The Use of Quantitative Observational Techniques in Anthropology [and Comments and Replies]
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Reviewed work(s):
Source: *Current Anthropology*, Vol. 26, No. 3 (Jun., 1985), pp. 323-335
Published by: The University of Chicago Press on behalf of Wenner-Gren Foundation for Anthropological Research
Stable URL: http://www.jstor.org/stable/2742731
Accessed: 11/04/2012 17:27

CURRENT ANTHROPOLOGY Vol. 26, No. 3, June 1985

The Use of Quantitative Observational Techniques in Anthropology[1]

by Monique Borgerhoff Mulder and T. M. Caro

QUANTITATIVE METHODS OF DATA COLLECTION in anthropology are rapidly gaining widespread acceptance. Anthropologists concerned with developmental issues (e.g., Blurton Jones 1972), economic activity (e.g., Johnson 1975, Minge-Klevana 1978), evolutionary questions (e.g., Chagnon and Irons 1979), nutritional assessment (e.g., Messer n.d.), and psychological anthropology (e.g., Whiting and Whiting 1975, Rogoff 1978) have deliberately adopted methods of animal ethology in investigating their own areas of research. Unlike descriptive narratives, quantitative methods allow specific hypotheses to be tested statistically and rigorous cross-cultural comparisons to be made on a quantitative basis. The new precision increases confidence in the robustness of results. This is especially important where societies are undergoing rapid change and observations cannot be repeated. It also makes anthropology more acceptable to other disciplines. However, observational techniques borrowed from the field of animal behavior cannot be directly applied to the study of human society without serious consideration of the new issues this raises. To date, these issues have received inadequate attention. In this paper we (1) discuss ways of measuring human activity, (2) enumerate the sources of bias involved in the use of observational techniques in the study of humans and suggest ways of dealing with them, (3) tackle the problem of definition in describing human activity, (4) discuss additional data which can be derived from the ability of the observer to communicate verbally with the subjects of a behavioral study, and (5) outline a code which can be used by anthropologists to facilitate comparison of behavioral studies across cultures. If workers are aware of the constraints and character of observational techniques at the start of their study, measures of association, activity, and interaction will be recorded more accurately.

[1] We would like to thank Pat Bateson, Laura Betzig, Tim Clutton-Brock, Robert Hinde, Paul Turke, and Richard Wrangham for helpful comments on earlier drafts of this paper. Borgerhoff Mulder is very grateful to the Office of the President, Nairobi, for permission to conduct research in Kenya and especially to the Kipsigi people of Tabarit and Abosi and to the family of Philip Arap Bii for their exceptional hospitality and cooperation. Borgerhoff Mulder was in receipt of a grant from the National Geographic Society and Caro held a Royal Society Scientific Investigations Grant when this paper was written.

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The present paper was submitted in final form 22 VII 84.

THE MEASUREMENT OF HUMAN ACTIVITY: BEHAVIORAL SAMPLING

Anthropologists agree that a key goal of their discipline is to explain similarities and differences, and stability and change, across human cultures. Given this goal, it is surprising to find that the cross-cultural data available for comparative purposes are often inadequate. While compiling and coding the *Ethnographic Atlas* (Murdock 1967), Murdock himself expressed serious concern over the lack of agreed-upon standards for ethnographic reporting (Murdock 1972). This lack of standardized reporting techniques has taken its toll on the level of cross-cultural theory (Johnson 1978). Nevertheless, because recent improvements have been made in controlled and theoretically oriented cross-cultural work (e.g., Naroll 1970, White, Burton, and Dow 1981), relationships are now being exposed between subsistence arrangements and descent (Aberle 1961, Textor 1967), warfare (Ember and Ember 1971), fertility (Em-

ber 1984), polygyny (Ember 1974, White, Burton, and Dow 1981), residence (Driver 1956, Ember and Ember 1971), female status (Whyte 1978), socialization techniques (Whiting and Whiting 1975), infant care (Nerlove 1974), slavery (White, Burton, and Dow 1981) and adult initiation ceremonies (Schlegel and Barry 1981).

In spite of these exciting theoretical and empirical developments, scepticism remains over the validity of comparing ethnographic reports published with little or no discussion of methodological procedure. Theoretical advances are possible only if reliable standards exist for the comparison of data from different cultures. Indeed, the most pressing problem in improving anthropological research design has been identified as data collection (Pelto 1970), specifically concept definition (Goodenough 1970) and measurement (Johnson 1978). This has led to the recent emphasis on the measurement of human activity, usually termed "time allocation studies."

Measurement of human activity is not an entirely new development within anthropology. Extensive analysis of time use has been made by ecological and economic anthropologists (Rappaport 1967, Lee 1969, Winterhalder and Smith 1981). Looking more specifically at the allocation of time by individuals, the new household economists, commencing with Becker (1965), began to investigate the evolution of family labor under differing socioeconomic conditions. Yet, in a recent review of time allocation studies, Minge-Klevana (1980) reached the overall conclusion that the present data on the time allocation of individuals in different societies were largely incompatible, a view supported by Nag, White, and Peet (1978) in their comparison of child labor in two communities. This incompatibility derives from the lack of standardized methodology. Two major problems emerge: (1) a lack, until recently, of a standardized manner of collecting time allocation data which addresses problems of bias and (2) insufficient rigor in the definition of behavioral categories (see below).

Time budgets have been assessed in various ways: (1) using interviews (respondent recall), with the subject recalling his or her activities over a period of anything from a day (e.g., White 1976) to a month (e.g., Hart 1977); (2) asking subjects to keep diaries (respondent report) of their activities (e.g., Minge-Klevana 1978); and (3) checking on the subjects' activities by observational means (e.g., Johnson 1975, Rogoff 1978). Errors (cf. Quizon-King 1978) deriving from insufficient and excessively patterned recall (D'Andrade 1973, 1974), from inaccurate reporting (particularly amongst people who do not use clocks) (Minge-Klevana 1980), and from the desire to conceal certain activities from the anthropologist (Gladwin and Gladwin 1971) seriously call into question the validity of time budgets based on data of the first two types.

Anthropologists have therefore been receptive to the pioneering work of Erasmus (1955) and Johnson (1975) and are beginning to adopt the method of spot observation, also called instantaneous sampling (Altmann 1974) or point sampling (Dunbar 1976), used in ethology. This method can be used to record the activities of a number of individuals (scan samples, Simpson and Simpson 1977) or the activity of a single individual (focal sample, Altmann 1974). A spot observation consists of the observer's recording the pertinent features of a subject's activity as soon as he or she is first observed. This technique has been found to give reliable estimates of the proportion of time spent in an activity (Dunbar 1976) and normally provides better estimates than other available methods (Simpson 1979*a*, Simpson and Simpson 1977; but see Harcourt and Stewart 1984 for a demonstration of large discrepancies between scan and focal sampling).

Studies that focus on specific problems such as personality development (Whiting and Whiting 1975, Draper 1975, Chisholm 1983) both within and between societies or that aim at obtaining accurate records of the duration and sequencing of activities (e.g., Evenson, Popkin, and Quiron 1978) have employed focal subject sampling. Here, a particular individual is followed for up to a day at a time, and records of specific activities are made during the period. Focal sampling has only recently been employed in anthropological studies; it can be used in conjunction with scan sampling (Chisholm 1983, Konner 1976, Munroe and Munroe 1971). Focal studies that employ continuous sampling techniques (Altmann 1974) are particularly well suited to the study of social interactions and qualitative differences in work patterns because a sequence of activities can be recorded. Subsequent discussion refers to spot observations used mainly in scan sampling, although much of the discussion can apply to spot observations that are used in focal sampling as well. We suggest that quantifiable direct observation of human activity collected through spot observational techniques provides a solid data base for cross-cultural comparison. Two important provisos must, however, be made explicit.

