viet Central Asia, where a psychological expedition was carried out in the 1930s:

> The non-technical economy (gardening, cotton-raising, animal husbandry) was replaced by more complex economic systems; there was a sharp increase in the communication with the cities; new people appeared in the villages; collective economy with joint planning and with joint organization of production radically changed the previous economic activity; extensive educational and propaganda work intruded on the traditional views which previously had been determined by the simpler life of the village; a large network of schools designed to liquidate illiteracy was introduced to a large portion of the population and, in the course of a few years, the residents of these villages were included in a system of educational institutions, and at the same time were introduced to a kind of theoretical activity which had previously not existed in those areas. . . .
>
> All of these events placed before psychology a fundamental question. Did these changes lead only to changes in the contents of conscious life or did they change the *forms* of consciousness as well? (1971, pp. 266, 267).

Experimental studies comparing traditional, nonliterate villagers with other residents of the same villages who had gone through a brief literacy course and who had participated in the newly formed collective farms found major differences in the way the two groups formed concepts and drew logical conclusions from verbal syllogisms:

> Not the abstract significance of words but concrete-practical ties reproduced from the experience of the subject play a direct role [among the nonliterate villagers]; not abstract thought, but visual-motor recollection determines the course of thinking. All of these facts have nothing in common with the biological features of the people that we have studied. They are a completely social-historical feature of psychological activity—it is only necessary for the social-historical conditions to change in order for these features of cognitive activity to change and disappear (1971, p. 269).

The implications of this social-historical view for developmental psychology have not been systematically explored. However, it suggests certain specific testable hypotheses about the relation between cognition and particular social institutions and activities. In particular, Vygotsky's distinction between simple basic processes and functional systems, which are composed of organized groupings of basic processes for application to particular cognitive problems, may offer the possibility of achieving a productive synthesis in cross-cultural theory and research.

Cognitive Capacities and Mental Tests

It might seem from the presentation thus far that arguments over primitive mentality and the evolution or development of mind have been a main concern of cross-cultural psychological investigations. This has certainly not been the case. Much of the psychological research, as we shall see, has been concerned with testing the generality of specific hypotheses about perception, language, and thought, and has frequently involved carrying overseas, in some adapted form, experimental questions and procedures that were created in the American laboratory or other institutional setting. Psychologists engaged in cross-cultural work have put heavy emphasis on formulating hypotheses that could be evaluated by *quantitative* means. Perhaps for this reason, comparative studies of mental capacities, using standardized tests, have long been popular and relatively numerous.

Research on mental capacities or abilities generally asks the question: Do these people (the group being studied) have less (or more) of a particular capacity than the group at home on which the test was standardized? It has too often been assumed that questions about the difference in some *capacity* ("intelligence" is the capacity most frequently investigated) are the same as questions about the difference in cognitive *process*. But this equation is not valid, as is vividly illustrated in the following passage from Levy-Bruhl, whom we presented as an advocate of irreconcilable differences between primitive and Western mentalities. Levy-Bruhl insisted that the differences in mentality that he described in no way justify the conclusion that primitive man is any less *intelligent* than his Western counterpart.

> Why is it that primitive mentality shows such indifference to, one might almost say such dislike of, the discursive operations of thought, of reasoning, and reflection, when to us they are the natural and almost continuous occupation of the human mind? *It is due neither to incapacity nor inaptitude* [italics added] since those who have drawn our attention to this feature of primitive mentality expressly state that among them are "minds quite as capable of scientific thought as those of Europeans," and we have

> seen that Australian and Melanesian children learn what the missionary teaches them quite as readily as French or English children would do. Neither is it the result of profound intellectual torpor, of enervation and unconquerable weariness, for these same natives who find an insuperable difficulty in the very slightest abstract thought, and who never seem to reason, show themselves on the contrary observant, wise, skillful, clever, even subtle, when an object interests them (1923, pp. 29–30).

This is an important point to keep in mind, one that too often gets lost when observed differences are loosely interpreted in terms of hypothetical psychological entities like intelligence.

The first major comparative study of cognitive capacities was carried out jointly by anthropologists and psychologists among the inhabitants of the Torres Straits (between New Guinea and Australia) just before the turn of the century. W. H. R. Rivers and his colleagues conducted a wide variety of tests of perceptual function, as a result of which they concluded that perceptual *acuity* is not markedly different in the "savage" and the normal European. They explained the perceptual prowess attributed to nonliterate people by anthropologists as a result of the habit of attending to small details. This explanation is interesting for two reasons. First, it did *not* rest upon laboratory evidence. Second, it suggested that laboratory performance might not give a true picture of how well people could use their skills under other circumstances. This latter is an issue that will reverberate repeatedly in later research.

The work of Rivers and his colleagues also opened the question of possible cultural differences in the perception of color and in susceptibility to visual illusions, two problems that became the subject of a great deal of work in later years.

In the years just following World War I, interest in the testing of mental capacities shifted from the measurement of simple sensory functions to measurement of the higher functions; intelligence tests began to dominate the scene.

It is useful to remember that Binet and Simon began their work on the development of tests of mental ability in order to identify children not likely to profit from the kind of education offered in France at the start of this century. In the beginning, the tests were viewed quite pragmatically: Could they accurately predict whether or not a child would succeed in school? But very soon this practical question was mixed up with another, more theoretical question: Did the tests measure a fixed property of each child tested? Did the tests measure *intelligence?* Though Binet protested against those who regarded the test score as a fixed quantity, the principal American translators and users of his test from the outset claimed it was a measure of native ability, and early linked variations in score to "racially determined" mental capacities (Kamin, 1973). Much of the cross-cultural work on mental testing was inspired by the objective of discovering and measuring "racial" differences in intelligence, and to this day such tests are used to support claims of racial superiority of one people over another.

