necessarily an inconsequential difference given that Dodd and Prothro have apparently merged data from class-heterogeneous samples in two developing countries (Jordan, Lebanon) where the quality of pre-, peri-, and postnatal health care can be quite variable.

Another difficulty is that two of the three samples utilized by Dodd and Prothro are from urban areas (Beirut, Tripoli), where the possibility for intimate childhood association and proximity between patrilateral cousins is generally more limited than in rural areas like Bayt al-‘asir, even nine years after the Prothro-Diab study. In fact, elsewhere, Prothro and Diab (1974:70) support this urban/rural difference in residence patterns by concluding,

The “territorial dimension of the kinship structure” is still much in evidence in the villages, and showing no sign of disappearing. In the cities, however, only a minority live near kin. This holds for all classes alike. Moreover, those who do live near close relatives seem as likely to be near relatives of the wife as relatives of the husband. The traditional residence pattern is not predominant in the cities.

Of course, the fertility pattern Dodd and Prothro report in their Table 2 reflects that of adults at the time of data collection in the late 1960s. These authors do not provide information about the residence pattern (hence, degree of childhood association) of their informants when they were children. In sum, I again conclude that the “distant” nature of these data and their incomparability with mine do not justify Dodd and Prothro’s conclusion that the Prothro-Diab research contradicts my findings.

I conclude this rejoinder by noting that the responses to my paper have delineated an important human theme in Westermarck’s thesis on human sexuality and one that differs from the case of nature versus culture. As reflected in FBD and minor marriage, Westermarck’s position highlights the issue of tension between the individual and the group. As such, it is a topic tailor-made for anthropological fieldwork. Further attempts to determine whether familiarity breeds sexual disinterest must be conducted at close range, with particular attention paid to specific cultural contexts and (most importantly) to the feelings of those actually in siblinglike marriages, as distinct from normative preferences for such marriages. Thus, it may be that documenting cases of adultery and thwarted attempts to arrange FBD unions will provide more sensitive indices of the Westermarck hypothesis than divorce and fertility rates. For now, I believe that Westermarck’s thesis provides an essential understanding in accounting for the general tendency of human beings to avoid mating with primary kin. It will be the task of future research to elaborate this understanding.

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Has Piaget Been Upstaged? A Reply to Hallpike

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Adopting that lively rhetorical style that many have come to love and expect in the Commentaries section of the American Anthropologist, C. R. Hallpike has written a reply (AA 85:656–660, 1983) to my review of his book The Foundations of Primitive Thought (see Shweder 1982; Hallpike 1983). It is a useful reply, offering as it does several qualifications of his original thesis and revealing much about his conception of primitive thought. As a de-
fense of Piaget's stage theory, however, it is
less useful and cannot be read as a substitute
for a balanced assessment of the recent (post-
1970) cognitive development literature. For
what an assessment of that literature reveals is
that Hallpike's Piagetian account of the primiti-
ve mind has been upstaged by the evi-
dence.

Hallpike's book presents the thesis that the
collective representations or ideational for-
mations of illiterate, unschooled rural peoples
(the underdeveloped or premodernized Third
World) are best understood as expressions of
preoperational thought processes characteris-
tic of Western children two to seven years old,
as described in Jean Piaget's stage-theory of
cognitive growth. Four objections to that the-
esis were raised in my review, the first two be-
ing variations on the theme that Piaget's
stage-theory of cognitive growth is in deep
trouble among cognitive developmentalists.
More specifically I pointed to recent evidence
suggesting that no single stage level is a gen-
eral or characteristic property of an individu-
al's thought and that Piaget seriously under-
estimated the operational capacity of illiter-
ate, unschooled Western children (two-to-
seven-year-olds). In my review I also noted
that Hallpike's references do not include a sin-
gle citation since 1966 from any major devel-
opmental psychology journal, strongly imply-
ing that Hallpike, inattentive to the last 10 to
15 years of cognitive developmental research,
had imported a somewhat outmoded mode of
thought. I pointed to an irony: Hallpike tries
to convert anthropologists to the Piagetian
orthodoxy at precisely the moment (1979) when
the field of cognitive developmental psychol-
ogy is close to abandoning Piaget's stage the-
ory.

