

Can there be an anthropology of children? A reply

I am spurred to action by the article in J.A.S.O., vol. IV, No. 2, 1973 by Charlotte Hardman. She poses a question in her title, and repeats it at the end in the form: "Should there not then be an anthropology of children?" My short answer to both questions is "yes". The long answer occupies the rest of this article, which is not intended as a rebuttal of Hardman's thesis nor as the basis for a bitter confrontation, as my agreement with her questions clearly indicates, but rather as a modification and to some extent a critique of her position.

Let me begin with a brief description of the way I see the problems she has raised. It will help if I start with a single-sentence autobiography. I studied social and physical anthropology (at U.C.L.) from 1956-59, rhesus monkeys and chimpanzees from 1959-68, and human school-children from 1968-now; I intend to go on studying human schoolchildren for at least the next three years. As a result of this hybrid background there can, for me, never be a social anthropology of young children (in the sense of a comprehensive explanation of why they do, say and think what they do) along the same lines as there can be and is a social anthropology of adults. Nor is the reason that children are 'immature' adults (in which case one might expect a sort of social anthropology of immaturity); I fully accept Hardman's view (derived mainly from her observations at St. Barnabas School playground, Oxford and the work of the Opies) that there is a very real sense in which one can talk of a 'children's subculture' or something like that in which there are ideas, rules, values and so on that belong strictly to the children and aren't part of or prior to (except in the obvious temporal sense) the adult world of meaning, and so on.

The problem as I see it is another one, and is essentially a developmental one. In a nutshell it is as follows:- the infant is born in an extremely animal-like state, with a number of behavioural capacities known in the medical literature as 'reflexes' (for a listing and description of these see Illingworth 1972). It develops socially during the first year in close association with the mother (or mother substitute) and its interactions with her are non-verbal and based on a number of non-verbal processes involving oral, tactile, visual, auditory and olfactory processes. These processes have been studied by ethologists and others (e.g. Wolff 1963). As time goes by the infant both continues to interact with the mother, father and peers on a non-verbal basis, and also to develop cultural, meaning-laden actions, a consciousness of itself and of others, pre-occupations with appropriateness and with the definition of social situations, most of all this being linguistically mediated. What interests me is how this set of transitions from animal-like beginnings to uniquely human endings comes about in the early years. Thus the problem presented by Charlotte Hardman's paper is that she has come into middle childhood (the age of her children isn't specified but I understand it to have been 6-11 years) armed with an array of concepts from the history of social anthropology and from the psychology of Piaget but has largely overlooked the ethological literature on her subject, for instance two recent books, Blurton+Jones (ed) (1972) and McGrew (1972), plus a goodly number of papers, reference to which can be found in those books. Did she feel those works were irrelevant? Can they possibly be irrelevant, dealing as they do with the social interactions of young children?

Let me just give a few hints of the quality of the ethological writings, so that readers can judge for themselves the intellectual and linguistic distance between them and the kind of descriptions found in Hardman's article or the Opies' works. I'll start with a brief summary of some of Blurton-Jones' findings in his earlier (1967) study.

The social environment he studied was a rather loose, unstructured one, in a nursery school, not unlike that studied by Hardman, but his age group (3-5 year olds) was lower than hers (6-11 year olds). He found there were some friendships between children, some rather submissive children, but no clear 'hierarchy'. Children's behaviour to adults depended on who the adults were. In the case of the teacher, some children stayed near her, showed her paintings they had done, etc., and clung to her if they got hurt. Others (called 'little mothers' by McGrew 1972) led a child who was in need of help to the teacher, often with one hand behind the led child's back. Strangers were stared at, and shown things. Responses to parents took two forms: either the child smiled, ran to the parent and touched her (it was usually the mother), or the child walked to the parent and gave her an object such as a painting. A third, less frequent variety was to ignore the parent altogether.

In their relations with each other, Blurton-Jones described two types, agonistic, and rough-and-tumble play. Agonistic (i.e. fight/flight) behaviour occurred mostly over objects. A 'beating movement', bringing down the hand or fist on to a child, was common. Biting occurred, especially in the case of girls. A 'fierce' expression, with lower teeth bared and mouth corners down was interpreted as inhibited attack. A defeated child would scream, call for help, then weep with puckered brows and a reddened face, staying immobile in one spot. There was no wrestling and punching in real quarrels.

'Rough-and-tumble play', by contrast, did include wrestling and punching, and gave the appearance of violence and assault, especially to adults. Facially it was quite different, since it went with an 'open-mouthed smile with teeth covered', an expression similar to the expression described by Van Hooff (1967) and Loizos (1967) as the play face of chimpanzees and macaques. Alternation of roles, another feature of primate play, was common in rough-and-tumble play.

McGrew (1972) observed children aged 3-5 in two nursery schools, one in Oxford and one in Edinburgh. In his book he compares his findings with those hitherto published on behaviour in this age group, and with relevant comparative data on nonhuman primates.