First, advantages are reaped from quantitative measurement only where variables have been satisfactorily operationalized (see Bateson 1968 for an ethological discussion of this point). Unless quantitative techniques are grounded in sound theoretical ideas, correlations can become mechanistic and meaningless (e.g., Kroeber 1952 and more recently Driver and Coffin 1975). The validity of operationalization depends, of course, on the question asked and must be addressed by each researcher in the context of a specific ethnographic situation. Clearly, quantitative measurement can never entirely replace sensitive ethnographic description. Among anthropologists, sensitivity to the shortcomings of quantification has resulted in the almost exclusive use of nominal measurement, termed "butterfly collecting" (Leach 1961, but see Firth 1946 and Richards 1939): people either are or are not patrilineal, do or do not practice ancestor worship. Systematic quantification beyond the nominal level has substantial advantages (Johnson 1978): it increases the reliability and compatibility of cross-cultural research; it preserves negative cases and undermines the subconscious quest for patterns; it exposes intracultural diversity; and it permits statistical rigor, thus facilitating theoretical precision.

Second, cultures differ greatly in the social value placed on time; whereas in Western society "time is money," in many parts of the world haste is considered inappropriate for the execution of any activity. The use of time as a common currency for making statements about comparative productivity, for example, must therefore be questioned (Gross et al. 1979) because of differences in such factors as rates of work, different technologies, and even the differing enjoyment derived from work. For instance, that the adult males of one culture spend twice as many hours a day in agriculture than those of another culture does not necessarily reflect substantial differences in their productive roles. Nevertheless, we suggest that some of these problems, such as differences in rates of work, can be overcome (see below), and we stress that time budgets should be used not alone but in conjunction with other variables in the analysis of human productive activity (e.g., Collier 1975).

Given an awareness of general anthropocentric biases affecting the quantification of cultural data, we now discuss specific biases that emerge from the use of spot observations to measure the allocation of time in human society. We then go on to discuss the question of how to define behavioral categories. We believe that the arguments presented below go some way towards countering the more general criticism made of quantitative work by anthropologists within the humanist tradition—that quantification, being mechanistic and depersonalized, is wholly inappropriate to the study of culture. While we do not wish to belittle the importance of the humanist tradition, we do believe that controlled quantitative observational techniques generate effective knowledge.

Choice of Subjects

The intensive observation under which the subject of a behavioral study is placed, particularly in the case of focal sampling, can be disturbing or annoying to the individual concerned, animal or human. But choosing subjects according to their tolerance of repeated visits or daily follows may produce a bias towards, in humans, individuals who are lonely, eccentric, or trying to please the anthropologist. This kind of bias has been perceived by anthropologists for a long time, but it is difficult to define and measure.

Some observational studies take a geographic or socially defined community (e.g., a village) as the unit of study, thereby largely obviating a sample bias of this type. Hames (1979) visited the households of all members of a village. Johnson (1975) visited all members of the households within 45 minutes' walk of his house, but if subjects live in close proximity for a particular reason this kind of sample may not be representative. Often families are chosen on socioeconomic or structural criteria (e.g., Minge-Klevana 1978), and it is not clear how much personal bias is operative in selection. Only occasionally is cooperativeness on the part of subjects explicitly a criterion for selection (Branch 1977).

Clearly, a total sample of a socially or geographically limited area is preferable in that it avoids a sample bias towards those individuals who are in some way trying to please the anthropologist. However, total samples should also be chosen with care. In field studies of baboons, observers have selected the biggest troops available to them, up to a limit of 80–100 animals; insofar as group size affects baboon behavior, analyses from such studies are likely to be biased (Sharman and Dunbar 1982).

In all cases, it should be made clear in publication whether individuals have been dropped from the sample and whether this exclusion was due to demographic factors (death or emigration), to an insufficient number of observations, or to concealment of activities resulting from low cooperation. An insufficient number of observations or reported activities within specific age- or sex-classes is in itself interesting, suggesting, at most, the presumable inadmissibility of certain of their activities to the outside observer.

Time of Day

In all societies, human activity is structured according to time of day. To gain relative measures of activity, most studies have used randomly timed visits, following Johnson (1975). However, in many parts of the world, settlements are highly scattered, making randomly timed visits to households not entirely feasible. Peristiany (1939), without even an observational schedule in mind, describes a Kipsigi valley in western Kenya as "a scene of desolation to the prospective anthropologist. Miles and miles of shrub or grass, then a dark patch—a field—and two brown spots—a hut with its granary and, sometimes, a kraal; then more bush and another sign of human habitation 300 or 400 yards away." Walking past a homestead in the pouring rain to reach a randomly selected house on the other side of the valley is not only logistically but socially unacceptable. Other sampling methods need to be developed. Faced with this dilemma, Johnson (1975) divided his sample of 13 households in half, thereby establishing a stratified sample within which particular households were visited at randomly selected hours. With larger samples of scattered households, randomly timed visits are impractical; rather, the day can be divided into blocks and each household visited a standard number of times within each block.

Randomly timed visits in human studies have two advantages. The human subjects cannot predict the observer's arrival, and, with the schedule predetermined prior to the day's work, the observer cannot change the route, consciously or unconsciously, in order to avoid an individual or situation he or she finds particularly disturbing to the schedule (for example, a group of heavy drinkers who insist on the observer's joining them). Where standardized time-block visiting becomes necessary, the advantages of random visiting within those blocks can be preserved by selecting a route from a large number of predetermined routes prior to the day's work.

Nonetheless, randomly scheduled visits are subject to bias if the observation points are not independent. In animal studies, where many point samples may be made during a day it is important to calculate the frequency distribution of bout length of the activity in question before sample points can be treated as independent. If the distribution of bout lengths has a sharp discontinuity—for example, if most bouts of beating maize are under an hour in length—then point samples more than an hour apart can be treated as independent for this activity. Alternatively, where, for instance, visits between household members usually last one to two days, point samples recording association data should not be less than two days apart. Ideally, analysis should be performed on all major activities to be scored before the intersample interval is chosen, on the basis of the time interval that best fits all activities.

Moreover, randomly scheduled visits are subject to bias if time intervals between spot observations vary widely, perhaps because of time taken to find subjects. Hawkes et al. (n.d.) elegantly demonstrate that group activities can become enormously overrepresented because many people can be found and observed in a group. Similarly, if houses are used as a means of locating individuals, household activities will become exaggerated. Hawkes et al. suggest that in order to maintain the independence of observations each point sample on each individual should begin only after a specified time has elapsed since the previous observation; if subjects are not found within a specified search time, this should also be recorded.

In animal studies, frequencies of behavior and association have been found to change according to the predominant activity at the time (Harcourt 1978, Simpson 1979*b*). Ideally, studies of individual time allocation should be based on 24-hour focal samples, but this is rarely practical. If the day is divided into time blocks, classes of individuals should only be compared using the same time blocks or using observations derived in equal proportions from different time blocks.

Seasonal Effects and Demography

Seasonal changes can dramatically affect types and rates of behavior. Both the primate and the anthropological literature provide a wealth of examples of how time spent feeding, food availability, and food distribution vary according to season (see Clutton-Brock 1977, Winterhalder and Smith 1981). In accordance with this, anthropological studies, especially those concerned with agricultural labor inputs, have covered the complete annual cycle (Johnson 1975, White 1976, Branch 1977, Hart 1977, Minge-Klevana 1978, Werner et al. 1979) or have, at least, sampled from different seasons (van Deenan 1964).

