# DISTRIBUTIONAL INSTABILITY AND THE UNITS OF CULTURE[1]

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**The analytical approach is familiar, powerful, and compelling, but not all scientific understanding builds upon discrete elemental units and their combinatorics. The question this essay addresses is whether the analytical approach is appropriate for the study of human culture. Does culture have clearly identifiable, distributionally stable parts sufficient to justify the particulate mode of understanding? Is culture comprised of elemental units, or is it merely convenient to think of it this way? The essay suggests that the quest for natural units of culture is a doomed undertaking. There will be no periodic chart for culture grounded in stable, essential properties, whether at the level of culture traits and complexes or at the cognitive level of ideas and schemata. On the other hand, various methods of data elicitation can produce replicable and superficially discrete results, which gives some hope for the possibility of a methodological particulatism. (Units of culture, cultural boundaries, traits, methodological particulatism)**

Description presupposes comparative categories, just as comparison presupposes adequate descriptions. When anthropologists assess cultural similarities and differences in an overtly comparative study, the relatively few underlying categories and units of analysis become a focus of attention, such that studies of this sort often begin with operational definitions of the traits, institutions, or cultures to be compared. When caught up in the ethnographic mode, descriptive accounts likewise rest on underlying categories and units, but perhaps because these are so numerous and wide ranging, they mostly remain implicit and not subject to conscious reflection or definition. Nonetheless, all anthropological accounts rest on some conceptualization of the units of culture in terms of which descriptions and comparisons are made. Rather than siding with the ethnographic impulse over the ethnologic, or vice versa, this essay reflects on something anthropologists of all persuasions have been struggling with (or, all too often, taking for granted) for at least a century. The issue of cultural partibility—the units of culture—remains an unsolved problem lying at the core of anthropology.

Does culture have parts, and if so, what are they? More specifically, how is culture distributed through space and time? There are two main ways of construing this question of units of culture: 1) human culture is distributed in cultures (whole cultures are the units); and 2) human culture is distributed in trait complexes (trait complexes are the units). With either, the initial impression is that human culture is distributed in neat and tidy packages. Cultures sound like well-bounded entities, as do traits, but I argue that these impressions are false and misleading. Neither cultures nor traits are well-bounded, well-defined units. Rather, they are distributionally unstable, and their identification as units involves arbitrary judgments. In short, Lowie (1936) was correct when he wrote, "There is only one cultural reality that is not artificial, to wit: the culture of all humanity at all periods and in all places"



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(Lowie 1936:305). In proceeding, this article quickly reviews problems with the notion that whole cultures are discrete entities, then concentrates on the trait-complex mode of thinking, because it is more fundamental. My conclusion is that human culture is not really particulate. Thus, while methodological particulatism enables making some headway in the short term, eventually we will need to develop nontypological, nondiscrete modes of describing cultural phenomena.

## THE FUZZINESS OF CULTURES

How many cultures are there? This familiar rephrasing of Galton's question, first asked in 1888, concisely cuts to the heart of the matter, for if cultures are well-bounded entities, then they must at least be enumerable. (Whether cultures in a list can be regarded as independent events, in the sense of probability theory, is a secondary issue; one that has received much more attention. Here the focus is on the preliminary question of whether cultures can, in principle, be listed and enumerated.) There seem to be two general ways of thinking about answering this question. The first is to consider cultures as definable by the contents of socially transmitted traditions. The other way is to consider cultures to be definable by their social-system vehicles of transmission. Below I outline an answer strategy from each of these viewpoints.

