# IN PURSUIT OF CULTURE

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■ **Abstract** In this brief, autobiographical account, I trace the development of my intellectual and theoretical interests, especially as they relate to culture. How can we account for culture's being learned by individuals and yet apparently shared by members of a community? How do cultures as shared within communities change and evolve? How does what we know about languages, themselves a kind of cultural tradition, contribute to understanding culture and cultural evolution? Are processes of cultural and linguistic evolution analogous to those in the evolution of biological species and, if so, in what ways? How, also, do genetically based behavioral proclivities manifest themselves in social arenas that are structured by language and culture?

The education that has informed much of my work as an anthropologist began in my childhood, when, in the fourth grade, I had a teacher who excited me about ancient Egyptian civilization. Later grades exposed me to ancient Greece and Rome; I was soon fascinated by the Scythian, Thracian, Celtic, and German "barbarians," wondering how I might learn more about them.

My secondary schooling involved six years of Latin, three years of Greek, three years of French, and two years of German, as well as courses in European history. In my junior year, I discovered Old Icelandic (Old Norse) literature, and I decided that was what I wanted for my major subject in college. I opted for Cornell University because I could take Beginning Old Icelandic there as a freshman and not wait until my junior year for it, as at Yale or Harvard. I was more interested in what I could learn about old Scandinavian culture, mythology, and history from the Eddas and sagas than in the literature as literature. At Cornell I continued Latin, Greek, and German and took courses in Middle High German, Old High German, Gothic, and Swedish. I also had a good course in Indo-European historical linguistics.

By the end of my junior year, it was obvious to me that there was no point in going on for a doctorate degree in Scandinavian languages and literature. In the late 1930s there were very few academic positions in that field, and most of them were filled by native Scandinavians. When I talked over this situation with my father, a professor of history at Yale, he asked me if I had thought of anthropology as a possible field of study. I had never heard of it.

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"Anthropology," I asked, "What's that?"

"Well," he said, "as I understand it, you can be interested in almost anything, and it's all right." I have repeated that to students many times since.

So, as a senior at Cornell, I took a course in cultural anthropology from Lauriston Sharp and, on my future wife's recommendation, a course in personality theory from Leonard S. Cottrell, Jr. While I was writing my term paper for the latter course, something became evident to me that has influenced my approach to cultural anthropology ever since.

I saw that psychologists had made considerable progress in developing a theory of instrumental (means-ends) learning; but they had no way to describe the cognitive content of what had been learned. They had no way to get at the contents of the "black box." Social psychologists had a good theory of the interactive process by which people acquired a sense of self; but they had no way of describing the cognitive content of that sense of self. Anthropologists were concerned with describing culture as a product of learning in social interaction. Some of that content informants could explain, but much of it they could not. Its content had to be inferred from observed behavior and from informants' applications of their cultural knowledge to specific situations. But the methods for doing this were poorly developed. How to objectify rigorously the content of what people have subjectively learned struck me as the major challenge to anthropological and behavioral science.

As a beginning graduate student at Yale, I was urged by my advisor, George Peter Murdock, to take a year-long course in phonetics and phonemics from George Trager. Edward Sapir had recently died, and Trager was a visiting lecturer in linguistics. It dawned on me halfway through this course that structural linguists had developed a rigorous method for describing the content of what speakers of a language must learn in order to speak in a manner that fellow speakers find acceptable, and this at the most basic level, namely phonology and morphology. The method was that of systematic contrastive analysis. Linguistics, classed as a humanity, was revealed to me as the most advanced of the behavioral sciences.

Basic to this method was making a transcribed record of speech that was fine-grained enough phonetically to catch all of the phonetic contrasts that made a significant difference to a language's speakers. The categories of sound that made such a significant difference were the language's phonemes. Phonetic differences that did not have contrastive value for speakers were allophonic variants of the same phoneme. An alphabetic system of writing required a symbol (a letter) for each phoneme. Morphology and syntax involved the ordering of phonemes into meaningful forms and of combinations of forms into words and words into phrases, clauses, and sentences. To describe all of this was to make what in current jargon would be called an emic description of a language.

