

COMMENT

MUNDA KINSHIP AND TRANSATLANTIC ANTHROPOLOGY: A RESPONSE TO SHAPIRO

IN his review of my book *The Munda of Central India* (Parkin 1992), Warren Shapiro adopts a wider agenda than is usually expected of a book reviewer (see *JASO*, Vol. XXIV, no. 2, pp. 218–20). He takes the opportunity to talk up everything American in anthropology and sneer at everything European, leading one to wonder whether the days of a recognizably common transatlantic discipline are finally over. This has led me to break with normal practice and respond directly, as the author, to a book review.

Shapiro's basic tactic is to use the depth of history as a means of expressing distance and disapproval, an interesting scholarly application of the deprecation of the 'other' with reference to time exposed by Johannes Fabian. However, this tactic is applied only to the Europeans, namely Radcliffe-Brown, Fortes, Lévi-Strauss, Dumont and Needham. By contrast, Shapiro lionizes his American colleagues Lowie, Murdock, Goodenough, Lounsbury, Scheffler, Keesing and Schneider, even though those of them who are still alive are hardly much less long in the tooth. In reality, the intellectual histories of the two groups run largely in parallel. For example, with the publication of the original French edition of his *Elementary Structures* in 1949, Lévi-Strauss beat his contemporaries Lounsbury and Goodenough into print by only seven years (both the latter published key texts in 1956), while Dumont, with his influential article on Dravidian kinship terminology (1953), did so by a mere three. Also, given so much mutual influence, I find the impression Shapiro leaves, whether intended or otherwise, that there is a bloc of American theory consistently opposing a bloc of European theory very misleading. The fundamental question surely is not which continental traditions have contributed most to anthropology, but what constitutes progress in the subject and who has produced it. A short comment such as this cannot hope to give authoritative answers, but some remarks may help restore the balance.

As far as I am concerned, Needham's remarks on formalism (1971: xxi–xxxiv) are just as cogent now as they were a quarter of a century ago. That formalists were and are mostly American is no more than a fact of history and does not mean that domestic critics have been lacking. Needham himself points to Coult as 'an exceedingly acute *American* critic' of them (*ibid.*: xxx; emphasis added), and he was not alone (cf. Trautmann 1981: 59ff. on Scheffler). As ways of describing a classification, neither the componential analyses of Goodenough nor the formal semantic analyses of Scheffler and Lounsbury can be considered wrong, but they do not add anything that cannot be provided by a conventional analysis using genealogical denotation. Claims that such analyses enable behaviour to be

predicted are at best confused and at worst fatuous. While behavioural stereotypes may correspond in part to a classification, trying to read off actual behaviour from a terminology is futile. In short, those who have been trying to unite the spheres of 'structure' and 'grosser human behaviour' (Shapiro, p. 219) have an awful lot more sweating to do.¹

Given also Scheffler's blank refusal to consider the affinal terminology as anything more than an epiphenomenon of terms for consanguines, the limitations of the approach are clear.² Really, it was such figures as Leach, Dumont and Needham who showed more clearly than Lévi-Strauss how certain terminologies express forms of affinal alliance, if not actually determine them. Yet they were all really working in the tradition of Morgan (to whom indeed Lévi-Strauss dedicated his *magnum opus*), much modified it is true but still recognizably Morganist. Morgan's status as a pioneer helps excuse his mistakes, and he remains an inspiration for many, not only in Europe, as the work of Trautmann (1981) shows. I am happy to associate my modest efforts with this tradition, at least in part, though I did not feel it necessary to advertise the fact specifically in a book in which the urge to theorize was kept mostly within bounds.

Now Morgan and Trautmann are both Americans. I am thus far from being anti-American, as Shapiro evidently suspects. It is simply that I think my Americans are better than his. Nor does it matter a whit to me that Morgan belongs to what might be called the proto-history of the discipline. In itself, being younger than Morgan—or for that matter, younger than Lévi-Strauss *et al.*—is no guarantee of academic excellence: what matters is whether the work is still of relevance. As it happens, Morgan's basic approach has become the mainstream form of analysis in kinship studies. This, it seems to me, is what is important, not when or where a person pursued his career. Similar remarks, *mutatis mutandis*, apply to Mauss, who often seems to have thought of everything in his long and productive career. One of the virtues of his essay on the person is precisely its demonstration of 'the pervasive equivalence of alternate generations' in certain ideologies. Nothing I know of in subsequent disquisitions on the person—not even in America—has managed to undermine this demonstration. This is because, like 'two-section' systems,³ such equivalences are not difficult to locate in contemporary ethnography. This in itself is an argument neither for nor against the

1. Shapiro has misread my text at this point: 'positive marriage rules' are not contrasted with 'considerations of wealth and status', the two are listed together as factors that may restrict choice of marriage partner. They are not, of course, mutually exclusive.

2. I develop these points further in an as yet unpublished manuscript entitled 'Genealogy and Category: An Operational View'.