Recently it has been shown that it is not only behavior related to food acquisition that is affected by seasonal change. In primates, marked differences in the amount of play have been found to mirror changes in monthly rainfall in vervet monkeys (Lee 1983), as have differences in competitive interactions in other species (Oliver and Lee 1978). This implies that cross-cultural psychologists may need to spend a complete year in the

field before they can make generalizations about rates of behavior in a particular society.

Indeed, it has been found that the number and kind of social relationships can depend on demographic factors such as differences in sex ratio, survivorship, or age-classes present (see Dunbar 1979 for such evidence in baboons). Thus anthropologists may need long-term studies to reach a better understanding of patterns of marriage (e.g., Chagnon 1982) or maternal care (Turnbull 1973).

Spatial Effects

A further problem in scan or focal sampling is that subjects, either consciously or subconsciously, limit some activities to situations in which the observer cannot see them. For example, certain types of behavior may be performed only outside the village (hunting, agriculture on rented fields, visiting). To some extent, this problem can be overcome by making an effort to seek out the subject, although the amount of time that can be given to any one individual is necessarily limited. The problem is compounded in human studies in that certain activities are seen as highly sexually specific, for instance, washing or the preparation of ceremonial items, preventing the observer from witnessing certain activities of subjects of the opposite sex. Although anthropologists are often attributed a degree of sexual neutrality (Bowen 1954), their sex can still cause problems. Sexual biases can be assessed if there are both male and female observers in the study.

If individuals do work at night or out of sight of the observer during the day, total time spent in work will be underestimated. Because the type and amount of activity performed out of sight will differ between sexes and between societies, comparisons will at best only approximate the real situation and at worst be guesses. Two methods can be used to circumvent this problem: (1) Random or fortuitous point samples can be made to check on verbal reports of absent individuals, allowing reported data to be used to calculate time budgets (but see below). (2) A limited number of special trips can be made to check on the activity of absent individuals on hunting or fishing parties (Gross et al. 1979), and these can provide a quantitative subsample by which total work budgets can be revalued.

Observer Effects

Taking spot observations involves moving along a random or predetermined route recording the activities of all subjects when they are first encountered. Unfortunately, subjects can change their behavior very quickly when they see the observer, as do nonhabituated nonhuman animals (see Underwood 1982). In contrast to the latter, however, humans may try to impress or mislead the observer, and this is potentially a serious form of bias. One way to measure this observer effect is to code recordings on the observer's judgment in terms of whether the observer saw the subject first (fig. 1, *1*) or the subject sighted the observer (fig. 1, *2*). The numbers in table 1, taken from a current study of Kipsigi in western Kenya, are divided for simplicity into two sorts of behavior: being active or being idle. It can be seen that different individual subjects were idle significantly more when they had seen the observer before the point sample was taken ($\chi^2 = 909.4$, df $= 1$, $p < 0.001$, two-tailed). On the surface this result suggests a bias towards subjects' stopping activities when they saw the observer, but it can be explained in another way. Idle subjects were sitting or lying more than were subjects who were active; sitting permits a greater degree of vigilance, making subjects more likely to see the observer (table 2, $\chi^2 = 44.7$, df $= 1$, $p < 0.001$, two-tailed). Nevertheless, it is possible to measure observer bias with such caveats in mind. In practical terms, it may be best to carry out frequent visits over a long period of time; eventually it will become inconvenient for the subject to modify or suppress his or her habitual behavior, although in some contexts this may be very slow.

Baseline data

Date
Time
Weather
Location
Subject code

Type of observation

(1) Observer sights subject first
(2) Subject sights observer first
(3) Informant report
  (4) Identity of informant
  (5) Informant alone/with others
  (6) Informant report accurate/inaccurate
  (7) Informant report found accurate/inaccurate on basis of self-report

Activity

(8) Physical description of activity
(9) Context or purpose of activity
(10) Presence of other synchronous activities

Association data

As required

Fig. 1. Information to be collected on checksheet.

TABLE 1

Activity of Different Individual Subjects and Sighting of the Observer

	Number of Individuals Scanned	
	Active	Idle
Observer sights subject first	111	43
Subject sights observer first	51	97

TABLE 2

Position and Activity of Different Individual Subjects

	Number of Individuals Scanned	
	Lying or Sitting	Travelling or Standing
Active	40	100
Idle	91	39

INTEROBSERVER RELIABILITY

Differences in the way that observers record behavior will be determined by many factors including their classification of behavior, their linguistic competence, and their personality or cultural rapport. While interobserver reliability testing in psychological studies can satisfactorily measure the accuracy with which behavior is scored, by comparing the results of two observers recording the behavior of a subject simultaneously (e.g., Caro et al. 1979), this method is not feasible in anthropological studies. The presence of another, less familiar

observer (essentially a visitor) may lead to gross changes in the subjects' activities; for example, children may run away, women may suffer from embarrassment, or men may rush to make the acquaintance of the visitor. This will limit the size of the sample of undisturbed behavior available for interobserver comparison.

Reliability can, however, be tested in ways which do not necessitate a second observer. One measure is the consistency in an observer's records over time (Anastasi 1968). Another method, and one that has been used in anthropology (Whiting et al. 1976), is to test for consistency between observers as a form of pre-fieldwork training. Even in the Whitings' study, however, one which exceeds usual standards of anthropological rigor, the concepts are not unambiguously defined and replicability would not be easy (Johnson 1978). At present most anthropological journals do not require measures of interobserver reliability or even the unambiguous definition of variables. This has serious implications for the accuracy of cross-cultural research because reliability based on subsequent replication cannot be established in societies undergoing rapid secular change.

THE PROBLEM OF DEFINITION

Cognitive categories differ from one society to another. This variation has for a long time convinced anthropologists of the value of cultural relativism, which requires that concepts derived from our own culture (e.g., work, leisure, politics, sport, etc.) not be imposed upon the interpretation of what happens in other societies. The scientific method does not provide an entirely satisfactory solution to this objection, but pure cultural relativism as regards the description and measurement of human activity precludes any comparative cross-cultural work and can only invite the scepticism of scientists in other disciplines.

In this section we introduce a method of defining behavior which is adequate to a variety of purposes for which ethnographic description is made. In our examples, we concentrate on problems of definition involved in the categorization of activities in relation to productive and reproductive ends (e.g., agriculture, horticulture, husbandry, household maintenance, food preparation, hygiene, and child care). This is because recent quantitative observational research has centered on these aspects of social life. The conceptual points we make are, however, generalizable to other specializations within anthropology in which scan sampling has been used (e.g., socialization and nutritional studies). Lastly, we have found that defining and recording behavior in the way we propose is relatively easy, and we introduce the skeleton checksheet of figure 1 as a summary of the ideas presented here.

Ambiguities in Definitional Categories

Ethologists have at least two methods of describing behavior. Physical description refers to patterns of limb or body movement such as crying, suckling, or lying; ultimately these are descriptions of short-term muscular changes (Hinde 1982). Descriptions by consequence define patterns of behavior in terms of their specific outcomes, such as garden labor, child rearing, or cooking; where a variety of antecedents lead to a common end, description by consequence proves particularly convenient (Bateson 1968). Difficulties arise in the latter method of description when there is uncertainty as to whether a behavior is actually leading to an outcome; for example, in predator studies it is often difficult to determine what behavior patterns actually constitute hunting.

