lasso, of weapons using compressed air, such as the blowpipe, and of poisoned darts and the very weak development of trapping techniques.

2. The capacity, depending on environmental conditions, for adopting an agro-pastoral economy (I summarize Testart 1981:203–11). In America, it is apparent that the hunter-gatherer regions are those ill suited to

agriculture. For North America Kroeber (1939) showed long ago that the geographical limits of Indian maize cultivation corresponded very closely with natural limits of rainfall and sunshine. Hunter-gatherers thus existed in the New World only where agriculture as traditionally practised by the Indians was impossible. Nevertheless, the immense plains of the Pampas and the Canadian forests were propitious for stockraising; in particular, the caribou hunted in Canada is an animal of the same species as the reindeer of Siberia, which has been domesticated. This is a general difference between the Old and the New World, where stockraising, even when it was possible, has been only slightly developed. In view of these differences between the two continents in traditional techniques and economic orientations, we can say that hunter-gathering occurs in America only where agriculture and stockraising, *to the extent that they have been practised and developed on this continent*, are impossible. I believe a similar point could be made about the Bushmen (or San) in relation to the Hottentots (or Khoi-Khoi), stockbreeders with whom they were coupled within a single vast cultural region: hunter-gatherers seem to have lived only where there was an absence of year-round surface water, in the central and southern Kalahari, in the deserts of Namibia, on the Cape Province plateaus—in other words, where stockbreeding as practised by the Hottentots was impossible. In both America and Africa, hunter-gatherers seem to have occupied only residual regions where the domestication of plants and animals as traditionally developed by neighbouring agro-pastoralists was impossible or, at least, impracticable by reason of natural factors such as cold and drought. We find nothing of the kind in Australia, where prehistorians such as White (1971:185) and Golson (1972:387–88) have shown that the horticulture practised in New Guinea could easily have been introduced into tropical northern Australia. Here there is no ecological reason for the persistence of a hunter-gatherer economy, simply a cultural barrier.

These differences are important precisely because they concern this techno-economic level that appears so similar from one hunter-gatherer society to another. Nevertheless, they are not decisive. As a matter of fact, we do not know how to interpret them. It is out of the question to have recourse to any form of technological determinism, first of all because it is difficult to see how a social structure could be consequent on such simple technological facts as I have put forward. To put it crudely, the use of the bow seems as compatible with exogamy and totemism as with any other social form. It might be objected that these simple technological facts may be embedded in a broader technological context, more complex and structured, that is yet to be discovered and that this would represent the only correct level on which to seek an explanation. Certainly, but there are also other arguments against technological determinism. The technical data are difficult to explain as a first cause. We have to admit that the technological development of a society is blocked or favoured by the environment in which it finds itself: now, in the Australian case

it is clear that it is not the *natural* environment that prevents the adoption of the bow or of horticulture but the *social* environment, and we are sent back to social conditions as the determining causes of the diffusion of techniques. As a last attempt to save the idea of a technological determinism, I have mentioned the possibility that our technological facts belong to a larger ensemble. This is in fact what we find: in respect of hunting as well as horticultural techniques,²⁶ Australia appears a backwater in relation to the possibilities offered by neighbouring peoples with whom there is contact, possibilities that are undoubtedly compatible with conditions of the natural environment. One could say, in summary, that in Australia we find a cultural ban on technological innovation, but at this point we reverse the scholarly reasoning about the connections of cause and effect: far from being determining, the technological facts seem determined.

We are thus sent back to something other than the techno-economic domain, without knowing what that "other" is. We can clearly see different social forms in Australia and elsewhere, but we do not see how these differences can allow us to explain the observed differences in techniques and economy. Why is this? Evidently because these forms—clan organization, exogamy, totemism, etc.—are non-economic, having no effects whatever on production. This indeed seems to be the problem, but, given anthropology's traditional neglect of the study of economic structures, can we be certain about it? Well, comparative study of the various forms of game-sharing systems among hunter-gatherers has demonstrated that they can be divided into two major categories (I summarize Testart 1985b:53–96; 1987a). Everywhere apart from Australia there is always, as a result of complex procedures that vary greatly across cultures, one individual among those who have contributed to or participated in some fashion in the success of the hunt (through having sighted the game, brought it down, provided the decisive weapon, etc.) who is considered the owner of the game taken, presides over its distribution, and is entitled to the best of it. An initial distribution occurs among the participants in the hunt, and it is only secondarily that the pieces are redistributed outside the circle of hunters, particularly to their kin.²⁷ In Australia, while forms of sharing differ significantly from one region to another, those who have priority rights to game and ideally preside over its distribution are *other* than the participants in the hunt, typically of the other generation from that of the hunters

and/or the other moiety, affines and not kin of the hunters. These latter receive lesser portions or even, in some cases, nothing at all. This difference can be summarized as follows: Except in Australia, the distribution process begins with those who have taken part in the hunt; in Australia, it begins elsewhere, and this "elsewhere" is defined by the complex play of several binary oppositions (between alternate generations, between kin and affines) inherent in the kinship system and social organization (moieties, sections, etc.).

These results are extremely important, for the following reasons:

1. This difference between the Australians and other nomadic hunter-gatherers is *equally* an economic difference.

2. It is related not to techniques, the mode of adaptation to the environment, or anything else of this nature but to a difference in the *social form of production*—a difference in structure. In Australia, the product escapes the producer (the game escapes the hunter) to the benefit of *another* who disposes of its distribution.

3. The social form of production peculiar to Australia presupposes precisely the intervention of the social forms that appear to constitute the specificity of Australia: the *other* is effectively defined only by the operation of a social organization that divides the members of society into several classes (marriage classes, moieties, sections, etc.) that are distinct and opposable in terms of several major oppositions (between moieties, between generations, between kin and affines, etc.).

4. Finally, and most important, all these social forms apparently serve to uphold a single law: exogamy decrees that a man may not dispose sexually of the women of his own clan: totemic prohibitions enjoin the non-consumption of an animal of his own totemic species unless there is a prescription to reproduce the species symbolically for the benefit of other clans;²⁸ game-sharing systems prevent the hunter from disposing of an animal he himself has killed. One may no more dispose of one's game than of one's totem or one's sisters. Contiguity (between hunter and game, between totemist and totemic species, between brother and sister) always translates as an advantage for *others*. It seems hardly necessary to say that drawing a parallel between exogamy and totemism is not new; what is new is discovering that the same law present in these two classical domains of anthropology also structures material production.

This law according to which one may not dispose of what is one's own (or what one is "closest" to) seems to me to represent something like *the principle of intelligibility* of Australian society *conceived as a whole*.²⁹ At once social schema and schema of thought, it applies to

26. To which one could add the techniques of preservation (by drying and smoking) of animal flesh which, though known and practised in funerary rites during the treatment of the corpse, are used very little to preserve game or fish for food (Testart 1982a:170–72).

27. Let us note in passing the extremely important result that kinship has only a *secondary* place among these hunter-gatherers. It is entirely different among the Australians. In particular, it follows that not only the *form* (the terminological system) but also the role of kinship cannot be the same for the one as for the other (Testart 1985b:231–47).

28. I merely mention in passing a subject of much controversy that deserves much fuller development (Testart 1985b:257–343).

29. This law also holds in Australia for certain well-known aspects of ritual: one does not initiate the young of one's moiety, who must be initiated by the other moiety; the same for funerary practices, etc.

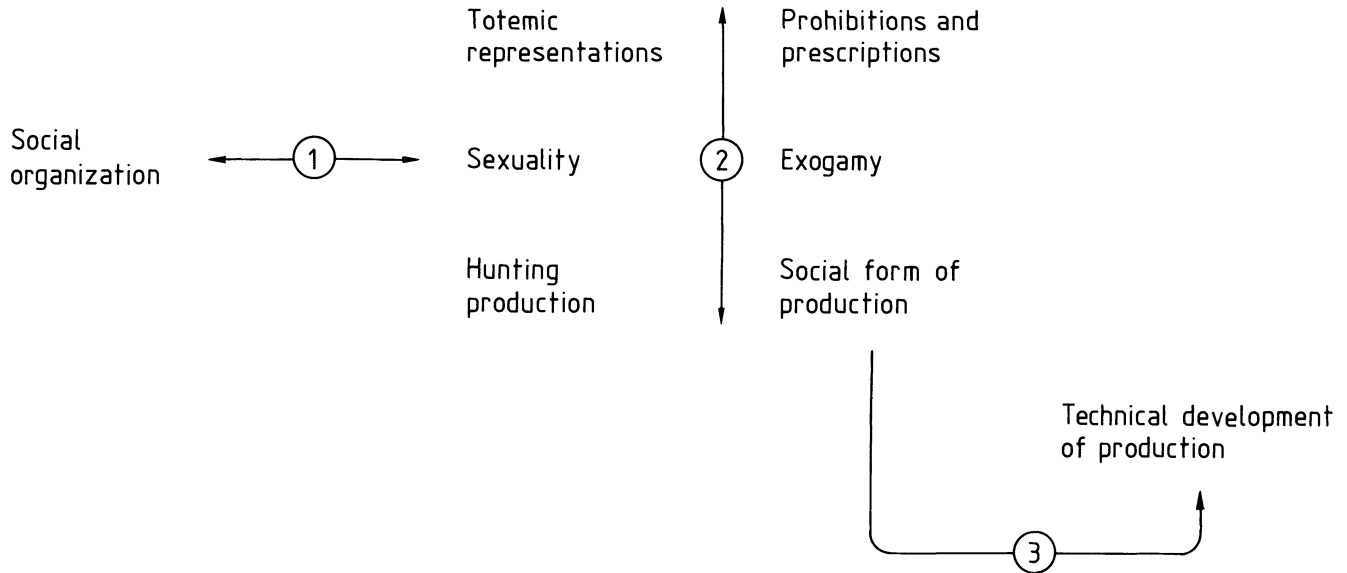


FIG. 1. *Hypothesis on the relationship between the economic and other aspects of society. 1, provision of the framework (categories of kin, matrimonial classes, etc.) in terms of which the common principle or law for structuring the various domains of society is defined; 2, isomorphism (structuring by a common principle); 3, determination or causal relationship.*

at least three domains: sexuality, including matrimonial exchange and the production of children; an important ideological sector, totemism, in which men think and mime on the ritual plane their imaginary relation with Nature, which they eventually reproduce symbolically; and an important sector of the economy, the hunt. Between these three domains it is vain to ask which determines the other. Instead of seeking a causal relation between them, I shall speak of "isomorphism."

Where does all this leave the question of articulation between the economic and the social? It is clear that the privileged aspect of the problem, in which everything is interconnected, cannot be other than the form or the social structure of production. On one side we see an isomorphism between the economic domain and domains of the social, including that of representations; this common structure presupposes the existence of well-defined social forms constituted by the diverse aspects of social organization that divide the society into marriage classes and into distinct categories of kin (fig. 1). So much for the social side. What about the other side, the economic? How are we to conceive the connection between the social structure of production and the technical development of production?