While the usefulness of carefully designed intelligence tests in predicting school performance has been repeatedly demonstrated, they have proved of little value as a technique for evaluating the relation between culture and mental processes. This is in part the result of conceptual difficulties: emphasis on race and the racial determinants of intelligence inevitably leads in the direction of deemphasizing culture and social-environmental determinants of intelligence (see the discussion on race and culture in the early part of this chapter). Similarly, a concern with the measurement of *capacities* (how much verbal ability does A have compared with B) leads away from an examination of *process* (how does A go about solving this problem as compared with B?). In part the difficulties are methodological. Some of the basic problems in cross-cultural application of mental testing were summarized more than thirty years ago by Florence Goodenough, a leader in the field.

> Examination of the literature in this field over the past twenty years shows that approximately two-thirds of all the publications dealing with racial differences in mental traits have been concerned with the measurement of intelligence by means of tests designed for use with American or European whites. . . . Now the fact can hardly be too strongly emphasized that neither intelligence tests nor the so-called tests of personality and character are measuring devices, properly speaking. They are sampling devices.
>
> When, however, we leave the field of direct measurement, and endeavor to classify individuals or races on the basis of some presumably general trait that cannot be measured directly, we are faced with another and much more difficult problem of sampling. Not only must we be sure of the adequacy of our sampling of subjects, but we must also be sure that the test items from which the total trait is to be judged are *representative and valid samples of the ability in question, as it is displayed within the particular cul-*

> *ture with which we are concerned.* The reason that the ordinary intelligence test works as well as it does for American urban populations is simply because the items of which it is composed are fairly representative samples of the kind of intellectual tasks that American city dwellers are likely to be called upon to perform. The principle involved is essentially the same as that employed by the thrifty housewife who takes a handful of beans out of the barrel from which she is to purchase a supply and judges the quality of the total on the basis of this sample. Considered as a sample, the intelligence test, with its variety of short tasks selected from out the infinite number that the individual is likely to be called upon to perform in the course of his daily life, differs from the handful of beans in only one important respect. The handful of beans is taken at random; the items comprising the intelligence test have been carefully selected with a view to their representativeness for the cultural requirements of the group for which the test was designed.
>
> The wise housewife, engaged in a search for a good value in beans, would not make the mistake of judging the quality of one lot on the basis of a sample taken from another lot. She would not, moreover, make the further error of assuming that the standards applied to her judgment of beans are fully valid for the judgment of potatoes. Nevertheless, errors of both these types and particularly of the latter, are all too common in much of the published work on racial differences. A part of the difficulty, as I have indicated before, seems to be due to the unfortunate use of the term "measurement" in this connection. We may *measure* certain kinds of mental performance with an encouraging high degree of accuracy, regardless of the group upon which the measurement is taken. But the inferences to be drawn from such measurements will vary with circumstances (Goodenough, 1936, pp. 5, 6).

In addition to the important issues raised by Goodenough, psychologists critical of assuming that differences in test performance can be interpreted as differences in inherent capacities have pointed to a host of situational and nonintellective factors that are known to influence test performance. For example, LeVine (1970) lists such factors as fear of foreigners or adults who administer the tests, differential familiarity with the test situation or the task itself, or lack of interest. The reader can probably provide his own list of factors that, though they might produce differences in performance, he would not want to attribute to differences in intelligence.

These problems, combined with the fact that the test items have been picked for their success in predicting school performance

rather than their diagnostic value as measures of particular kinds of psychological processes have led to a de-emphasis on the use of IQ tests as devices for studying cultural variations in cognitive processes. The limited usefulness of this line of work and the inherent difficulties in interpretation of IQ test data led us to exclude such research in the remainder of this book.

In the chapters that follow, we turn from grand theory to research. We will present the major lines of evidence currently available on the relation between culture and cognition, devoting each chapter to one particular cognitive area. In the final chapter, we will try to grapple with the problem of whether-and-how the research we have reviewed can illuminate the profound questions about human nature and human thought that lie at the base of all classical and contemporary explorations in this field.

chapter 3 Culture and Language

Any attempt to understand the relation between culture and cognition must consider the question of language at an early stage in the inquiry. Language is both the medium through which we obtain a great deal of our data concerning culture and cognition and, according to some theories, the major determinant of our thought processes.

The first point is obvious: almost all of our data concerning cultural differences in cognitive processes are obtained via verbal reports or other linguistic responses. Each of the examples given in the introduction makes use of linguistic evidence, although the particular nature of the evidence differs from case to case. This condition imposes on the investigator an obligation to disentangle those differences in performance that may be the result solely of linguistic differences from those caused by differences in the cognitive operations under investigation. We will deal with some of these difficulties and how they have been handled when we

discuss the various problem areas that have been the subject of cross cultural research.

The second point requires extensive consideration. It is not only *not* obvious, it is counter to most of our intuitions. To say that language is a cause of the way we perceive or think seems to put the cart before the horse; most of us conceive of language as the vehicle through which we give expression to our perceptions and thoughts and look upon the particular language used for the purpose of expression as an unimportant accident of birth. Nevertheless, it can be argued that just the opposite relation holds true.