This set me to thinking about social behavior. In every human society people must learn how to conduct themselves in ways that are acceptable to their fellows. People articulate what must be learned as rules of conduct and lists of "dos and don'ts." But much of what they learn remains subjective. They cannot explain to someone else the working principles for which they come to have a feel any more

than they can explain to others the working principles of their language's grammar. They can apply their subjective knowledge to correct people in specific situations, but they cannot explain the underlying understanding by which they make their immediate judgments.

Learning how to behave, it seemed to me, must be much like learning how to speak. For culturally appropriate behavior to be readily learnable, its content had to be reducible to organizational principles analogous to those of a language's grammar. I presumed, therefore, that the methodological strategy of descriptive linguistics should be applicable to getting at those underlying principles. So I proposed as my doctoral dissertation project an exploration into the possibility of formulating a "grammar of social behavior" while doing ethnographic fieldwork.

While at Yale, I had the opportunity to study under Bronislaw Malinowski in 1940–1941 and, on my return to Yale after World War II, under Ralph Linton in 1946–1947. I took courses also from G.P. Murdock, Clellan Ford, John Dollard, and Cornelius Osgood and archaeology courses from Irving Rouse and Wendell Bennett. From November 1941 to December 1945, I served in the Army, where I had the good fortune to work for three years doing attitude and opinion research in the Research Branch of the Army's Information and Education Division, under the sociologists Samuel Stouffer and Leonard S. Cottrell, Jr. There I learned about sampling and questionnaire survey methods, and, most importantly for me, I learned Guttman scaling, which led to my first publication (Goodenough 1944).

My dissertation fieldwork was done in 1947 in Chuuk (formerly Truk) in Micronesia. I was part of a team that went there under the National Research Council program called the Coordinated Investigation of Micronesian Anthropology (CIMA), funded by the Office of Naval Research. Our team, led by Murdock, also included Isidore Dyen, as linguist, and fellow graduate students Thomas Gladwin and Frank LeBar. Under our division of labor, LeBar (1964) worked on the traditional material culture, Gladwin on the life cycle, life histories, and personality and culture (Gladwin & Sarason 1953), while I was assigned social behavior and religion. Murdock took on social organization, but he had me working with him because he was having trouble with the Chuukese language and I was making good progress in it. I learned a great deal about fieldwork from him in the process. After he left, I continued working on social organization as well as my other topics (Goodenough 1951).

In accordance with my linguistic (emic) methodological approach, I found that study of the traditional property system required learning what one needed to know to do a search of title in the system. This required knowing the different kinds of entitlements individuals and corporate groups could have, the transactions that could occur with these entitlements, and the new entitlements that could result from the different possible transactions. It also required knowing what were the rights and duties associated with each of these entitlements. As far as I know, the resulting ethnography stands almost alone as an account of how a property system is culturally constructed and actually works (Goodenough 1951). My emic approach led me also to try to learn what were the choices that the Chuukese saw

available to them in making decisions about marital residence. These choices could be mapped into the standard anthropological (etic) categories, but these categories did not describe their choices. Similar experience in fieldwork in Kiribati and New Britain led me to formulate the need for emic description in doing ethnography and at the same time attend to how the emic formulations could be mapped into the etic concepts needed for comparative, cross-cultural research (Goodenough 1956a). Some years later these considerations led me to examine anthropology's etic concepts in relation to marriage, family, kin groups, and kinship terminology with the object of refining them for comparative purposes (Goodenough 1970a).