He found that, overall, most child-child interactions were dyadic: 81% in the case of agonistic behaviour, 91% in the case of non-agonistic (friendly or neutral). Thus it seems at this age children interact nearly always with one other, and perhaps are not able to cope very often with the greater complexity of triadic or multiple interactions. The mean time of interactions was 12.9 seconds. Thus young pre-school children seem incapable for the most part of engaging in prolonged interaction, an ability that in most children would seem to develop at the primary school stage. Boys formed all-male groups with a frequency greater than could be attributed to chance, while this was not true of girls. 31% of all interactions involved the transmission or manipulation of an inanimate object.

McGrew, unlike Blurton-Jones, was able to rank his children into a dominance rank order, on the basis of predictable wins/losses in fights, especially over objects. The dominant boys were significantly older, heavier and more nursery-experienced than the subordinates, but were not taller or more intelligent.

Among the more valuable contributions of McGrew's study was his close analysis of the first 7 days' experience of one of his nursery schools by 8 children. They were observed from the moment they entered school, usually with their mothers; all of the newcomers were aged 3, and there were in addition 5 nursery-experienced children, aged 4, in the group. McGrew observed the whole group at once for this study.

At the very outset behaviour was characterised by crying, slow locomotion and an orientation towards the Teacher. Indications of 'social stress' or 'anxiety' such as digit sucking and automanipulation were greatest at the outset but declined during the 7-day period. In contrast, there was an increase in object struggles, and in aggressive acts such as 'push', the latter being more common in boys than girls.

Newcomers' behaviour after arrival was characteristically to suck objects or their fingers, to look away from other children and avoid eye contact, to move around with a sidling, shuffling, hesitant gait. They observed the activities of others intently, but declined offers to engage in social interaction or kept it brief. They avoided all boisterous activity and any kind of competition. In most cases the voice was quiet or silent but 3 children (all girls) were garrulous. In the case of the silent majority, verbalisation increased subsequently, whereas in the case of the three noisy girls it declined.

Some resident girls displayed maternal attentiveness - a soothing tone of voice when talking to a newcomer, tactile comforting e.g. holding hands, or putting a hand on the back or an arm round the shoulders, or patting or kissing. These were the "little mothers", one of whom was aged 3, who made efforts to cheer up sad newcomers. Boys, by contrast, seemed for the most part indifferent to newcomers' tears or questions like "when's mummy coming back?" It has been found in studies of rhesus monkeys that juvenile females are more responsive to infants than juvenile males.

We can note as primary features of the ethological descriptions (a) their clear focus on observable, quantifiable and well-defined non-verbal actions, and (b) their zoological orientation, with an especial tendency to refer to non-human primates for comparative purposes.

Despite the fact that there is an age difference between the 3-5 year olds described by Blurton-Jones or McGrew and the 6-11 year olds described by Hardman, the fact is that 6-11 year olds can be described in an ethological way. But even were this not the case, the fact remains that 3-5 year olds do grow into 6-11 year olds and at that age a Hardman-type analysis shows us a completely different world, so different that we seem to be confronted by a different order of being. Yet I'm willing to bet that if Hardman had studied either Blurton-Jones' or McGrew's nursery school children she would have found them expressing ideas, thinking and talking, in a younger but essentially comparable way to that of her St. Barnabas children.

In other words we have at least in part to do with a severe contrast of methods of study.

And second we have to do with a real development, the development of the human organism.

Can we hope, even try, to synthesise the methods, in order the better to understand the development?

There are precedents, of a sort. Certainly Piaget has tried to build up an image of child development that starts from organic principles and builds outwards in a sort of dialectical spiral that moves between an organic development on the one hand and contact with a structuralist idea-world on the other over time. Piaget however says nothing about non-verbal interaction, even less than Charlotte Hardman who does at least tell us that "I was soon made aware that the bio-physical environment constituted the main equipment also for communication, as I later found out (p.95)". What did she later find out, exactly? It's not too clear, but it seems to be that certain physical objects in the playground, plus certain parts of the children's bodies "especially their arms, fingers and feet all show immense potential for possible play. Each object will acquire meaning or value through its relative position with other objects or the specific context"... "The contexts which define the meanings of the environment are the imaginary situations agreed upon by the group." (pp.95-96)

Excellent! It seems that we have to deal with just that stage in development where hits of the body and physical environment are used for social communication, not in the 'animal' way but rather in the uniquely human, meaning-laden way. That's just fine as a description, and in so far as social anthropology is content with description then it's fine social anthropology. Also, in so far as social anthropology is concerned with explanation, if it's content with Lévi-Strauss type structuralist explanation, then Charlotte Hardman has arrived; she's found her anthropology of children and her question is answered. But I don't think anthropology (and note that I say 'anthropology', not 'social anthropology', and note too that Charlotte Hardman says 'anthropology', not 'social anthropology' in her title and last sentence) should or can afford to rest content with explanations that simply take people's ideas, whether adults' or children's, and relate them on a to-fro basis to the world of knowledge in which they live. It's no great trick to do this, although it may be fashionable, or have been so. There remains the stubborn fact, which anthropologists by the generation have chosen to ignore or demote to irrelevance, that humans, children and adults, are biological entities with nervous systems, eyes, ears and so on.