Linguistic Relativity: The Whorfian Hypothesis

Benjamin Whorf, an American authority on Indian languages, maintained that language is not a way of expressing or packaging thought but rather is a mold that shapes our thoughts. The world can be perceived and structured in many ways, and the language we learn as children directs the particular way we see and structure it. This view, which for many years was influential in the social sciences, is forcefully stated in the following passage by Whorf:

> It was found that the background linguistic system (in other words, the grammar) of each language is not merely a reproducing instrument for voicing ideas but rather is itself the shaper of ideas, the program and guide for the individual's mental activity, for his analysis of impressions, for his synthesis of his mental stock in trade. . . . We dissect nature along lines laid down by our native languages. The categories and types that we isolate from the world of phenomena we do not find there because they stare every observer in the face; on the contrary, the world is presented in a kaleidoscopic flux of impressions which has to be organized by our minds—and this means largely by the linguistic systems in our minds. We cut nature up, organize it into concepts, and ascribe significances as we do, largely because we are parties to an agreement to organize it in this way—an agreement that holds throughout our speech community and is codified in the patterns of our language. The agreement is, of course, an implicit and unstated one. *BUT ITS TERMS ARE ABSOLUTELY OBLIGATORY*; we cannot talk at all except by subscribing to the organization and classification of data which the agreement decrees. . . . We are thus introduced to a new principle of relativity, which holds that all observers are not led by the same physical evidence to the same picture of the universe, unless their linguistic backgrounds are similar, or can in some way be calibrated (Whorf, 1956, pp. 212–214).

The Whorfian hypothesis of the language–cognition relationship actually contains two propositions, which are best analyzed separately. The first maintains that the world is differently experienced and conceived in different language communities. This proposition has come to be known as *linguistic relativity*. The second proposition goes beyond the simple statement that there are differences in cognition associated with differences in language to claim that language actually *causes* these differences. This doctrine of *linguistic determinism* is essentially a conception of a one-way causal sequence among cognitive processes with language playing the directing role.

This conception clearly transcends the issue of cultural differences in thought, which first intrigued Whorf, and zeroes in on a kernel problem in psychology. The question—Which is primary, language or conceptual thought?—has historically been, and to this day remains, one of the most controversial issues in psychology and one that has involved the world's leading developmental psychologists in theoretical combat. The language–thought problem provides a vivid illustration of how concern with cultural variation inevitably draws the social scientist into consideration of basic developmental processes that are presumed to occur in all human beings in all cultures.

Extreme forms of linguistic relativity and determinism would have serious implications, not only for mankind's study of himself, but for his study of nature as well, because it would close the door to objective knowledge once and for all. If the properties of the environment are known only through the infinitely varying selective and organizing mechanisms of language, what we perceive and experience is in some sense arbitrary. It is not necessarily related to what is "out there" but only to how our particular language community has agreed to *talk about* what is "out there." Our exploration of the universe would be restricted to the features coded by our language, and exchange of knowledge across cultures would be limited, if not impossible.

Perhaps it is fortunate that evidence related to the Whorfian hypothesis indicates that language is a less powerful factor in its

constraints on perception and thought than Whorf believed it to be. It is most convenient to review the evidence in terms of the different aspects of language that Whorf thought might influence cognition. The first is the way in which individual units of meaning slice up the nonlinguistic world (the vocabulary or lexicon of a language). The second is "fashions of speaking," or rules for combining basic units of meaning (the grammar of a language). Whorf also suggested that these aspects of language were related both to other *cultural* characteristics (such as cultural attitudes toward time, toward quantification, and the like) and to *individual* characteristics (the single person's perception and thought).

The cultural phenomena that might be related to language characteristics are most commonly investigated by anthropologists, whereas individual behavior is primarily the province of psychologists. Because our aim in this book is to acquaint the reader with cross-cultural research in psychology, we will be reviewing only the data relating to the level of individual behavior. It is important that the reader keep in mind the fact that any generalizations suggested by this evidence do not necessarily apply to Whorf's insights about the integrated nature of various aspects of *culture*, nor do we mean to depreciate the importance of cultural analysis in its own right.

Our discussion will also be limited to the question of linguistic *relativity*—that is, that the world is differently experienced in different language communities—and will ignore the claim that language causes these differences. We think that propositions about causal relations among language, perception, and thought, such as those asserted by the doctrine of linguistic *determinism*, require study in a developmental perspective. To determine whether language or thought is the prior or more basic cognitive capacity, we would want to investigate how *changes* in either class of operations (linguistic or conceptual) affect the other. The cross-cultural data thus far collected on the Whorfian hypothesis are not of this kind. They are correlational in nature—that is, they show the association of one behavior with another, but they do not show whether either behavior causes or determines the other.

The Lexicon

Whorf's writings, supplemented by much anthropological data, contain numerous examples of how languages differ in the way

their vocabularies segment the perceptual world. A classic illustration is the fact that languages vary widely in the number of color terms they possess and the parts of the color spectrum to which the terms refer. Some early observers of this phenomenon attributed the unfamiliar color categories to conceptual confusion on the part of their informants. When it was discovered that Homeric Greek, was deficient (by our standards) in color names, a debate ensued as to whether the early Greeks were color-blind. And, as we have seen, psychologists such as Werner have drawn conclusions about the "primitive" and "syncretic" level of perception among tribal peoples from an analysis of their color terms.

Here are some additional examples given by Whorf: The Hopi use a single word to name all flying things except birds (airplanes, insects, aviators), whereas our language has a separate word for each of these things. On the other hand, the Eskimo have many different words for snow—flying snow, slushy snow, dry snow—while we get along with one.

What is the significance of such lexical differences? Does the fact that a language does not have separate terms for certain phenomena mean that the users of this language are unable to distinguish these phenomena from others? Are Americans unable to see the differences that Eskimo see in snow? Or, to take an example that seems absurd on the face of it, is the Hopi unable to make a visual distinction between an aviator and an insect?