Writing my ethnographic account of Chuuk's social organization, I encountered a problem involving the order in which things were to be described. To describe kin groups seemed to require describing property first, but describing property seemed to require describing kin groups first. The problem resolved itself when I saw that both entitlements and kin groups depended on property transactions. Describing transaction first made it possible to treat entitlements and kin groups as emergent forms resulting from previous transactions. An orderly, linear rather than circular account of social organization thus became possible. From this I learned that customs and institutions were not only largely interconnected and to be understood in terms of one another, as Malinowski (1922) demonstrated long ago, but also that the understanding of some was dependent on the understanding of others. Finding the logical starting points for orderly description of interconnected cultural systems was something requiring attention for an emic ethnographic account (Goodenough 1951).

With an eye to my premise about underlying principles specific to a particular culture's ordering of social relationships, I was able to come up with two empirically based examples from my fieldwork in Truk in 1947. One of these examples involved the application of contrastive analysis to the sets of genealogical relationships that were designated by the same kinship terms to arrive at a set of cross-cutting criteria that allowed me to use every kinship term correctly by informants' standards in every relationship to which the term denotatively applied. Having the genealogical connections among all the community's members already recorded, I went through the roster of the community's members and listed for one informant what kinship term, if any, I predicted he would apply to every person in the community. Subsequently, I had him say what kin term he applied as I called the roll of the community's members. He called them just as I predicted, except for one sublineage whose members I had predicted as his kin but who he said were not kin. When I inquired why he classed them as not kin, he confided that they once were his kin, as I had predicted, but because he had been caught in a love affair with one of them, in violation of incest taboos, the community had decreed that henceforth he and that sublineage were no longer kin. The unexpected exceptions to how one thinks things work can be illuminating.

The other example involved the distribution of obligations and taboos in different social relationships. Through the application of Guttman scale analysis I found that distribution of some of these was a function of deference, marking different

degrees of deference. Others were a function of sexual distance. There was, indeed, something akin to a grammatical ordering of the distributions of dos and don'ts in different social identity relationships according to a limited set of variables of which these distributions were functions.

Preliminary accounts of these findings appeared in my published dissertation (Goodenough 1951). Subsequently, I had the opportunity to present them more fully in articles. Presentation of my semantic analysis of Chuukese kinship terms under the title "Componential Analysis and the Study of Meaning" (1956b) led to adoption of the term componential analysis (a term I had borrowed from linguistics) by fellow anthropologists for this method of contrastive analysis of the complementary distribution of words over fields of denotata. In the same issue of *Language*, Floyd Lounsbury published a similar approach to the semantic analysis of kinship terms, also inspired by the linguistic method of contrastive analysis. I subsequently explored applying the method to the analysis of other kinships systems (1964, 1965a, 1967, 1968). Important work along these lines in regard to color categories and the cultural classifications of plants and animals was subsequently conducted by fellow anthropologists (Berlin & Kay 1969, Berlin et al. 1974, Witkowski & Brown 1977, MacLaury 1997). And Lounsbury (1964) went on to adapt Chomsky's (1957) method of transformational grammatical analysis to the semantics of kinship terms as well.

My early analysis of the distributions of dos and don'ts in different social identity relationships subsequently led me to reappraise the concepts of "status" and "role" (1965b) and to use my data from Chuuk (Truk) to suggest a method for systematically describing a cultural "grammar"of social behavior. I was able to illustrate how knowing such a grammar illuminated behaviors that were in violation of it, providing a way to measure the seriousness of breaches and even insight into what could be understood as incidents of what we call poetic justice. I then undertook to make a more systematic application of the method in a follow-up study in Chuuk in 1964–1965, when I collected a large inventory of identity relationships and recorded how one informant applied his understanding of how a large number of different kinds of behaviors should be distributed between different pairs of egos and alters in those relationships. The idea was then to sort the distributions into sets that were functions of the same variable and therefore conformed to a Guttman scale. As I gathered the data, it became evident that the distributions were not confined to different Guttman scales. There clearly were scale-like patterns, but they exhibited complexities that I had not anticipated. Unfortunately for me, there seemed to be no computer program that was designed to do what I wanted, so the data sat until this past year, when Kimball Romney indicated he would like to see what he could do with them.