It seems impossible to elicit the complex social construction I have just outlined from the technical facts; I have already admitted this. Neither can there be any question of isomorphism: the technical facts form a system, but they cannot be ordered within a structure analogous to that which I have put forward. From another side, I have already expounded the reasons that led me to believe it was social factors that influenced technical development to the extent of eventually blocking it, but

this idea must remain vague in that I have yet to discover the social form that has such an effect on production. One such has been mentioned so far, and there is good reason to believe that it is indeed the social structure of production that determines technical development. That is just about as far as my reasoning has gone, although I have to admit that I do not find it entirely satisfactory.

The capture of game occasioning no advantage, either material or moral, for the hunters (they derive no prestige from it and do not oversee its distribution), there is no incentive for them to increase labour productivity, to experiment, or to adopt new and eventually more efficacious techniques. This explains the weak development in Australia (compared with other hunter-gatherer societies) of the weaponry and techniques of hunting. On the other hand, the fact that no one appropriates the prize in no way prevents hunters from cooperating in collective hunts; on the contrary, among other hunter-gatherers the principle of appropriation of game by one of the hunters can only give rise to conflicts with other members of the hunting group when it comes to deciding who has rights and particularly who is the owner. Without in any way reducing the size of the hunting group, this principle has the effect of causing each hunter to rely more on his own skill and the efficacy of his weapons than on the help of others. This is clearly not a question of the size of the group but rather one of the relative weighting, within forms of hunting that may be collective, of the factors of cooperation and efficacy of weapons. This relation is inverted in the case of Australia and of the other hunter-gatherers because the different social structures of production favour conversely

recourse to one factor or the other. We thus find that in Australia, while the range of hunting weapons is oddly restricted, on the contrary all forms of collective hunting are represented, including the use of barriers, surprising indeed in a country where the fauna is so sparse and ungregarious.

To summarize, then: The discovery of a social structure peculiar to Australia, inherent in the economy but equally connected with all social domains, has allowed us to account for a certain number of facts relating to hunter-gatherers. Can these facts be placed in a diachronic perspective? In other words, can this new view of things lead us to a new concept of evolution? I think it can, for reasons I would call *a priori* in that any attempt to theorize about an object of scientific study implies certain hypotheses on the laws of its movement.

The equipment of an Australian hunter is, as we have already observed, notable for certain deficiencies. There is no cultural or archaeological evidence of regression. The technical level attained by Australia as regards the weapons and other implements of hunting is thus slightly but significantly inferior to that attained in the other regions of hunting and gathering; it seems as if the Australian condition perpetuated an earlier technological stage.

This hypothesis, which is at the very most reasonable on the basis of the ethnographic data, can become convincing only if underpinned by data from prehistoric archaeology. It appears that the results of a study of these data (Testart 1985b:131–56), diverse and fragmentary as they are, allow us to propose two general conclusions: (1) during the greater part of the Palaeolithic and until the very end of the Upper Palaeolithic, weaponry remained rudimentary, particularly as regards throwing weapons, the use of spears being attested in the Upper Palaeolithic but the harpoon, spear-thrower, and bow not being evident before the Magdalenian and Mesolithic; and (2) the most widely accepted and best-founded hypotheses about prehistoric hunting have recourse to cooperation as a decisive element in its success. Archaeological data are always subject to revision and subtle reinterpretation, and these conclusions should therefore be taken as provisional and regarded with extreme prudence. Nonetheless, they appear to bear witness to the same inverse relation between weapons and cooperation as I have proposed for Australia. All this would seem to support the idea that Australia in some sense perpetuated the technical stage of the Palaeolithic.

To this hypothesis I would add another resulting from my earlier reflections, namely, that technological development is determined by what I have called the social structures of production. Here, then, is how I see things:

1. The social structure of production peculiar to Australia determined no significant development of productive techniques beyond what they had been in the Palaeolithic. At the most we can record the adoption of the spear-thrower, the semi-domestication of the dog (the economic importance of which is contested), and

what the Australian archaeologists call the small-tool tradition (three items that were unknown in Tasmania, probably the most conservative part of the Australian ensemble since the island became separated from the continent by the Bass Strait). The economy remains a hunter-gatherer economy. The major technological characteristic is rejection—of the bow, of horticulture, of techniques for preserving meat, etc. Structures and social forms analogous to those observed in Australia were probably present in Palaeolithic societies. This is only a hypothesis, but it does allow us to account for the very slow technological development of the Palaeolithic. We cannot, however, assert that these forms and structures were the only ones.

2. Everywhere else, new social structures appeared with or after the end of the Palaeolithic.³⁰ I cannot give either the exact date or the reasons for these changes, but I see them as mutations, structural ruptures, that are unlikely to have occurred suddenly and were probably the result of a slow transformation that affected not isolated elements but global totalities. The result of these mutations was the development of the productive techniques of the society, among the techniques of food preservation and the beginning of domestication. This development attained different levels according to environmental conditions; sometimes it led to agriculture, sometimes to a sedentary hunter-gathering economy based on storage. Where the environment or the state of technical development made these types of economy impracticable or impossible, societies remained nomadic hunter-gatherers. This hypothesis accounts for our earlier observation that nomadic hunter-gatherers remained such, outside Australia, only where the environment precluded the adoption of a storage economy or of an agro-pastoralist economy as it was practised in the region. Perhaps I should emphasize here that my argument inverts the current notion that an earlier condition is best-preserved in an unfavourable or degraded region, an argument that is simplistic in that it envisages the society as all of a piece, overlooking the possibility of lack of correspondence between social form and economic level. In fact, the hunter-gatherers of the degraded regions tell us nothing about past social forms, because whatever social forms may have existed among hunter-gatherers they could not, given the unfavourable environment, have been translated into anything other than a hunting-and-gathering economy; they are just as likely to be the expression of the greatest novelty. If we seek to know about the past, a field of study that has never

30. These forms that are present among hunter-gatherers other than those of Australia remain to be enumerated. I have done no more than characterize them in a very cavalier fashion, in contrast to those of Australia. There is evidently much material for research here. Such research should also be decisive because the theoretical proposition that I have put forward in respect of Australia cannot be considered valid unless it can be generalized. Its validity thus depends, for us, on the possibility of eventually theorizing the San, the Athapaskans, the Algonkin, etc., in the same terms as the Australians, in terms of social forms, social structures of production, isomorphism, etc.

seemed dishonourable to any discipline other than social anthropology, the point of departure should be hunter-gatherers in favourable regions, hunter-gatherers who might not have been such and probably remain such only by reason of restrictive social forms that for them are quite possibly a distant and glorious heritage.

Comments

BERNARD ARCAND

Département d'anthropologie, Université Laval, Cité universitaire, Québec, P.Q., Canada G1K 7P4.

24 VIII 87

This article is probably best understood as part of a much broader enquiry. It follows Testart's rather massive and scholarly research in the literature on hunter-gatherers and represents perhaps something of a pause, an effort to reconsider some fundamental theoretical issues, that might ideally lead to a better tomorrow.

The questions addressed here should concern all those interested in hunter-gatherers (I could hardly claim otherwise, since I raised the same points at the international conferences on hunting and gathering societies in Québec in 1980 and London in 1986). Does the category "hunter-gatherers" make any analytical sense? or Why should grouping people on the basis of their mode of extracting food from nature inform us about the nature of their societies? Is there any meaning in using this category beyond its obvious contrast to "agro-pastoralist" and "industrial" societies? Testart has already offered a major contribution to these questions with his convincing argument on the importance of food storage, which can generate radical social differences between societies otherwise equally based on hunting and gathering. He told us then that the concept of a Neolithic Revolution is inadequate in that there are often no sociologically significant contrasts between some societies of hunter-gatherers and some of the societies producing food by cultivating. Recognizing the importance of food storage was undeniably a step forward in refining the problem at hand. Unfortunately, much of the present article can be seen as taking us a few steps back.

The argument is difficult to follow and often downright confusing. Each time Testart leads us to understand how diverse and complex the reality of hunter-gatherers really is and how any generalization can only be partial and simplistic, he himself, after a brief moment of doubt and hesitation, leaps back to an interpretation that we thought had died with the French school of historical materialism of the past two decades. This makes for some rather blatant contradictions. For example, after telling us that in Australia game-sharing procedures are coherent with totemic organization and exogamy (the contrary would have surprised) and after

warning against searching for any simple causal relation among these domains, he immediately proceeds to declare that "it is clear that the privileged aspect of the problem, in which everything is interconnected, cannot be other than the form of the social structure of production"—which does not follow logically and is even directly contradicted by what precedes. Elsewhere, after suggesting a correlation between low development of techniques and cooperative efforts in food producing as characteristic of the Upper Palaeolithic, he adds to it a second hypothesis on the determination of technological developments by the "social structures of production," a quite separate hypothesis with no link to the statistical correlation and one that seems to make little sense given that these unspecified "structures" are now plural.

The article testifies to the intellectual honesty of a writer who wishes to respect the ethnographic facts he knows well. The problem arises from his adherence to a rather monolithic theoretical framework that seems quite incapable of dealing with these facts. At worst, this may even be damaging, as when the model fails and for lack of a better way out we are told that Australians suffered from a "cultural barrier" and that other hunter-gatherers have had "restrictive social forms." This hardly seems the way to restore evolutionism to respectability.

Testart appears obsessed with drawing a causal link between economy and social organization. His knowledge of the ethnography forbids any such straightforward relation, hence his numerous hesitations and claims that the issues are far from settled. At the same time, he frequently refers to "determination," which in this article runs every conceivable way to and from technology, environment, structures of production, subdivisions of society, etc. But he wants to remain faithful to the model, and this leads him even to suggest that if societies with similar economies have generated different forms of social organization, it is probably because their economies were never really similar. This is the kind of argument enjoyed by harsh critics and the kind of which Baudrillard wrote, some 15 years ago, that it reminded him of Baron Münchhausen trying to extricate himself from quicksand by pulling his own hair. One is left with the impression that the search can only be endless and the only way out would be for Testart first to reconsider his deeper conviction of the existence of separate levels of society, the "economic form" and the "social form," which can usefully be correlated.

Testart's earlier research has significantly modified our ways of looking at hunting-and-gathering societies. It should be obvious by now that the present article has not impressed me to the same extent. Yet, through his uncertainties, his doubts and even anxieties, which are never really helped by resorting to too narrow and simple a model of human society, he may well be approaching a rather promising reformulation of his original question: Has anthropology itself become the determining factor behind the categorization of people on the basis of their economic and/or social organization?

TIM INGOLD

Department of Social Anthropology, University of Manchester, Manchester M13 9PL, England. 24 VII 87

I share Testart's view that the anthropological study of hunting and gathering societies rests on the premises that such societies are essentially comparable and that some relationship exists between those of the ethnographic present and those of the more or less distant past. And I agree that to justify these premises, we are called upon to specify in what this relationship consists and to demonstrate that there is more in common to societies of hunters and gatherers than the mere fact that their members spend much of their time hunting and gathering. I cannot, however, go along with his reformulation of these crucial problems, which seems to me to put the clock back rather than forward.