### *Definable by Distinctive Contents*

In this framing of the problem, the first step is to draft an initial list of candidate cultures. As the strategy is to winnow out false candidates, any proposed culture whose contents can be specified should be included. The second step would be to devise a checklist of cultural features as well as their possible values and, using this list, to construct an overall cultural similarity index scaled 0 to 1. Constructing such a composite index would be fraught with problems. We would have to: 1) integrate items measured on different scales—nominal, ordinal, and interval; 2) decide whether some items should be weighted more than others; 3) determine a finite list of cultural features to include as items. (This raises the devilish issue of how many traits or trait complexes there are, discussed in the following section.) Presuming the formidable problems of index construction are resolved, we would still have to specify a threshold value which, if met or exceeded, would justify collapsing two candidate cultures into one. What value should this be—.99, .90, .75? A choice must be made, but there is no principled reason for selecting one threshold over another.

Pairwise comparisons among the initial candidate cultures in terms of an overall cultural similarity index would take the form of a matrix. Initially the matrix would be large, but whenever comparisons achieved the threshold value, the matrix would be trimmed by iterative collapsing of pairs[2] until all the similarity values in the matrix remain below whatever threshold was chosen. The number of rows by

columns that is left after this winnowing procedure would serve as the answer to the original question.

*Definable by Social-System Vehicles of Transmission*

The premise in this second framing of the problem is that there are as many cultures as there are social systems. And for social systems to be more than metaphors, they must have detectable boundaries. The issue, then, is to assess "the status of aggregates of persons as social entities" (Campbell 1958). Campbell (1958) proposes using five quantitative indices to identify social entities. Each of these indices measures a different property by which any given aggregation might qualify for the status as a social entity (i.e., a social system). These five indices are: 1) common fate—the degree to which individuals presumed to be in the same social entity are co-present in space and time more among themselves than they are with individuals not in the presumed social entity; 2) similarity—the degree to which individuals, two at a time, resemble one another in a multitude of cultured characteristics (note: this is very like the first viewpoint above, only the units are persons rather than candidate cultures); 3) proximity—the degree to which individuals are in contemporaneous spatial contiguity; 4) reflection or resistance to intrusion of external energy, matter, or diagnostic probes—the relative permeability of the presumed social entity to nonmembers or to the ideas and practices of nonmembers, etc.; and 5) internal diffusion, transfer, communication—the relative rates at which matter, energy, or information passes within the presumed social entity compared to rates between presumed entities.

For each index, persons are the rows and columns in a matrix, and social "entitativity" (Campbell 1958) is very much a matter of degree. If the values in the matrix form smooth, almost continuous gradients, then there is relatively weak social entitativity. On the other hand, if the values fall into noticeably different ranges, then each block of similar values signals a relatively strong social entity, and the number of such blocks is the number of discerned social entities. Campbell's indices of social entitativity would detect ethnic boundaries even where the groups' lifeways appear very similar to an outsider, such as Nuer and Dinka. If Nuer and Dinka feel they are different (and such a sentiment has behavioral consequences), this should show up at least on the "reflection to intrusion" measures. Hence, there is no need to consider subjectively felt ethnic identities as a separate way of answering how many cultures there are. It is a special case of Campbell's more general approach.

With the Earth's population having just exceeded six billion, this answer strategy might be a bit tedious and time consuming, even with supercomputers doing most of the routine comparisons. In principle, however, it could be done, and the full person-by-person matrix would contain a wealth of information relevant to social entities. Unfortunately, extracting a simple count from the matrix would be less than satisfying. The number of discerned social entities within the full matrix would depend on what is judged to be "noticeably different ranges" and "blocks of similar

values." Different quantitative specifications of these notions would produce different counts, yet there is no a priori basis to choose one threshold over another.

*The Non-Denumerability of Cultures*

Most thinkers who take Galton's and Flower's[3] questions seriously end up confronting the issues of culture sharing[4] and the distributive locus of culture.[5] I have tried to circumvent these familiar problems by clearly separating the notion of content-defined cultures and the notion of social-system vehicles of transmission. Nonetheless, whichever route we take, we come to the same conclusion: cultures are very fuzzy things whose purported existence rests on arbitrary qualitative and quantitative judgments. Under such circumstances, the notion of counting cultures makes about as much sense as arguing over how many colors are present in a finger-painting. Lowie was right; there is only one cultural reality that is not artificial.