Certain aspects of language behavior challenge Whorf's thesis that the absence or presence of a lexical distinction can be taken as an indicator of a corresponding perceptual or conceptual distinction. His own linguistic behavior—his ability to translate the Eskimo terms for snow into English phrases—is evidence to the contrary. While it may not be possible to translate one language into another with term-for-term correspondence, while much may be lost in the process, the preservation and expression of at least some part of the original meaning argues against any hard-and-fast identification of word categories with thought categories. Nor is language interchangeability a skill confined to trained linguists; there are bilinguals among the general populace in most language communities. The importation of words from one language into another is a further example of the flexibility of languages in respect to vocabulary and a demonstration that the existing lexicon does not exhaust the discriminations of which the language users are capable. Rivers, in one of the earliest cross cultural studies in

perception (Rivers, 1901) cites the example of Murray Islanders who had no indigenous term for the color blue but borrowed the English term and modified it to resemble the other members of their color vocabulary (*bulubulu*). On the basis of these facts and comparative language studies, the linguist Charles Hockett (1954, p. 122) has concluded that the question of lexical diversity can best be expressed as follows: Languages differ among themselves not so much as to what *can* be said in them but rather as to what it is relatively *easy to say*.

This formulation disposes of sweeping conclusions relating *all* lexical differences to differences in the way people perceive and think about the world, but it does not help us determine whether any *particular* set of distinctions encoded in a language lexicon are apprehended by individuals whose language lacks this set. To test this question requires some means of measuring perceptual and conceptual discriminations independently of language discriminations. If individuals give differential nonlinguistic responses to specifically different stimulus dimensions, we can infer that they are discriminating these dimensions even though they may lack terms in which to express them. An example would be accurate performance by a Zuni Indian in judging whether two colors in the orange-yellow range of the color spectrum are the same or different according to their measurable physical attributes in spite of the fact that his language does not contain separate terms for colors in this range. Since we know also that sometimes under a particular set of circumstances, individuals may not make distinctions they actually are capable of making, a further test would be a training experiment to determine whether individuals can learn to apply different lexical terms to classifications not expressed in their natural language. (See Heider, 1972, for the report of a successful learning experiment of this kind conducted among the Dani, a New Guinean population still living in a stone age culture.)

Most of the studies conducted by psychologists to test the impact of lexical distinctions on cognition have proceeded from the weaker version of the influence of vocabulary differences stated by Hockett (that it is *easier* to say something in one language than in another). Brown and Lenneberg (1954), who carried out one of the first experimental studies in this area, reasoned that the ease with which a distinction is expressed in a language is related to

the frequency with which its referent perceptual discrimination is required in everyday life. For example, Eskimos are constantly required to make judgments about snows, whereas Americans may need to make such judgments only under rare and special circumstances. There should be a relation, then, between the more nameable perceptual categories and their availability for various cognitive operations, or as these authors put it, "The more nameable categories are nearer the top of the cognitive 'deck' " (p. 456).

For their perceptual domain they chose categories in the color space. Besides the classical interest in this domain, color space commended itself for investigation because it has been exhaustively mapped and measured and offers a set of physical dimensions against which varying color terminologies can be matched. The three dimensions of physical variation—hue, brightness, and saturation—are treated in the color space as continuous gradations that can be segmented more or less arbitrarily by language —a seemingly ideal representation of Whorf's general conception of the relation between language and reality.

Brown and Lenneberg chose memory as the cognitive process to relate to the linguistic variable of nameability or *codability*. Part of the way we remember an experience such as a color, they thought, is by remembering a word or name for it. Therefore, those color experiences that can be easily and adequately described in words should be more available in a memory test than others less easily verbalized.

Their first experiment was performed with English-speaking subjects on the assumption that a relation between codability and memory demonstrated within one language should also hold within other languages, and between languages as well. The subjects were presented with 24 color chips one at a time and instructed to name the color as quickly as possible. Several measures of the subjects' responses were found to be systematically related: the longer the name, the longer it took the subject to begin to say it and the less agreement there was among subjects in the terms used to name that particular color. The amount of naming agreement among subjects was selected as the most useful measure of codability.

The relation between codability and memory availability was then studied in a recognition experiment with a new group of subjects. Four of the 24 color chips were presented to a subject for a

5-second inspection period; then the chips were removed and the subject was asked to pick them out from an array of 120 colors. The number of correct identifications made by the subject was expressed as a recognition score. Under these circumstances there was a small correlation between codability (agreement on the name for a color) and recognition. When the memory task was made more difficult by introducing a delay period, filled with distracters, between the presentation of the color chips and their later identification, the correlation was much stronger. When the memory task was simplified by presenting one color for later identification and by having an immediate recognition test, the correlation almost vanished. Under these latter conditions a measure of *visual discriminability* correlated significantly with recognition, emphasizing the close relation between the physical event and memory instead of the relation between language and memory.

The relation between codability and recognition under difficult memory conditions was confirmed in a second study (Lenneberg and Roberts, 1956) conducted among the Zuni Indians of the southwestern United States. The authors hypothesized that the Zuni would have trouble remembering colors in the yellow-orange section of the color spectrum, since their language does not distinguish between these two colors. In this carefully conducted experiment, they found that monolingual Zuni did indeed make the most errors in recognition of these colors followed by subjects who spoke both Zuni and English, with monolingual English-speakers making the fewest errors.

These experiments were widely quoted as evidence for a weak version of linguistic relativity. But further investigation showed that the demonstrated relation between codability and recognition did not hold up for all colors. Burnham and Clark (1955) secured recognition data for another array of colors that did not differ as distinctively in hue as the array in the original study. Lenneberg (1961) took these recognition data and correlated them with codability data he had secured independently for this color array. He found that correlation was a negative one—the better the naming agreement, the lower the recognition score! Evidently, a short distinctive lexical term like *blue* is useful for remembering a color blue when it is surrounded by colors of distinctive hues (red, yellow, green, etc.), but it does not help in the selection of a particular blue from an array of blues of different brightnesses and saturations. Here a phrase—"the cloudy blue with a gray tinge"—may be more useful.