Testart begins with a defence of what he calls a "reasoned" evolutionism, which apparently consists in noting that between present and past hunter-gatherer societies there exist both similarities and differences. But like the evolutionism of an earlier anthropology, the object remains a *comparative* one: to establish certain essential types of society and to arrange these types in some (not necessarily unilinear) order of progression. Whatever the validity of this kind of exercise, it is quite different from attempting to document, from ethnohistorical and archaeological evidence, the actual connections between particular societies as unique historical entities. Yet Testart seems to think his evolutionism covers that as well, maintaining in a footnote that the relations and connections he seeks between present and past societies are "necessarily historical." As I have argued elsewhere (Ingold 1986a), both "history" and "evolution" can be understood in at least two fundamentally distinct senses, and whether the terms are identified or opposed depends on which of these senses one chooses to adopt. But in asking "in what respects existing hunter-gatherer societies are continuous with those of the past" it will not do to interpret continuity at one moment as *formal similarity* and at the next as *unbroken genealogical connection*. The confusion is as elementary, and as damaging, as that between analogy and homology with regard to biological species.

Turning to the second major question, whether hunting-and-gathering societies have enough in common to render them comparable at all, Testart rephrases it by asking whether any relation exists, and if so of what kind, "between the techno-economic level of a society and the various aspects of its social organization." I wish to make three comments about this formulation. The first concerns the characterization of hunting-and-gathering as technique, the second its conjunction with economy in the hybrid "techno-economic," and the third the dichotomy between the latter and "social organization."

Testart recognizes hunting-and-gathering by "absence of domestication in respect of subsistence" such that nourishment is obtained from wild resources. He holds that this is a "technical definition" and prefers it to "a

definition in purely social terms, which would mix . . . terms referring to technical activities ('hunter-gatherers') with social forms." I challenge him to demonstrate that the distinction between domesticated and wild resources can be made without implicating social relations. As he well knows, both Ducos (1978:54) and I (Ingold 1980b:133; 1986b:133, 233) have argued that domestication carries an essential connotation of *social appropriation*. Testart is surely deceiving himself if he thinks that in speaking of hunting-and-gathering as the exploitation of non-domesticated ("wild") resources, he is referring only to the technical and not to the social aspect of practical activity. Where I have elsewhere defined hunting-and-gathering as the practical concomitant of a system of collective appropriation (Ingold 1980a; 1986b:222-42), I have not mixed the social with the technical but merely given explicit recognition to the fact that it is the social form of appropriation and not the morphology of the objects appropriated that characterizes the productive practice. Eliminate this social component of production and hunting-and-gathering reverts to predation-and-foraging, strictly comparable to the extractive behaviour of nonhuman animals.

If the meaning of the technical remains unclear in Testart's account—referring interchangeably to the activity (e.g., "hunting-and-gathering") and to its instruments (e.g., "bows and arrows")—the meaning of the economic is still more obscure. Hunting-and-gathering has been defined as technique, yet he goes on to speak of an economy defined in terms of hunting and gathering. Unable to decide whether it is economic or only technical, Testart conflates the two: thus again, hunter-gatherer societies "are grouped in terms of techno-economic level." If there is anything more to the economy than the technical form of activity, Testart does not tell us what it is—not, that is, until the closing passages of his paper, when he suddenly introduces the "social form of production," which governs "an important sector of the economy: the hunt."

Evidently, this social form is integral to the economy and in no sense technologically determined. Has Testart just discovered what many economic anthropologists (including students of hunting-and-gathering) have been saying for the past 20 years? If so, it is not apparent from the preceding pages, in which the key problem is repeatedly phrased as one of identifying "the causal connection between economic form and social form," as though the one were wholly *external* to the other. So it may be, if the economic is reduced to the technical. But that can hardly be squared with Testart's final restatement of the problem, where it appears as one of conceiving the connection, *within* the domain of the economy, "between the social structure of production and the technical development of production." And to return to my earlier point: if hunting-and-gathering is definitive of an economic form, and if the economy comprises both social relations and techniques of production, why is Testart so averse to introducing a social component into the very definition of hunting-and-gathering?

Having dealt with his two leading questions concerning relations between past and present hunter-gatherer societies and within such societies between the economic and social levels of organization, Testart proceeds to rehearse two arguments that he has already presented elsewhere, concerning the significance of storage and the apparently unique features of Australian Aboriginal societies. As regards the storage argument, I can only repeat my previous accusation (Ingold 1982) that Testart fails to show why *practical* storage, occasioned by the non-concurrence of production and consumption schedules, should lead to *social* storage, or the convergence of rights to resources upon a specific proprietorial interest. Likening storing hunter-gatherers to agriculturalists, he now maintains that "the former [do] with wild resources (products of gathering, fishing, etc.) *exactly* what the latter do with domesticated ones." This is a pretty strong claim to make, and there seems no *a priori* reason that we should accept it, rather than the contrary claim that storing hunter-gatherers treat their harvested resources exactly as "nomadic" hunter-gatherers treat unharvested ones. The ethnography can be read either way.

When it comes to Australia, though not a specialist, I have my doubts about the accuracy of Testart's gross characterization of Aboriginal economy and society. His rendering of totemism, for example, is more redolent of modern French than of traditional Aboriginal thought, more informed by a reading of Durkheim and Lévi-Strauss than by contemporary ethnography. On the subject of kinship and descent: cases of non-unilinearity seem to represent more than "doubtful exceptions" (see, for example, Layton [1983] on the Pitjantjatjara); indeed, the contrast between unilineal and non-unilineal systems may constitute an important axis of variation in Australia, as it does elsewhere (see, for example, Stuart [1980] on the comparable cases of the Yahgan and the Ona of Tierra del Fuego). It is decidedly odd that in setting up a distinction between Australian kinship systems and those (for example) of the Canadian interior, Testart makes no reference to the work of Turner (e.g., in Turner and Wertman 1977:96–110), who has already done this with rather greater sophistication and with the advantage of a first-hand knowledge of both ethnographic regions. Lastly, I object to Testart's negative characterization of Australian Aboriginal technology in terms of its deficiencies. The idea that Australia has "perpetuated an earlier technological stage" is preposterous, given that many essential elements of the Aboriginal toolkit (not found in Tasmania) were introduced as a result of outside influences no more than 5–6,000 years ago. By any standards, Aboriginal technology is quite elaborate, especially if one includes the knowledge that is just as vital as material equipment in the effective procurement of subsistence. Technological inferiority or superiority is notoriously hard to gauge, but had Testart chosen to emphasize the principles that Aboriginal hunter-gatherers *do* utilize rather than those they do *not*, he might have come to different conclusions.

In the 1950s, an important debate emerged among students of Australian Aboriginal society about local group

organization and the relation between bands and clans. Oblivious to the subsequent development of the debate, Testart writes in a footnote that though it has "shaken the foundations of Australian anthropology" it has "not perhaps received all the attention it deserves outside Australia." Within his peculiar time-warp, Radcliffe-Brown, Steward, and Childe are only just over the horizon, the ink is scarcely dry on the ethnographies of 30 years ago, and the signal advances in hunter-gatherer ethnography and theory that followed the publication in 1968 of the symposium volume *Man the Hunter* have yet to take place. Where the rest of us have moved forward since those days, Testart has gone into reverse. It is time he made it back to the future.

DOMINIQUE LEGROS

*Concordia University, Sir George Williams Campus,
1455 de Maisonneuve Blvd. West, Montreal, Que.,
Canada H3G 1M8. 26 VIII 87*

Testart's work on hunters and gatherers, of which the present article is the most recent development, leaves me with two contradictory feelings. One is of total intellectual excitement: his critique of the concept of the Neolithic Revolution is a masterpiece of epistemology, and it *de facto* imposes an entirely new way of looking at cultural evolution. I am not exaggerating, for an approach which demonstrates the flaws in concepts as basic as that of the band and of the Neolithic Revolution is bound to be both strongly contested by some and enthusiastically endorsed by others, leading to a revamping of the field. My other feeling is mixed. The present formulation of Testart's rethinking of the process of cultural evolution and of the relationships between economic, social, and cultural apparatuses is still not bold enough and seems to me to constitute a retreat from the headway he made in his book *Le communisme primitif* (1985b). As I said to him in the conversation he refers to, it is not that our ways of thinking are totally incompatible but that a renewal of the field requires us to distance ourselves still further from the classical anthropologists' conception of society. In other words, I go along with the critical part of his work and less with his counterproposal in this short article. In abbreviated form, here are my reasons:

First, there is the lesser problem of storers and inequality. Testart takes the position that there is a qualitative difference in inequalities between nomadic hunter-gatherers and storing sedentary hunter-gatherers. Although he is not too explicit about it, his reasoning seems to rest on the idea that among nomadic hunters inequalities exist only in the domain of prestige items, among which—it appears from his phrasing—are included women, while among storers the inequalities are in the spheres of production and food consumption as well as in that of prestige goods. However, this idea is erroneous. As fieldwork among Tutchone Athapaskans in the Subarctic (see Legros 1981, 1982, 1985) has shown me (unexpectedly, for I thought I had embarked on the

study of an egalitarian society), when individuals in a nomadic society take many wives it is, in a case like the Tutchone, not for prestige reasons alone but for the work output they represent. Similarly, when they capture members of their society, female or male, and make them their slaves, it is to make them work at productive tasks. Moreover, and more important, the same Tutchone case study demonstrates on a factual basis that nature provides human beings with "stores" of food "preserved live," such as year-round productive fishing holes, beaver colonies, etc. Even though such sites may have very low productivity by any standard, they take on tremendous importance when they are too few to feed the total population and no other resources are available. They may thus be, as among the Tutchone, very valuable "stores" which some subgroups appropriate as private property by controlling access to the locations where they are available. In fact the "store" analogy was spontaneously made by a bilingual informant in attempting to explain to me in 1972, long before Testart's thesis, the importance of such sites in the Tutchone past. As it can be demonstrated that the proximity of Northwest Coast society had nothing to do with these phenomena, one has to conclude that a nomadic hunting and gathering economy, and quite a poor one at that, provides just as many opportunities for the development of inequalities and of a "storage economy" as a sedentary hunting economy in a rich environment. If one argues that there is a difference of volume between the social surplus appropriated by the Tutchone elite and that accumulated by the elite of a Northwest Coast society I will agree, but a simple difference of volume in surplus extraction does not make a structural difference. Inequality is not a matter of surplus volume but one of structural relationships between the various components of a society. In consequence, I think it far more productive to take the position that all societies of hunters and gatherers have provided possibilities for the development of social inequalities in the full sense of the term; that all have had the potential for a "storage economy" of one kind or another; that in some societies poorly endowed in natural foodstuffs the volume of surplus extracted has necessarily remained quite low; that in other societies living in richer environments the volume of extraction has become much larger; and that in still other populations, not so well endowed with readily available natural food resources but well located for the development of horticulture or agriculture, surplus extraction volume has become quite large since the domestication of plants and/or animals. Since it is recognized, on the other hand, that societies of hunter-gatherers *did not have to* institute inequalities, and in fact some did not, but many obviously did at some point in the past with or without a rich environment, with or without the adoption of agriculture, and since it is evident that some groups in rich environments did not move toward stratification and that some who adopted agriculture did not institute inequalities, it seems to me that there is no point in trying to link the adoption of nonegalitarian structures to particular kinds of ecology and/or produc-

tive techniques. Following Balibar (in Althusser and Balibar 1970), who is himself in part following Marx, I think that what productive techniques determine is not whether a surplus will be extracted but only the social form in which a surplus may be extracted. In other words, like Engels (in Marx and Engels 1970:487), I take the minimal position that "we make our history ourselves, but, in the first place, under very definite assumptions and conditions." A stratified structure is a possibility that some people may succeed in instituting in any economic context. Its form will simply vary according to the sort of production techniques which are in place. While this thesis fits the ethnological and the archaeological record best, some anthropologists may object that it makes the emergence of social inequalities (note that I am not saying the form or the volume of surplus extraction) depend on the simple initiative of some subgroup in a society. However, I do not see why this should not be so. After all, when traditional cultural evolutionists link evolution to initial changes in production techniques, where do these changes come from if not from the will of some individuals in society? Are not new production techniques just as much a product of the minds of some human beings thinking and planning and others resisting the change as new social or cultural institutions?

