Some Comments on Formal Analysis of Grammatical and Semantic Systems

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INTRODUCTION

The relationship between the "native's" structuring of reality and the alien analyst's structuring of reality comes up from time to time as a serious methodological question in social science. It arises in a number of forms. It has recently been prominent in discussions of componential analysis and generative grammar but has also been of some interest in more general social theory (for example, Does a clan need to be a named group? Is there class structure in a society without class consciousness—and, of course, the much larger problems of the relationship between stated norms and observed practices).

The attribution of more fundamental "truth" to the native's structuring than to the analyst's structuring often goes unquestioned. The more fundamental truth, then, quite naturally, becomes the criterion of validity for the analyst's work. Consideration of all the ontological and epistemological assumptions underlying this approach would be a very large undertaking; but it may be worthwhile to consider the implications of a few of these assumptions, with particular reference to two somewhat related areas of work in which they seem currently to be especially relevant. To focus the discussion, major attention will be given to specific works by Wallace (1962) on semantic or componential analysis, and Chomsky (1957) on transformational or generative grammar.

The main issue hinges on the distinction between open and closed systems, and the relationship between empirical and formal analysis. Related to this are questions about the appropriateness of formal and empirical criteria of adequacy for structural analysis, about the nature of change and how it relates to structural analysis, and about the connection between logical derivatives and different orders of empirical consequences.

An example, particularly of the last point, may be useful. It can easily be demonstrated that there cannot be a system with all three of the following characteristics:

(a) Sub-groups are hierarchically ordered; if A is higher than B, and B is higher than C, then A is higher than C.
(b) A man marries a woman of a higher-status sub-group.
(c) There is no set of people who cannot marry.

Clearly, if (a) is true, then either (b) or (c) cannot be true, since the highest status group of men and the lowest status group of women could not
marry in accord with (b). But these characteristics are, in fact, described for Kachin marriage (Leach 1954). What happens, simply, is that the rules of the system are not empirically pushed to perfect consistency of application; at the bottom status levels (a) is weakened in that hierarchical distinctions are allowed to blur, while at the top, geographical distance "covers" for hierarchy and men marry non-local women. Thus, the empirical consequence that is derivable from the logical contradiction is that the system cannot operate consistently on the basis of the formulated rules and must involve departures from them at the logical "weak points," top and bottom.

Rather than discuss abstractly the questions raised above, this paper will consider them first in the context of Chomsky's criteria for grammar and then in terms of Wallace's discussion of "cognitive process" as revealed by componential analysis.

FINITE GRAMMARS AND "NATIVE SPEAKERS' INTUITION"

For Chomsky, a grammar is a theory of a language. He deals with a language as a set of sentences and states that in a natural language "... there are infinitely many sentences" (1957:13). The grammar of a language L is "... a device that generates all of the grammatical sequences of L and none of the ungrammatical ones" (1957:13). He goes on to say that "One requirement that a grammar must certainly meet is that it be finite" (1957:18); and later, "Clearly, every grammar will have to meet certain external conditions of adequacy (italics his); e.g., the sentences generated will have to be acceptable to the native speaker" (1957:49–50). Again, after stating that the proper goal of linguistic science should be that of "... developing an evaluation procedure for grammars..." (1957:53), he says "... it is necessary to state precisely (if possible, with operational, behavioral tests) the external criteria of adequacy for grammars" (1957:53). He then mentions two other kinds of criteria which are concerned with internal standards (primarily simplicity) and cross-language generality.

There is no logical difficulty in a finite set of rules governing the production of a non-finite set or sequence of units. Euclidean geometry may be infinite; its rules are a finite, relatively simple set. Arithmetic and geometric series are infinite, and are generated by application of a (presumably) finite set of rules. But is a grammar really finite? Certainly no complete grammar of a natural language has ever really been accomplished or, to my knowledge, seriously attempted. While this may be a function of the unwieldy length that would be required or the relative lack of elegance, it is perhaps not even theoretically true that a grammar of a natural language is finite.