In an attempt to resolve this contradiction, Lantz and Stefflre (1964) developed a new method of measuring codability which they called *communication accuracy*. Viewing memory as a situation in which an individual communicates to himself through time, they argued that items communicated accurately *inter*personally (that is, to another person) would also be more accurately communicated *intra*personally (to oneself). They presented test colors to a group of subjects who were asked to describe them in such a way as to enable others to pick them out of an array. The descriptions were then read to a second group who tried to find the colors from among the recognition array. This procedure yielded very high and statistically significant correlations between communication accuracy and recognition scores for *both* the Brown-Lenneberg and Burnham-Clark color arrays. On the other hand, communication accuracy and naming agreement (the original measure of codability) were not highly correlated.

These results were replicated and extended for non-English-speakers in a study by Stefflre, Vales, and Morely (1966) conducted in Yucatan, Mexico. They worked with two different language groups—Mayan Indians, whose native language, Yucatec, contains relatively few color terms, and students at the University of Yucatan, whose native language, Spanish, has a color vocabulary similar to English.

For each language group a clear correlation was established between communication accuracy for particular colors and the errors that subjects *within that language* made when trying to recognize colors a short time later. The speakers of the two languages found different colors easy to communicate, so that the recognition errors of Yucatec-speakers were not the same as those of Spanish-speakers. Here is clear evidence that errors in recognition are associated with the linguistic, or communication, code more strongly than with the physical attributes of the colors being recognized.

The same general results were obtained by Wang (1972) using the Lantz and Stefflre technique with American college students. Wang first obtained communication accuracy scores for a large set of colors. Then for each color he picked two color names that produced low accuracy; one of the two names biased selection to

one side of the test color, the other to the opposite side. When a new group of subjects was presented the colors using the biasing color names, recognition scores were found to err in the direction predictable from the name.

Lantz and Stefflre explained the superiority of the communication measure in predicting recognition scores by the fact that it allows flexibility in the particular verbal expression (single- or multi-word name, phrase, etc.) used to characterize the target stimulus.

> The kind of formulation presented here of relation between language and behavior emphasizes the productivity of language—*new descriptions may be formed spontaneously* [italics added] and function to encode stimuli effectively. . . . Any description of the relation between language and behavior or language and thought that does not take this into account and emphasizes only the role of dictionary words and/or the grammatical categories will find it difficult to deal with the facts found in a particular experimental context (Lantz and Stefflre, 1964, p. 481).

In addition to their contribution to the language–cognition problem, the Lantz-Stefflre study shows the limitations of any attempt to relate cognitive behavior to *static* characteristics of language without taking into account the dynamic functions that language can serve within various problem-solving situations. Their communication measure points to a whole new set of language variables connected with language *use* that might be expected to influence cognition. If *intra*personal communication is related to *inter*personal communication, then the social processes of communication within various cultures need to be studied: What form do they take and what aspects of experience are most commonly verbalized and communicated? Looking back at the original Brown-Lenneberg study, we note that their hypothesis of the relationship between codability and memory rested on the assumption their subjects actively applied and stored verbal labels for the test colors. This, too, is an instance of language *use* in a particular situation and an additional demonstration of the fact that the activities of the subject are a crucial intervening variable in attempts to test language–cognition relationships.

In the last few years, the linguistic relativity thesis has been challenged even within the color domain. As we have indicated, the color space was long considered a source of uniform, physical

variation, which languages partition arbitrarily into color-name categories. Research conducted by two anthropologists (Berlin and Kay, 1969) suggests that this is not the case. They asked speakers of 20 different languages to choose the best examples of their languages' basic color terms from an array of color chips, and to indicate, in addition, all the chips that could be called by that name. As expected, the boundaries of the color terms varied widely, but the best examples (Berlin and Kay called them the *focal colors*) were stable; instead of being randomly distributed throughout the array they were tightly clustered around 11 basic colors—8 chromatic colors, whose English names are *red, yellow, green, blue, brown, orange, pink,* and *purple*—and 3 achromatic colors, *black, white,* and *gray.* Berlin and Kay argue that the emphasis on cross-cultural differences in linguistic encoding of colors has stemmed from investigators' preoccupation with variable color *boundaries* to the neglect of common *focal color referents.*

In a series of studies, Heider (1972), explored the psychological implications of these reputedly universal focal colors. After refining the location of each of the focal colors in the color space, she tested to see whether these colors were the most codable *across language families.* Subjects spoke languages belonging to the Indo-European, Austronesian, Sino-Tibetan, and Afro-Asiatic families, plus Hungarian, and Japanese. The results were quite clear: focal colors were given shorter names and were named more quickly than nonfocal colors (the two measures of codability used in this study). A third study, modeled after the Brown-Lenneberg experiments demonstrated that focal colors could be remembered more accurately than nonfocal colors *even by speakers of a language that lacks basic hue terms.* The Dani of New Guinea, whose color lexicon is restricted to two basic terms meaning, roughly, dark and light, showed memory superiority for focal colors over nonfocal colors similar to that shown by the comparison group of American subjects whose language has a term for each member of the entire set of focal colors. What does this imply about the role of language in this task? Data from another series of experiments (Heider and Olivier, 1972) has led Heider to conclude that there may be a *visual* rehearsal process in the recognition task which is separable from a *verbal* rehearsal process. Visual memory images may be isomorphic to visual images of colors that are physically present, and thus more responsive to perceptually salient characteristics of

the stimuli and more resistant to language-related distortion. In Brown and Lenneberg's easy memory task, recognition could be accounted for, as we have seen, by the perceptual property of discriminability. Just which memory tasks call out visual rather than verbal memory processes in which populations, what the nature of the interaction may be between these two processes and what kind of "verbal encoding" is employed in a given situation are all important questions which studies of this kind must answer.