There have been two important events since
the publication of my review. The first is the
publication of John Flavell's 1980 presenta-
tion and evaluation of stage-theory at the Min-
nesota Symposium of Child Psychology (Flavell
1982). The second is the publication of the 1983
Mussen's Handbook on Child Psychology,
which includes a major review article by Gel-
man and Baillargeon on Piagetian concepts.

Flavell is a leading Piagetian scholar and
his discussion of cognitive stages is balanced
and even-handed. After presenting an over-
view of potential explanations for heterogene-
ity and homogeneity in cognitive functioning,
he concludes that it is his "reasonable best
guess" that "human cognitive growth is not
very 'stage-like', in the horizontal-structure,
high-homogeneity meaning of the term"
(Flavell 1982:17) and "that the word [stage]
connotes more homogeneity to most audiences
than its user may want to claim—and more
than actually obtains within the child's mind"

Gelman and Baillargeon's (1983) review ar-
ticle brings together much of the relevant post-
1966 evidence. Their "here's the state of the
art" assessment of Piaget's stage theory goes
something like this: "the experimental evi-
dence available today no longer supports the
hypothesis of a major qualitative shift from
preoperational to concrete operational
thought'" (Gelman and Baillargeon
1983:167). Preschool children are not funda-
mentally egocentric or percept-bound
(1983:174, 175). "In our opinion there is little
evidence to support the idea of major stages in
cognitive development of the type described
by Piaget. Over and over again, the evidence
is that the preoperational child has more com-
petence than expected" (1983:214).

Hallpike's reply to my review gives no in-
dication of engagement with this recent cog-
nitive developmental literature, and my var-
ious objections to his thesis (Shweder 1982) go
unanswered. He does, however, advance four
arguments, each of which I shall consider in
turn.

Hallpike's first argument (p. 656) goes like
this. Since I (Hallpike) learned anthropology
from Evans-Pritchard and Needham I am not
unaware of the problems involved in reducing
collective representations to individual
thought processes. If there had been a prob-
lem in my argument, the social anthropolo-
gists who reviewed my book would have noti-
ced it, "but none has done so" (p. 656)"; it
would not have required child psychologist
Shweder to have discovered this" (p. 656).
Hallpike references reviews by Beideman,
Gellner, Hicks, Howes, deRuijter, Southwold,
and Willis. He does not mention the review by
the anthropologist Lave (1981) that appeared in
Contemporary Psychology.

The principle advocated here by Hallpike
(your objections must be wrong because oth-
ers didn't make them) is of course ridiculous.
What's far more curious, however, is that
what Hallpike says about his anthropological
reviewers is not accurate. Willis (1981:14), for
example, quite explicitly describes Hallpike as
"vulnerable to attack" precisely because he is
"heedless of Evans-Pritchard's famous admo-
nition that attempting to construct social an-
thropology on the foundations of psychology is
to build a house on shifting sands". There is
also a double irony in Hallpike's comparison
between "child psychologist Shweder" and the
"social anthropologists" who reviewed his
book; for Shweder is not a child psychologis
and it was Hallpike (1979:32) himself who, in

Commentaries

139
his book, described the concepts of anthropology as vague and confused compared to those of child psychology. With regard to his first argument, Hallpike would be well advised to get straight his facts and his message.

Hallpike's second argument (p. 656) is that I (Shweder) do not do justice to the full scope of his theory and that I present only minor aspects of his thesis. The first two pages of my ten-page review are an extensive exposition of Hallpike's thesis; that exposition begins with the statement that the book is "dedicated to the principle that 'the collective representations of a society' must 'reflect' the 'level of cognitive development of the great majority of the adult members of that society' " and that "the collective representations of illiterate, un-schooled, rural peoples are expressions of preoperational thought processes characteristic of two-to-seven-year-old Western children, as described by Piaget."