The second point is more conceptual. Since I have already elaborated this point in two articles (Legros 1977, Legros, Hunderfund, and Shapiro 1979), I will only summarize. Two types of causality are at work in any society. The first may be called synchronic causality. It refers to the interdependence between an already constituted economic system and the sociocultural apparatuses which insure its maintenance or reproduction over time. The second may be termed diachronic causality. It refers to the matter that leads to the emergence of structurally new social facts. When we are dealing with human societies, any economic system is necessarily both and immediately a technological and a social phenomenon with rules for controlling labour, means of production, and the products of labour—rules which may or may not be egalitarian. Any society may include several socioeconomic systems as just defined, even a society of nomadic hunters and gatherers (see Legros 1978 for an Inuit group and Testart 1985b:224–25 for Australia, where the rules for apportioning products of the hunt and products of gathering as well as other spheres seem to have been different). Hence, questions of causality can never be posed in terms of an interrelationship between the *overall* economy of a given society and the *totality* of its sociocultural apparatuses. On the synchronic plane, causality is a question about how *each* of a society's economic systems is *synchronically* interrelated to *some* of its superstructural apparatuses, each constituting in the process a given mode of production in the extended sense of the word. On the diachronic plane, causality is a question of how disparate elements stemming from one or several of the economic systems and/or sociocultural apparatuses may by chance offer the building blocks for a new socioeconomic structure

and of whether opportunities thus offered are taken by at least some of the social actors making history themselves under the constraints and potentials of their milieu. I do not see why cultural evolution should be any less chancy and yet rule-governed than biological evolution.

To sum up, my dissatisfaction with the present state of the reconstructive part of Testart's article is that it remains embedded in the questions of traditional cultural evolutionism, which, as raised, are unanswerable; that it keeps the whole society as the unit of analysis when the dissecting of societal wholes into component parts seems a more heuristic approach for understanding cultural evolution; that it nowhere attempts to construct concepts of modes of production which in the long term would allow for a finer and much different probing of the ethnographic record. However, the critique bears only on the present article as it stands and only on what I call its reconstructive part. In the first place, *Le communisme primitif* utilizes in part the much more productive theoretical framework that I am referring to, and I strongly recommend it for the advances it makes (in fact, I am surprised that we do not yet have an English translation). His present reversion to the theoretical framework of traditional cultural evolutionists may have been intended to provide his CURRENT ANTHROPOLOGY Anglo-Saxon audience with a mode of thought it is generally more familiar with, but it takes away too much of the sharpness of his own previous terms of analysis. In the second place, but just as important, my critique in no way diminishes my admiration for his present debunking of the Neolithic Revolution concept.

ANTJE LINKENBACH

Reichensteinstrasse 48, 6903 Neckargemünd, Federal Republic of Germany. 19 VIII 87

Social anthropologists still call "hunter-gatherers" a large number of societies that show great similarities in the labour process and whose economy is characterized by the non-existence of cultivating techniques and stockbreeding and therefore represents a low level of technological development. To Testart this grouping seems unsatisfying and leads to what he calls the two major questions in the anthropology of hunter-gatherers: the relation between economy and society, as well as the existence and conceptualization of socio-cultural differences among hunter-gatherers, and the possibility of an analogy between hunter-gatherers of today and of the past. Obviously, these are problems in the discipline, but I think we have to admit that nowadays most social anthropologists are aware of them, especially of the first. Even Testart recognizes this, and I wonder why he does not mention the work of, for example, Cohen (1968), Fried (1967) and Godelier (1973a, b), who discuss evolutionary problems as well as problems of the articulation of economic and sociocultural phenomena in the con-

text of hunter-gatherer societies. (In addition, Cohen distinguishes between hunter-gatherers and hunter-gatherer-fishers and discusses the question of the Neolithic transformation.)

The major problem with Testart's treatment of hunter-gatherers, in my opinion, is his dichotomous conception of economy and social organization. On the one hand, this necessarily leads to (social or economic) determinism; on the other, it reduces man's complex relation to nature to labour or instrumental action, social relations mainly being viewed as relations of the labour process. What we need for the conceptualization of "primitive" societies (and not only these) is an approach which allows us to get detailed information on the main relationships of man—to nature (the labour process being only part of this), to the Other, and to the Self. Between these relationships there is a specific articulation, since they are based on a common principle, which in the case of hunter-gatherers without storage may be generally characterized as solidarity and communication between man and man as well as between man and nature (Linkenbach 1986). For each society the specific modes of relationship and their articulation must be formulated so that possible differences between societies will become apparent (for example, the role of women and their level of autonomy seem to be different in Inuit and !Kung societies). In addition, the possibility of appropriation of surplus as a precondition of social stratification and inequality seems to be only one aspect of the explanation of evolutionary processes. Above all, it implies a negation of the principle of solidarity and thus a change in consciousness.

In his analysis of the Australian Aborigines, Testart tries to isolate an interpretive logic, "at once social schema and schema of thought": the rule of not disposing of what is one's own, which applies to the three domains of sexuality, totemism, and economy. In my opinion he does not consistently develop this really important idea. He is presenting a key concept of Australian culture, and it would be interesting to relate it to these societies' concept of nature. The classification of "men and things" in "totemism" is based on a specific world view that denies essential barriers between man and nature (Lévi-Strauss 1962).

Finally, Testart presents the hypothesis of a similarity between Australian Aborigines and Paleolithic societies. For him, the reason for technological stagnation lies in the social conditions of Australian society, which allow no advantage for the hunter from the capture of game. In my opinion one should be careful in assuming a profit-oriented logic, which can easily become a constant element in human history. Moreover, to claim or deny an analogy between hunter-gatherers of today and of the past is always a question of one's conception of history. If one sees history only as a history of progress, societies that "lack" innovations (new technologies or aspects of social stratification) seem stagnant and therefore comparable to societies of the past. But we must bear in mind that man's inventiveness is not confined to those dimensions which seem significant to us (Lévi-

Strauss 1972) and that the processes of historical change need not be similar or even uniform in character.

JOHN MORTON

*School of Behavioural Sciences, Macquarie University,
Sydney, N.S.W. 2109, Australia. 10 VIII 87*

Testart has been masterfully and provocatively synthesizing data on hunter-gatherers for some time, and this paper testifies once more to the breadth of his knowledge. He is concerned with broad evolutionary questions and their relation to social theory, but a good deal of the paper approaches the specific question of the place of Australian Aborigines in hunter-gatherer studies. I will confine my remarks to this latter issue, particularly to Testart's conclusion "that Australia in some sense perpetuated the technical stage of the Palaeolithic."

Australian Aborigines have long presented a puzzle to anthropology: why do they have such elaborate religious practices and social forms when other groups, apparently with similar levels of techno-economic organization, do not? Testart suggests that we should first consider that Aborigines appear to have been technologically conservative and that this conservatism takes the form of a "cultural barrier" which prevents the adoption of new economic strategies in otherwise favourable conditions. In coming to grips with the why of this situation, he states that we may be mistaken in thinking that Australia's elaborate social and religious systems have no effects on production. To the contrary, meat sharing, for example, can be shown to be intimately connected with moiety and generation distinctions, with the injunction on a hunter to give up his produce for "the benefit of another who disposes of its distribution." Moreover, this injunction is part of a generalized principle of "surrender": "one may no more dispose of one's game than of one's totem or one's sisters." The elaborate social and religious dimensions of Aboriginal life thus appear to be governed by a single "*principle of intelligibility*," with three diverse domains—economy, kinship, and religion—brought together in "isomorphism." Testart admits, however, that he does not quite know how to place this isomorphism in an explanatory context. All that he will venture to say is that the rule of giving up game for others to distribute means that the hunter receives no material or moral advantage from increasing production and labour input and that there is therefore "no incentive . . . to experiment or to adopt new and eventually more efficacious techniques."

Conservatism is indeed a feature of Australian societies, and it was precisely in relation to the techno-economic field that Strehlow (1947:35) observed that "all occupations originated with the totemic ancestors; and here the native follows tradition blindly: he clings to the primitive weapons used by his forefathers, and no thought of improving them ever enters his mind." Similarly, Maddock (1970:177–78) has observed in the context of an analysis of myths of the origin of fire that

There is among [Aborigines] . . . a profound resistance to crediting themselves with their own cultural achievements. All they will claim credit for is fidelity to tradition or, as they put it, for "following the Dreaming," the cultural features of human societies having been established entirely by the acts of mythical beings who . . . are alone conceived of as active and creative, men being passive beneficiaries of unmotivated generosity.

If there is a technological conservatism in Australia, it is echoed loudly in the religious domain.

In addition, and as Testart suggests, religion does not simply account mythically for the origins of economic goods; it also in large measure takes credit for reproducing them (though not directly in the case of technological items). As Hamilton (1982:91) notes: "within the Aboriginal perspective 'religious' property has an aspect of 'economic' property, since reproduction of [totemic] species is held to depend on human actions over certain objects, jealously guarded and kept from all but their owners, at certain places from which all but the owners are excluded." Through "increase rites" each "local group was believed to perform an indispensable economic service not only for itself but for the population around its borders as well" (Strehlow 1970:102). Though the issue is more complicated than I can spell out here, this reproduction for others is closely tied to restrictions on the consumption and use of one's own totemic species.

Participation in "increase ritual" and similar cults is largely restricted to senior men, and the usual prerequisite for participation, initiation, is also the precondition of marriage. After initiation a man's "marriage must conform to the laws of his group; a perverted desire for women who are forbidden to him is one of the greatest bars in his struggle and search for further knowledge and the power that comes with wisdom" (Strehlow 1947:112). Thus, another level of Testart's "isomorphic" organization, marriage, is also closely related to the religious domain. Economy, religion, social organization, and even technological innovation all fall within the ambit of the Dreaming for Aborigines.

