SAHLINS AND A KIND OF ANTHROPOLOGY

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This considers *Stone Age Economics* in terms of anthropological research carried out in Melanesia and in terms of the kind of anthropology that the book and that research presented. Researchers in the New Guinea Highlands found societies that did not have patrilineal groups and so did not fit the dominant structuralist model developed in work on lineage societies in sub-Saharan Africa. This led to a search for a different basis of social order, and researchers settled on exchange. They kept, however, the structuralist idea that there are a few principles that shape and explain the social order, principles of the society's exchange system. Sahlins focussed on exchange and was interested in structure, but his was the structure of empirical patterns and regularities shaped by contingent factors, not the result of a few underlying principles. Also, he attended to the ways that people could manipulate the system and how this revealed its in-built limitations. The result is social orders that do not reproduce themselves as the Africanist model implies, but are unstable. These points are developed with reference to Sahins's analysis of the Vitiaz Straits trade and big men, complemented by Andrew Strathern's description of competitive ceremonial exchange in the New Guinea Highlands. The paper ends with a brief description of the fate of Sahlins's kind of anthropology.

There is much in *Stone Age Economics*, a work that influenced economic anthropology and the discipline as a whole. The work can be approached in different ways, and two of them are important here.

One way is straightforward. Together with the article that I take as its companion, "Poor Man, Rich Man, Big-Man, Chief" (Sahlins 1963), the

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book influenced work on the Western Pacific, which is its regional focus. Here I approach the two works in term of anthropological research on Melanesia and especially what is now Papua New Guinea (PNG). Melanesia features strongly in the book and it is where Achsah Carrier and I started doing field work in 1978, a few years after *Stone Age Economics* appeared (e.g., A. Carrier and J. Carrier 1991; J. Carrier and A. Carrier 1989). I will approach the book in terms of its place in changes in Melanesian anthropology in the 1960s and 1970s, which were shaped by the changing geographical scope of research in PNG, described below.

The other way I approach the book is related to the first, but is less ethnographic and more analytical. Just as work in Melanesia was changing, so too was work in anthropology as a whole. The 1960s and 1970s saw an increasing dissatisfaction with one of the important strands of anthropological thought, also described below. That dissatisfaction slowly turned into a broad criticism of what many took to be the dominant strands in anthropological thought, criticism encapsulated in Sherry Ortner's 1984 paper, "Theory in anthropology since the sixties". The second way that I approach the book is in terms of that dissatisfaction and criticism.

1. ANTHROPOLOGY AND MELANESIA

The Torres Strait Expedition visited Melanesia in 1898, but the area became unavoidable in anthropology when Bronislaw Malinowski's *Argonauts of the Western Pacific* appeared in 1922, describing life on Kiriwina in the Trobriand Islands to the east of the main island of New Guinea. Subsequently Melanesia was the location of work by people like Reo Fortune (1932), Margaret Mead (1934) and Gregory Bateson (1936), and has remained important into the present. In the years before *Stone Age Economics* appeared, however, the geographical focus of anthropological interest in the country changed, and the analytical orientation changed as well.

Before the 1930s Westerners thought that the Highlands region of New Guinea was mountainous and pretty much empty, and anthropological work resembled Malinowski's, being focussed on coastal and island regions. Westerners however, learned that they were wrong. In 1930 a gold prospector, Mick Leahy, and his companion, Mick Dwyer, mounted an expedition that walked across the Highlands and discovered that the area had many basins and plateaux worked by something like a million settled agriculturalists. Although it turned out to have been populated, the area remained little explored before the start of the Second World War and the Japanese invasion of 1942. This meant that the Highlands region was closed to anthropologists and to civilian Westerners generally until late in

the 1940s. Researchers went into the Highlands shortly thereafter and significant publications on Highlands societies began to appear in the second half of the 1950s.

Many of the early researchers in the Highlands had been steeped in classic British social-anthropological work on sub-Saharan Africa, with its concern with social structure and its stress on lineages as the basis of social order. They approached the region expecting to find roughly what the Africanist work described. Like those Westerners who had thought that the Highlands were mountainous and empty, those early Highlands researchers learned that they were wrong. I want to explain what they learned. Doing so requires attention to research on and debates about kinship, not as common an anthropological concern as it was. As I hope to make clear, however, behind the research and debates was a change in the kind of anthropology that many thought of as sound and that we ought to pursue.