Here is how Lave (1981:790) describes Hallpike's thesis: "the book addresses only one proposition, and in a one-sided form that might be paraphrased as 'the ethnographic literature suggests the proposition that primitive thought is retarded at a universal preoperational stage of cognitive development. ' " Here is how Gellner (1980:911) describes Hallpike's thesis: "The central idea seems to be that Piaget's psychology can help us define the similarities and differences between our thought and that of savages. " Here is how Willis (1981:13) describes the thesis: "By unconditionally adopting Piaget's linear-evolutionist schema he commits himself to a central thesis that will almost certainly turn out to be importantly wrong. " Here is how Howes (1981:145) describes the thesis: "Hallpike's basic contention is that the thought processes of most individuals in primitive societies are arrested at the late pre-operatory stage (about age 5 and 6). " Here is how deRuijter (1981:187) describes Hallpike's thesis: "He uses Piaget's conceptual framework, which is the result of studying children in complex societies, to explore adult thinking in primitive societies." Here is how Southwold (1981:491) describes the thesis: "The collective representations of any society are necessarily shaped—though not wholly determined—by the kinds of cognitive processes that prevail in that society. " It is not difficult to "sum up" Hallpike's thesis, and most reviewers have agreed on what it amounts to. Thus it seems to me curious and slightly disingenuous for Hallpike to state in his reply (p. 656) that his discussion of the relationship of collective representations and individual thought processes is restricted to a mere 60 pages (a not insignificant amount of space) in the first two chapters. Almost every chapter in the book returns to the main thesis or is used to illustrate it, and one need only turn to the first page of the index (Hallpike 1979:513), where one finds a "Note": "The name 'Piaget' occurs so often throughout this book that its inclusion in the index would not be informative."

Hallpike is not lacking in cleverness, and in this context we find one of his several ingenuities. Quoting from Hallpike's reply (p. 656): "the best he [Shweder] can do by way of presenting my theory is to refer, repeatedly, to an alleged '50% rule': 'The mastery of a conceptual problem must be far above the 50 percent level in adults if a notion is to be incorporated into the collective representations of a society' (Shweder, p. 355)."

An alleged "50% rule?" What Hallpike has done here is make it look as though he is quoting me and that the "50% rule" is my summary formulation of his thesis. In fact, the statement of the "50% rule" (above "the mastery of a conceptual problem") is a direct quotation from Hallpike's book (1979:61) and appears as a quotation in my review. In his reply Hallpike leaves out my page reference (p. 61) to his book.

Hallpike's third argument is that the studies cited in my review are not relevant for assessing his thesis or Piaget's theory. One might have given some weight to this part of Hallpike's reply if he had undertaken to assess the now substantial experimental literature concerned with Piaget's stage theory. This he has not done. (For a systematic, comprehensive and critical appraisal of that literature see Gelman and Baillargeon 1983; also see Flavell 1982). Instead we are subjected to a barrage of piecemeal debunking that is as troubling as it is irrelevant for any rational appraisal of Piaget's stage theory.

For example, Hallpike does not like a particular study I cited by Bullock and Gelman (1979) on causal reasoning. He thinks they do not understand Piaget and that the study does not address relevant issues of causal thinking in children or primitives. Yet Hallpike must know, or at least he ought to know, that there have been several recent studies of causal thinking in children and primitives and they tell a similar story. A 1982 monograph by Schultz (1982:5), published by the Society for Research on Child Development, summarizes the findings of this body of research as follows: "Human rules [of causation] can be used appropriately by children as young as about 3-4 years of age" (also see Trabasso, Stein, and Johnson 1981).