It is interesting to observe how a line of research originally inspired by notions of linguistic relativity has now led to the claim that there are certain universals or invariants in the relation between one area of perceptual experience and language lexicons. In spite of the great variety of terms for colors and the unstable boundaries separating one color class from another, certain colors seem to be universally salient and easier to remember. On the strength of this evidence Heider (1972) suggests that the customary understanding of the relationship between language terms and concepts may be the reverse of what it is customarily understood to be. "In short, far from being a domain well suited to the study of the effects of language on thought, the color space would seem to be a prime example of the influence of underlying perceptual-cognitive factors on the formation and reference of linguistic categories" (p. 20).

Grammar

Not only do languages differ with respect to the way in which their vocabularies cut up the world, they also differ with respect to the way in which individual units of meaning get combined. Whorf was especially fascinated by these structural features of language, which he called "fashions of speaking," and he emphasized their importance in molding, unconsciously, the language community's view of reality. He pointed out, for example, that English verbs take different forms in accordance with the temporal distinctions, past, present, and future. These obligatory temporal references fit in with our culture's concept of time as a never-ending line and our preoccupation with its measurement (as witness our calendars and clocks in almost infinite variety). However, Hopi words that function as verbs—including words that we clearly treat as nouns, such as lightning and puff-of-smoke

—emphasize duration rather than time of occurrence. Another example of a structural fashion of speaking is supplied by Lee (1938), who describes verbs in the Wintu (California Indian) language as being classified by "validity modes." If the event being spoken of is a matter of hearsay, one word is used; if it is an event actually observed by the *speaker* (not the subject of the sentence), another verb is used. Hence, different words for *to hear* might be used by a witness to a crime who "heard" the gun go off and by the policeman relating the witness's claim of having "heard" the gun go off.

As in the case of the linguistic evidence relating to lexical differences, we are not sure what to make of these instances. Whorf and others would have us believe that they reflect inescapable constraints on our thinking, but the evidence relevant to thought is all via language; no independent indicator of cognition is offered. We have to infer thought processes from general cultural indices (whose meaning we find it difficult to agree upon) or from other linguistic evidence, which we also believe to be related to cognition. In either event we are treading on very thin ice.

We know of only two experiments that present nonlinguistic evidence relevant to the influence of grammar on cognitive activities. The first was conducted by Carroll and Casagrande (1958) on a Navaho Indian reservation. In the Navaho language certain verbs that refer to manipulation of things require special forms, depending on what kind of thing is being handled: there is one verb form if the object is round and thin, another for a long flexible object, still another for a long rigid object, and so on. Since the Navaho grammar forces attention to the shape, form, and material of things, it is reasonable to assume that the behavior of Navahos toward things might be guided by these particular attributes to a greater extent than is the behavior of non-Navaho speakers. So Carroll and Casagrande reasoned.

They chose to investigate the saliency of these attributes in the object-sorting behavior of matched age groups of Navaho children, one speaking only Navaho, the other speaking only English. The children's actual task was to match an object with one of a pair of objects shown by the experimenter. A presentation pair might be a yellow rope and a blue stick, as shown in Figure 3–1. The child would then be shown a yellow stick and asked which one of the presentation pair it belonged with. Results con-

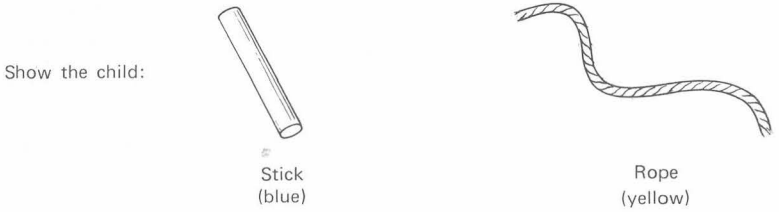

Figure 3-1. Objects used to study the influence of grammar on cognition (fashioned after Carroll and Casagrande, 1958).

firmed expectations about attribute saliency: Navaho-speaking children tended to match the items on the basis of form rather than color at younger ages than the English-speaking children did. Unhappily for the theory, when the same matching task was given to middle-class English-speaking children in metropolitan Boston, they too showed a preference for form over color—a preference that Carroll and Casagrande accounted for by the abundant experience with shapes and forms which these children had acquired in the course of playing with toys. They concluded that in this particular task, form choices could be mediated by *either* language or nonlanguage experience and that, overall, the results show that grammatical categories do influence matching. Note, however, that this is a very benign form of linguistic relativity, much more consistent with the idea that concepts are differentially available in different cultures rather than with the idea that they are exclusive to some one particular culture.

A recent experiment by Cole and his associates (1969) reinforces this interpretation. Their experiment took advantage of the fact that in the Kpelle language of Liberia, comparisons of size are not symmetric as they are in English. Thus, in comparing a large and a small person, a Kpelle would always refer to the larger, his remark translating as "John, he is big past Joe." Although it is possible for him to say the equivalent of "Joe is smaller than John," the Kpelle expression translates as "Joe, in smallness surpasses John," and it is rarely if ever used.

This observation was combined with a standard experiment, known as a *transposition experiment,* which has been used extensively to study the development of conceptual behavior in children. The experiment is most easily understood when we consider a particular example, such as that shown in Figure 3–2.