Let us first consider the listing procedures utilized in many "rules," in conjunction with the non-finiteness of vocabulary (whether for an individual or a language community). If it is possible to increase the number of units in the relevant "pool" (i.e. to make new words), then a comprehensive list of units governed by the rule in question is not only practically impossible, but theoretically impossible. Grammatical finiteness would then be preserved only
if there is a finite set of rules governing all potential increases to the relevant pool (like the number system’s “+1”), on the basis of which absolute prediction were possible of the lists in which each possible new unit would be included and the lists from which it would be excluded. If the linguistic “behavior” of any unit added to a language (or idiolect) were subject to “purely” linguistic conditions, such absolute prediction might be theoretically possible (though presumably not feasible). If, however, it is subject to influence from factors which are accidental from the point of view of the language, then absolute prediction depends, even theoretically, not only on the finiteness of the linguistic rules but on the finiteness of rules of the total complex of extralinguistic systems whose influence has now entered consideration.

If it is agreed that no natural system—physical, biological, or cultural—is a closed system, then the demand for a finite set of rules governing the operation of the system—particularly if formulated in absolute terms—must be rejected as inapplicable.

The difference is important. In the search for a finite, generative grammar formulated in absolute terms, Chomsky holds the native speaker as ultimate criterion of grammaticalness. Disagreement among native speakers then must signify different languages; error of judgment is defined out of existence, and, along with it, change. Novelty is retained, but not change. New utterances can, as promised, be generated infinitely, but there is no way to modify the rules themselves. Yet it is clearly the case that the rules governing a language do change, and it may be suggested that they change not randomly but also according to rule. It is perfectly allowable to treat a language as if it were a static and closed system, to construct a system which is modeled on the basis of selected characteristics of a natural language, but precisely and exhaustively bounded to make it an eternal entity, subject to logical criteria of analysis. But it is not then permissible to use real speakers to provide the criteria for correctness and adequacy of analysis. It is, in fact, not possible to do so. Whatever may be the “locus” of the sentences that have not yet occurred but would be grammatical if they did, it may be assumed to be in the same vague terrain which also includes the locus of the rules that are in process of changing; and both loci must in some sense be attributed to these native speakers.

That the intuitive linguistic knowledge of the native speaker is not a simple criterion can readily be demonstrated. Hill (1958) has shown disagreement as to the grammaticalness of a set of sentences used as illustrations by Chomsky (1957); years earlier, Leonard (1938) showed marked disagreements within a sample of educated, native English-speaking judges including linguists, English teachers, writers, and others, on a number of expressions including very common ones such as “It’s me.” If it is objected, as Chomsky seems to do, that linguistic variations, even within a language community, are theoretically beside the point, since they merely indicate that the grammars appropriate to a number of idiolects are not identical, two new problems arise: first, one must somehow account for communication, since people do in fact speak to each other, and the grammaticalness of an utterance is relevant to
such communication; and second, I think it can be shown that even our criterion individual has more than one grammar. He will accept sentences in speech that he will reject if they are written; he will accept in conversation sentences he will reject in a formal speech; he will produce sentences that he himself finds unacceptable if he listens to them; he will make judgments about grammaticalness inconsistently. (As a matter of fact, much of the time he doesn’t speak in “sentences.”) As a methodological problem this variability need not be an insurmountable difficulty, but in terms of linguistic theory it cannot be ignored. Which set of norms is “the grammar” even for an individual? Must not the grammatical rules be conditional on extra-linguistic context?

If a grammar is sought which is not pragmatically conditional, one possible approach is the setting up of two connected grammars, one of maximal range and one of minimal range. The maximal range grammar would presumably be the simpler and more general set of rules governing the body of productions—let us restrict ourselves here to sentences—which might under some (but unspecified) conditions be considered acceptable in the language. The minimal range grammar would add those restrictions necessary for the production of only those sentences which are always formally acceptable. (It is of course not logically necessary that the set of rules be simpler for the maximal range grammar; it may well be the reverse. In any case, the relationship between these two grammars, as well as among the several grammars relevant to differing pragmatic definitions, needs exploration.) This approach, however, while not quite pragmatically conditional, is still not sufficiently freed of the speakers of the language to produce a finite grammar. Limitation to the minimal range grammar at a given time does not itself create the required speaker-free conditions for a finite grammar, but does perhaps provide an explicit basis for the construction of an artificially bound system completely comprehended in a finite set of rules. The criterion for any rule would not be the responses of the native speaker but the rule’s contribution to coherence or elegance (etc.) of the set of rules for the constructed system; and, ultimately, its contribution to the construction of a system which permits manipulations yielding statements of relationship that cannot be worked out by dealing directly with the empirical material (e.g.—“holes” in the phonemic system).