In 1952, I was invited by Leonard S. Cottrell, Jr., of the Russell Sage Foundation, and Alexander Leighton to prepare a book of guidelines for people engaged in what was then called "community development" in cross-cultural settings. Russell Sage Foundation had already published two books of case studies (Spicer 1952, Paul 1955), and my book was to be a companion to them. The book, with

my wife's valuable help, grew into a discussion of the nature of culture and the social psychological processes involved in cultural and social change, especially when such change was being engineered by outside change agents (Goodenough 1963a). Why on some occasions do people welcome change and on others resist it? Why, when people begin to take charge of their own change processes, do outside agents of change so often find themselves dismayed? Does what we know about the process of individual identity change provide any insight into the process of collective identity change? How can we use ethnography to forecast the ways in which an introduced change will set in motion ramifying effects and what those ramifying effects will be? What relation do programmatic efforts at change have to the cultural revitalization process described by Wallace (1956)? What did I as an anthropologist have to say about such questions? Trying to address such questions was challenging, indeed. The book took eleven years to write instead of the two initially projected.

In the course of writing it, I was able to refine my developing understanding of how to think about culture in the classic anthropological sense that it consists of what humans learn as members of societies, especially in regard to the expectations their fellow members have of them in the context of living and working together. Culture, in this sense, did not consist of patterns of recurring events in a community, though in practice it was often taken to be such and was argued by cultural materialists to be only such (e.g., Harris 1964). Rather, as something learned, culture was like a language, which is not what its speakers say but what they need to know to communicate acceptably with one another, including constructing utterances never made before yet immediately intelligible to others. Learned in the same way culture is learned, language was thus to be seen as one kind of cultural system (Goodenough 1957, 1981a).

This led me to a definition of culture that has guided me ever since, consisting of (*i*) criteria for categorizing phenomena as meaningful stimuli, (*ii*) criteria for deciding what can be, (*iii*) criteria for deciding how one feels about things (preferences and values), (*iv*) criteria for deciding what to do about things, (*v*) criteria for deciding how to go about doing things, and (*vi*) the skills needed to perform acceptably (Goodenough 1963a, pp. 258–59).

Because it consists of what each individual has made out of his or her experiences, what is learned must be located in people's individual minds and bodies. It follows that no two people have exactly the same criteria or exactly the same understanding of what they perceive to be the expectations (the criteria and standards) of those with whom they have dealings. As long as the variation in their individual knowledge and understandings does not interfere with their ability to interact readily with one another, they have the sense that they share their knowledge and understandings and therefore have a common culture. Just as no two people have exactly the same way of speaking what they perceive as their common language, so no two members of a community have exactly the same understanding of what they perceive to be their community's way of doing things, its culture. Thus people see communities as having languages and cultures, but, in fact, these are collections

of individual understandings of what the languages and cultures consist of. As long as these differences do not get in the way of people living and working together (sometimes, indeed, they do), people ignore them and may be unaware of them. We can legitimately speak of a community's language and culture, just as we can legitimately speak of biological species or subspecies (Goodenogh 1981a). The collection of individual understandings comprising a community's culture are analogous to the collection of individual genotypes comprising a breeding group's or deme's gene pool. The cultural variance within a community and the genetic variance within a deme are considerably less than the variance across communities and across demes. Most individuals, moreover, are likely to have knowledge of more than one cultural or subcultural tradition as a result of interactions with different sets of others in his own and other societies (Goodenough 1976).

For any community, then, it is more precise to speak not of its culture but of its cultural makeup: the content of its culture pool and the distribution of that content, subject matter by subject matter, among the community's various subgroups as well as individuals (Goodenough 1981a, p. 112). An ethnographer seeks to develop his own understanding or version of that community's culture. If it enables him to grasp the meaning of events in the way the community's members grasp them, then his account of that culture is a valid one but not the only possible valid one. It is a model of what one needs to know to function acceptably as a member of that community in the same way that a grammar and dictionary of a language aim to provide a model of what one needs to know in order to speak the language acceptably.