For some years there had been growing concern among those working in different parts of Oceania that the Africanist conception of, and stress on, kin groups defined by descent did not seem to accord with what they saw in their field sites. The Africanists had described mostly societies that traced descent only through male links (patrilineal or agnatic), less commonly only through females (matrilineal). However, many of the societies that people working on Oceania studied were cognatic, tracing descent without regard to sex, indifferently through both male and female links. In such societies descent by itself could not be the basis of social structure. It could not define distinct social groups in the way that it did in unilineal African societies, nor could it unequivocally locate individuals in those groups. So, something in addition to descent had to be important in creating social groups and locating people in them.

Societies in the Highlands did not seem to present the same challenge, for most of them had lineages that people said were agnatic, with descent traced only through males. However, on inspection those lineages turned out to be more fluid and less clearly genealogical than villagers said and than the Africanist literature described, and people's position in the lineage structure constrained them less. Indeed, in some cases the Africanist model seemed to be stood on its head. It was not that being an agnatic descendent of the lineage founder gave you a place in the social order and told you how to act. Rather, the way you acted defined your place in the social order and turned you into an agnate. Referring to the Bena Bena, the Highlands group that he studied, L.L. Langness (1964: 172) put it this way: "the sheer fact of residence in a Bena Bena group can and does determine kinship".

The Fortesian Africanist model held that descent was a biological reality, that descent groups were the basis of social structure and that descent unambiguously located people in those groups. However, work on cog-

natic societies in Oceania suggested that descent might not be an adequate basis of social order, while work in the Highlands suggested that descent might not really be descent. It is understandable that, in 1962, a paper by J.A. Barnes appeared, summarising and crystallising the growing belief that African models were not really suited to the New Guinea Highlands.

As I said, the overt issue in all of this was the nature of kinship, especially in the Highlands. However, underlying this was something more basic that was part of the Africanist approach. That was the question of the basis of social order, people's place in it and anthropological models of it, a question laid out and pursued at the time by David Schneider (1965). Rigorous Africanists like Meyer Fortes (esp. 1953) pursued a kind of anthropology that took a structuralist view of society, based on the assertion that a society "can be apprehended as a unity made of parts and processes that are linked to one another by a limited number of principles of wide validity" (1953: 39). Illustrating what this means in practice, he (1953: 35-36) said:

The most important religious and magical concepts and institutions of a lineage-based society are tied into the lineage structure serving both as the necessary symbolical representation of the social system and its regulating values… every significant structural differentiation has its specific ritual symbolism, so that one can, as it were, read off from the scheme of ritual differentiation the pattern of structural differentiation and the configuration of norms of conduct that goes with it.

This structural vision extended down to the people who make up such societies, for they are not defined by their lives, actions and personalities, but by their position in the structure: "any person… can be substituted for any other person of the same category without bringing about changes in the social structure" (1953: 36). Further, in the societies that concerned people like Fortes, the structure was defined by unilineal descent. Not only does it define a set of mutually-exclusive kin groups and unequivocally locate every member of the society in one or another of them in the structure, descent is based on the biological facts of human reproduction and so is directly and equally accessible to researchers and the people they studied.

So, for the patrilineal societies that Africanists described, those principles revolve around descent and the areas that they determine are the structure of the society: group formation and relationship, religion, politics and more. Further, the rules of descent reflect the objective facts of biology. Work in Oceania showed that kinship by itself could not be the source of those principles and work in the New Guinea Highlands showed that the facts of biology may have been objective, but did not seem very important in placing individuals in groups that putatively were patrilineal.

With no single, clear set of rules to generate social structure and govern social life, and with no objective facts to which the rules could be applied, researchers were at a stand. Groups of people who called themselves agnates often were not so in the anthropological sense; groups that called themselves kin groups turned out on inspection not to be so. The apparent shortcomings of the Africanist model meant that researchers had to find other ways to account for what they saw in the field.