Especially important for the present discus-
sion is Schultz's own experimental work (experiment 4) on causal thinking among the Bambara of Mali. The research is conducted with unschooled Bambara village children who had no contact with Westerners or technological gadgets, as well as with Bambara children who attended Western-style French schools. Schultz (1982:38) reports that "the essential similarities between the results for Malian children and those obtained for Western children are many and striking." He concludes that "causal reasoning is a very basic mental function not easily influenced by even substantial cultural variations" (1982:39) and that "the child as young as 2 or 3 years of age possesses a much more sophisticated conception of causation than has heretofore been realized" (1982:47). Hallpike simply ignores all recent evidence on causal reasoning.

Hallpike also does not like my interpretation of the implications of a study by Goldberg et al. (1974). I stated in my review that the Goldberg et al. study indicated that children as young as two years of age have the intellectual capacity to "detect categorical structure" and that "as Goldberg et al. note, children can cluster things categorically as soon as they can talk." Hallpike has several objections to my characterization of the Goldberg et al. study.

First, Hallpike makes it appear that I misreported the study. Quoting Hallpike (p. 657): "the children were actually in the age range 29–35 months (hardly 'as soon as they can talk')." The Goldberg et al. study is a study of 29–35-month-old children; that, as I reported, they studied two-year-olds. And, as I reported, Goldberg et al. do note that children can cluster things together as soon as they can talk. The evidence for this, as noted by Goldberg et al. (1974:6), comes from a study by Rossi and Rossi (1965). Goldberg et al. view their own work with two-year-olds as an extension of the Rossi and Rossi findings. (For further evidence on this point see Huttenlocher and Smiley 1983.) Why does Hallpike try to make it appear that the Goldberg et al. study was misreported when it was not?

Hallpike's second objection (p. 657) is that the purpose of the Goldberg et al. experiment was "primarily to investigate memory rather than the classificatory ability of young children." Here we have another of Hallpike's inanities. Hallpike knows that "the ability to remember is closely associated with the ability to classify," for that quotation is from Hallpike's (1979:191) book. Moreover, Goldberg et al. begin their article by explicitly noting that they are studying the use of categorical organization in free recall. The ability to classify is often studied in exactly this way (see Bousfield 1953; Cole et al. 1971:115; Huttenlocher and Liu 1979). Why does Hallpike try to make it appear that the Goldberg et al. study was not a study of categorization capacities?

Hallpike's third objection is that the Goldberg et al. study is not an exact replication of Piaget's procedures, where children are asked to sort objects into groups of things "that belong together." One hopes that Hallpike is not here advocating a form of operationalism in which Piagetian concepts are defined by the procedures that Piaget happened to use. Nevertheless, let's for the sake of argument accept Hallpike's stricture. In an important and well-known study, Rosch et al. (1976: experiments 8 and 9) examined the classificatory behavior of three- and four-year-old children using a three-item oddity task. Children were presented with sets of three colored photographs of objects representing four categories of animals (cats, dogs, butterflies, and fish) and four categories of vehicles (cars, trains, motorcycles, and airplanes). The children were asked to point to "the two that are alike, that are the same kind of thing." Some trials examined the child's "basic level" sorting of categories (e.g., a cat, a truck, and another cat) and some trials examined the child's "superordinate level" sorting of categories (e.g., a dog, a train, and a fish). For "basic level" categories (cats, dogs, cars, trains), the performance of three- and four-year-olds was virtually perfect, 99% and 100% correct. For "superordinate level" categories (e.g., animals, vehicles) three-year-olds were correct 55% and four-year-olds, 96% of the time. Rosch et al. (1976:417–418) conclude that "there do exist objects which even small children will classify in the same manner as adults."