In the study of human behavior, perhaps because we study a conspecific with whom we can communicate and readily empathize, we tend to classify according to some higher-order criterion, particularly the purpose of the activity. Imputed purpose is more open to misinterpretation on the part of the observer and misinformation on the part of the subject than is physical description or a rigorous use of description by consequence. Two examples of the ambiguity between the description of behavior and the attribution of purpose are taken from the Kipsigi context (fig. 2). A woman found chasing her chicken (a description of consequence) may be (*a*) visiting, taking the chicken as a gift; (*b*) engaged in a cash-gaining activity by selling the chicken; (*c*) involved in a stage of food preparation; (*d*) protecting her sleeping infant from disturbance; or (*e*) forestalling the chicken's defecation on the food dishes. Similarly, a girl gathering leaves (a physical description) in the scrub around her mother's house may be planning to use them (*a*) to scour the cooking pots, (*b*) to clean a baby's running nose, or (*c*) to mop up feces from the floor of the hut. These examples highlight the many purposes which can be ascribed to an observed behavior. The extent to which anthropologists use complex or simple descriptions by consequence and the extent to which they correctly impute purpose to their observations must vary enormously from one study to another (see Drummond 1981 for a general discussion), causing serious problems for the comparison of time budgets derived from different time allocation studies.

A particularly pertinent example of the ambiguities involved in introducing a concept which goes beyond the simple description of activity is the variable delineation of what activities constitute "work" (cf. Erasmus 1955). Depending on which of the current anthropological definitions of work is employed, Machiguenga males spend less than 2½ or more than 8 hours a day in essential subsistence activities and females less than 1 or more than 9 hours a day (Johnson 1975). Some studies of time allocation fail to define key categories such as work (Lee 1969). Others use explicit but divergent definitions: for example, Minge-Klevana (1978) defines cow-herding as leisure for Swiss children (they enjoy it), while Smith Obler (1977) defines the same activity as work for Nandi children (if the children did not look after the cows someone else would have to). Similar problems concerning the definition of time spent in political decision-making or ritual have yet to be addressed (Carlstein 1980).

The problem of classifying behavior according to concepts which are not equivalent to indigenous cognitive categories is accentuated by the vast diversity of societies that anthropologists study. It is particularly interesting that the allocation of time to labor in postindustrial societies exhibits far less variance between studies than in preindustrial societies (Minge-Klevana 1980). This may be a real phenomenon, but two other

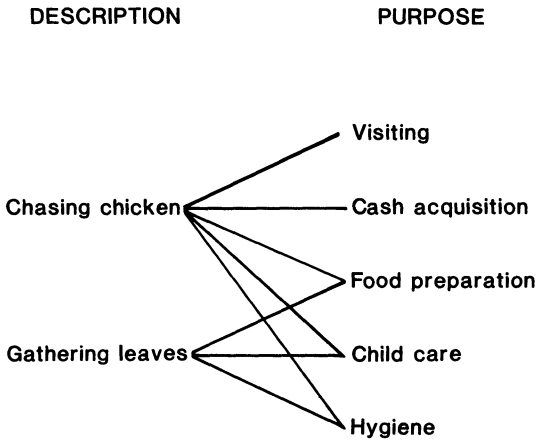

FIG. 2. Examples of purposes that can be ascribed to two observations.

possibilities are raised. Observers, almost exclusively from postindustrial societies, are most consistent with each other in the definition of work in post- as opposed to preindustrial societies. Alternatively, observers engage in extreme cultural relativism in classifying the activities of their subjects. Both of these effects may be occurring. We should therefore recognize that existing time allocation studies are most validly compared within cultural areas or subsistence types (see Gross et al. 1979 for an elegant demonstration).

A Binary Code

Problems in defining behavior in such a way as to permit cross-cultural comparison without losing cultural meaning suggest the importance of using a binary, two-tiered code for recording and classifying behavior. Both physical description and a more interpretive description by consequence can be made for each observation.

Physical description. A small number of descriptive terms can be used to categorize the physical aspects of all human activity. Categories such as "lie," "walk," and "carry" (fig. 1, *8*) allow anthropologists to compare activities across cultures, albeit at a relatively nonspecific level of interpretation. Physical descriptive categories can be made more specific according to the question addressed in the study: for example, estimated weights carried may be of importance in studies of subsistence effort.

Description by consequence. Description by consequence (fig. 1, *9*) entails varying amounts of interpretation on the part of the observer as to the outcome of an activity. Such description is subject to bias derived from the observer's understanding of the culture, linguistic ability, social skills, and rapport. Ethologists usually spend time at the beginning of their studies making long-hand notes of the animals' behavior (Hinde 1973), and detailed description can also be useful to the anthropologist (Johnson 1975). Recording subjects' explanations of their various activities is, moreover, socially acceptable for a newcomer to the community and is linguistically instructive. In the Kipsigi study, all of the categories of description by consequence were based on indigenous terms; for example, some categories used under the heading of primary food production were *sore bandek,* "go and get maize from the store"; *mowe bandek,* "dry maize (in the sun)"; and *wendi tinga,* "go to the flour mill."

A binary method of recording behavior at each spot observation has a number of advantages:

1. Classifying behavior by its independent physical and consequential attributes ensures that the measurement of a specific behavior pattern is not constrained by a preconceived notion of its function. In particular, decisions regarding what activities constitute work or what activities constitute leisure need not be made prior to data collection.

2. A dual code allows examination of the different components of work patterns both within and between societies. In the past, studies have rarely specified walking as a discrete activity but have subsumed the activity of walking under the purpose for which the subject claims to be travelling. Individuals both within and between cultures can be compared with respect to the amount of time they must put into reaching their place of work. Similarly, if a woman standing in a half-harvested field of millet is scored as "stand/harvest millet," the extent to which activities such as harvesting are broken up by periods of rest is now open to comparison. Again, the components of work for a particular job may differ by age and sex within a culture. In the Kipsigi case, it was found that although adult males were often found sitting watching cows it was children who drove stray animals back to the herd (table 3; $\chi^2 = 14.8$, df $= 1$, $p < 0.001$, two-tailed).

3. A binary recording system also addresses an obvious problem of time allocation studies, that of work density (Erasmus 1980) or differences in tempo and intensity of work patterns. Kipsigi children were often found in the vegetable garden with "children's hoes." The mothers rightly stated, as observed by the anthropologist, that the children were helping them cultivate, and as such the children's contributions were recorded. However, when cultivation was broken down by activity, children were found to be less active than their mothers (table 4, $\chi^2 = 10.31$, df $= 1$, $p < 0.01$, two-tailed).

TABLE 3

Husbandry Activities of Different Adult Males and Children

	Number of Individuals Scanned	
	Sitting/Husbandry	Driving/Husbandry
Adult males	34	13
Children	27	50

TABLE 4

Cultivation Activities of Different Mothers and Children

	Number of Individuals Scanned	
	Standing/Cultivation	Laboring/Cultivation
Mothers	11	29
Children	19	8

4. A binary recording system distinguishes between physical activity and responsibility. In agropastoral societies in which crops are inadequately fenced, cattle and goats must be kept under supervision (Smith Obler 1977). A herdsboy might, for example, be found sleeping on a hillside, awakened only by his mother's shouting "Where are the cows?" Given that the cows do not have to be watched continuously, should the boy's activity be recorded as idleness or animal husbandry? Although sleeping, the boy bears a responsibility someone else would have to take if he were not there. By preserving both aspects of activity, one can address questions about both energy budgets and the child's role in the economy.

5. Similarly, a dual recording method can be readily expanded to encompass the problem of synchronous activities. Opportunities for synchronous activities in the human context are vast. A woman may be carrying firewood, nursing her baby, and calling to her neighbor to watch her cows. Such a situation could be scored as "walk/firewood, nurse, husbandry." (If there is too much information to record, the observer need score only certain activities that directly address the problem or, at least, indicate synchronous activities were present (fig. 1, *10*).

6. Placing a behavior in a single category on the basis of either the subject's assertion or the observer's assessment of purpose can be open to inaccuracies resulting from the subject's attempt to impress or mislead the observer. A binary code at least preserves that which the researcher actually observes.