Two ways turned out to be important. One, my main concern here, was to find another realm of social life that was the foundation of social order. The realm that many researchers turned to was exchange. The other, pursued most visibly by Schneider [1980 (1968)], was to see the facts of biological life as cultural constructions. If groups that people said were agnatic were not actually so, those people were not wrong but instead meant something different by agnatic descent. In this, researchers separated what the Africanist model had joined, culture, people's assertion that groups are agnatic, and society, the actual membership of those groups. As it turned out, this had the implication that we should approach societies in terms of the perceptions and beliefs of those who lived in them, The Native's Point of View, rather than in terms of the intellectual armoury of the discipline and the facts on the ground that researchers confronted (Carrier 2016: chap. 2, 3).

2. Changing structures

Stone Age Economics is no conventional ethnography based on primary field work. Instead, it is secondary, relying on other people's published work; as Sahlins put it (1963: 285 n.), his research method was "reading the monographs and taking notes". That made it relatively easy for him to look outward from the village or society that was the field worker's focus. The most obvious example of this is the first part of the chapter "Exchange value and primitive trade" (1974: 280-295). It is obvious because it is concerned with regional trading systems and so transcends individual societies.

In it, Sahlins is trying to understand the value of specialist items traded in such systems, while at the same time criticising the idea that the conventional notion of exchange value can be applied to those items. In that notion, value is determined by the transactions in which items are traded, and particularly the interplay of supply and demand. He demonstrates the inadequacy of that notion by looking at trade patterns in the Vitiaz Straits, between New Britain and the New Guinea mainland, from the perspective of Siassi Islanders, as described by Thomas Harding (1967). In one unexceptionable trading sequence a Siassi who starts with 1 pig can end up with 5 to 10, because trad-

ers confront different sets of people who are willing to trade particular items at a significant loss, both in Sahlins's terms and in theirs.

To some degree such gain rests on the fact that Siassi Islanders dominated the straits trade and so could, in present-day parlance, extract rent from it. However, Sahlins accounts for such trade differently, in a way that echoed the Africanists and their structuralism. That is, in terms of the structure and logic of the regional system as an integrated whole. People from different villages were willing to trade some items at a loss in order to get items that they could use to gain access to other parts of the system that would, ultimately, benefit them. So, people from Village A were willing to pay too much of their specialist item for the specialist item of Village B, because they needed B's specialist item to trade for the specialist item of Village C, which they greatly desired. If you will, the initial overpayments were effectively a loss leader, in which a shop prices an item below cost in order to get what the shopkeeper ultimately wants, in this case people in the shop who will buy other things.

Sahlins also argues that societies at the edge of such systems often find themselves at a disadvantage, for the only items that they can use in the trading systems are their own or items acquired from other societies in the system. That may be adequate for their immediate needs, but if they want to improve their position they need to develop ties with societies outside the system and so gain access to additional specialist items not already available to societies in the system. As Sahlins (1974: 293) puts it, "the continuity of the trade system as a whole depends on its expansion", an expansion that can not go on forever.

In what I have described of "Exchange value and primitive trade", Sahlins was trying to make sense of exchanges that are anomalous in the conventional economic view. This attention to exchange was not distinctive to him. In the late 1960s and 1970s many Melanesianists who were looking for an alternative to descent as the route to understanding social organisation settled, as I said, on exchange. An early advocate of this view was Roy Wagner. In his ethnography of the Daribi people in the Highlands he (1967: 231) said that there is "a principle of exchange which is capable of forming [social] units on its own terms". A few years later, in his Malinowski lecture Anthony Forge (1972: 539) concluded that "in terms of both ideology and practice rules of descent and the groups formed in their name take second place to the principles of exchange". This view became so widespread that one influential writer could say that "exchange itself is the central dynamic" of Melanesian social organisation (Whitehead 1986: 80) without feeling the need to argue the point.