The adequacy of the constructed system as a grammar for a particular language cannot really be evaluated. On the reasonable assumption that no grammar can ever perfectly predict the speakers’ behavior (either in the production of sentences or in judgments about them), evaluative criteria would require greater specification than the seemingly simple requirement that all and only the “grammatical” sentences of the language be generated by the system. Putting aside the problem that this “simple” requirement begs the question of grammaticalness, the problems of selection of test-sentences, test-speakers, and weighting of results must be dealt with. Do we prefer a grammar which produces no errors, but has not the capacity to produce many of the sentences speakers consider perfectly correct, or one which allows the produc-
tion of all correct sentences but also many that are not, or one with some intermediary degree of error and omission? We obviously cannot say, without some higher-order criterion. It is perfectly possible that very different grammars will be found most adequate for the criteria we would want to use in the differing contexts of teaching English to foreign students, decoding electronically recorded messages, writing poetry, assisting in psychiatric diagnosis, and in mechanical translation, for example. Within the last-mentioned, the grammars most adequate to translation from Hebrew to English, French to English, and English to French may again differ. The point here is not that a grammar must be "useful," but only that if its "adequacy" is to be evaluated, it must be evaluated with respect to something more exact, less varied, than a speaker's intuitive knowledge.

To put the matter in different terms, if there is no clear single external criterion of adequacy—and I believe there is not—then the choice among possible grammars must remain a function of the interests of the investigators (and those they communicate with). It would seem that there may be partial exceptions to this: (1) in the case of choice between two grammars which formally handle a selected body of material equally well, but only one of which also handles an additional body of material, the more inclusive grammar seems clearly preferable, and (2) in the case of choice between two grammars which formally handle the same selected body of material, one more simply than the other, the simpler grammar seems clearly preferable. More generally, then, choice between grammars which differ only (or primarily) along a single dimension can be made. This is primarily a formal criterion, even though it involves the handling of a body of empirical material. At this point, however, a distinction should be made among formal models. We are not dealing with purely formal models like mathematics. The elements of the models of interest here derive from empirical concerns: thus the choice of units, their definition, the operations and relations utilized, are all initially influenced by the empirical interest that elicits the model. Nevertheless, the criteria of evaluation for the model must be formal; external criteria will necessarily be arbitrary.

To summarize briefly before turning to componential analysis, it has been argued here that finite formal grammars must be evaluated fundamentally in formal terms, that the rules involved in linguistic behavior are not a finite set because natural languages are not closed systems, and that the speakers of the language are not an appropriate source of evaluative criteria for a formal, finite grammar.

COMPONENTIAL ANALYSIS AND "PSYCHOLOGICAL REALITY"

Similar considerations apply to componential analysis of kinship. Let us first look at several statements from the introductory section of Wallace's article on "Culture and Cognition" (1962:351).

One of the products of modern studies in ethnographic method has been an increasing awareness that the research operations of the ethnographer produce primarily not naturalistic
or statistical descriptions of regularities in overt behavior but descriptions of the rules which
the actors are presumably employing, or attempting to employ, in the execution and mutual
organization of this behavior. A second product of these methodological studies is the rec-
ognition that a set of such related rules forms a calculus which describes cognitive process....
What he (the ethnographer) does, ... is to infer the system of rules which these people are
attempting to apply. The assurance that he is on the way to an adequate understanding of
these rules will be given him by the logical completeness of the system he infers and by his ability,
when using it, to produce behavior which an expert will reward by saying, in effect, “That’s
right; that’s good; now you’ve got it.” Sometimes, of course, a sociologist or a psychologist
will say to him, “But it is the behavior that is real, not the abstract system which no one
actually applies perfectly and completely and which is merely the asymptote of the real
curve of behaviors.” To this the investigator simply replies that culture—conceived in this
sense as a collection of formal calculi—is just as real as algebra, Euclidean geometry, and
set theory, which are also “merely” the asymptotes of the “real” behavior of fallible stu-
dents, professional mathematicians, and machines (Italics mine).

Wallace, like Chomsky, thus sets forth for the evaluation of the analyst’s
system of rules both formal criteria—“the logical completeness of the system
he infers”—and external or empirical criteria—“his ability ... to produce
behavior which an expert will reward ...” (the expert, of course, being native
to the cultural group studied). (There is, incidentally, confusion here of the
kind of knowledge involved in familiarity and skill with the kind of knowledge
involved in analysis. An entomologist is, after all, not expected to carry honey
around in his belly.) Wallace further makes explicit the relationship between
the cultural “native” and the desired product of analysis: the latter is “the
system of rules which these people are attempting to apply” and it “describes
cognitive process”.