In summary, then, use of descriptive observational categories alone minimizes ambiguity and does not require linguistic proficiency, but it loses enormous amounts of information. Categories defined by consequence are subject to bias derived from the length of the field study and the observer's social skills. Combining these recording techniques ensures less ambiguity and facilitates cross-cultural comparison. Most importantly, it allows different questions to be addressed at the same time. For example, in two hypothetical cultural contexts adult men might devote equal percentages of their time to ritual activity. In the first culture this might involve regular slaugh-

tering of meat for feasts, in the second repeated chanting. The contribution of the men in the first culture would be greater in the material sense than those in the second but would be equal in terms of time. This should have emerged from qualitative ethnographic observations, but a binary recording system allows both differences to be measured quantitatively.

THE USE OF VERBAL INFORMATION WITH OBSERVATIONAL TECHNIQUES

Recently an explicit distinction has been drawn between the conceptual framework used by the indigenous members of a culture (emic) and that used by an outside observer (etic). The former is based on empathy and interviews with the people, while the latter derives from direct observation and historical records (Harris 1969). Gellner (1960) has drawn attention to the overlap and disjunction of emics and etics, but anthropologists have tended to view the two approaches as methodologically exclusive and incompatible, engaging in considerable debate over which approach is the appropriate tool of the anthropologist (Needham 1960).

An exclusive reliance on either observational or interview techniques in anthropology is unlikely to be helpful, judging by the benefits that both nutritionists (e.g., Waxman and Stunkard 1980) and psychologists (e.g., Gottman, Markman, and Notarius 1977) have enjoyed from conbining the two techniques.

Additional Data from Verbal Reports

Verbal reports can shed light on the activity of absent individuals. While reporting by informants is liable to inaccuracies deriving from the concealment of certain activities, from misinformation on the part of the informant, or from a concern to please the anthropologist, the reliability of informant reports can sometimes be verified both by subsequent self-report and by the fortuitous locating of the subject within a specified time limit. Informant reports gain greater credence if they are kept separate from observational data (fig. 1, *3*) and note is taken of the informant's identity (fig. 1, *4*); knowledge acquired later in the study may lead to reassessment of the informant's reliability. It is possible that informants will misreport or report more accurately when other people are present, possibly with a view to manipulating events; thus presence of others can be easily scored (fig. 1, *5*) as well.

Reports can be verified either by locating the subject who is encountered in the reported or other activity within a given time limit (fig. 1, *6*) or, less satisfactorily, by a subsequent self-report from the subject who is encountered in a different activity (fig. 1, *7*).

These checks go some way towards assessing the accuracy of verbal information, but because informant reports give only a general description of activity, never exactly on the point sample, correspondence between the two is likely to be weak.

Combining observations and verbal reports in one measure is a procedure that should be avoided. It should be employed only as a last resort, and even then stringent checks must have been made using the kinds of methods outlined above.

Inconsistencies between Informant Reports and Observed Activities

Discrepancies between what people do and what they say they do have long been recognized in the anthropological literature (Firth 1951, Barth 1966, Cancian 1975). Verification of informant reporting on a subject's activity can provide insights into the personal relationships between different members of the household and also into the organization of family labor. Table 5 shows four aspects of the relationship between subject and informant. In Case 1, the reported activity of a subject is verified within a specified time, indicating considerable communication between informant and subject. Case 2 indicates a high degree of communication as well, but here the reported activity was short-lived. Less communication is suggested in Case 3, where the informant's report proves wrong, and Case 4, where the informant admits ignorance of the subject's activity.

Analysis of a subsample of data from the Kipsigi time allocation study showed that when different mothers were reporting on the activities of adult or adolescent male household members, 76% of all reports fell into Cases 3 and 4 (table 6), whereas 78% of other mothers' reports fell into Cases 1 and 2 when they were reporting on their own children who were members of the household or on adult females. The significant difference in the number of different reports falling into the first two or the second two cases ($\chi^2 = 16.6$, df $= 1$, $p < 0.001$, two-tailed) is open to a number of interpretations, such as that males perform a greater number of activities than do women and children or that mothers try to hide the activities of men. Nevertheless, there is a possibility that relative lack of knowledge of the whereabouts of household males implies that the role of men in maintaining the household economy is rather small or at least not coordinated with that of women.

Inconsistencies between Observational Data and Cultural Norms

Discrepancies between the way people perceive or justify their situation and what actually happens provide some demonstration of the strength of cultural norms and prescriptions (i.e., traditional wisdom) in influencing behavior. For example, it is common wisdom among Kipsigi that women have a large number of children "so that they can help with the work," but in fact it appears that women with many children are busier than those with few children. It is also common for a woman who favors polygynous status to cite the help that she can expect from her co-wife as an important advantage of polyg-

TABLE 5

PERMUTATIONS OF INFORMANT REPORT RELIABILITY

		Subject Later Encountered by Observer, Who		
Case	Informant Report	sees subject in		and is told (self-report)
1	Activity X	Activity X		...
2	Activity X	Activity Y	and	Activity X
3	Activity X	Activity Y	and	Not Activity X
4	Activity ?	...		...

TABLE 6

INFORMANT RELIABILITY OF DIFFERENT MOTHERS WHO WERE REPORTING EITHER ON ADULT AND ADOLESCENT MALE HOUSEHOLD MEMBERS OR ON CHILD HOUSEHOLD MEMBERS AND OTHER ADULT FEMALES

	CASES FROM TABLE 5	
	1 and 2	3 and 4
Reports on males	10	31
Reports on women and children	21	6

yny, whereas it appears that co-wife cooperation is not strikingly high, with women choosing, in preference, their mothers-in-law and sisters-in-law as work partners. Interestingly, it is the men more than the women who cite the importance of co-wife cooperation in polygynous households, suggesting that men could be manipulating cultural prescriptions to their own reproductive or productive advantage.

A combination of sophisticated interview techniques with intensive observational techniques could amplify many of the inconsistencies between cultural prescriptions and actual behavior, with the aim of demonstrating to whose advantage the cultural prescriptions operate (see Cohen 1974).

THE VALUE OF AN OBSERVER UNABLE TO COMMUNICATE WITH SUBJECTS

There are a few instances in which an observer unable to speak the language is at an advantage over the bilingual observer. In the Kipsigi study, focal sampling with certain subjects was impossible because the subject and his or her associates constantly wanted to talk to and teach the observer. However, a focal study on infants' interactions with their mothers and caretakers was successfully performed by an observer who had mastered only the basic greetings. The observer was introduced to a family for a morning or afternoon session and then, to her relief, largely forgotten about.

CONCLUSION

The list of information to be collected within a scan sample (fig. 1) is daunting. Scan sampling is extremely taxing on the observer. During the early stages of the study, when neither the individuals nor the modes of cultural interaction are well known, it is clearly impossible to complete this schedule. Initially, however, only baseline data, descriptions of the physical activity (fig. 1, *8*), its purpose or context (*9*), and informants' reports (*3*) need be recorded. As the observer gradually masters the baseline data and the behavioral categories, which in a large sample could take up to three months, additional codes can be added. Which individual sees the other first (*1* and *2*) and whether there are synchronous activities (*10*) are easy to note. Finally, when all the individuals are easily recognized, the details on informant reports (*4–7*) can be filled in. After a few months of conducting scans, most observers find it relatively easy to make mental notes of the scene on their arrival at a household. After the preliminary greetings and exchange of news, these can be written onto specially designed checksheets with minimal effort.

If cross-cultural comparisons are to be meaningful, sources of bias need to be removed from the data. The amount of unambiguous data recorded in a study should be maximized. This is achieved when the physical description method of recording is used. Recording behavior according to its consequence or purpose is more difficult, and explicit discussion as to how dilemmas of categorization are resolved should be included in publication. Nevertheless, if rigorous definitions are made at the start of the study, these sorts of data can also be used quantitatively. Informant reports can be exploited whenever they are required, but details of each report should be noted. Observational and verbal information should not be combined. If the observer can satisfy his or her readers that the reported data are accurate, then this type of information can be used as an adjunct to observational data.