While Sahlins and those Melanesianists were alike in abandoning the Africanist model and its focus on descent, he differed from most of them

in an important way. That is because those Melanesianists continued to practice, or at least aspire to, the kind of anthropology Fortes advocated. He focussed on descent, but as I have described, his approach was structuralist, looking for the handful of principles that govern society as a whole and maintain it over time. In their influential comparative analysis of Highlands societies, Paula Rubel and Abraham Rosman (1978: 320) concluded that exchange was the location of just those principles: "the structure of ceremonial exchange also organizes behaviour in other cultural domains, which is why it can be singled out as the dominant sphere".

Sahlins also was concerned with structure, but not in the Fortesian way. In his description of the Vitiaz Straits trade, for instance, there is a structure, but it is not in any obvious way the manifestation of those fundamental principles endlessly reproducing the social order over the course of generations. Rather, it is made up of empirical patterns and regularities that appear to arise from contingent events rather than from a small number of principles of social life. As well, Sahlins's structures do not seem very good at reproducing themselves but instead seem unstable, like the trading system that survives only because it expands and only for so long as it can do so. Further, the people involved in that trade have goals and desires, but they can pursue them only in the face of constraints. These are not, however, the constraints inherent in their society that reflect fundamental principles. Rather, they arise from things like whether villagers two islands away have managed to establish trade relations with a third and more distant village, and so incorporate it and its produce into the regional system of trade.

3. PROCESS AND PROBABILITY

As I noted, Sahlins was not a conventional field worker, which set him apart from most anthropologists in PNG through the 1970s and 1980s. Such researchers confront a common problem, how to make sense of a confusing set of people in an alien place. Sahlins, however, could take a different perspective and look at what was going on beyond the village. His discussion of the Vitiaz Straits trade illustrates this, for it approaches societies and their economic activities in terms of their links with other societies. In his work on big men, a summary of which (Sahlins 1963) appeared before *Stone Age Economics*, he indicated how those links could come about. The activities that led to those links had straightforward material consequences, particularly the intensification of agricultural production. However, Sahlins's main concern is the social and political nature and results of those activities.

Aspects of what he had to say influenced work especially in the New Guinea Highlands, notably in Andrew Strathern's (1971) work on *moka*, based on field work among the Melpa people, near Mt Hagen. *Moka* is the name of a form of competitive exchange which was, under different names, fairly common in the Highlands (e.g. Feil 1984), and I want to sketch how it worked.

Generally *moka* grew out of a ceremonial exchange, such as a bride-price payment and its associated return prestation. The expectation was that the return prestation would be a particular proportion of the bride-price payment, and if it was that proportion, then the ceremonial exchange was complete. However, and in line with Sahlins's observations about the importance and consequences of generosity (e.g. 1974: 207-212), it could be that the person making the return would give more than that, which is to say that the return gift contained an increment. Technically *moka* referred to that increment, though as I said it also referred to the form of competitive exchange as a whole.

The person (whom I call A) who received the return prestation that included the increment was placed in a subordinate position to the person (called B) who gave it, and would remain so until it was reciprocated. If, eventually, A simply reciprocated the value of the increment, then he (those who carried out these prestations were men) would no longer be subordinate to B and their sequence of exchange would end. However, A could reciprocate with a gift larger than the increment. In that case their positions would reverse: B would be subordinate to A until B made a return prestation. If this sequence of prestation and counter-prestation with more than the increment that the giver had received were to continue, A and B would become what I call partners, making *moka* with or, perhaps better said, against each other.

In *moka* men sought to mount larger and larger prestations to more and more partners. The goal was prestige, becoming a big man rather than a rubbish man, renown and respected over a large area in a region where, before colonial control, there were no effective mechanisms for maintaining peace and good order. The region was "primitive" in Sahlins's (1974: 188) definition of the word, stateless: "cultures lacking a political state" and that "have not been modified by the historical penetration of states". As a corollary of the absence of such a mechanism, villages in the Highlands often were dangerous places to be if one did not belong or have friends who would offer protection. Inter-group fighting was common in the region, and even in the 1980s it recurred, under the name of "tribal fighting".

Moka modified this. Because individual Highland societies were small, seldom more than a thousand or so members, with villages even smaller, men had to seek partners in other villages and societies. *Moka* may have

been a competitive system, but partners were known and linked to each other, and provided legitimacy to each other. Consequently, partners and their important supporters could visit each other assured of relative safety in a region where this was not very common.