## THE FUZZINESS OF CULTURE TRAITS

What is culture composed of? What are its parts? From about 1870 through 1940, most anthropologists thought the best answer to these questions was "culture traits." There was considerable disagreement, however, concerning the criteria by which such traits should be defined and for what purposes.

*German and American Culture Historical Schools*

Both the German and American ethnologists of the era were interested in unraveling historical relationships among nonliterate peoples. The German historical school theorized that there were just a few culture centers (*Kulturkreise*), or places where genuinely distinctive lifeways had originated, and they referred to each center's distinctive cultural developments as a culture complex (*Kulturkomplex*). Once these centers of origin and their identifying culture complexes had been determined, the present distribution of culture around the world was to be explained in terms of varying combinations and overlays of cultural strata through diffusion from the *Kreise*.[6] For example, the Moiety complex in Oceania (one of six strata defined for the area by Graebner) is defined by such diverse elements as yam cultivation, plank boats, gable roofs, fire-saw rather than fire-drill, and heavy war clubs (Lowie 1937:181).

With respect to the actual determination of cultural traits (a necessary initial procedure before plotting distributions), the Germans used only the criterion of form and firmly rejected a trait's psychological associations as relevant to its definition. For example, the definition of "bow" as a culture trait would be specified only with respect to morphological characteristics, and the more detailed the formal definition of the trait, the better, such as distinguishing the self bow, composite bow, and sinew-backed bow. On the other hand, the Germans would not care whether the bow

was used for hunting or warfare, whether its use was limited to adult men or considered a toy for children, whether bows were made by individuals for private use or produced by specialized craftsmen. Thus, if it was found that many highly detailed and logically unrelated traits co-occurred in diverse locations, then the case for historical relations among the locales would be strong, and the region that best exemplified the whole package (the Kulturkomplex) would be identified as the culture center (the Kulturkreis). These principles of ethnological analysis were published by Fritz Graebner (1911), a leading figure of the *Kulturkreislehre*.

As early as 1896, Boas (1966a) had begun publishing views of culture and culture change that distinguished his own historical method from what he called the "comparative method" as practiced by both diffusionists and parallel evolutionists. His review of Graebner crystallized the differences between German and American historical anthropology. A key contrast was Boas's belief that similar results could be reached through different histories or causal sequences (Boas 1965:169, 1966a:273, 280, 1966c:282, 1966d:258, 1938:4). Boas's framework for explaining cultural similarities allowed for convergent evolution[7] as well as historical transmission and parallel evolution. But to make room for convergent forces to produce similar culture traits, Boas emphasized the relevance of the psychological dimensions of a trait (its contextual meaning, purpose, and functions), precisely those aspects that Graebner rejected as irrelevant to historical analysis. In his review of Graebner's book, Boas (1966b:299-300) wrote:

> The theory of convergence claims that similar ways *may* (not *must*) be found. This would be a truism, if there existed only one way of solving the problem. . . . Nobody claims that convergence means an absolute identity of phenomena derived from heterogeneous sources; but we think we have ample proof to show that the most diverse ethnic phenomena, when subject to similar psychical conditions, or when referring to similar activities, will give similar results (not equal results) which we group naturally under the same category when viewed not from an historical standpoint, but from that of psychology, technology or other similar standpoint. . . . The concepts of comparability and homogeneity, as I understand them, have to deal not only with historical relationships, but to a much higher degree with psychological similarity, for only as elements of the mental make-up of society do ideas or actions become potent and determining elements of further development.

Boas's review prompted Lowie (1912) and then Goldenweiser (1913) to enter the fracas, both siding with Boas's view concerning convergent forces in culture history, and hence arguing that purely formal characteristics are insufficient to properly define culture traits. This emphasis on the psychological dimensions of culture traits culminated in Linton's (1936:402-05) distinctions among four aspects of a culture trait: form, function, use, and meaning.