One further quotation is necessary:

The commitment to describe the psychological reality of culture requires that not just
any model which predicts some overt class of action be accepted, but only that model which
is used as a system of reckoning by the actor. Not infrequently it can be demonstrated that
two systems of reckoning will yield the same result in overt behavior. For example, there
are several different ways to compute the square root of a number; the task in culture and
cognition would be, not simply to find a way, but to find the way being actively employed
by a person or a group. The technical problem of determining which of two equally predic-
tive models corresponds best to the model actually being used by the subject requires the
introduction of problems of choice which were not a part of the originally predicted behavior
and which precede it in the chain of reckoning (1962:356).

At this point, Wallace and Chomsky do not correspond—Chomsky does not
(at least on the surface) demand that the speaker use the grammarian’s rules,
but only that both ultimately arrive at the same sentence-judgments. Wallace
goes further, and asks that the rules themselves correspond to “psychological
reality.”

The arguments against using concepts like “psychological reality,” in this
way are too well known to need repetition (i.e., one cannot get at it directly,
or know when one “has” it, etc.). In view of this fact, however, and of the fact
that it nevertheless continues to be used, it may be worth dealing with the
values of such a concept, and considering possible ways of retaining those
values without entering the domain of invisible, undefinable criteria. Reference
to psychological reality (or cognitive process) is presumably not meant by
Wallace to exclude overt behavior; it is meant to transcend it, in the sense that no one set of behaviors can be specified which is regularly the critical set, so that some overriding criterion must be introduced. Since we are unable to define the overriding criterion, we name it in a way which does not lend itself to being identified prematurely with concrete behavior. The obvious alternative solution is to define the overriding criterion. This solution is not suggested facetiously, although such a criterion would be exceedingly difficult to define.

Wallace illustrates the method of componential analysis using American-English consanguineal reference terminology. He shows that the 15 important kin types (or terms) may be formally defined along the three dimensions of sex, generation, and lineality. To decide whether this is an "adequate" analysis let us consider several empirical observations that might be made of American-English kin terminology:

(1) Reference terms like "uncle" and "aunt" are used by natives to refer to individuals with no consanguineal relationship to Ego. These individuals include the spouses of parents' siblings (even when they are childless), sometimes parents' siblings of one's own spouse, and for some, but not all Egos, long-term close friends of Ego's parents. Ego readily responds to specific questions about his uncles and aunts by distinguishing parents' friends from the others as "not really related."

(2) Some Egos at some times distinguish named categories according to generation and lineality within the "cousin" category. The terms used include "first cousins" and "second cousins," and sometimes "cousins once removed." Questioning of natives about the definitions of these terms, and the kin to whom they refer, elicits highly varied responses.

(3) When there are large age discrepancies within the same kin category, Ego tends to qualify his use of the reference term for some of them by assertions like, "He's really my uncle but he's my age," or by including the "deviant" relative in the "wrong" kin category such as "cousin."

(4) Some Egos qualify their use of the reference term for specific individuals in terms of their history of interactions—for example, "He's more my brother really than my cousin because my mother brought him up."

(5) Natives give vague and inconsistent responses to questions involving extension of kinship to distant cousins and such persons as first cousins' spouses or mother's brother's wife's sister and her husband. A plausible hypothesis is that extension of kinship terms to such individuals is a function of the family's network of interactions, rather than any combination of the three dimensions cited.

Many other observations could be added to reinforce the conclusion that sex, generation, and lineality are not adequate as dimensions of the American-English kinship terminological reference system. A recent article by Romney and d'Andrade (1964) suggests a fourth dimension, reciprocity, as an improvement in terms of native speakers' (in this case, high school students,) responses to subsets of kin terms.
However, such a conclusion about inadequacy is initially in the wrong domain if the dimensions formally work for the phenomena selected as within the boundaries of the system analysed. The question of *adequacy to the selected system* must be asked in formal terms (e.g. is there a simpler or more elegant or more general solution?) The question of *adequacy to empirically observable usage* can only be asked with reference to some specified kind of utility (e.g. what is the system’s relevance to some sets of kin-behavior other than terms, or to variation in term-usage with variation in situation, etc).