Moka allowed a degree of peace and security in another way as well. An aspiring big man who got a prestation from another received a significant number of valuable items. If he were able and industrious, he would use them to mount a prestation to a different man with whom he was, or hoped to be, in a *moka* relationship. In other words, when our hypothetical player A made a prestation to B, B would try to use at least some of it in a prestation to C who, if possible, would give it to D and so on down the chain. In this way, *moka* relationships often formed chains, which Strathern pointed to in the title of his book, *The Rope of Moka*. While it was not necessary that A and C knew each other, as aspiring big men they would know of each other. Also, it was to their advantage to meet, to co-ordinate their dealings with their intermediary B. The same, of course, applied to B and D, C and E and so on.

In the chapter "The sociology of primitive exchange", Sahlins attends primarily to the flow of items in the relationship between leaders and followers, and to the difference between big men and chiefs, which is to say the way that a social group is shaped by the give and take of things. Strathern's description of *moka* shows something different, the broader social and political implication of Sahlins's (1974: 209) observation that a leader like a big man "acts as a shunting station for goods flowing reciprocally between his own and other like groups". As such, *moka* produced a degree of sociality and security across boundaries that otherwise were dangerous to cross.

I said that researchers going into the Highlands after the War often had the general structuralist orientation found in Africanist work. One aspect of this was the tendency to approach people in terms of their statuses and roles, their location in the social structure and the ways that this constrained or shaped their behaviour [e.g. Radcliffe-Brown 1952 (1924)]. In the broadest sense, any effort to understand social groups requires approaching people in this way to some degree. Failing to do so reduces what researchers see to a set of autonomous individuals who can be grasped only in a statistical sense, the sum of their personal thoughts and actions, in the way that the economist's market price is seen as the consequence of the personal preferences and resources of a mass of individual market actors.

Sahlins's conception of the big man, however, indicates that a different view of people might also be useful. In that view, the individual is not simply the occupant of a social location who conforms to the expectations associated with it, status and role. Rather, to some degree the individual in Sahlins's conception consciously manoeuvres in order to achieve a desired

end; in present-day parlance, exercises agency. The big man's freedom to manoeuvre was not absolute. He was constrained by circumstances and his own limitations, and in fact most of them failed to achieve their ultimate goal, even if they avoided being reduced to rubbish men. Even so, the manoeuvring was real.

This ability to manoeuvre introduces a degree of indeterminacy that often is muted in work inspired by the Africanist model. In turn, that implies that Sahlins and those inspired by him, like Andrew Strathern, were using a conception of causation that is probabilistic rather than deterministic. A deterministic model holds, in effect, that A causes B, whether A is Wagner's principle of exchange or a person's position in the structure of kinship relationships. A probabilistic model, on the other hand, holds only that A is likely to cause B, and recognises that contingent factors can affect the outcome. Big men lived in a contingent world and operated in a system that, like the Vitiaz Straits trade, has an in-built tendency to instability. This will be come apparent in the paragraphs that follow.

I have described how a *moka* relationship could emerge out of a ceremonial exchange such as a brideprice payment and its return prestation. Valuable items like decorated shells and ceremonial axes could pass in these prestations, but the dominant item was pigs. Some were butchered to be cooked and eaten as part of the ceremony and festivity of the prestation, but a larger number were tied to stakes on the ceremonial ground and given as live animals. There were only two legitimate ways to acquire those pigs: receive them in a prestation and rear them.

Rearing pigs for *moka* required land and labour and brought about the intensification of agricultural production mentioned previously. Some land was required for pens and pasture, other land was required for gardens to grow food for pigs, primarily yams. Similarly, some labour went to tend the pigs and some to making gardens and growing yams. A man and his wife or wives, and perhaps children, could grow a few pigs, but an aspiring big man needed more than that and had to recruit additional labour. Usually this began with the man's sisters, extending then to near affines, perhaps his brothers' wives and sisters' husbands. This might be enough to produce the pigs for a minor prestation, but no more than that.