Do I then advocate some sort of Robin Fox-Lionel Tiger approach, by which I mean a consideration of man as a 'cultural animal', a creature evolved and pre-programmed with a 'biogrammar' that predisposes him to develop in certain directions, both in face-to-face interactions and in his social arrangements? (see e.g. Fox 1967; Tiger and Fox 1966, 1972). No, I don't, and I think an article such

as Charlotte Hardman's is sufficient to dispose of most of the Tiger-Fox arguments, which seem to me (although for a while I myself was attracted by them and even engaged in them) to be metaphorical, un-productive of empirical research and too speculative for comfort. Having lived with and thought about that approach for a few years I have felt myself forced to reject it or at least modify it drastically. Charlotte Hardman's approach, however, is much more enlightening and leads to a direct need for more empirical research and more theorising. (Incidentally, she might be interested in a little piece of research I recently did with Anne Guest, on children's conceptions of the meaning of Easter (Guest and Reynolds 1972)).

But to return to the basic problem. What of the complex and skilfully worked out approach of Piaget to children's mental development? Hardman writes "In certain other aspects Piaget is surprisingly anthropological in his approach, or rather he links with anthropology through structuralism. He sees his own theory of cognitive structure as intimately connected with Lévi-Strauss' doctrine of the primacy of structure in social life, and like Lévi-Strauss is seeking that conceptual structure which lurks behind the social structure" (p.94)...."We might perhaps link the works of Piaget and Lévi-Strauss as a means to understand child thought". (p.95). An excellent idea, but it leads to certain problems which are elucidated in an article to which Hardman does not refer, namely Howard Gardner's recent paper "Structure and Development" (1973), which contains a step-by-step comparison of the methods of and results achieved by Piaget and Lévi-Strauss.

So relevant is this article to the issues here discussed that I quote from it at length:

'Piaget poses a crucial question: "Le problème central de tout structuralisme: les totalités par composition, sont elles composées de tout temps, mais comment ou par qui, ou ont-elles été d'abord (et sont-elles toujours?) en voie de composition? Autrement dit, les structures comportent-elles une formation ou ne connaissent-elles qu'une préformation plus ou moins éternelle?" (Piaget 1968).

'Here Piaget is challenging structuralism of the Lévi-Strauss variety, for he goes on to maintain that a full comprehension of the structure can only result from the realisation that a structure is always in the process of being formed and that one cannot understand the structure without appreciating the nature of its formation and its course of continuous transformation and auto-regulation'. (Gardner 1973 p.56). And further on:

'Piaget's approach, then, involves a continuous dialectic between the flux biological processes and the formal precision of structural models... In a way suggestive of Lévi-Strauss, the structures discerned are viewed as intermediate between the nervous system and conscious behaviour.' (p.57).

Gardner goes on to compare and contrast Piaget and Lévi-Strauss in a number of respects, but note his comments on the developmental issue: 'Lévi-Strauss is explicitly not concerned with...the manner in which, over time, the individual member of the society acquires the cultural system... In his disregard of individual actions Lévi-Strauss's thoughts about development are reflected in a very instructive way. Lévi-Strauss believes that the five-year-old in a society has already acquired the ways of thought

of adults...(Piaget) scorns the kinds of a priori formulations about the nature and quality of thought which Lévi-Strauss finds attractive." (p.58-59).

Gardner concludes: "This review of the two positions suggests that the discrepancies between Lévi-Strauss and Piaget predominate. Yet it is only because they are in many ways close to one another that a detailed comparison is even possible." (p.60).

Gardner continues with a discussion of some of the work of Jakobson, indicating that it might well provide a bridge between Piaget's developmentalism and Lévi-Strauss's structuralism. To go into the details of this is not my intention, in any case Gardner's article is in print. However I will just give his conclusion: "Indeed, combining the developmental perspective of Piaget and the structural linguistic approach of Jakobson and Lévi-Strauss would seem a promising step for students of psychology and anthropology. It should bring into closer alignment those approaches which stress the sensory aspects in relation to specific cultural codes, and those which stress the active, organising aspects in relation to the world of objects and persons." (p.66). Clearly, Gardner would feel that Hardman's idea of combining Piaget and Lévi-Strauss was a good one, and that the way to do it would be via Jakobson.

As for my own comment at this stage, I feel that there is here a problem of extraordinary interest for anthropologists, but one which needs widening out further than authors have hitherto been prepared to do. I want to see much more early non-verbal communication brought into the developmental picture, and a concern among anthropologists for a frame of reference that will do justice to the amazing transformations involved in child development. And as if that alone were not enough I want to see the actual underlying (neuro-) physiological processes brought in as well. I don't want to see a quick jump into genetic or para-genetic arguments or evolutionary rationalisations.

Just to arouse the reader's curiosity, let me end by saying that my own current research concerns the estimation by biochemical means of the catecholamine content of children's urine. Children like those studied by Charlotte Hardman. Why? Let us continue at another time. But let us work together and not create artificial barriers. If we do, children of all ages will have a right to laugh at us.

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