This way of understanding culture opens up insights into cultural evolution and cultural change. Individuals' understandings of their society's culture change through time. They are different, moreover, with each new generation of learners. Thus there are constant small mutations in these understandings, each mutation making a small difference (and sometimes a big difference) in the content of a community's culture pool. As new generations come along, learning at first from parents and other seniors and then, very importantly, from peers, new mutations diffuse across the channels of communication and social interaction. In the course of ethnographic description we create portraits of communities' cultures, which are, indeed, informative and useful; but these portraits, like snapshots, do not reveal the ongoing processes that are constantly affecting the contents of these communities' culture pools.

These insights led me to become interested not only in cultural evolution as a process but also in cultural phylogeny and the methodological problem of recognizing cognate cultural traditions (Goodenough 1997a, 1999). It became evident to me that a community's cultural makeup as a whole, though changing through time in response to a number of different processes, was not the basic unit of cultural evolution. Discrete bundles of how to do things, such as build a house or celebrate a marriage, become relatively distinct traditions as they are passed down across generations. These traditions are the main units of cultural evolution and change. Thus the community meeting house, its organization, and protocol in Kiribati (Gilbert Islands) constitute a tradition that was introduced by Samoan

immigrants several centuries ago (Maude 1963). As such it has been integrated into the makeup of Kiribati culture, but it is cognate phylogenetically with the tradition in Samoa relating to its community meeting house, the *fono*. The Kiribati and Samoan versions of what was once a common ancestral tradition have diverged over time, but they remain cognate traditions. Other traditions in Kiribati and Samoa have different evolutionary histories, though they too may be more distantly related phylogenetically.

Traditions are tied to activities. When I was trying to formulate a method for predicting the probable ramifying effects, if any, of an introduced cultural or technological change, activities were the key. I found that Malinowski's (1944) model of the structure of an institution, appropriately elaborated, was perfectly suited to the structure of an activity. By taking inventory of all a community's activities that involved more than one person and seeing how they were structurally interrelated (e.g., involving the same standing groups), it would be possible then to posit any change and game out its structural effects. Retrospective analyses of changes in the availability of traditional outrigger sailing canoes in Kiribati (the Gilbert Islands) (Goodenough 1963a, pp. 337–43; 1963b) and the introduction of outboard motors in Kapingamarangi in Micronesia (Lieber 1994) illustrated the utility of this approach. Looking at a community's cultural organization of activities proved very useful also, in describing the political organization of a stateless society (Chowning & Goodenough 1966). The social organization of that community's many activities followed one or another of a very few designs. Lines of authority were clearly revealed in these designs.

A long-standing concern of anthropologists has been the search for cultural universals. In thinking about what, in practice, we treated as cross-cultural categories for comparative purposes, I saw that these were not, in themselves, specific to any given culture as a part of its emic makeup. In emic terms the categories of one culture were not exactly the same as those of any other. For comparative purposes we map these emic categories into functional types. Thus we map particular emic categories of containers into bowls, jars, etc., or particular emic criteria for residence choices in marriage into patrilocal, matrilocal, etc. Then we say that containers are cultural universals, instead of saying that in all cultures there are things that people use as containers, recognizing that the universal is a functional category rather than an emic cultural one. The common denominator of cultures is thus to be seen as composed of functional categories, such as shelter, food quest, food preparation, socialization of children, treatment of illness, disposal of the dead, religion, and so on (Goodenough 1981b). The *Outline of Cultural Materials* gives a detailed list of categories that are largely of this kind (Murdock et al. 1967).