For a *moka* prestation worthy of the name, the aspiring big man needed a larger circle of followers and hence a more complex way of recruiting them, one that extended beyond his relationships with near relatives and the obligations that went with them. One way of recruiting was to give a person a few piglets. That person would take on the work of rearing them, on the understanding that they would be returned to the aspiring big man in due course. The person doing the rearing gained two things by this. One was the right to keep a few of the piglets. The other was to become asso-

ciated with the future *moka* prestation of which the pigs would be a part, and consequently have a claim both on a share of the prestige accruing to the big man and on a share of the counter-prestation by which that future prestation would be reciprocated.

As this suggests, recruiting supporters would be easier for a big man who had already achieved some renown, for the prestige of being associated with his *moka* would be greater. As well, an established big man would be making *moka* with several partners, would be receiving prestations from them more frequently than if he had only one partner and so would be in a position to encourage supporters with small parts of return prestations more quickly than would otherwise be the case. Because success in *moka* made it easier to attract followers, it tended to feed on itself.

There was an in-built contradiction, however, because success made the big-man's life more difficult. The more successful he was the more his existing and future supporters expected of him, which meant that he had to work harder. For one thing, he had to oversee and assist in the rearing of pigs and for another he had to spend time visiting supporters, encouraging them to work on his behalf and perhaps giving them small gifts by way of inducement. Moreover, because *moka* was competitive, there was no natural stopping point. Big men sought to be even bigger, with more supporters rearing more pigs to be given to more partners, ideally in numbers so great that the recipient would not be able to reciprocate.

A big man, in other words, had to manoeuvre. The general rules of the system in which he operated were clear enough. To be a truly big man, however, he had to bend the rules or finesse them. So, the desire to gain additional supporters could lead the big man to divert some of the return prestations that he received, in order to attract potential supporters, rather than using them to reward existing supporters. The desire to make *moka* with a new partner could have the same effect. The greater the aspiration and the bigger the stakes the greater the tendency to finesse obligations to supporters.

A truly astute big man could carry this off better than the less able. However, the competitive nature of the system and the desire to be an ever more renown big man made it harder and harder to do so. At some point, existing supporters would become disaffected, new supporters would become scarce and the bubble would burst. As Sahlins (1963: 293) says, a big man with a sizeable following "comes under increasing pressure to extract goods from his followers, to delay reciprocities owing them, and to deflect incoming goods back into external circulation. Success in competition with other bigmen particularly undermines internal-factional reciprocities", leading to an "inevitable tide of discontent".

Understanding Highland big men, then, requires looking beyond status and role. Those things existed and were important. Being the sort of leader

that Sahlins describes was a status, and it had roles associated with it, including being generous. However, attending simply to these is not enough. Unlike the Polynesian chiefs that Sahlins compares to them, Melanesian big men had no durable institutional structure that secured their status. Instead, they were, so to speak, only as good as their last *moka* prestation, and because of the competitive nature of the system their last prestation was never really good enough.

4. Situating Sahlins

As presented in Sahlins's synthesis and Strathern's description, the Melanesian big man stands uncertainly between autonomy and constraint. Big men are not autonomous actors free to do as they choose. They make their transactions in the context of common understandings about how important people are supposed to behave, about how *moka* and like prestations are to be made and reciprocated and about how supporters can be recruited and encouraged. They make them also in the context of other existing and would-be big men and their relationships with them. Equally, they are not social persons defined by their status in a structure and governed by the roles associated with it. Rather, while they operate in terms of expectations and constraints, they manoeuvre in terms of them, manipulate and finesse them, to pursue the individual goal of becoming the biggest man.

Sahlins was not the only social scientist to approach collective life in terms that departed from the idea of the socialised person, occupant of a status and conforming to its role. Around the time that Sahlins was writing the material that ended up being *Stone Age Economics*, Dennis Wrong (1962) produced "The over-socialized conception of man in modern sociology". Wrong said that this conception was a feature of structural functionalism, important in his discipline as it was laid out by Talcott Parsons (e.g. 1937). He echoed the approach that I have portrayed in Sahlins and Strathern, that people are motivated to act for reasons that lay beyond the realm of status and role. He differed from them, however, in rooting those motivations in a fairly Freudian psychology.