We should not underestimate the practical significance of the Dreaming: it is not some rarefied philosophical system or something that is "just symbolic." Testart's three "isomorphic" levels are very much practically integrated. For example, hunting in Central Australia is closely tied to the later stages of initiation, when groups of initiates are sent away to kill game and bring back meat to exchange for secret knowledge revealed to them by the elders in secret/sacred acts. "It is with meat that the old men have to be 'loosened,' so that they will reveal their great tjurunga [sacred objects, verses, etc.] which they are clutching so tightly" (Strehlow 1971:677). These prestations are part of a general scheme of appropriation whereby young hunters normally present game to older men in the band. This often takes the form of bride service: as a young man, a hunter is likely to be living with and working for his

father-in-law (cf. Peterson 1978), who also invariably has a key part to play in his ritual career, particularly as circumcisor. Gifts, especially of game, that flow from a novice to his circumcisor are "partly a recompense for the ritual services that have been rendered to the boy's [patri]lodge and partly an anticipatory payment of his bride-price" (Meggitt 1962:308). It is only after initiation and marriage that participation in "increase ritual" occurs, and such participation represents an identification of ritual actors with ancestral beings and their authority (Morton 1987).

This is a brief picture of a complex situation, but it is sufficient to highlight a problem with Testart's analysis. Australian hunters do indeed accrue advantages, both "material" and "moral," from their capture of game. However, these are not direct, and it remains true that hunters do not get prize cuts of meat or the prestige that comes from "ownership" and distribution of game. Rather, returns have to be measured in terms of *access to human resources and religious knowledge*: "Wealth in the Aboriginal social formation is assessed in terms of control of the reproductive and productive capacity of women and the religious property" (Bern 1979:123). In fact, the situation is yet more complex. Valued resources are not simply constituted by women and sacred knowledge: they also consist of all the social relations that are created through marriage and all the territorial access that comes from being attached to different places through knowledge of the Dreaming, sacred stories, songs, and objects always being associated with specific sites in the land. Aboriginal religion in this way "symbolically constitutes the society as a structure of reproduction" (Myers 1986:228), creating and re-creating ties between people, between "countries," and between the two. There is thus a great deal at stake in hunting: the ability of hunters to fulfil obligations to old men is inextricably bound to their general life prospects. Hunting to some extent determines with whom and where one may live, that is to say, the access one has to *labour and land*. At the very least, then, any "cultural ban on technological innovation" is unlikely to stem from a lack of interest in the products of hunting (cf. Sackett 1979), and the roots of conservatism have to be sought elsewhere.

Most of the examples I have drawn on here are from Central Australia, arguably the most conservative part of the continent. But such conservatism is a corollary of the strength of attachment to the tenets of the Dreaming. "Western desert people," for example, "are known throughout Australia for their conservatism and the strength of their adherence to the Law [Dreaming]" (Myers 1986:297), and it was the same pride in and devotion to the Law that caused Lévi-Strauss (1966:89) to remark that Aborigines sometimes appear to be "real snobs." But the key point (Lévi-Strauss 1966:89, my emphasis) is that

Australian societies have probably developed in isolation more than appears to have been the case elsewhere. Moreover, *this development was not undergone passively. It was desired and conceptualized,*

for few civilizations seem to equal the Australians in their taste for erudition and speculation and what sometimes looks like intellectual dandyism, odd as this expression may appear when it is applied to people with so rudimentary a level of material life.

Conservatism is not only a matter of inertia: it is also commitment to "social organization and marriage rules [that] require the efforts of mathematicians for their interpretation" and to a "cosmology [that] astonishes philosophers" (Lévi-Strauss 1966:243).

The incentive to invent and reproduce inventions stems from a sense of scarcity, and in spite of the popular image of Australia as an arid, unproductive land, there is no evidence whatsoever of Aborigines' having taken on a view of their country as unproductive. To the contrary, Aboriginal people view the land as wholly fertile, often waxing lyrical about its productive virtues, especially in myth. It may well be that to us "Australia appears a backwater in relation to the possibilities offered by neighbouring peoples with whom there is contact," but to Aborigines themselves it is no such thing. When it comes to considering conservatism, this latter view is all-important, since any "cultural barrier" to innovation is matched by the high value placed on the status quo. In Australia, there have been severe limitations on possible challenges to this high value, since inter-group contact has been restricted to an island that afforded few areas ecologically ripe for techno-economic development and few sustained contacts with peoples of "superior" ability. On the other hand, if Australia has developed less than any other part of the world in terms of the cultural appropriation of nature, it has developed far superior methods for dealing with other key factors of production—access to labour and land (or, in the Aboriginal idiom, access to people and country).

The adaptive value of Aboriginal social and religious organization is rarely underscored in hunter-gatherer studies, mainly because of the latter's abiding concern with techno-economic development. Testart is not especially guilty of ignoring the significance of the social and religious domains: indeed, he has often been at pains to draw attention to them. This particular paper, however, will contribute, perhaps unintentionally, to a certain myopia. Aboriginal ethnography testifies time and time again to the importance of people and place as resources, yet technology still holds pride of place in the general field. I have attempted to counteract that view because an overemphasis on technology tends to lead to the notion that this is the only kind of development worthy of study. However, to say that a society or culture is conservative is to say no more than it does not develop in a particular way. Conservatism in Australia is indicative of a "coldness" (Lévi-Strauss 1967:46–47) that describes not lack of development but a particular kind of development—a way of abolishing "the possible effects of historical factors on . . . equilibrium and continuity" (Lévi-Strauss 1966:234). The Law that applies in Australia is based on the dictum "once a precedent; twice a tradition" (Myers 1986:285). Development is here a case of

adapting to novel circumstances and giving those adaptations the weight of authority which comes from an eternal Law, enforced by men identified with it.

It is important to understand that Aborigines have been constantly modifying their social and territorial relations. These can change very quickly, and Aborigines themselves are only just becoming aware, particularly through the documentation of land claims, of the disjunction that often exists between people's assertions about their occupation of a territory and the historical record. In the former case, people tend to say that, because the Dreaming is "forever" and they now have the religious knowledge for particular places, they and their ancestors have always been at those places. In short, they are as reluctant to allow history into their myths as we are to allow myth into our histories. However, in giving techno-economic development pride of place in hunter-gatherer studies we are surely mythicising people to a grand degree. Hunter-gatherers are more remarkable for the way they allocate natural resources (the land and its products) through social relations which have as their basic "materialist" teleology the reproduction of people, not goods.

Testart may be correct in seeing Aborigines as representing a picture of "a distant and glorious heritage": this is indeed how they see themselves. But to concentrate only on the nature of "restrictive social forms" is to draw attention away from the genius of those forms in dealing with historical contingency. Let it be said that Aborigines may be the closest thing living to "natural man," but only if it is simultaneously recognised that "natural man" has a culture which is largely in accord with what he is rather than what he will become. Aborigines, having perhaps more than anyone restricted the expansion of cultural forces of production, have also developed to the greatest degree the means for managing natural contingencies (as opposed to cultural ones, which call more and more upon technological invention). The vagaries of demography and the environment are with each Aboriginal generation subsumed under the rubric of the Dreaming, in a scheme which is fundamentally religious and "world-accepting" (cf. Stanner 1963) and whose general principles have stood the test of time. One senses that if hunter-gatherer experts spent as much time classifying modes of mythical consciousness and religious artefacts as they do technological items, the reconstruction of the past might be considerably enhanced. Moreover, this is perfectly possible, because the study of transformations over space is on an equal footing with the study of transformations over time (Lévi-Strauss 1966:256-62). There is no history without myth, just as there is no myth without history.

NICOLAS PETERSON

*Department of Prehistory and Anthropology,
Australian National University, G.P.O. Box 4,
Canberra, A.C.T. 2601, Australia. 17 VIII 87*

It has long been argued that Australian Aboriginal societies are unique. In the 19th century this uniqueness

was unproblematic and accounted for by an evolutionary paradigm that saw Aborigines as transitional beings on the bottom rung of the human ladder. Their ethnographic interest arose from the fact that they were thought to open a window onto the origins of religion, marriage, and property. With the demise of the evolutionary paradigm, the world at large lost interest in Australian ethnography and left it to regional specialists.

The claim of uniqueness emerged again at the Man the Hunter conference in 1966, when Murdock, in discussing whether hunter-gatherers are a cultural type, commented: "The Australian evidence, however, makes this seem dubious. I suggest that we recognize the near uniqueness of Australian social organization and pay more attention than before to attempts to explain their sharp divergence from similar societies elsewhere in the world" (1968b:336). Instead of being bilateral with a prominent nuclear family, said Murdock, the Australians are unilinear, largely characterised by double descent, and have residence rules that are overwhelmingly patrilocal and polygyny in a more developed form than is found among other hunter-gatherers. The Australians also posed problems for Woodburn's (1980) dualistic classification of hunting and gathering societies into immediate- and delayed-return, since they had most of the features of immediate-return societies but uncharacteristically protracted marriage contracts that led him to classify them with the delayed-return societies. For Testart it is unilinearity, pervasive dualism, and multiplicitous forms of totemism that mark out the Australians. Generally, then, the problem is that the Australians' elementary forms of religious and social life are nothing like as elementary as once thought: indeed, one well-known anthropologist remarked at a recent conference that Durkheim should have written about the people he worked with for really elementary forms.

What is the reality of this perceived difference? Undoubtedly there are some unique aspects to the Australians' situation: principally their occupation of an entire continent for a long period of time with minimal external contact. But is this sufficient to account for the perceived difference, or are there distinctive principles underlying Australian social life? Could it not be that the way we have constituted and reconstructed Aboriginal life is part of the problem? The basic ethnographies of Aboriginal life were carried out in the heyday of structural-functionalism by anthropologists working with Aboriginal people who had given up the hunting-and-gathering life to live in settled communities of one kind or another. The normative focus of structural-functionalist accounts was thus reinforced in two ways: residence and land-use practices had to be reconstructed, with the result that common strategies and particular periods of the life cycle were emphasised by informants and converted into rigid rules by ethnographers; and the variability of social practices across the continent was frequently elided so that the difference, for example, in polygyny rates between the desert and northern Australia was obscured in the creation of an Australia-wide norm. These points having been made, the distinctiveness is not dissolved, but is it a quantitative or a

qualitative distinctiveness? Are Aboriginal cultural and social organisations not merely distinctive but disjunctive with these of other hunter-gatherers to the extent that they challenge the loose unity assumed to be created by material constraints? Do they manifest intensification and formalisation of features commonly found amongst other hunter-gatherers, or are there really unique principles underwriting their way of life that put them in a category all their own?