Seeing religion as a functional category struck me as having important theoretical implications. Definitions of religion have always centered on belief in supernatural or spirit beings. Atheists were presumably without religion, yet the great salvation-promising movement of the twentieth century was atheistic communism. People were converted to it as to other visionary religions. Salvation is, of course, the achievement of an ideal state of being, whether in life or after death,

a transformation of self whether through individual endeavor or through collective effort to transform society. When we stop to look at what the concerns are that people are addressing through prayer, ritual, magic, etc., we find that they have to do with the state of their selves and the selves of others who matter to them, including the state of the groups with which people identify themselves. What is addressed is the maintenance of selves as people wish them to be, the repair of damage to selves (as from pollution and illness) and the enhancement of selves (as with rites of passage and rites to earn merit). These rites may be elaborate or they may be as simple as avoiding stepping on the cracks in the sidewalk or carrying a rabbit's foot. Our folk wisdom recognizes this when we speak of people doing their morning exercises religiously or making a religion of their business. A customary practice that is readily abandoned in favor of another lacks religious value for people. Their selves are not threatened by it. The greater the emotional distress, exhibited by the suggestion that a custom be abandoned, the greater the religious (i.e., self-maintaining) value it has for those who are distressed. The equivalent of children's security blankets are legion. Horace Minor recognized this years ago with his much-cited article "Body Ritual Among the Nacirema" (1956).

Looked at functionally in this way, the ethnographic description of a people's religious life requires examining all of their institutions and customary practices with an eye to how they function religiously, if at all, and for whom. This is no different from what we must do when describing a people's economic life or their political life. The same institution may function economically, politically, and religiously. I followed this approach to describing a people's religious life in my recent book on pre-Christian religious tradition in Chuuk (Goodenough 2002). I was able to do this because of the availability of psychological test materials that provided a profile of the major concerns that were generated by the way people experienced themselves in the framework of Chuuk's social culture (Gladwin & Sarason 1953).

In recent years I have become interested in looking at how genetically programmed behavioral tendencies from our animal heritage are manifested in the complex symbolic world stemming from language and culture in which we humans exist. A surprise to me in this regard was the realization that the bristling response we call moral outrage is the human equivalent of what ethologists refer to as the territorial response (Lorenz 1963; Ardrey 1966, p. 3). Among humans, the rights, privileges, and immunities they have in their various social identity relationships are symbolic territories. Trespass on these territories evokes the bristling umbrage of the animal territorial response (Goodenough 1997b). In this regard, it is evident in the ethnographic record that there is no human society whose culture of interpersonal relationships does not involve the definition of kinds of social identities and kinds of possible inter-identity relationships. These relationships are organized in terms of what are formalized in jural relationships as the complementary conceptual pairs, right versus duty, no right versus privilege, power versus liability, and no power versus immunity (Hohfeld 1919), as was observed years ago by Hoebel (1954). Of these, right and duty are fundamental, the other concepts being derivable from them.

Rights and duties are affected by what among the four universal interactive modes described by Fiske (1991) is appropriate to the context of a social interaction. These modes are communal sharing; authority ranking or priority ranking (the cultural ordering of dominance); equality matching (everyone getting or owing exactly the same); and market pricing (negotiation and contractual arrangement). All but equality matching appear to be present in rudimentary form in chimpanzee interactions (de Waal 1989, 1991, 1994). Although these modes are present in all human societies, the subject matters to which they apply and the relative valuation placed on them vary considerably cross-culturally, again illustrating the complicated ways in which behavioral proclivities already present in higher primates are elaborated and transformed by language and culture. People unconsciously shift from mode to mode in accordance with their cultural grammar of social interaction in the same way they compose grammatical sentences (Goodenough 1997b).

I have also had occasion to explore how other proclivities and cognitive capabilities present in other higher primates required only the addition of language and, with it, the ability to state propositions to produce the ability to formulate beliefs (Goodenough 1990).