3. This phrase must be Shapiro's. It is certainly not mine. This makes Shapiro's linking it with the phrase 'fundamental properties of the human mind', which actually comes in the middle of a lengthy discussion on reincarnation, doubly spurious.

evolutionism Shapiro derides. History may or may not have more examples of these equivalences: it certainly does not monopolize them.

To return to kinship in a narrow sense, Schneider's position is somewhat different from that of the formalists. His approach can be related to the argument that anthropologists should concentrate on 'culture'—broadly speaking, symbolism—and leave social structure to sociologists. His stress on indigenous representations is certainly to be appreciated, though it is hardly remarkable in itself. More questionable is the attempt to ring-fence anthropology and thus limit its comparative scope. Anthropologists have always accepted the influence of other disciplines, including linguistics, philosophy, psychology, history and sociology, radically adapting the latter in particular. Since my own interest was largely social-structural, I did not feel Schneider provided any more of a model than the formalists had. Rather, the ethnography practically imposed the terms in which I wrote the book. This is where Shapiro's attempts to read between the lines frequently lead him astray. For example, of the three passages in which I cite Lévi-Strauss, two are critical (1992: 163, 184–6).⁴ A key finding, ignored by Shapiro, is the fact that affinal alliance among the Munda characteristically does not fall exactly into any of the models identified by Lévi-Strauss. In its modest way, this represents a fresh development within the Morganist tradition.

Finally, a point about my association of 'bifurcate merging' with 'Dravidian' or 'symmetric prescriptive' terminologies (Shapiro, p. 219; Parkin 1992: 122). The first phrase was coined by Lowie to describe the sort of pattern that links parallel and lineal kin under the same term(s) but has a separate term for cross kin (e.g. F = FB ≠ MB; Lowie 1928). As such, it is often encountered in symmetric prescriptive terminologies. Certainly, as Shapiro indicates, this does not exhaust its distribution: for instance, many so-called Crow–Omaha terminologies can be said to have this pattern, without this saying very much about them. This makes it unreliable for diagnostic purposes and is the reason I never use it, save to criticize or to attribute it to others (I will not labour the point that it is of American origin). Whether, as Shapiro further hints, the separation of affinal terms renders a terminology less 'symmetric prescriptive' is another matter. According to the orthodoxy of 'Anglo-French structuralists', it probably would. But what is truly surprising is that he has completely missed my discussion, in parts of chapters 7 and 8, of this separation and its significance in the Munda terminologies. In using arrogance to cover his inattention to what I wrote, Shapiro has succeeded only in producing an object lesson in how not to review a book.

ROBERT PARKIN

4. If Shapiro really wants to know how little I think of Lévi-Strauss's efforts to come to terms with the Munda ethnography, he should consult Parkin 1983.

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NEW THOUGHTS ON THE PIG MEAT TABOO

As a biologist who has already trespassed into the field of anthropology in these pages (albeit in collaboration with the late Bryan Cranstone) with a discussion of the biological reasons why pigs have never been milked (Cranstone and Ryder 1987), I am pleased to be allowed to do so again. I wish now to draw the attention of anthropologists to a possible biological basis for the rejection of pig meat in the Middle East, having already put this forward in a biological context (Ryder 1993).

Although the earliest record of this prohibition is apparently that in the Old Testament, the rejection of pig meat is not restricted to Judaism. It is in fact part of a wider distaste for pork in the Middle East. According to Darlington (1969: 123), pork was forbidden to the aristocracy in the Egyptian New Kingdom, a fact he attributed to influence from the Hyksos, who invaded from the east in the eighteenth century BC. The pig is also regarded as ritually unclean in Islam, which began more recently. What is it about pigs in that area that makes whole groups of people consider them unclean on what is now regarded as religious grounds?

When I first became interested in this question thirty years ago and suggested to a veterinarian colleague that the reason for the prohibition might be the proneness of pigs to become infested with the roundworm *Trichinella spiralis*, he repeated the nineteenth-century view that 'primitive' people are unlikely to have

understood the link between human disease and the eating of infested pork. The parasitic nematode in question reproduces in the intestine of the pig and the larvae produced bore through its walls and enter the muscles. Here they create cysts in which they lie coiled and dormant, causing a condition known as trichinosis. People eating pork diseased in this way themselves become infested with the intestinal worms, which in turn invade the human muscles, often fatally. The connection between the porcine and human diseases was not thought to have been recognized (and then in Europe) until 1860 (Douglas 1966: 30). We now know that the larvae are killed by adequate cooking, that is sufficient time at 137 degrees Fahrenheit (58.3 degrees centigrade) to ensure that those in deeper parts of the meat are killed.