*Culture Traits as Units*

To agree with Graebner and define culture traits by purely formal characteristics results in rather arbitrary definitions of them. For example, why should the self bow be distinguished from the composite and sinew-backed bows? If specificity is the

guiding principle, then note that the English longbow (see Kaiser 1980) and the short bow of the Comanche (Linton 1936) are both self bows. But the English longbow was made of yew and D-shaped in cross-section, with waxed hemp or flax string, whereas the Comanche short bow was made of Osage orangewood taken from the heart of the tree, highly polished, and rectangular in cross-section, with sinew string. Close examination of a collection of English bows (all five of them) and Comanche bows would reveal formal variations at finer levels of detail.

How specific should definitions of traits be? There is no nonarbitrary answer to this question. On purely logical grounds, definitions of culture traits become only more arbitrary and murky when relevant criteria include meaning, use, and function as well as form (i.e., four dimensions of variability create a larger joint-value space). By expanding the number of relevant criteria, however, some Boasians hoped they might actually reduce definitional arbitrariness. If subjective manifestations of a trait are relevant to its definition, then alternative definitions could be evaluated by stipulating that the "natives" recognize the proposed trait as a single entity. But such a procedure overlooks the variable participation of individuals in their culture. Which native or natives? For example,

> the average Comanche certainly thought of the bow as a single entity, a thing which he could use in certain ways. A professional bow-maker, on the other hand, was fully conscious of all the items which went to make up the bow since he had to assemble them into a useful whole. To the average man the bow was a trait, to the specialist a trait-complex. (Linton 1936:399)

There are ways to get around such intracultural variability, using some operationalization of consensus analysis (Romney, Weller, and Batchelder 1986) to identify the typical Comanche's sense of trait entitativity, but one still must deal with what is commonly called cross-cultural variation in the manifestations of a trait. For example,

> actual studies of diffused complexes show that form may persist with only slight modifications in the face of wide differences in other qualities. Thus the Sun Dance, which occurred in the cultures of a whole series of Plains tribes, varied much more in meaning, use, and function than it did in its form. Although there were marked similarities of procedure wherever the dance occurred, it might be given for quite different purposes. (Linton 1936:405)

A critical question is why Linton thinks these similar-but-different ceremonies performed among Plains tribes are merely variations or versions of the same thing, in this case the Sun Dance. To cling to the criterion of native endorsement means that all the tribes would have to agree, more or less, that their various so-called Sun Dance ceremonies are essentially alike. Failing that, it is only selected aspects of the ceremonies' forms that underlie Linton's assessment, but as already discussed, even formal similarities are a matter of judgment and degree. Indeed, if one reads carefully a sample of these early distribution studies, there is a typical four-step progression going something like this:

Step 1: The author names the cultural entity to be studied; e.g., aboriginal maize culture (Wissler 1916), the concept of the guardian spirit in North America (Benedict 1923), bear ceremonialism in the northern hemisphere (Hallowell 1926), the cattle complex in East Africa (Herskovits 1926), or double burial (Gatewood 1986). The act of naming is significant because it is the name that remains constant, thereby sustaining an illusion of stability and entitativity.

Step 2: The author produces an initial definition of the named trait complex, a list of salient features by which instances of the complex will be recognized when encountered.

Step 3: The bulk of the work then consists of discussing and evaluating accounts of behaviors and beliefs from local cultures that seem relevant to the initial definition of the trait complex. Invariably, each local manifestation differs in some ways from the others, and these differences are duly noted.

Step 4: The author concludes by mapping the distribution of the named trait complex, perhaps infers something about the directions and chronologies of diffusion, and waffles about the amazing variety of manifestations that undermine simple definitions of the trait complex, often ending with a revised definition.

From these exercises there is one inescapable conclusion: culture traits are distributionally unstable; i.e., for any such unit of culture, variability is the norm rather than the exception. This is true even for traits that involve largely utilitarian[8] behaviors and practices, such as maize cultivation. Only by rather arbitrary definitional abstraction can the variations in local manifestations be glossed over and the essential similarity affirmed.