The classic “application” of componential analysis of kinship to ethnographic data (Kroeber 1909) may be paraphrased as follows: (1) assuming the same set of biologically related individuals for all humans; (2) assuming the culturally specific sets of terms of reference and persons to whom they refer as given in ethnographic descriptions; (3) defining a limited set of dimensions on the basis of which these biologically related individuals may be categorized by these terms of reference; and (4) assuming (implicitly) the basic formal criterion of economy of analysis, Kroeber finds that the kinship systems he deals with differ in terms of the particular dimensions used, the number of dimensions used, and the internal consistency—or relative redundancy—of use of those dimensions. The number and internal consistency of dimension-use, according to Kroeber, reflect a bi-polar distribution which corresponds to the older dichotomy between “descriptive” and “classificatory” systems, or to “political” and “kin-based” societies (Morgan 1877). It should be noted that while the particular purpose of Kroeber’s analysis does provide an external criterion of adequacy of the underlying formal analyses—i.e., a way of distinguishing two general types—the external criterion in this case has nothing to do with the behavior (or cognition) of the native informants. They, of course, were the source of the data on kinship terms and their referents, but they were not in any other way relevant to the analysis. If the data were correct and the analyses were formally proper, the native informant’s confession or denial that he uses those dimensions would have no bearing on Kroeber’s results. Morgan’s own use of kinship data may be seen as componential analysis which was then organized within a theory involving premises about covariation of named categories and social distinctions and about differential rates of change in cultural characteristics. One source of difficulty in evaluating some of Morgan’s work is the lack of separation between the formal and the empirical aspects of the theory.

One further kind of comment on componential analysis should be considered here. Burling (1964) has provided the simple arithmetic for the complex issue involving the very large number of componential analyses possible for even a small number of items. Hymes (1964) has argued that independently elicited information on the informants’ principles of “sorting” can eliminate from consideration many logical possibilities. It should be said, first, that componential analysis does not deal with a number of separate items, but with an informant-produced sorting of those items into named sets (such as mo bro-fa
bro+mo si hu+fa si hu). The analyst extracts possible principles for such a sorting. On the whole, the number of possible alternatives is reduced rather than increased when the number of items is larger, since the analyst is not free to sort them. But let us look again at the nature of "cognitive reality." If my informant sorts a series of items in such a way that all the yellow objects (triangular and circular) are in one group, and all the blue objects (triangular and square) are in another, and he tells me he is sorting on the basis of shape, he may, of course, be joking, lying, or stupid, or he may be rationalizing (poorly) a set of behaviors whose abstract logical principles he never learned, does not consciously use, and does not really know. The example above may be too simple to be believable, but anyone who has ever tried to give road map directions to a place he gets to with ease, or explain the construction of plural nouns to a child, or the basis of his recognizing as Elizabethan a particular piece of music he has not heard before will realize that people can often "sort" reliably without having information on their sorting principles. Far worse, they may readily give information on principles they are simply not using. Verplanck (1962) summarizes relevant experimental work on this question, indicating that subjects' statements of the rules they were using and the sortings they actually made, could be experimentally controlled essentially independently of each other. If I want to know how he will talk about his rules for sorting, I need to study that; but if I want to know how he will sort, I had better not study his stated rules.

In the same article already cited, Wallace states with perfect clarity a position which seems to me to contradict the demand for psychological reality. He points out:

Now, just as the ethnographer may invent a taxonomic model which will predict satisfactorily how a speaker will refer to his kinsmen but which does not describe how the speaker reckons kinfolk, so it is possible that two members of the same society may produce similar or complementary behaviors without sharing the same cognitive model (1962:356).

His resolution of the apparent paradox is to ask for a "metacalculus" which deals with the "diverse calculi of particular individuals or subgroups cooperating to maintain stable systems of relationships. . . ." But such a calculus cannot be presumed to have any "psychological reality" for anyone except, perhaps, the ethnographer.