While writing my book *Sheep and Man* (Ryder 1983) during the 1970s I came across an explanation offered by the American geographer C. S. Coon that seemed very convincing at the time. He suggested that the religious prohibition of pork among Middle Eastern nomads is more likely to have had an ecological explanation (Coon 1952: 346). Pigs are unattractive to nomads because they are not easily driven and do not adapt well to the hot dry conditions of the Middle East. Unlike other livestock, pigs supply little more than meat and are difficult to skin. This explanation was accepted by Zeuner (1963: 261), who quoted Antonius as pointing out that the pig was valuable to the settled farmer only; nomads, who have always felt superior to farmers, came to despise the pig as well as the farmer who bred it. In due course the nomads developed religious prohibitions against the animal they could not keep. Marvin Harris (1986) considered that since pigs can compete with man for food, they may threaten the whole subsistence economy. Other domestic animals also provided milk, wool or transport, and sheep and goats could survive on the little food available in the austere environment of the area (Ryder 1983: 195).

During the writing of the paper on pig-milking, I became aware that anthropologists saw this religious prohibition of pig meat as a symbolic taboo. Indeed, I understand that for anthropologists a taboo is by definition symbolic. The basis for the taboo against pigs is thus thought to be not their over-rich fat, nor their habit of wallowing in mud to keep cool; neither is it their habit of scavenging for food. Mary Douglas (1978: 54–7) has pointed out that in Leviticus there is no reference to the scavenging habit of pigs. She sees the answer in ritualistic and symbolic terms, pointing out that the Israelites considered as ritually unclean animals that did not fall clearly within certain categories. In the first few verses of chapter 11 of Leviticus it is stated that only beasts which 'parteth the hoof and chew the cud may be eaten'. The pig is anomalous because, although cloven-hoofed, it is not a ruminant. Douglas (1972: 78–9) has further suggested that the pig symbolized prohibited exogamy, since it was eaten by neighbouring peoples with whom intermarriage was forbidden.

But did 'primitive' peoples really not recognize the link between diseased pork and human illness? They must for instance have learnt by trial and error

which plants are poisonous.¹ More recently I have come across other evidence that suggests that a direct connection between diseased pork and human illness may well have been understood in prehistory. The *Trichinella* cysts are produced by the host animal as a reaction to the parasite and are coated with calcareous matter. I discovered that older livestock books describe how these grit-like bodies can be felt when a piece of affected meat is cut with a knife, and that the cysts are actually visible to the naked eye. Tacitus embraced this explanation when he wrote, c. AD 100, that the Israelites did not eat pork because it carried 'a kind of leprosy' (quoted in Zeuner 1963: 261). Zeuner dismissed this explanation in favour of the ecological explanation (as discussed above) because Tacitus had got the wrong disease, whereas it is not uncommon for a grain of truth to be hidden among the garbled writings of classical authors.

Finally, these 'pork worms' were in fact described by the scholar San Isidoro who lived in Seville c. AD 560 to 636. He called them *usia*, and stated that their danger to man was the reason why the Jews were forbidden this meat. The late George Ordish (1976: 41) thought that San Isidoro's knowledge of the worms, plus his discussions with rabbis, might have led him to the conclusion that there was a practical foundation for the Mosaic prohibition on the consumption of pork.

How do anthropologists view this evidence for a completely different and more direct basis for the Middle Eastern prohibition of pig meat, with hints that this basis was known in antiquity?

M. L. RYDER

1. According to research reported recently in the *New Scientist* (Hutson 1994), sheep are able to learn which plants in a pasture are poisonous. The human ability to learn which foods are harmful may, therefore, have a longer evolutionary history than suggested here.

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CHICKEN GOLOK

On 7 September 1982 I attended the erection of a new clan temple for the clan Golok in the hamlet Lamanuk, just above Lamelera, Lembata, Indonesia. At an associated feast, two *ata mola*, or ceremonial specialists, Laga Doni Ten Or and Yosef Gogok, cooked a chicken according to the following recipe:

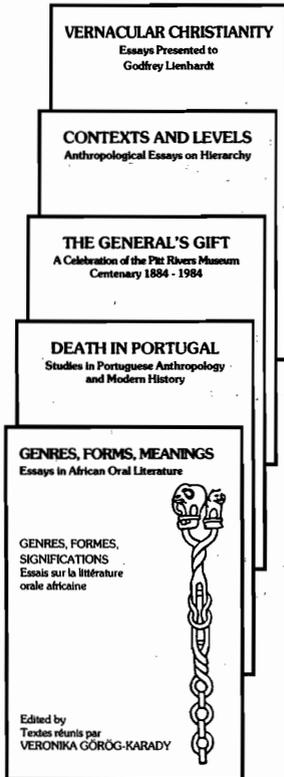
Throw the chicken into the fire, scorching off the feathers in places and melting the rest to the skin. Cut open the chicken along the back bone. Take out the intestines and then rub *ajinomoto* (monosodium glutamate) into the inside of the chicken. Next, take a stone and carefully pound the wings, legs and the rest of the carcass until all the bones are finely splintered. Throw the animal back into the fire and roast [until, as I recall, less than half cooked]. Cut into small pieces, leaving on the blackened skin, soot and feathers. Serve with salt and red peppers.

I wonder if *JASO* might be interested in publishing this recipe, perhaps as the first in a series called 'Culinary Discoveries from the Field' or some such.

R. H. BARNES

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