Comments

by JAMES S. CHISHOLM

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I agree with virtually everything Borgerhoff Mulder and Caro say and applaud the observational techniques they describe and advocate. While many of their points have been made before, they present them here in the concrete context of anthropological fieldwork, and they make a unique contribution in discussing ways to avoid or control for observational bias in the field. For example, noting in spot observations whether observer or subject saw the other first seems both easy and potentially useful.

They argue that quantitative observational methods are better than those of what they call the "humanist tradition," and if by "humanist tradition" they mean only the collection of nonquantitative data I agree. They hold, as have others, that simple nominal descriptive categories are an insufficient basis for a science of anthropology. While anthropologists firmly ensconced in some branches of the humanist tradition may remain unpersuaded, feeling that quantified data are "mechanistic and depersonalized," a scientific anthropology based on rigorous measurement is likely to be a more *useful* anthropology, better able to "generate effective knowledge." Research using the scientific methods that Borgerhoff Mulder and Caro advocate has already, for example, resulted in improved health care in many parts of the world. Scientifically minded anthropologists, because their explanations are based on measurement and susceptible to test, may thus, at least in principle, be in a better position to contribute to the welfare of the people who put up with them during their fieldwork. Indeed, many anthropologists have begun using quantitative observational methods precisely because of their deep humanistic motivation and dissatisfaction with the irrelevance of much traditional anthropological emphasis on interpretation alone.

On the other hand, if by "humanist tradition" they mean a concern with verbal data, "personalized" data, and emics, then I think they are being unnecessarily harsh. As Goodenough and others have shown, it is possible to use quantitative methods with culturally relevant data about what people say they think, feel, and value. The emic approach is based on a good deal more than "empathy and interviews with people." Among other things, it is based on the notion that before there can be a science of culture there must exist a basis for making rigorous cross-cultural comparisons. At least some people following the emic research strategy argue that it is only by describing beliefs, feelings, and values in culturally relevant terms that we can ever apply the natural history approach to human cognition. From my own experience I agree that verbal data cannot reliably be collected at the same moment one is making behavioral observations, but there is no reason (other than our own tradition of rigidly separating behavior and ideas) that one cannot conduct rigorously quantified research on people's beliefs and values about observable behavior. Good emics is good science, just as good definition of observable behavior is good science.

by JEAN-PAUL DUMONT
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Borgerhoff Mulder and Caro tell us that "quantitative methods of data collection in anthropology are rapidly gaining widespread acceptance." This quantity of quantity, that of the methods and that of the converts, is impressive indeed. Even I have been known to count people, households, fields, yields, and whatnot. I still fit more readily somewhere else, however, preferring the domain of quality to that of quantity. It would be fine if Borgerhoff Mulder and Caro could have their quantity and I my quality, but things are not that simple.

It is certainly reassuring to me that they "do not wish to belittle the importance of the humanist tradition." It is very nice of them, very polite indeed, even though the quantitative bulk of their paper makes me wonder what use they might have for that tradition. But my anxiety rises again when in the same sentence they pursue their point with the statement "We do believe that controlled quantitative observational techniques generate effective knowledge." Their belief, in itself most "unquantitative," is emphatic since, as they put it, they "do believe." The use of such a stylistic turn, that is, reliance upon the strength of the belief suggests immediately an appeal to faith—oops!—rather than science. And what is it that is believed by our authors? That "controlled quantitative observational techniques generate effective knowledge." *Et voilá!* Even though it may be difficult to imagine how techniques generate knowledge or to see through the advertised necessity of "controlled techniques," the words "controlled" and "generate" go a long way toward indicating how anthropologists fabricate what they believe to be inherent in their data. Moreover, in using the adjective "effective," which qualifies positively the knowledge to which it applies, the authors have now placed a claim over the domain of quality. Without wishing "to belittle the importance of the humanist tradition," they leave nonbelievers in quantity with a presumably "ineffective" knowledge and presumably a very unenviable position. But what does "effective knowledge" mean? If it is effective, for what is it so? and for whom? and so on. The answer to these questions remains in effect affective, that is, an ideological pronouncement. The iteration is the thing, the assertion says it all: In quantity we trust. Quantification, controlled and all that, may lead its practititioners to salvation, but of course in this process science deteriorates into its own travesty, namely, scientism.

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If time allocation studies offer any special advantage, it is the ability to differentiate between what an informant thinks is important, what he/she says is important, and what his/her behavior and that of other members of the society indicate is important. Thus, these studies may help differentiate between the ideology and the reality, the sacred and the commonplace. As the authors note, these studies may also help to identify interpersonal, gender, age, and seasonal aspects of behavior and thus show the variance of a trait as well as its modal value. Such studies may help anthropologists to focus upon change and innovation, particularly if yesterday's rare behaviors can be shown to be today's modal ones.

Helpful and obvious as the suggestions made by Borgerhoff Mulder and Caro are, however, they do not lead to quantitative solutions to problems posed in anthropological research, nor do the authors imply that they do. Their real value lies in the effect upon the observer—in bringing clarity to the observation and forcing the anthropologist to be more aware of the screen through which observations are made, coded, and evaluated. The authors have provided guidelines for the use of ethological techniques, but the interpretation and implementation still lie with each researcher.

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That certain (though not all) types of anthropological enquiry could profit from quantitative data collection is indisputable. Borgerhoff Mulder and Caro are performing an important service in emphasizing the issue and in pointing to some ways in which adequate quantification can be achieved. The following nit-picking comments should therefore not be taken as detracting from their achievement:

1. They draw their inspiration principally from animal studies. Yet quantitative observational techniques have been used in psychology, and especially developmental psychology, since the '30s. Their revitalization over the last three decades (partly under the influence of ethology) has included recognition of some of the special problems raised by human subjects.

2. Spot observation, which they define as the recording of data as soon as the subject is first observed, is not to be equated with instantaneous sampling, which usually involves recording at successive points in time.

3. The discussion refers primarily to descriptive techniques useful for the study of activity budgets and related problems. The reader must remember that other techniques, mentioned only briefly, may be valuable in other contexts.

4. The distinction between physical description and description by consequence, basic for ethologists, is an important one, and their combination in a two-tiered code, advocated by Borgerhoff Mulder and Caro, clearly has great potential. However, the example of physical description given here is one that an ethologist would regard as description by consequence: "gathering leaves" refers to the consequence of the behaviour (leaves gathered) and implies that the observer has already made a judgment in rejecting the alternative description "clearing a patch of ground." This suggests that the two-tiered system should be seen as referring to different levels in a hierarchy of goals rather than the two types of description. Furthermore, there is a certain ambiguity in their discussion of description by consequence. At different points in the paper "consequence" concerns "outcome," "goal," and "function." These are not necessarily equivalent, and it is necessary to be clear which criterion is being used in any particular case.

5. The question of independence between successive observations is more difficult than the authors suggest. They argue that "if most bouts of beating maize are under an hour in length, then point samples more than an hour apart can be treated as independent." But if maize beating is limited to Mondays, Wednesdays, and Fridays or to the period of the full moon, this criterion for independence is inadequate.

6. The problem of reliability is inadequately discussed. Interobserver reliability cannot be proven by the consistency of a given observer's data over time and in any case is not the only issue: for instance, the adequacy of the sample must be assessed.

But it must be emphasized that these are all points of detail: the message of this paper is an important one, and it is to be hoped that it will get the attention it deserves.