All of the features deemed distinctive are found among one or more hunter-gatherer societies elsewhere: lineality as ideology, if not as actuality, is common, if often only weakly developed and usually more filial than lineal; dualism is not unknown; totemism was "discovered" in North America; and polygyny is found from the San to the Inuit. Thus in these features, at least, there are no discontinuities but only intensifications. Testart, however, moves beyond listing distinctive traits to argue for a distinctive underlying principle of Australian social life: people may not dispose of what is their own or what they are "close to." Thus members of a clan cannot dispose of the women of their own clan in marriage, nor can they eat their own totem where they are responsible for reproducing it for other clans; the hunter cannot distribute the game he kills; and, although Testart does not put it in this way, effectively missing the major example of this principle, the owners of rites are frequently dependent on categories of non-owners to perform them. Mad-dock (1974:42) speaks of these kinds of principles as fostering mutual dependence, balancing the parochialism created by local rights and sentiments, while for Testart they are evidence of the collective appropriation of nature and a communal organisation. This communal organisation is also manifested, he argues, in the preference for cooperative modes of hunting over technological innovations that are inimical to such cooperation. Setting aside reservations about and/or qualifications of all these points, it does not seem to me that either the allegedly distinctive traits or Testart's principle of intelligibility strike at the core of the distinctiveness of Australian cultures.

I would have thought that the distinctive features of Australian societies and cultures are the elaboration of the religious life, the complexity of the cultural structuring of the landscape, and the marriage exchange systems. Fundamental to all of these is the importance of an economy of knowledge. This is integrally involved in the existence of polygyny, the distribution of meat, the functioning of unilineal groups, and the organisation of ceremonies. It is also related to territorial organisation and the elaboration of rights in land. Economies of knowledge, such as are found in Australia, are quite antithetical to the collective appropriation of nature and the communal organisation which Testart is arguing are unique to it. They in fact lay the grounds for powers, rights, and interests in ceremonies, land, and people that are exclusory and the basis of inequality. I would also argue (see Peterson 1986 for elaboration) that they help create a distinctive demographic regime which makes generations traced through males twice the length of those traced through females, laying the foundation for

ideologies of patrilineal continuity. Thus I suggest that if there is a key to understanding Aboriginal societies it lies in the centrality of economies of knowledge. Whether or not such economies of knowledge were characteristic of Pleistocene hunter-gatherers must surely remain in the domain of speculation, but, adopting the kind of evolutionary argument used by Testart, elaboration and complexity would suggest that they are relatively recent appearances.

In sum, I am not persuaded by Testart's central argument, although I find his analyses highly stimulating and the questions he asks provocative and worthwhile. It is a pity that his more extensive treatment of these issues is not available in English to stimulate the debate they deserve.

D. R. RAJU

*Department of Archaeology, Deccan College
Postgraduate and Research Institute, Pune 411006,
India. 13 VIII 87*

At the outset let me applaud Testart for adding a new dimension to the study of hunter-gatherers. His critique of the 19th-century unilineal evolutionism based on biological models and of Gordon Childe's Neolithic Revolution is refreshing vis-à-vis Kabo's (1985) article on the food-producing economy, wherein it is argued that the Neolithic Revolution is valid. His concept of the economic structure of storing hunter-gatherers as a separate category capable of explaining "agriculture not as an economic factor of radical or universal importance but as a technological factor that becomes decisive only under certain environmental conditions" seems plausible and renders Childe's grand conceptual framework untenable.

Testart's review of hunter-gatherer characteristics—economic, technological, and social—from a global perspective and of the peculiarities of the Australian situation, where social organization plays a pivotal role, is impressive and instructive and should be of the utmost usefulness for prehistorians all over the world. It is unfortunate, however, that he dismisses the hunter-gatherers of the tropical regions of Africa and Asia as subsisting as such only through links with neighbouring agro-pastoralists. The Cholanaickens of Kerala lived until recently in caves remote from agro-pastoralists, and so did the Kadar of the Anamali Hills of Tamilnadu. The Chenchus of the Nallamalais (von Fürer Haimendorf 1943) and the Yanadis of the south-east coastal plains of India were until the beginning of this century hunter-gatherers par excellence (Raju 1981, Murty 1981).

While Testart's development of the theme of the articulation of the economic and social factors is cogent, the second theme, relating the ethnographic present to the prehistoric past, seems as elusive as ever. The last quarter-century has witnessed a methodological battle and theoretical debate in archaeology, especially ethnoarchaeology (Moore and Keene 1983), and a spate of publications has emerged as a result in both the New World and the Old (Asher 1961; Binford 1978, 1983; Binford

and Binford 1968; Flannery 1972; Orme 1973; Jochim 1976; Lee and DeVore 1976; Yellen 1977; Yellen and Harpending 1977; Gould 1978, 1980; Schiffer 1978; Wobst 1978; Clark 1980; Hodder 1981; Winterhalder and Smith 1981; Vogel 1986). Testart conspicuously evades discussion of this literature in trying to relate the modern hunter-gatherers to those of the past. His global conclusions are difficult to accept. He says that Australia in some sense perpetuated the technical stage of the Palaeolithic. I assume that he means the European Upper Palaeolithic (there is no Palaeolithic in Australia). It should not be forgotten that the Palaeolithic is not the same everywhere in the Old World and that the European Upper Palaeolithic was a glorious period compared with the Mesolithic. And what about the environmental conditions of Pleistocene Europe? In evaluating the Australian hunter-gatherers, Testart takes for granted environmental differences in different parts of Australia in favour of his argument. Moreover, he attempts to piece together Australian ethnography and European Palaeolithic archaeology to arrive at global conclusions. What a pity! While Sollas's (1911) ideas are undeniably naive, I wonder if Testart has not fallen into the same trap as regards his "reasoned evolutionism," the idea that technological development is caused by the social structure of production in Palaeolithic times—guesswork, pure and simple. Is this yet another case of grab-bag analogy, and a general comparative one at that?

CARMEL SCHRIRE

*Department of Human Ecology, Rutgers University,
New Brunswick, N.J., U.S.A. 27 VIII 87*

We are indebted to Testart for resuscitating the question of whether modern hunter-gatherers and their Toyota-owning descendants reflect significant aspects of Palaeolithic life. It was, I feared, a question whose time might have come and gone when I wrote the essay Testart cites (Schrire 1984). Clearly the matter is still of concern, and consequently I shall try and answer some of Testart's arguments.

Testart concentrates his attention on Australian Aborigines who continued to hunt and gather their food using a simple technology, no storage, and no farming, despite the absence of constraints on a tropical agricultural system such as is and was practised just to their north in places like New Guinea. He sees a link between the Aboriginal practice of letting credit for production accrue to someone other than the producer himself and their patent ban on cultural innovation. Thus their simple technology coupled with their cooperative behaviour links them to Palaeolithic societies in their pre-farming state rather than to societies found in degraded regions where farming could not have been practised because of environmental constraints.

Interesting though this stance may be, it is flawed by outdated sources. Testart's Australian sources are only three in number. One dates back some 80 years, and the other two were written some 15 years ago and have been

superseded by new and better findings. The rich literature of the past decade includes an extensive review of the environmental differences between North Australia and Papua New Guinea (Jones and Bowler 1980) that is at pains to explain why transferring tropical horticulture to Arnhem Land was hardly as easy as it seems in Testart's outdated sources (Golson 1972, White 1971). In addition, recent publications reveal the importance of Aboriginal harvesting strategies with respect to their ability to accumulate enough food to provision large gatherings of people in an intensification of their social life; work by Meehan (1982:66), Beaton (1982), and Lourandos (1985) includes a small sample of such findings and casts interesting light on the proposition that Aboriginal people never stored food or, more important, never had enough surplus to maintain more complex social relations. Finally, there is the question of the simple Australian technology: Testart observes that, lacking bows and blowpipes, they never grasped what can be done if you understand the principle of compressed air power. He forgets, of course, that in their use of artefacts such as trident fish spears and boomerangs, Australians had mastered what to do about distortion due to refraction as well as some rather tricky problems of aerodynamics. It occurs to me that a major problem in the anthropology of hunter-gatherers may be that its proponents are not quite as familiar with the literature as they might be.

ERIC ALDEN SMITH

*Department of Anthropology, University of
Washington, Seattle, Wash. 98195, U.S.A. 21 VIII 87*

Although much that Testart has to say is reasonable (if ponderously expressed), I have several problems with his argument. My criticisms fall into two categories: those concerning his general theory of the articulation of economic and social forces and those specifically concerning his discussion of Australian Aboriginal societies.

Setting aside the evolutionary issue (which is given only a rudimentary sketch), the major *problématique* in the paper is the relation between technoeconomic factors and social organization. While we can hardly expect a short paper to resolve issues that have concerned scholars in hundreds of volumes, we can expect better than we get here. Testart's simplistic notion of causal explanation—a linear determinism whereby either the economic base determines the superstructure or vice versa—leads him to two unfortunate (and incompatible) conclusions. First, in searching for such linear determinism he relies on highly aggregated categories ("technoeconomic level" vs. "social organization"); as a result, causal mechanisms remain hopelessly obscure. Arguments over the causal priority of base vs. superstructure are inevitably sterile unless these categories are disaggregated to the point where specific causal explanations can be tested. Realizing the difficulty, Testart takes a second tack: rather than causal explanations, he often rests content with stating correlations (which he some-

times terms "isomorphisms") between economic and social factors. But correlations are not really answers to anything; they are questions in search of answers.

I see two ways out. One is via a more rigorous form of functional analysis (Cohen 1978), preferably involving a specification of the way in which the preferences and capabilities of individual actors generate social trends and constraints (Elster 1982). (Testart makes some very tentative moves in this direction in his discussion of Australian game-sharing rules.) Another escape from simplistic linear determinism is via the recursive logic of natural-selection theory, wherein the set of traits existing at any one point in time and the conflicting goals of the individuals bearing these traits constrain (partially determine) the strength and direction of various selective forces (Maynard Smith 1978, Boyd and Richerson 1985). In either case, it *may* turn out that what we call "technoeconomic" factors have greater influence on evolutionary trends than do other sorts of factors, but this is a far cry from one-way determinisms, on the one hand, and acausal correlations or systemic "articulations," on the other.

Turning to the Australian Aborigines—Testart's "test case"—I find that the problems of general theory just noted are less evident when he deals with ethnographic material, but other problems take their place. The key hypothesis is that certain social factors in aboriginal Australia, specifically rules of game division, constrained or blocked technoeconomic development. In Testart's view, given a rule (expressive of a general structural opposition or complementarity in Aboriginal culture) that the hunter has no rights to the game he kills, "there is no incentive . . . to increase labour productivity, to experiment or to adopt new and eventually more efficacious techniques." Testart concludes that social factors have blocked or constrained Aboriginal technoeconomic development; specifically, he claims that the sharing rule has impeded (1) efficiency-enhancing improvements in hunting tools, (2) diffusion of horticulture, and (3) storage, specifically meat preservation.