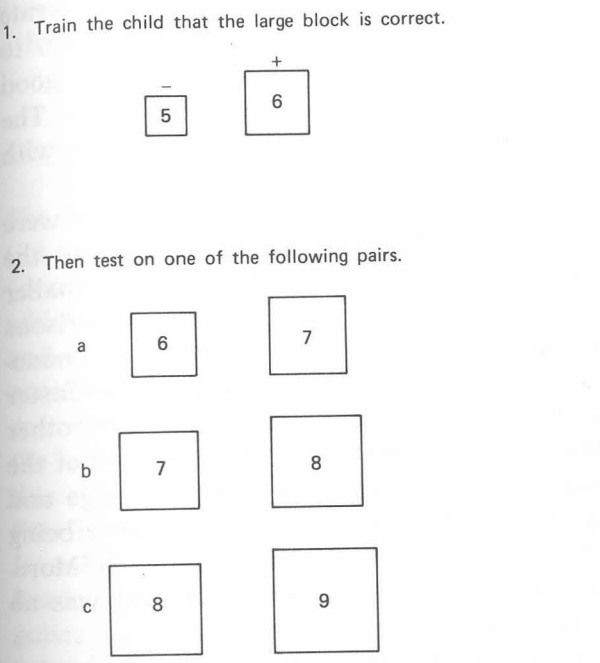

Figure 3-2. Design for a transposition experiment.

In this example, the child is first taught always to choose the larger of two blocks presented to him by the experimenter. In successive trials the physical placement of the two blocks is randomized so that size is the only reliable cue for determining which block is correct. After the subject reliably picks the correct block, he is presented with two other blocks also different in size. The question is: Will the subject choose the block that is the same size (or closest to the same size) as the block that was correct during training, or will he pick the block that bears the same size *relation* to its paired mate as the correct block did to its mate during training? For example, if, as shown in Figure 3–2, block 6, the larger block, is correct during training, then when the subject is presented with the pair of blocks 6 and 7 (condition A) will he choose 6 (the same) or 7(the larger)? When the block bearing the correct size relation is chosen, the subject is said to show transposition.

Three groups of Kpelle children participated in the experiment:

monolingual Kpelle-speaking children aged 4 to 6, nonliterate children aged 6 to 8 who spoke a little English in addition to Kpelle, and 6 to 8-year-old first-graders who could speak a good deal of English and who were beginning to read and write. The same blocks and the same testing procedures were used also with a group of 4- to 5-year-old American nursery school children.

When the experiment was actually run, some groups were trained to choose the smaller block and others to choose the larger, and they were tested on different combinations of smaller or larger blocks. If the Kpelle are used to making size comparisons by singling out the larger member first, we might expect monolingual Kpelle speakers to learn the larger-than relationship faster and to transpose it to test trials more readily than any of the other comparison groups. But this was not the case. Virtually all of the children showed transposition, regardless of their language and school background, and regardless of whether they were being tested on the larger-than or the smaller-than relationship. Moreover, learning to choose the larger block during training was no more rapid than learning to choose the smaller block.

These findings imply that the Kpelle preference for comparing the larger of two things to the smaller has no influence on discrimination learning. Here, as in the work reported earlier on perception of focal colors, we seem to be dealing with stimulus properties and relationships that exert a strong control over behavior. Two aspects of the Kpelle children's behavior in this task did seem affected, however. On the very first trial of training, before the subject had any information about the problem, all the Kpelle children showed a significant preference for the larger block, while the American children did not. Second, when the test phase was over and subjects were asked why they had made the choices they did, Kpelle subjects were better able to justify their responses if they were trained to choose the larger block, but American subjects showed no difference in the adequacy of their justifications as a function of which training block was correct.

Like the result of Carroll and Casagrande's research, and in fact like virtually all of the experimentally derived results relating to language and cognition in a cross-cultural context, these data point to limitations on the generality of the linguistic relativity hypothesis. We will defer any attempt to summarize the present status of the hypothesis until we look briefly at some proposals that all languages, in spite of their heterogeneity, share certain

common ways of coding experience. These propositions constitute a hypothesis about *linguistic universality.*

Linguistic Universality

The Whorfian hypothesis is primarily concerned with the referential aspect of language: how it maps experience, what it points to (denotative meaning). But there is another aspect of language, which expresses the qualities of experience—the feelings, images, and relationships that words arouse (sometimes referred to as connotative meaning). One of the largest, most systematic, and sustained cross-cultural investigations of language and thought in the last decade has been concerned with testing the generality of this aspect of meaning. With the help of cooperating social scientists in twelve countries (Japan, Hong Kong, India, Afghanistan, Iran, Lebanon, Yugoslavia, Poland, Finland, Holland, Belgium, and France), Charles Osgood (1964), a leading American psycholinguist, has been studying affective meaning systems through the use of a special measuring instrument he devised, called the *semantic differential.*

The basic procedure of the semantic differential is this: a subject is presented with a list of verbal concepts: *mother, bread, communism, teacher,* for example. Then he is given a list of antonym qualifiers (represented by adjectives in English) such as *good–bad, honest–dishonest, hot–cold.* The subject has to rate each concept against each qualifier pair using a number from 1 to 7, with the 1 standing for an extreme quality of the left-hand member of the pair (*good* in the first example given), the 7 for an extreme quality of the right-hand member of the pair (*bad*), and the other numbers for intermediate qualities. In a dozen or more factorial studies conducted with American English-speaking subjects, Osgood and his associates kept finding that the rating results could be described in terms of three dominant factors or dimensions of meaning: an *evaluative* factor (represented by scales like *good–bad*); a *potency* factor (represented by scales like *strong–weak*); and an *activity* factor (represented by scales like *fast–slow*). The problem then arose: Is this semantic framework limited to Americans speaking the English language or is it "shared by all humans regardless of their language or culture?" To find out, Osgood and his associates prepared a list of 100

familiar concepts that had been selected by linguists and anthropologists as "culture fair." This list was translated into the indigenous language, and from then on the work was conducted entirely in the various native tongues. Qualifiers and their opposites were elicited from groups of high school boys in each country. Scales were constructed, based on their responses, and then new groups of subjects were asked to rate the original 100 concepts against these scales.