In conclusion, I should say that my early interest in languages has remained with me over the years, especially in historical linguistics. The methods for reconstructing the ancestral vocabularies of languages ancestral to genetically related languages, like those in the Indo-European and Austronesian language phyla, produce insights into prehistoric cultures that complement the data from archaeology. This interest has led me to examine problems relating to proto-Indo-European culture (Goodenough 1970b) and Proto-Austronesian origins (Chang & Goodenough 1996). It has also led me to the reconstruction of the proto-languages of two subfamilies in the Oceanic group within the Austronesian phylum (Goodenough 1997c, Bender et al. 2003). This interest is what I expect to continue to pursue in the future.

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## LITERATURE CITED

Ardrey R. 1966. *The Territorial Imperative.* New York: Atheneum

Bender B, Goodenough W, Hsu R, Jackson F, Marck J, et al. 2003. Proto-Micronesian reconstructions. *Ocean. Linguist.* 42:In press

Berlin B, Breedlove D, Raven P. 1974. *Principles of Tzeltal Plant Classification.* New York: Academic

Berlin B, Kay P. 1969. *Basic Color Categories.* Berkeley: Univ. Calif. Press

Chang K-C, Goodenough W. 1996. Archaeology of southeastern coastal China and its bearing on the Austronesian homeland. In *Prehistoric Settlement of the Pacific,* ed. W Goodenough, pp. 36–56. Philadelphia: Am. Philos. Soc.

Chomsky N. 1957. *Syntactic Structures.* Janua Linguarum, Ser. Minor No. 4. The Hague: Mouton

Chowning A, Goodenough W. 1966. Lakalai political organization. *Anthropol. Forum* 1:412–73

de Waal F. 1989. Food sharing and reciprocal

obligations among chimpanzees. *J. Hum. Evol.* 18:433–59

de Waal F. 1991. The chimpanzee's sense of social reciprocity and its relation to the human sense of justice. *Am. Behav. Sci.* 34:335–49

de Waal F. 1994. Sex differences in the formation of coalitions among chimpanzees. In *Primate Evolution*, ed. G Schubert, R Masters, pp. 128–40. Lanham, MD: Univ. Press Am.

Fiske A. 1991. *Structures of Social Life: the Four Elementary Forms of Human Relations* New York: Free Press

Gladwin T, Sarason S. 1953. *Truk: Man in Paradise.* Viking Fund Publ. Anthropol. No. 20. New York: Wenner-Gren Found. Anthropol. Res.

Goodenough W. 1944. A technique for scale analysis. *Educ. Psychol. Meas.* 4:179–90

Goodenough W. 1951. *Property, Kin and Community on Truk*. New Haven, CT: Yale Univ. Publ. Anthropol. No. 46

Goodenough W. 1956a. Residence rules. *Southwest. J. Anthropol.* 12:22–37

Goodenough W. 1956b. Componential analysis and the study of meaning. *Language* 32:195–216

Goodenough W. 1957. Cultural anthropology and linguistics. In *Report of the Seventh Annual Round Table Meeting on Linguistics and Language Study*, ed. P Garvin, pp. 167–73. Washington, DC: Georgetown Univ. Press

Goodenough W. 1963a. *Cooperation in Change: an Anthropological Approach to Community Development*. New York: Russell Sage Found.

Goodenough W. 1963b. Ecological and social change in the Gilbert Islands. *Proc. Ninth Pac. Sci. Congr.: Anthropol. Soc. Sci.* 3:167–69. Bangkok

Goodenough W. 1964. Componential analysis of Könkämä Lapp kinship terminology. In *Explorations in Cultural Anthropology*, ed. W Goodenough, pp. 221–38. New York: McGraw-Hill

Goodenough W. 1965a. Yankee kinship terminology: a problem in componential analysis. In *Formal Semantic Analysis*, ed. E Hammel, pp. 259–87. Spec. issue of *Am. Anthropol.* 67(5, pt. 2)

Goodenough W. 1965b. Rethinking 'status' and 'role': toward a general model of the cultural organization of social relationships. In *The Relevance of Models for Social Anthropology*, ed. M Banton, pp. 1–24. ASA Monogr. 1. London: Tavistock