At the general level, Testart's argument has a number of interesting features. From one point of view, it is fundamentally a rational-choice (self-interest) explanation: individuals (rationally) choose not to invest in technical innovation because the profits (in food and prestige) from doing so would not accrue to them (given the game-distribution rule). From another angle, it is a variant of Marx and Engel's thesis that contradictions will arise between existing social relations of production and potential development of the productive forces; what seems to be novel (or, considering "Oriental despotism," maybe not so novel) is the thesis that this contradiction, rather than leading to social change, remains frozen from Paleolithic times on.

I leave questions regarding the accuracy of Testart's ethnography to the Australian specialists and those on the technical backwardness of Australia to the archaeologists. Some severe problems with the argument remain. First, Testart has "explained" the anomalous status of Australians among nomadic hunter-gatherers

by reference to an *existing* social/symbolic structure, which itself remains unaccounted for. Rather than being an evolutionary or historical materialist argument, then, it is a static one. Second, of the three factors Testart lists as evidence of social blockage of technoeconomic development (see above), only the first fits his thesis; I fail to see how rules depriving a hunter of the benefit of his own kill can provide disincentives for modification of vegetable foods (not widely shared, and in the domain of women's production) in the direction of horticulture or for the preservation of meat *once it has been distributed within a local group*. Third, I am not convinced by the sparse evidence supplied that the rules governing division of game are actually a result of a general Australian proclivity for social/symbolic oppositions; alternative explanations, some ecological, should certainly be considered as well.

In conclusion, I suggest that Testart needs to develop a more rigorous argument, one that specifies causal mechanisms in more detail and that does a better job of accounting for the data bearing on his main claims regarding the effect of game divisions on labor productivity. I have no problem in principle with the notion that existing social practices can constrain technoeconomic development or that sharing rules in particular can have great effect (Smith 1985), but Testart's argument is simply too loose to be convincing.

M. SUSAN WALTER

*Department of Anthropology, Saint Mary's University,
Halifax, N.S., Canada B3H 3C3. 20 VIII 87*

Testart argues that since storing foragers are often characterized by sedentism and social inequality, the "Neolithic Revolution" was not solely responsible for the emergence and spread of nonegalitarian societies. Rather, domestication of food sources allowed for a wider geographical distribution of nonegalitarian societies and the development of states. These are good points. However, the extent of departure from egalitarianism that foraging has supported is still under debate (Moseley 1975, Wilson 1981). Also, the storing foragers identified by Testart (1982) vary markedly in degree of social inequality and sedentism.

Localization and predictability of resources should perhaps be added to the conditions Testart considers necessary for storing foraging. Also, sporadic natural disasters may provoke storage even where resources are not seasonal. Testart considers storing incompatible with hunting of land fauna because of the greater time entailed in processing meat. However, storing foragers are frequently fishing people, perhaps because fishing is more often a low-risk, high-return activity (Lee 1968), and fishing resources are more often localized. Hunting requires more mobility *in general* than fishing and transportation of processed or untreated meat over longer distances. Localization of resources near a home base or in the vicinity of fixed camps occupied in rotation facilitates women's participation in food processing. Among

storing foragers women do all or much of the food storage.

Testart questions the usefulness of the category "forager" because it ignores significant differences among peoples lacking plant and animal domestication and similarities between storing foragers and food producers. There has been a tendency also to accentuate differences between egalitarian foragers and horticulturalists. For instance, Service (1978) assigns the Arunta to a "band" level of societal complexity despite the presence of clans that crosscut residential groups, while egalitarian horticulturalists like the Jivaro are designated "tribal." Although horticulturalists are usually more sedentary than nonstoring foragers, both may be markedly egalitarian and have minimal emphasis on food storage.

Testart links the underdevelopment of Australian Aboriginal hunting equipment and techniques to a lack of motivation of hunters occasioned by their methods of distributing game. There is ethnographic evidence, however, that excellent hunters fared better in competition for wives. A woman was less likely to object to marriage arrangements if her betrothed was a superior hunter. Adult male suitors, who frequently arranged their own marriages, could favourably impress potential in-laws if they outdid rivals in gifts of meat (Tonkinson 1978). Polygyny brought prestige, economic advantage, political alliance, and greater security in old age (Berndt and Berndt 1964). Failure to provide meat for older initiated men occasioned punishment of various sorts including withholding of wives (Strehlow 1970; Birdsell 1975:377-79). There was reason for Aboriginal men to want to excel in hunting.

The most sophisticated hunting technologies would be expected in areas where hunting constituted a major food source yet presented considerable challenge, as in the Arctic. Is the Australian hunting arsenal so different from that of other predominantly gathering foragers? Testart says that it is "slightly but significantly inferior." Nonetheless, Australian hunting tools, techniques, and knowledge of game appear to have met aboriginal needs. Much of Australia is desert, population densities were low, and some devices might not have been useful. Relative isolation may have limited diffusion.

The proposed "cultural ban on technological innovation" is not an altogether convincing explanation of the failure of horticulture to spread to Australia. Processes whereby foraging has been replaced by food production have probably involved competition for land as well as voluntary adoption by foragers of horticulture. Horticulturalists may frequently have prevailed over *nonstoring* foragers in such contests because they often have advantages of numbers and more formalized leadership. Since no indigenous development of horticulture occurred in Australia, foragers there lacked competition from immediate neighbouring horticulturalists while foragers on other continents did not. Perhaps this relates to the persistence of foraging in northern Australia.

Testart comments on the considerable diversity among foragers that cannot be accounted for by refer-

ence to the storing/nonstoring distinction. For example, he sees Australians as virtually unique among nonstoring foragers in that they emphasized unilinearity, dualism, and totemism. He acknowledges that totemism and clan organization occurred among western Athabaskans and southern Algonkins but attributes this to influence from neighbouring cultivating and/or storing peoples. But these non-Australian examples of unilinearity among nonstoring foragers should not be dismissed in this way, since foragers like the !Kung also lived close to food producers without adopting unilinearity. In addition, it is questionable whether western Athabaskans should be classed as nonstoring (Testart 1982). A precise way of distinguishing storing and nonstoring foragers and/or of measuring *degree* of emphasis on food storage is needed.

Nonetheless, Testart points to issues requiring investigation: why do many more storing foragers than nonstoring foragers emphasize unilineal descent, and why do some nonstoring foragers (including Australian Aborigines) deviate from the more usual nonstoring pattern of bilateral descent reckoning?

MAREK ZVELEBIL

Department of Archaeology and Prehistory, University of Sheffield, Sheffield S10 2TN, England. 20 VII 87

Testart's contribution can be divided into two major sections. In the first, inspired by the work of Childe, he contrasts hunting-gathering with farming as two different modes of production. He goes on to distinguish between storage-using foragers and the more common nomadic hunters as two distinct forms of hunter-gatherer adaptation. In the second section, he considers kinship as a socio-economic mechanism that promotes or hinders economic intensification. Using the Australian Aborigines as an example, he argues that their kinship structure and ideology effectively prevented economic intensification and the development of food production. The underlying question is, of course, why some societies develop or adopt farming (or a storage-using sedentary foraging economy) while others do not.

Most of these points have been raised before, either by Testart himself (1981, 1982a, b) or by others (Sahlins 1974; Bender 1978, 1981; Rowley-Conwy 1983; Zvelebil 1986). The new elements are Testart's skillful combination of archaeological evidence and anthropological observations and his insistence that the social structure of production, mediated by kinship relationships, has acted as a means of social control on the development of food production. In my view, however, Testart goes too far in drawing the distinctions between nomadic hunter-gatherers, storing hunter-gatherers, and farmers while not going far enough in considering the implications of social control over the use of resources in a hunter-gatherer society.

I agree with Testart that lumping all hunter-gatherers together as one type of society, characterised by a "band level of organisation," a "hunting-gathering economy," and "nomadism," does not do justice to their diversity,

especially when seen in diachronic perspective. But rather than stressing “ruptures,” as Childe did and as Testart has done, one should stress the continuity and variation that more appropriately describe the evolution of hunter-gatherer societies towards intensive foraging or farming in many parts of the globe. Neither of the two “ruptures” observed by Testart—that between nomadic foragers and sedentary storers and that between farmers and hunter-gatherers in general—can be supported either by archaeological evidence or by more theoretical considerations.

Regarding the first “rupture,” Testart exaggerates the difference between storers and nomads. It is not true that the connection between sedentism and storage is simple and obvious (footnote notwithstanding). On the contrary, it is complicated and subtle. Nomadic hunters, primarily dependent on meat, do in fact make extensive use of storage in northern latitudes (Eidlitz 1969, Binford 1978). Other foragers can make only occasional use of storage, even though they appear to be sedentary or transhumant. The issue is further complicated by the difficulties of the recognition of storage, sedentism, and socio-economic inequalities in the archaeological record. Contradictory claims for the presence or absence of such features have been made for both the Palaeolithic and the Mesolithic.

The second “rupture,” generally known as the Neolithic Revolution, is equally implausible in many parts of the world. Ethnographically, we have in many areas of the world societies dependent on semi-domesticated resources (the Lapps, some tropical cultivators) or societies utilising a mixture of wild and domesticated resources. Neither of these fit Testart’s normative categories. Archaeologically, as often as not we have faunal assemblages with a mixture of wild and domestic fauna in cultural contexts that are clearly transitional between the Mesolithic and the Neolithic (Zvelebil 1986). The question arising here is at what juncture the organisation of production shifted from the hunter-gatherer to the food-producing mode. The shift was probably gradual, and the evidence for the most part does not support the notion of a clear break.

Further, it is not true, to my mind, that storers “have the same economic structure as cultivators of cereals, the former doing with wild resources (products of gathering, fishing, etc.) *exactly* what the latter do with domesticated ones.” Hunter-gatherers harvest *wild* plants, fish, and game; at best, they manipulate the environment to increase the productivity of their resources. Cereal farmers manipulate *domesticated* plants themselves: they control their reproduction and can increase their productivity through selective breeding as well as environmental alteration. As Testart notes, forager storage and agriculture were in some ways parallel developments (in that they increased productivity or reliability of resources), but this point has been made before (Harris 1977, Zvelebil 1986).

In summary, one can find little evidence for Testart’s assertion of a double rupture in the development of hunter-gatherer societies. It seems, in fact, that Testart

has chosen to emphasise one of the more controversial and probably mostly erroneous aspects of Childe’s work, a distinction, moreover, that is unnecessary to the more original and interesting feature of Testart’s model, the notion that kinship relationships and ideology control resource intensification in hunter-gatherer societies. This notion is certainly not new (see, e.g., Sahlins 1974; Bender 1978, 1981), but Testart adds to the discussion by giving us a concrete illustration of the way such forms of social control worked and by insisting that the social environment, the social structure of production, was responsible for the rate of technological change towards the end of the Palaeolithic and in the Mesolithic—in other words, by bridging (in some cases) or failing to bridge the ruptures he has defined earlier. And here we return once again to the archaeological problem: how to recognise in the archaeological record kinship patterns, ideology, and social structures to the extent of allowing us to decide whether or not they aided intensification. No solution has yet been found to this problem.