Between them, Sahlins and Strathern showed how the actions of big men and the social ordering that results are a consequence of the interplay of their motives and the social order in which they operate and which they manipulate in their effort to become even bigger men. In doing so, they provided an answer to the question that Sherry Ortner (1984: 148) posed a decade later: how are we to understand "the relationship(s) that obtain between human action, on the one hand, and some global entity which we may call 'the system,' on the other".

Ortner's paper was perhaps the most influential of a set of criticisms of structuralist intellectual orientations. Critics argued that such orientations saw societies as orderly and rule-bound, allowing no place for people and their everyday lives, desires and uncertainties. In a way, the critics were arguing at the societal level what Wrong was arguing at the individual level: reducing life in societies to the rules of the system, like reducing individuals' lives to status and role, left out too much. Equally, however, when Ortner (1984: 126) reduced anthropological thought to "a few large categories of theoretical affiliation, a set of identifiable camps or schools", she left out things like *Stone Age Economics*.

Whether or not her simplifying omissions made her argument easier to think, what she had to say was timely. Many in anthropology were wrestling with problems cast as "structure vs process" and "structure vs agency" (e.g. Bourdieu 1977), as did those in adjacent disciplines (e.g. Giddens 1984). However, the question that Ortner posed and the kind of anthropology that Sahlins practised ended up being abandoned.

Stone Age Economics appeared one year after Clifford Geertz's *The Interpretation of Cultures*, and much of anthropology ended up embracing the Cultural Turn. As Geertz (1973: 453) explained his kind of anthropology, our job is only to grasp meanings, cultural texts, and we are to approach what people do in terms of its symbolic content, the cultural meaning it expresses, not its practical consequences or the factors that might shape it. Such a view is likely to preclude attention to what concerned Sahlins, patterns and regularities, and their instabilities, perhaps visible only to the external observer. Adam Kuper (1999: 118) put it this way: "if the texts pass over politics and economics in silence, then those matters can safely be ignored". This in turn yielded to postmodernism and poststructuralism, what Sahlins (1999) was to call "afterology", which seemed to go further than Geertz in ruling out the kind of anthropology found in *Stone Age Economics*, not least because it challenged both the researcher's understanding of what is going on in the field and what I called the intellectual armoury of the discipline.

Work in Melanesia followed a somewhat different path. The country became independent of Australia in 1975, and there was a flurry of interest in the effects of colonisation on social and economic life in societies there, marked especially by the appearance of C.A. Gregory's *Gifts and Commodities* in 1982. However, shortly afterwards Melanesianists mostly embraced the combination of postmodernism and intense focus on culture in Marilyn Strathern's *The Gender of the Gift*, which appeared in 1988.

In the years through the 1980s Sahlins himself followed some of the intellectual changes that I have sketched. Four years after *Stone Age Economics*, his *Culture and Practical Reason* (1976) appeared. While the earlier book

is concerned with people's transactions, their patterns and consequences, the later one is concerned more with structures of meaning, drawing on the work of Jean Baudrilliard [e.g. 1981 (1968)], himself heavily influenced by Claude Lévi-Strauss's work on totemism (1969). This was followed by work on Hawaii, especially the arrival of Captain Cook and his death there in February of 1779.

Here again, the intellectual focus is culture, both how it is maintained and how it is changed. Sahlins (1985: 153) said that "all events are culturally systematic", in that their significance "is dependent on the structure [of culture] for its existence". However, events also shape culture, because "the world is under no obligation to conform to the logic by which some people conceive it". Because it need not, those events can act back on culture: "cultural meanings are thus altered. It follows that the relationships between categories change: the structure is transformed".

While this argument attends to change in the way that, for example, Geertz frequently does not, it lacks serious attention to the possibility of inherent instability and individuals manoeuvring within a system. As well, Sahlins ignores the sort of structure that had concerned most of the earlier anthropologists that I have mentioned, social structure. Like many in the discipline, then, he practised a kind of anthropology that abandoned a matter that those Africanists had sought to address, how society is organised, as well as a matter the he himself had sought to address, how people manoeuvre in their society and what the consequences are.

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