There are similar instabilities with respect to the psychological manifestations of culture within the long-term memory of individuals. In one's consciousness, thoughts, images, and feelings neither occur all at once nor randomly intermix. That is, introspection reveals a non-homogeneous but also non-atomistic mental make-up. Conscious experience is partible but not rigidly so; it consists of distinguishable aspects or currents or flows, but these subjective sensations are not reliably distinct from one another. Although one's inner life seems familiar, no one can say for certain whether he or she has thought the same thought or felt the same feeling twice.

Thus, while radically different in method, both distributional studies of overt culture and introspection come to similar conclusions. The units of culture are fluid and complexly congealing, not well-bounded and stable.

### *The Nature of Cultural Content*

In summary, tracing the spatial and temporal distributions of culture traits and trait complexes reveals complexly variable pseudoentities. The important lessons from distribution studies can be summarized under three points.

1. *Culture traits are distributionally unstable.* Traits are clumpings of culture content, not well-bounded entities. They are polythetic in Needham's (1975) sense. They are

n-dimensionally variable, permitting a variation approaching continuous gradation of similarity and difference in their distributions (Gatewood 1978:312).

2. *Culture traits are seldom reliably replicated.*[9] Whereas all cultural phenomena are learned, all learning is fallible. Thus, variability exists not only with respect to the overt expressions of culture, but also with respect to the underlying and internalized knowledge. Further, patterns of similarity among individuals concerning what they have learned do not necessarily identify the knowledge required to replicate their way of life. For example, a random sample of Americans would show that most people are familiar with scissors, but they would be unable to make scissors themselves. Indeed, the knowledge required to actually make scissors is distributed among several specialist groups (miners, metallurgists, tool-makers, etc.). Shared, consensual knowledge is a proper subset of the knowledge required to reproduce the culture.

3. *There is no "atomic level" for culture, no periodic chart of mutually exclusive entities with stable properties from which cultural compounds are formed.* A trait refers to no precise level of cultural stuff. The trait concept functions like an adjustable cookie-cutter, creating artificial boundaries around pliant content. Virtually any clumping of culture, from whole subsistence efforts to decorative elements on a moccasin, can be regarded as a trait. The usefulness of the concept is that it functions as a place-holder in the analyst's thinking, signifying the lowest level of cultural content that the analyst cares to consider at a given time for a given purpose (Gatewood 1978:312; see also Kluckhohn 1953:517-18). Humans routinely create such ad hoc, temporary, task-relevant categorizations; e.g., the phenomenon of "chunking" in short-term memory tasks (see D'Andrade 1995; Kronenfeld 1996). But the myriad information-processing units people generate each day are quite unstable and certainly do not partition culture in the way the periodic chart partitions matter.

## CONCLUSION

If the traits Mendel studied in his garden peas had exhibited the sort of multifaceted variability and instability that characterize culture traits, he never would have proposed his particulate theory of inheritance, and for good reason. The plausibility of proposing that discrete genes were the units of inheritance rested on the existence of clearly distinguishable, countable phenotypic traits. In biology, there are abundant examples of discrete variability in phenotypic traits. By contrast, very few if any culture traits exhibit discrete variability in their expressions. Hence, unlike Mendel, anthropologists have no legitimate basis for theorizing that cultural transmission is intrinsically particulate. Perhaps it is time they think deeply again about the nature of culture while looking to fields other than chemistry, genetics, and linguistics for inspiration. For example, field theory in physics, as Kurt Lewin and others suggested years ago, may be a more relevant theoretical example for social scientists to build upon than the combinatorics approach of chemistry and genetics.

Culture, whatever else it is, rests on patterned flows of activation in humans' neurological substrates. Thus, the findings and models from cognitive neuroscience

would seem a good place to start for those who seriously wish to address the units of culture problem. In the meantime, there is no quarrel with those who in the interest of measurement reliability continue studying culture as if it were particulate. Much headway can be and has been made in this fashion. But such work should be construed as methodological particulatism, not a revelation of the true nature of culture.