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Borgerhoff Mulder and Caro's attention to the ambiguity of the data of time allocation studies, which have made rapid advances and become widespread among anthropologists work-

ing in various fields (e.g., Gross 1984), is timely. The problems of bias to which they point and propose solutions have consistently concerned researchers. In particular, the statistical examination of their data on the agropastoral Kipsigi (as shown in tables 1–6) will compel researchers to reconsider the reliability of time allocation data. Nonetheless, a question concerning the "observer effect" illustrated in table 1 arises, despite the authors' deliberate attention to it. Was it possible to confirm the observer's judgment on whether he sighted the subject first or vice versa, and how? The Kipsigi open habitat may have made observer's first sight possible. In contrast, in my experience among the lowland Papuans, who inhabit a wooded environment and subsist on sago exploitation, slash-and-burn horticulture, and hunting (Ohtsuka 1983), without a doubt the subject was almost always able to perceive my presence even before I came within sight. Typically, for example, as I approached a subject's garden by a rough path, he (she) would be listening carefully to the sounds I made to determine who or what was coming. The difference between these settings leads to the following generalization: A subject's response to an approaching observer varies from person to person depending on sex and nature of activity and from population to population depending on environmental conditions and subsistence pattern, although frequent visits may stabilize it to some extent.

Regarding the binary coding of activity, the addition of a physical description to description by consequence is valuable for understanding and reporting what is actually done and how; I wonder, though, whether it is possible for the physical description in scan sampling to represent patterns of limb or body movement proportionately. Because of the different likelihood of discovering a subject in different body movement patterns (e.g., sitting vs. walking) and because of the abovementioned higher probability of first perception (or sight) by the subject, the stationary and less-active patterns may be observed with more frequency than they actually occur. Again, the degree of this inconsistency inevitably varies among populations. Thus comparison of frequency of body movement patterns and the resultant energy expenditure across cultures based on scan sampling data still seems problematic even if the binary code is used.

Reply

by MONIQUE BORGERHOFF MULDER and T. M. CARO
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Our paper has drawn a variety of epithets: "unique" (Chisholm) "important" (Hinde), "helpful" (Hall), "timely" (Ohtsuka), "obvious" (Hall), and "nothing new" (Chisholm). Each of these descriptions is fair: we review the pitfalls uncovered in the field of ethology in order to provide the first detailed, and we hope useful, set of guidelines for anthropologists to take into the field (if they are interested in quantifying what their subjects do). We do, however, disagree with Ohtsuka's remark that biases of observation have "consistently concerned researchers." In a very general sense he is right: anthropologists are always troubled by their own impact on the people with whom they live and work; but in our opinion anthropologists do not pay adequate attention to the systematic effects their presence may have on the material they record.

Anthropology, particularly social anthropology, has been stubbornly resistant to quantification. Dumont's views alone represent the radical response we had anticipated and probably reflect those of many anthropologists. We therefore deal with them first.

Dumont implies that quantity is somehow incompatible, possibly irreconcilable, with quality. This we dispute. Indeed, the sole purpose of writing a paper which addresses problems of quantification is to help anthropologists improve the *quality* of their quantitative work. We do not dispute that the diverse and sometimes esoteric subject matter of anthropology raises all kinds of difficulties in recording, but these are not insurmountable, and the more common problems are dealt with in the paper. To label quantification "scientism" is not novel, but it is unfair. Numbers can of course be nonsensical, but so can words. Quantitative studies are valid only insofar as apt variables are sensibly operationalized. Quantification is not an end but a means to an end, as is recognized by Hall, and good methods constitute "good science," as Chisholm notes. To equate numbers with "scientism" would render scientistic the outstanding quantitative comparative work of the Embers and White and his colleagues (e.g., White, Burton, and Dow 1981).

Dumont does not support his claim that quantitative and qualitative approaches are incompatible but proceeds to accuse us of holding a messianic belief in quantity. This perception of quantitative anthropologists as evangelical is possibly widespread among anthropologists and must be taken seriously.

Belief enters science (even the hardest) at every juncture, and it would be naive to suppose otherwise. Scientists are in the game to prove that their beliefs, or hypotheses, are right. We are constantly making choices about what is the most important question to tackle and how to tackle it; our decisions derive from personal predilections and feelings which ultimately lie outside of the logic of the discipline. Thus Dumont cannot condemn us both for appealing to faith and (albeit in French) for indulging in tautology when we state that we "believe that controlled quantitative observational techniques generate effective knowledge." He is of course on target with his accusation of circularity: it is as yet unproven that the use of quantitative techniques will deepen anthropological understanding of the variability in human behavior, and this is precisely why we preface our statement with "we believe." To accuse us of seeking salvation in numbers is surely unjust.

A rather more serious implication of our apparent faith in numbers is suggested by Dumont and is difficult to interpret. Does he insinuate that it is easy to cheat with numbers? If it is, then this objection must be levelled at all quantitative scientists. It is certainly not apparent to us why quantitative anthropology is more susceptible to fabrication than nonquantitative anthropology. On the contrary, if the methodology is made clear quantitative studies can be replicated and thus falsified.

More substantive is Dumont's questioning of what we mean by "effective knowledge," an issue also raised by Chisholm, and we apologize for our lack of clarity in this context. In the opening section of the paper we specified as a key goal of anthropology the explanation of cultural variability; this, we suggested, is achieved through well-conducted systematic cross-cultural comparison. By "effective knowledge" we therefore mean knowledge which helps attain this goal. Our key goal no doubt differs from that of other anthropologists, and we can only reiterate a fundamental methodological principle, a point also made by Hinde: different goals will necessitate the use of different techniques. As regards our ambiguous use of the term "effective knowledge," we appreciate Chisholm's ingenious interpretation: quantitative studies have indeed proved useful in achieving humanistic ends (effective health and nutritional programs in different parts of the world), and our juxtaposing of quantitative studies against those of the "humanistic tradition" in anthropology was perhaps unfortunate. Indeed, as Chisholm correctly infers, we were of course only referring to the divide between anthropology as one of the humanities and anthropology as a science, and we would not really want to imply that either quantitative or qualitative studies are necessarily more effective in achieving humanitarian ends.

Turning now to the positive, constructive suggestions: Quantitative studies of how people in different cultures spend their time are valuable for several reasons, as identified by the commentators: examining discrepancies between thought and action (Hall), revealing intracultural variability in, for ex-

ample, productivity (Hall), controlling bias (Chisholm, Hall), and providing reliable unbiased estimates of just what is done in different cultures, how, and by whom (Ohtsuka). We thank Ohtsuka for drawing our attention to Gross's (1984) compendious review of the use of the time allocation method, hailed as "a tool to examine a multitude of questions"; it anticipates and corroborates many of our arguments. Quantitative description provides essential data on which hypotheses about the nature of cultural variability can be tested. For example, the extent to which human action is shaped by economic rationale, leisure maximization, or reproductive consequences can be determined by comparing variations in the allocation of time under different social and ecological conditions. Thus deductions drawn from competing theories for human action can be confirmed or falsified, through hypothesis testing. We view such a development as essential if anthropology is to earn greater respect and credibility.

Some fields of anthropology have already developed highly sophisticated methodological techniques, for example, the rigorous emic analyses of the cognitive anthropologists (Chisholm's point). We did not include their work in this review because the biases we identify in observational recording are rather different from those encountered in the study of cognition and have received far less attention: hence our focus on quantitative *observational* techniques. We also omitted any reference to the literature on observational techniques in developmental psychology, as pointed out by Hinde. This omission no doubt reflects the regrettable isolation of developmental psychology from ethology, as Hinde himself has noted (Hinde 1983). In fact, some of the early studies on child development employed careful descriptive observational techniques (e.g., Smith 1933, Swan 1938, Washburn 1932), and anthropology would no doubt benefit from a more integrative relationship with other disciplines.