Hallpike's fourth objection to the Goldberg et al. study is that it is impossible to be certain that the two-year-olds were using Goldberg et al.'s taxonomic categories; the children might have been thinking complexly, not taxonomically. This is true; one can never be certain. Goldberg et al. provide us with a sufficiency proof. They select a "related pair" of items (elephant-giraffe) to represent a conceptual category (animals) and they show that two-year-olds are better at remembering pairs of items selected in that way. They do consider the possibility that there might have been other confounding forms of association among the items (Goldberg et al. 1974:3) and they explicitly rule out that possibility by referring to pilot work from another experiment. They argue that their results are consistent with the view that two-year-olds are able to organize memory around conceptual categories. Hall-
Jones does not try to develop his alternative complexive-thinking hypothesis, nor does he explain why a complexive thinking hypothesis should predict the same results as a conceptual category hypothesis. Given all the evidence on classification and categorization in young children reviewed by Gelman and Bailargeon (1983), Hallpike's objection seems both ad hoc and unlikely. As Gelman and Bailargeon remark, reviewing the relevant evidence: "Children as young as 3 years of age can sort objects according to a consistent criterion and without remainder or overlap. Indeed there is evidence that still younger children can do the same."

Aside from his "ingenuities," another troubling feature of Hallpike's reply is his somewhat selective reporting of evidence. Thus, in discussing Blurtom-Jones and Konner's (1976) work "Kung Knowledge of Animal Behavior," Hallpike argues (p. 658) that in one respect their work supports his thesis. He quotes Blurtom-Jones and Konner to the effect that the Kung are less interested in theorizing about animal behavior than in recording and discussing facts about the animals they hunt (e.g., that Kudu are afraid of cows). Hallpike sees this as support for his view that they (the illiterate, unschooled !Kung) are atheoretical and concrete while we (literate, schooled Westerners) are theoretical and abstract. What Hallpike never mentions is that Blurtom-Jones and Konner make their observations about the !Kung while arguing that Western ethnologists and the !Kung display quite similar patterns of thought. Blurtom-Jones and Konner (1976:346-347) in fact argue that animal behavior is context-specific and best understood concretely. They favor the following interpretation of the !Kung's fact-driven "turn of mind": "there cannot be any grand universal theories of behavior; the nature of the data, primarily the diversity of species, forces on !Kung and biologist alike a greater respect for facts and for the diversity of life than for attempts to explain them in a simple way." In summary, Hallpike has not engaged in a systematic assessment of the evidence against his thesis, and his selective discussion of evidence is full of "ingenuities" of the type described above.

Hallpike's fourth general argument is that I do not understand him (or anything). He seems to imply that there are two types of reviewers of his book: those who understand him and praise him and those who misunderstand him and criticize him. Into the second category he puts Scheffler (1981) and Shweder (1982), and I suppose we should add Lave (1981) and Willis (1981) as well. I have a different theory about the reception of Hallpike's book. Those who are most familiar with the cognitive development literature are critical of the book. Lave (1981), for example, notes in her critical review that Piaget's theory has strained under intense pressure; and Willis (1981), noting that the central thesis of Hallpike's book will almost certainly turn out to be wrong, points to "recent studies attacking Piaget on his home ground, the cognitive characteristics of children in Western society."

Those who praise the book seem unaware of these studies. Some of those who praise the book seem fascinated by Hallpike's anti-relativism, and of course "anti-relativism" is something of a fashion these days (Geertz 1984).

On a more positive note, Hallpike's reply to my review marks the beginning of the epicycle stage of his theory, and his theory of primitive thought may now be in danger of becoming uncontroversial. In the midst of all the derision and dismissing he actually concedes a good deal. Begrudgingly he grants (p. 659) that literacy is not all he made it out to be. He concedes (pp. 659-660) that collective representations (e.g., adult doctrine concerning the reality of dreams) may not be reflective of the underlying cognitive processes of a population. Having argued in his book (1979:94, 105, 109, 133) that the social environment of the primitive is conformist and dogmatic, that questioning and the expression of individual attitudes is repressed and that primitives do not think for themselves, he now states (p. 658) that he is not at all surprised that the !Kung disagree with each other, do not treat hearsay as fact, and are quite willing to confess ignorance when they do not know something. Conformist, dogmatic, and repressive?