Finally, Testart’s model has some interesting implications for the organisation of hunter-gatherer society in general. Implicitly, it challenges the notion of the egalitarian “band-level” society as the norm for hunter-gatherers, effortlessly maintained given a reasonable state of equilibrium. Rather, it seems to suggest that a degree of social control is required to maintain hunter-gatherer society in an egalitarian state—a set of rules to provide the disincentive to intensification and the accumulation of wealth. Taking the argument a step further, one could ask what effort and energy are expended in support of the egalitarian society. Are the relevant forms of social control embedded in the social structure, not requiring any special support, or does it in fact take additional effort to symbolise and enforce them? Considerations such as these will perhaps allow us eventually to view the social development diachronically in terms of the relative costs of maintaining an open, non-stratified society as opposed to promoting the development of social and territorial circumscription.

Reply

ALAIN TESTART

La Bosse, 60590 Sérifontaine, France. 25 IX 87

Certainly it was a perilous undertaking on my part to attempt to summarize in only a few pages the analyses and reflections that have taken up two books (the second of nearly 600 pages) and a number of articles. For briefly reporting the conclusions without presenting all the facts on which they seem to me to be based, I have been accused of jumping to conclusions. Thus Linkenbach is surprised that no mention is made of the works of other scholars on the articulation of economy and society; I would point to my second book (1985b:17–51), which begins with a long critical appraisal of prior work on

hunter-gatherers from a Marxist perspective. Similarly, I would refer Raju, who finds it "unfortunate" that I set aside the hunter-gatherers of tropical Africa and Asia, to my 1981 article, in which I developed my arguments in depth. Zvelebil reminds us that even nomadic hunters had recourse to storage, but this is a central issue in my argument that I have dealt with repeatedly, distinguishing different functions of storage depending on the type of society (1982a:161, 169, et passim; 1985a:11-12). Schrire takes me to task on the question of Australian techniques and does not hesitate to suggest that I am unfamiliar with the literature; doubtless she overlooks the summary character of this article and, if she were to take a look at the pages of which I have only summarized the conclusions, might judge otherwise (on hunting techniques, see 1982b:97-129, which contains a discussion of the boomerang that I have "forgotten"; on food preservation and harvesting techniques, see 1982a:167-73; etc.). The possibility of an aboriginal horticulture in northern Australia remains open to debate, but I do not think that the views of those who established the basic similarity of the tropical environments of southern New Guinea and northern Australia can be so easily labeled "outdated." Even if Jones and Bowler (cited by Schrire) have been able to show significant differences, it must not be forgotten that horticulture is eminently adaptable, as the extraordinary ecological and agricultural diversity of New Guinea attests. It is always advisable to specify under what conditions such-and-such development seems possible or impossible; it is all a question of nuance, something that is not much in evidence in Schrire's comment.

To this first difficulty is added another, that the theoretical perspective of my first work is not that of the second and that I have not, I think, concealed my uncertainties, my changes of mind, or my self-criticism. All this has made the commentators' task very difficult.¹ Those among them who are familiar with my earlier work (Walter, Arcand, Legros) seem somewhat less put out by the new perspectives that I propose. Walter offers temperate criticisms and poses sensible questions that I can at the moment answer only in part, and she takes due note of the question (which I consider central) of the different positions of unilinearity among the Australians and among other nomadic hunter-gatherers. Arcand seems especially charmed by the critical part of my work and would settle, I think, for a philosophical position close to a generalized scepticism. In spite of the generally sympathetic character of his comment, he occasionally raises some strange criticisms. Thus, he argues that my assertion "It is clear that the privileged aspect of the problem, in which everything is interconnected, cannot be other than the form of the social structure of production" does not follow logically from what precedes it and is even contradictory with my idea of isomorphism be-

tween game-sharing rules, exogamy, and totemism. But he has misread me here, overlooking the fact that after the passage on isomorphism of social forms I change the subject and return to the question of the relation between economy and society: *relative to this question* the point of resolution is obviously the social form of production because it is at once a social form and economic in nature (whereas the other isomorphic forms lack this dual character). I agree that I am "obsessed"—as he claims—but I would rather that it be with logic than with something else. I ask myself whether the "obsession" that he believes he detects in me "with drawing a causal link between economy and social organization" is not instead the echo of his own obsession with *not being able* to do so.

Legros's comment is certainly the best informed because he has read the two works that the present article attempts to summarize. He congratulates me on my critique of the notions of the Neolithic Revolution and the band. I am not sure, however, that he has entirely understood the theoretical reorganization that I propose in my second work. In terms of the first, the case of the Tutchone and the western Athapaskans more generally was mixed and difficult to classify (Testart 1982a:120); in particular, it was difficult to understand why these societies were so egalitarian when the development of storage was apparently so limited (compared with that on the Northwest Coast). This difficulty no longer exists with the new theoretical approach that I propose (1985b and the second part of this article) because here the social form becomes the first cause; thus it becomes conceivable for a social form to exist without the society's developing all its potentialities on the level of production. Supposing that the western Athapaskan societies and those of the Northwest Coast were structured by analogous social forms, one can conceive that the latter had been able to develop an economic structure of storage thanks to their favourable milieu, whereas the former displayed all the indices of a similar society (inequalities, attenuated forms of the potlatch, privileges associated with forms of property in certain places, etc.) but lacked as stable a productive base because of environmental conditions. It would simply be a matter of one of the effects of blockage of which I speak at the end of the present article. In the second part of his comment Legros seems disappointed that I have had no recourse to the concept of mode of production, but on this subject he allows himself to be misled by words; what I here call "the social form of production" is exactly the same thing that I called "social relations of production" in my 1985 work. The change of vocabulary was not to please CA readers but primarily to facilitate an intellectual exercise I considered salutary—determining whether my ideas would still seem valuable in a different formulation. It was, beyond that, an effort at simplification in view of the controversy that is always aroused by the employment of a concept borrowed from the Marxist tradition.

Ingold's comment proceeds from a more profound and well-known difference of opinion. Discussion of the

1. To these difficulties must be added that the translation of a text originally conceived in French was sent to commentators without an English version of its figure 1, which in my view represents the essence of what I wanted to say.

definition of hunter-gatherers seems to me in large part idle, and I do not see that Ingold's insistence that the economic implicates the social allows us to advance even a step. Who does not know this? And how does it preclude proposing a definition in economic or technical terms? In another connection, Ingold, who reproaches me for not being familiar with contemporary ethnography, seems for his part not to be familiar with the earlier ethnography; otherwise he would probably have known that the question of the unilinearity specific to central and western Australia, associated with clans called "local" and with conception or birth totemism, is a classic one that has long been recognized (see n. 17) if not well studied. And as he views me as moving backward, I will quite willingly allow him to forge valiantly ahead.

Astonishingly, Zvelebil criticizes me for conceiving the Neolithic Revolution as a fundamental rupture when my main conclusions consist in minimizing the importance of that rupture, but in another connection he raises the very interesting question of how a rupture might be identified from archaeological data. I would point out that there are two kinds of rupture. One is in the technical or productive apparatus of a society and is susceptible to identification in archaeological sequences. The other has to do with social forms, and this is the sort of rupture that I consider fundamental. It is never *directly* identifiable, for the emergence of a new social form may very well not be translated into any important technical or productive change where there is blockage by the environment or may be translated into such decisive changes only after a lapse of time that may be considerable. Taking these structural and diachronic shifts into account makes the construction of definitive archaeological models rather complex and their verification a difficult problem, but this would not be the first time that a science proposed hypotheses verifiable only through their indirect consequences and by means of an extended methodological detour.

Several commentators (Smith, Arcand, Linkenbach) find "my" dichotomy of economy and society too simple or too classical, but no one seems to notice that this opposition figures in the questions I ask but not at all in the answers I propose. What I consider determinant, in fact, is a social form identifiable at a number of levels—economic, sexual, and symbolic. This is very far from the simplistic determinism for which I am criticized in that it is a form that is *in itself not economic* that determines a causal relation within the economic domain. I think that my critics have allowed themselves to be led astray by the terms (or form) of the question, but the formulation of a question is always provisional insofar as the development of the concepts that permit an adequate response does not permit criticism of its terms.

The notion of isomorphism is, it seems, not very well understood, in particular by Smith, who takes it for correlation. Correlation is among facts; isomorphism is among forms, structures, articulated ensembles that can be accounted for in terms of a simple principle of intelligibility. In borrowing the term from the fields of math-

ematics and physics, I hoped to account for an identity of structure across domains without recourse to the idea of a causal link. Need I underline once again that the domains of which I am speaking are never global ones such as the economy or social organization in general but parts of them, pieces carved out of the ensemble for purposes of analysis? There is probably a fundamental philosophical difference, for instead of a microeconomic explanation (appealing to "individual actors" and their eventually rational choices) I seek mainly a macroeconomic one in terms of social structures and forms. Nevertheless, I grant Smith a criticism that I myself recognized as well founded in the course of writing the article: that of the three factors of social blockage of techno-economic development in Australia considerations of the game-sharing system in fact allow the direct explanation only of the first. This criticism arises from the fact that I have presented in this article only the simplified core of the otherwise complex argumentation developed in my 1985 work. Having already summarized too much, I refuse to summarize any more and will simply say this: if it can be shown (as I attempted to do in the work just cited) that gathering and other productive activities are subsumed by a social form analogous to that to which I have called attention with regard to hunting, is it not possible to speak of a general blockage of all technical development in hunting, gathering, or other sectors of the economy?

As for the specialists on Australia, Morton and Peterson comment especially on the part of my article that relates to that continent. I do not think that what Morton says about the importance of religion in Australian societies contradicts my theoretical proposals. I agree that the Australian hunter has *some* interest in the product of the hunt (Walter raises the same question), but the point is that this interest is, as Morton recognizes, *indirect*; in the long term it assumes the prior renunciation of his direct interest, that is, the renunciation of his catch. One way or another, every hunter—like every producer—must recover some part of his interest in any society, and it is the particular modalities whereby this interest is finally satisfied that vary from one type of society to another. It is these eminently social modalities that most interest us. I cannot judge the extent of the disagreement between Peterson and me with regard to the major characteristics that can appropriately be attributed to Australian societies. He points out that totemism has been "discovered" in America, but he does not overlook the extraordinary profusion of the phenomenon in Australia in comparison with the situation in the New World (on this subject see Testart 1985b:257–343). Among the peculiarities of Australia he cites the marriage exchange systems, but what are the moieties, sections, and other matrimonial classes if not compounds of unilinearity and dualism? As for his mention of "the complexity of the cultural structuring of the landscape," I again see no contradiction, for what is it that structures the cultural relationship to the land if it is not all that mythology and ritual that is expressed precisely in terms of clan and totemic affiliation? Per-

haps the difference between our characterizations arises from the fact that mine is more analytic, in a sense breaking down the salient aspects of Australian societies into what might be called their basic elements. One final word: I am surprised that Peterson finds the complexity suggestive of recent development "adopting the kind of evolutionary argument used by Testart," for I have employed no such argument and have always held that the idea of an evolution from simple to complex is one of the erroneous views of the evolutionism of yesteryear.

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