CONCLUSION

It also seems quite clear that cultural systems, in Wallace's sense of sets of rules, have actual indeterminacy, which is not merely a function of incomplete or inadequate analysis. There are, at any time, potential value conflicts (i.e., conflicts between operative principles) which simply have not yet arisen. When they arise, the conditions under which they arise will affect their resolution, and new principles will have been created. Furthermore, the systems of rules will be affected by the partly chance question of which conflicts arise, and in what sequence. A set of rules analyzed systematically has the power of pro-
ducing a number of logical derivatives which are not obvious in less formal descriptions of the same basic data. These cannot form the basis of simple predictions of future behavior, but they can make explicit the nature of the potential conflicts (still in formal terms) and thus the alternatives available within the framework of analysis, assuming sets of posited conditions apply when the conflict arises. In those cases where there is good empirical information on the changing conditions which affect the phenomena that are the source of the data assumed in the formal analyses, and where the logical derivatives of those analyses involve few alternatives, predictions may be quite good. Failing these requirements, they will not be. But the formal analyses provide the only possible basis of dealing systematically with this kind of indeterminacy. To insist that they can not be evaluated in terms of the native's cognitive structure is not, I think, to minimize their importance. On the contrary, their freedom from native intuition is a source of their power.

It may be suggested then that componential analyses, like grammars of a language, are finite and formal, and, thereby, unlike the behaviors utilized in their derivation. They may be evaluated in formal terms. Selection of external criteria of adequacy is necessarily and quite properly arbitrary. The arbitrariness (from the formal point of view) of the external criteria need not, however, be capricious. Formal analyses derive their advantage in large measure from the wide scope of their applicability and from the clarity with which internal characteristics such as consistency are revealed. The most effective use to be made of them, therefore, involves the subjecting of a diversity of conditions to the same essential analysis—as suggested in Chomsky's generality criterion—and the derivation of hypotheses about change of structure from the conflicts within a system or between systems when analyzed jointly.

The questions to which formal and empirical analyses are most appropriately addressed are fundamentally different. "Explanation" requires both to be joined; and the process of joining them raises the most complex and scientifically least well treated methodological questions.

It may be necessary to state here that I am exaggerating the degree of separation possible between formal and empirical study. Any empirical analysis must of necessity utilize formal structures, categories, modes of connection; and formal analyses cannot be entirely empty, even if they only use points, lines, numbers, and equal signs.

But they are distinct, and the blurring of the distinction is not a good thing for the study of human society. It leads to ethnographies with no behavioral data, and to theories with no generality.

Questions about what people say and do, where they live, how many of them there are, what parts of their environment they use and how they use it, all require empirical answers. These are not simple questions to answer, and the structuring the questions require in order to allow for investigation makes some formalization essential. They are, however, quite different questions from those dealing with dimensions of social categorization, or with principles of
maintenance and change of systems, or with structural definition of a code, or
with the formal changes implied by the addition of a fifth member to a four-
member unit.

Social explanation demands that we know both; improper merging of the
two produces neither. It is generally recognized that data pushed and muti-
lated at the altar of a theory is improper as an empirical report; it seems less
well recognized that formal systems must be judged primarily by formal cri-
teria. The crossing of the criteria is proper only as a secondary procedure, and
only partially. That is, for a given purpose these data or this formal system
may or may not be to the point, may be more or less satisfactory. Their valid-
ity must be evaluated in other terms.

NOTES

1 This work was supported in part by funds from Grant M 1541 from the National Institute
of Mental Health.
2 There is no “total vocabulary” for English, for example, since there are many processes
available for making new words.
3 Social ranking, bilingualism, weather conditions, endocrine imbalance, typographical errors,
and socialist revolutions, for example.
4 With the possible exception of the whole universe.
5 It might be added that this is too fascinating an area of study to be thrown away for the
sake of neatness; but this may be a matter of taste.
6 They apply more generally to analyses of color categories, biological taxonomies, etc.—or, to
use Wallace’s elegant phrase, “the calculi of culture” (1962:351)—but it will be simpler to deal
with only the one example which has received most attention to date.
7 Confession and denial are not meant to refer necessarily to responses to direct questions. A
very sophisticated methodology for eliciting information on the things an informant thinks about
while reckoning kinship would provide most interesting ethnographic material but it would remain
irrelevant to Kroeber’s analysis.
8 This, of course, treats “item” as a kind of relative rather than a term, which I believe is a
necessary first step.
9 An article by Dulany (1963) raises some questions about Verplanck’s conclusions, and indi-
cates that Verplanck is probably not correct where there is no ambiguity of classification of the
items being sorted. I suspect that some ambiguity is most commonly present in cultural sortings,
and that this difference in interpretation is therefore not directly relevant.

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