Goodenough W. 1967. Componential analysis. *Science* 156:1203–98

Goodenough W. 1968. Componential analysis. *Int. Encycl. Soc. Sci.* 3:186–92

Goodenough W. 1970a. *Description and Comparison in Cultural Anthropology*. Chicago: Aldine

Goodenough W. 1970b. The evolution of nomadism and Indo-European origins. In *Indo-European and Indo-Europeans*, ed. G Cardona, H Hoenigswald, A Senn, pp. 253–65. Philadelphia: Univ. Penn. Press

Goodenough W. 1976. Multiculturalism as the normal human experience. *Anthropol. Educ. Q.* 7(4):4–6

Goodenough W. 1981a. *Culture, Language, and Society*. Menlo Park, CA: Cummings. 2nd ed.

Goodenough W. 1981b. Some reflections on the common denominator of cultures. *Soc. Cross-Cult. Res. Newsl.* 8(1):10–18

Goodenough W. 1990. Evolution of the human capacity for beliefs. *Am. Anthropol.* 92:597–612

Goodenough W. 1997a. Phylogenetically related cultural traditions. *Cross-Cult. Res.* 31:16–26

Goodenough W. 1997b. Moral outrage: territoriality in human guise. *Zygon* 32:5–27

Goodenough W. 1997c. Proto-Kimbe: a new analysis. *Ocean. Lingist.* 36:247–311

Goodenough W. 1999. Outline of a framework for a theory of cultural evolution. *Cross-Cult. Res.* 33:84–107

Goodenough W. 2002. *Under Heaven's Brow: Pre-Christian Religious Tradition in Chuuk*. Philadelphia: Am. Philos. Soc.

Harris M. 1964. *The Nature of Cultural Things*. New York: Random House

Hoebel E. 1954. *The Law of Primitive Man*. Cambridge, MA: Harvard Univ. Press

Hohfeld W. 1919. *Fundamental Legal Concepts*. New Haven: Yale Univ. Press

LeBar F. 1964. *The Material Culture of Truk*. New Haven, CT: Yale Univ. Publ. Anthropol. No. 68

Lieber M. 1994. *More Than a Living: Fishing and the Social Order on a Polynesian Atoll*. Boulder, CO: Westview

Lorenz K. 1963. *On Aggression*. New York: Harcourt, Brace, and World

Lounsbury F. 1956. A semantic analysis of the Pawnee kinship usage. *Language* 32:158–94

Lounsbury F. 1964. A formal account of the Crow- and Omaha-type kinship terminologies. In *Explorations in Cultural Anthropology: Essays in Honor of George Peter Murdock*, ed. W Goodenough, pp. 351–93. New York: McGraw-Hill

MacLaury R. 1997. *Color and Cognition in Mesoamerica*. Austin: Univ. Tex. Press

Malinowski B. 1922. *Argonauts of the Western Pacific*. London: Routledge

Malinowski B. 1944. *A Scientific Theory of Culture*. Chapel Hill, NC: Univ. North Carol. Press

Maude H. 1963. *The Evolution of the Gilbertese boti: an Ethnohistorical Interpretation*. Memoir No. 35. Wellington, New Zealand: Polynesian Soc.

Minor H. 1956. Body ritual among the Nacirema. *Am. Anthropol.* 59:503–7

Murdock G, Ford C, Hudson A, Kennedy R, Simmons L, Whiting W. 1967. *Outline of Cultural Materials*. New Haven, CT: Hum. Relat. Area Files. 4th rev. ed.

Paul B, ed. 1955. *Health, Culture, and Community: Case Studies of Public Reactions to Health Programs*. New York: Russell Sage Found.

Spicer E, ed. 1952. *Human Problems in Technological Change: a Casebook*. New York: Russell Sage Found.

Wallace A. 1956. Revitalization movements. *Am. Anthropol.* 58:264–81

Witkowski S, Brown C. 1977. An explanation of color nomenclature universals. *Am. Anthropol.* 79:50–57