Most importantly, Hallpike grants (p. 659) an essential distinction between implicitly knowing how and the ability to explicitly state the knowledge one has; and, in his reply, he limits his thesis to the issue of explicit knowledge. If that is all that Hallpike wants to claim—that individuals and cultures differ in the extent to which they reflect on what they know and explicitly formulate it, and that those who get schooling are better at self-reflection—there would be little to quarrel about. Such a modified thesis has nothing to say about the type of intellectual operations available to a people and it is consistent with the findings of cognitive development research. The modified thesis merely asserts that there is a difference between implicit and explicit understanding of principles and that some cultures are more rationalized, although not necessarily more rational, than others.
Self-consciousness about thinking probably is a useful cross-cultural and developmental variable; not everyone has good verbal access to their own available intellectual processes or operational skills.

Exactly how far Hallpike is willing to carry the distinction between implicit and explicit knowledge is unclear from his reply. What is clear is that there is still a great discrepancy between Hallpike’s conception of mental processing and the concept of mental processing that has emerged in the recent cognitive science literature. The magnitude of that discrepancy can be gauged by Hallpike’s comments on Trobriand speeches and disputation. Quoting Hallpike (p. 659), “speeches and disputation in primitive society tend to take a vast amount of contextual knowledge for granted and to be conducted at a level of specificity that renders them largely opaque to the outsider—by contrast to our own judicial procedures, for example.”

Here Hallpike seems to stake his thesis on the claim that our judicial proceedings in particular, and our “speeches and disputation” in general are intelligible out of context and without the need for implicit background knowledge. If that is Hallpike’s thesis, we can dismiss it straight away, for there has been a widespread recognition among cognitive scientists during the past 15 years that all natural language discourse is abbreviated and condensed, and that comprehension always depends on context and a shared body of unstated knowledge, belief and presupposition. The basic principle is that the intelligibility of natural language communications depends on what’s unstated and tacit; and what’s unstated and tacit can never be fully explicated. The principle has been widely discussed. Among philosophers, see Cavell (1969:12) and Ziff (1972:chs. 4 and 6). Among sociologists, see Garfinkle (1967:ch. 1) and Mehan and Woods (1975:90–95). Among psychologists, see Bransford and McCarrell (1974). Among linguists, see Labov and Fanshel (1977:ch. 2). Among anthropologists, see D’Andrade (1984) and Tyler (1978:384–458). Among those who work on artificial intelligence, see Schank and Abelson (1977: specially ch. 3 on “Scripts”) and Winograd (1980:222–239). If primitive thinking is implicated whenever comprehension is dependent on context and tacit ideas, then “we” are “they” and the contrast between the primitive and the nonprimitive becomes meaningless. Even simple sentences (“the haystack was important because the cloth ripped”) become difficult to understand when bleached of context (“parachutist”), and ordinary language communications are never fully explicit (“Every morning he stands on his head”) and always leave room for misunderstanding by outsiders (decapitation?). A lot is taken for granted within all societies (see Bransford and McCarrell [1974] for numerous illustrations and experimental findings on tacit understandings and context-boundness among literate, schooled Western adults).

Had The Foundations of Primitive Thought been published in the 1960s during a decade when cognitive deficit theories and cognitive stage theories flourished, it would have been a bold and provocative challenge to anthropology, deserving of serious attention. Unfortunately, it was published at least ten years too late. By 1979, certainly by 1983, Hallpike’s Piagetian tome has been upstaged by the evidence.

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Reply to Shweder’s “Has Piaget Been Upstaged?”

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Shweder’s original review of my book (Shweder 1982) seemed at first sight to derive considerable authority from the long and imposing list of references quoted in support of his criticisms. I therefore examined a sample of these in my reply, and showed in every case that those cited (a) did not say what Shweder