Discussion of certain points in some detail will, we hope, provide further guidelines for the prospective fieldworker:

1. Ohtsuka rightly queries how we determined whether observer spotted subject first or vice versa. The observer could only positively determine that she (in this case) had first spotted the subject when she emerged from a densely wooded area or from behind a hill. Alternatively, she could only assign "subject spots observer first" when the subject was actually observing her as she came into sight. Only such unambiguous cases were coded. In the hilly and vegetatively varied terrain of the Kipsigi, many spot observations were amenable to coding on this measure. Ohtsuka's lowland Papuan example nicely demonstrates how observational techniques need modification in different, in his case thickly wooded, environments. The Papuan hunters are acutely vigilant, and the anthropologist is generally sighted first. This is unfortunately unavoidable, but we would maintain that cautious coding of sightings can still be used to measure the effect of the observer's presence on the subjects' activities. Where an unambiguous case can be made that the anthropologist sighted the subject first, this should be noted. The resulting sample of "observer sights subject first" will be small, but *some* measure of the magnitude and direction of observer bias can be obtained and then used as a corrective factor. Awareness of differential observer bias in different habitats may subsequently prove useful in evaluating vast discrepancies with the time budgets of (for example) other hunter-horticulturalists living in less wooded habitats.

2. Ohtsuka asks whether physical descriptions actually represent unbiased estimates of total physical exertion on two counts. The answer is no. They are subject to all the biases we enumerated. But Ohtsuka does raise an interesting point we did not consider. He suggests that moving subjects are more easily sighted than motionless subjects, and therefore categories of movement may be overrepresented by spot-observation sampling techniques. It is unlikely that this would seriously bias observations, but prospective fieldworkers should be aware of the problem, especially in forested habitats. The strength of the bias could conceivably be measured by making a number of full-day studies on a few focal individuals: the proportion of all physical activities observed could then be compared with the proportion represented in the spot-observation data on those same individuals (using a Wilcoxon matched-pairs test) to assess the magnitude and direction of the bias.

3. Hinde draws attention to the independence of data points. The issue of independence is complex, and we gave it insufficient attention. Although problems of independence usually arise during analysis, they should be anticipated in the structuring of observation schedules. Hinde points out that if maize beating is a patterned activity, spacing observations more widely than the length of the longest bout of beating is an insufficient precaution against nonindependence of data points. With this we entirely agree. We do in fact anticipate this problem in the paper when we warn against the problems of seasonal and time-of-day biases. If specific activities are clustered according to seasonal, lunar, or weekly cycles, the sampling period must of course include the whole cycle if real activity budgets are to be fairly represented in the sample. The clustering and patterning of specific activities arguably represents a form of nonindependence of data points, although we prefer to view it as a bias (seasonal or other). In brief, intersample intervals should exceed mean bout length, and sample periods must cover the cyclical patterning of the activity under investigation.

4. Hinde is right to point out that we have slightly confused the issue of observer reliability. While interobserver reliability can be assessed by measuring the consistency between observers' records, consistency in one observer's reports over time (cf. Anastasi 1968) measures intraobserver, not interobserver, reliability. Hinde also notes that reliability can be questionable on other grounds. For example, is the sample adequate for answering the question under consideration? Mother-infant relations might adequately be sampled over a period of a few weeks (cf. Munroe and Munroe 1971), whereas agricultural activities will require a year's data, at least in seasonal environments. Again, we anticipated this issue but treated it as a problem of sampling rather than of reliability.

5. Hinde is right to point out that spot observations are not to be equated with instantaneous sampling (cf. Altmann 1974). This paper deals with biases specific to spot observations (the most commonly used method in anthropology), although many of the points are also applicable to the recording of a focal individual's behavior at predetermined time intervals.

6. Hinde suggests an expansion of the two-tiered recording system. He proposes that all behavior can be categorized according to a hierarchy of levels of purpose (cf. Miller, Galanter, and Pribram 1960). We entirely agree. The extent to which behavior can be inclusively categorized will of course depend on the kinds of questions being asked. For example, "Are women more productive than men?" will require a more inclusive categorization of activities than "Do women play a larger role in household production?" It is at this level of data analysis and interpretation that the individual skills and experience of the researcher come into play, as is suggested by Hall. We maintain, however, that at the stage of data collection (the observational techniques with which this paper deals) less inclusive behavioral categories offer most flexibility for later stages of analysis. This is why descriptions by consequence are so useful. Thus while we agree with Hinde that consequence, outcome, goal, and function are by no means equivalent, we used them interchangeably with the express purpose of showing that descriptions by consequence can ultimately be used to address questions concerning the goals and functions of behavior.

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Calendar

1985

July 1–7. 45th International Congress of Americanists, Bogotá, Colombia. Write: 45 Congreso Internacional de Americanistas, Universidad de los Andes, Apartado 4976, Bogotá, Colombia.

July 8–12. 9th International Conference on the Social Sciences and Medicine, Korpilampi Forest Lake Hotel, near Helsinki, Finland. Write: Peter J. M. McEwan, Glengarden, Ballater, Aberdeenshire AB3 5UB, Scotland.

July 12–17. 13th International Congress of Gerontology, New York, N.Y., U.S.A. Theme: Aging—The Universal Human Experience. Write: IAG Congress Secretariat, c/o Gerontological Society of America, 1411 K St. N.W., Suite 300, Washington, D.C. 20005, U.S.A.

July 15–20. International Political Science Association, 13th World Congress, Paris, France. Theme: The Changing State and Its Interaction with National and International Society. Write: IPSA Secretariat, University of Ottawa, Ottawa, Ont. K1N 6N5, Canada.

July 30–August 8. International Council for Traditional Music, 28th Conference, Stockholm/Helsinki/Leningrad and on board ship. Themes: Formation of Musical Traditions and Traditional Music and Dance around the Baltic Sea. Write: Krister Malm, ICTM Conference, Musimuseet, Box 16326, S–103 26 Stockholm, Sweden.

August. European Society for Opinion and Marketing Research and World Association for Public Opinion Research, joint meeting, somewhere in the Federal Republic of Germany.

August 11–17. 12th International Anatomical Congress, London, U.K. Includes symposium on physical anthropology. Write: Bernard Wood, c/o Congress Secretariat, 100 Park Rd., London NW1 4RN, U.K.

August 20–23. Institute for Encyclopedia of Ultimate Reality and Meaning, 3d biennial meeting, Toronto, Ont., Canada. Write: Institute for URAM, Regis College, 15 St. Mary St., Toronto, Ont., Canada M4Y 2R5.

August 26–September 4. International Association of Agricultural Economists, 19th Conference, Malaga, Spain. Theme: Agriculture in a Turbulent World Economy. Write: Bruce Greenshields, Economic Research Service, U.S. Department of Agriculture, Washington, D.C. 20250, U.S.A.

August 29–31. New Zealand Association of Social Anthropologists, 10th Annual Conference, Palmerston North, New Zealand. Theme: Ethnography—History, Theory, Practice. Write: Henry Barnard, Department of Social Anthropology and Maori Studies, Massey University, Palmerston North, New Zealand.

September 11–14. 3d annual multidisciplinary symposium on American Studies in Africa, Gaborone, Botswana. Theme: Africa and America: Mutual Perceptions. Write: R. F. Morton, University of Botswana, Private Bag 0022, Gaborone, Botswana.

September 19–22. Society for the Scientific Study of Sex, 28th Annual Conference, San Diego, Calif., U.S.A. Theme: Sexuality across the Boundaries in Our Lives and Our World. Write: Dwight or Joan Dixon, co-chairs, P.O. Box 9902, San Diego, Calif. 92109, U.S.A.

October 30–November 4. 3d World Congress for Soviet and East European Studies, Washington, D.C. Write: Donald