## NOTES

1. Paper presented in a session entitled "Themes, Memes, and Other Schemes: What Are the Units of Culture?" at the 27th Annual Meeting of the Society for Cross-Cultural Research, February 3-7, 1999, in Sante Fe, New Mexico.
2. For interval- and ratio-scaled variables, a collapsed pair of candidate cultures might take the average value of the previous two. For nominal variables, the collapsing operation is itself problematic unless the ethnographic information for each culture is in the form of frequency distributions.
3. Professor Flower was present at the reading of Tylor's famous paper in 1888, and like Galton he asked a deep question in the ensuing discussion. Flower wanted to know why Tylor counted the culture of a Micronesian atoll, which had a population numbering only about 400, as an equivalent case (tallied as one case) to the culture of a province in China, which had a population of around 4,000,000 (also tallied as one case).
4. Culture is shared by whom? To what degree? There is everywhere variable participation of individuals in their socially transmitted traditions. Attempts to reconcile the idea that culture is shared with the facts of intracultural variation have a long history. A very incomplete list might include Durkheim's (1933) distinction between collective conscience and collective representations; Linton's (1936) universals, specialties, alternatives, and idiosyncrasies; Wallace's (1961) idea of end-linkage; Roberts's (1964) metaphor of an information economy and D'Andrade's (1981) information pool; Boster's (1985, 1987) and Romney, Weller, and Batchelder's (1986) focus on patterning of similarities; Gatewood's (1983, 1994, 1996) knowing of/knowing about/knowing how gradients and negotiations of ignorance via a common core of collective representations; and so on.
5. This problem emerges as soon as one contemplates how "superorganic" culture resides in and among living people (Spiro 1951). What is the distributional locus of culture? What is the culture-bearing unit (Schwartz 1978)? Societies, villages, neighborhoods, families, individuals, and social networks of interacting individuals have all been proposed as the vehicles of culture (see Barnes's 1971 critique of Murdock). Individuals have the most easily established entitativity, but because groups of individuals are necessary to replicate their distributed culture, social networks (of what scale?) is probably the best answer, even though networks are seldom sharply bounded.
6. This summary of the objectives, principles, and methods of the *Kulturkreislehre* come principally from their critics, such as Kluckhohn (1936) and Lowie (1937).
7. Ehrenreich (1903) appears to have introduced the idea of convergent cultural evolution, but Radin (1933:77) attributes the "thoroughly baneful concept" to Felix von Luschan and G. Thilenius.
8. Several of these early authors suggest that logical or functional relations rooted in survival needs may underlie the coherence and relative stability of utilitarian traits and trait complexes; e.g., Wissler (1916, 1923) and Goldenweiser (1913, 1937). Kroeber (1948) called these kinds of culture patterns "systemic patterns" to distinguish them from whole-culture patterns, the universal pattern, and stylistic patterns. The notion of functional or systemic patterns foreshadowed modern conceptions of "memes," for which some selective process must act to define the units (Wilkins 1998:10).
9. With the advent of mass-produced brand-name goods, quality-control experts have achieved a high degree of reliable replication. For example, despite widely varying water supplies, Coca-Cola tastes almost the same all over the world; i.e., Coca-Cola is amazingly homogeneous with respect to its formal properties. On the other hand, the meaning and use of Coca-Cola have not been constant. Originally

associated with home-remedy health care, it gradually became a purely recreational beverage, and more recently a symbol of cultural imperialism as well as unhealthy lifestyle. Thus, as the apparent homogeneity of brand-name products is relatively recent in human history, as tremendous social effort is required to minimize product variance, as such products still evidence variability in their cultural meanings, functions, and uses, and as such products almost always have similar rival products, even mass-produced brand-name goods confirm the generalization that culture traits are seldom reliably replicated.

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