The results to date indicate that the same three dimensions of meaning (evaluation, potency, and activity) describe the rating judgments in all the languages studied, although individual concepts are rated differently from culture to culture on these semantic factors. Stated a little differently, the structure of connotative meaning is the same from culture to culture, while the connotative meanings of particular concepts are culture specific. Osgood attributes this aspect of linguistic universality to the fact that his scales tap emotional feelings mediated by the affective nervous system which is "panhuman biologically" (Osgood, 1963, p. 320). A limitation in respect to "universality" to which we might draw attention is that his subject populations were all *educated* groups. In view of the strong homogenizing influence of education on the performance of cognitive tasks, to be described in later chapters, this restriction may be quite important.

Osgood goes on to suggest that universality of affective meaning systems may also account for the phenomena of metaphor and *verbal–visual synesthesia.* A classic study on metaphor conducted by Asch (1961) investigated how terms referring to physical properties of things *(hard, straight, hot)* are used to characterize psychological attributes of persons ("John is a very cold person"). He found strikingly similar metaphoric applications in such dissimilar languages as Hebrew, Greek, Chinese, Thai, Hausa, and Burmese.

Verbal–visual synethesia is a phenomenon in which words are regularly paired with certain pictorial representations rather than with others, as in the pairing of *happy* with an arrow pointing upward instead of downward. This was one of the fascinating results found by Osgood in a cross-cultural study demonstrating the generality of visual–verbal synesthetic tendencies among Navaho, Mexican-Spanish, Anglo, and Japanese subjects (1960).

Somewhat more research has been devoted to another related

phenomenon known as *phonetic symbolism*—the appropriateness of the relation between the sound of a word and its meaning. The *tinkle* of an icecube in a glass or the *boom* of the drum in the Salvation Army band might be considered to have appropriate verbal expression in the sense that the word sounds help to communicate some attributes of their referents.

Edward Sapir initiated research in phonetic symbolism in the 1920s by using a vocabulary of artificial words. (His and other early work are reviewed in Brown, 1958, Chap. 4). Brown, Black, and Horowitz (1955) carried out a well-controlled, specifically cross-language study which begat a series of investigations still under way. Twenty-one pairs of English antonyms *(warm–cold, heavy–light)* were translated into Chinese, Czech, and Hindi and were given to American college students unfamiliar with these languages. Told only the dimensions along which the words varied, the students were able to make better-than-chance discriminations concerning the meanings of the individual words in all three languages. To illustrate: given the pair of Chinese words *ch'ing* and *ch'ung* and the information that one means light and the other heavy, subjects tended to correctly guess *ch'ing* as light.

In many variations of this task using different languages and different methods of word presentation, investigators have repeatedly demonstrated above-chance matching of word meaning to word form. Correct matchings have been made even when each of the two members of a word pair was presented in a different language—*light* in Czech and *heavy* in Japanese, for example (Klank, Huang, and Johnson, 1971). A start has been made in identifying *which* sounds give cues to *which* meanings. Some evidence links vowel sound to meanings of magnitude: it has been found that high and front vowels occur proportionately more often in words denoting smallness, and low back vowels in words denoting largeness in *both* Chinese and English.

The first indication that the correspondence between the sound of a word and its meaning may influence cognitive processes comes from a recently reported Russian study on verbal memory (Baihdurashvili, 1972). Two groups of subjects were required to memorize lists of word pairs composed of a Japanese word and a word in the subject's native language. For the first group, the Japanese word was paired with a native word of the same meaning; the second group had a list in which the same Japanese words

were paired with native words of different meanings. The first group learned more rapidly and showed greater retention of the material, evidencing, in the investigator's words, "the lawful character of naming in a natural language" (p. 411).

Taken together, work in the semantic differential, synesthesia, metaphor, and phonetic symbolism seem to offer impressive support for the argument that certain qualities of experience are given common expression in many languages and cultures, differ as they may among themselves in other characteristics.

At this point we might stop to consider for a moment the implications of work in *language universals,* which we mentioned briefly in Chapter 2.

Joseph Greenberg (1966), G. A. Miller (1970), and others have singled out for attention various features of phonology (sound systems), grammar, and lexicon that all languages seem to share. Miller refers to these as "general design features" of language and suggests that their existence points to common physiological and psychological processes or capacities shared by all men. Chomsky (1968) maintains that these commonly shared features are themselves derived from base structures, which are built-in components of the human mind. These base structures make possible, and at the same time constrain, all language development. The task of psychology, he contends, is to search out the nature of these mental mechanisms underlying linguistic competence. But while it may be relatively simple to identify the basic mechanisms accounting for universal features in phonology (the limited variety of articulates possible to human speech apparatus, for example), it is another matter entirely to identify the psychological processes that might account for universals in grammar and lexicon. To add to the difficulty, the question of the relation between underlying psychological processes and linguistic competence has become the subject of a nonproductive debate pitting genetic innate mechanisms against learning mechanisms. On the other hand, developmental psycholinguists, studying the acquisition of language in the first few years of the child's life, are contributing information suggesting that there may be certain sequences in language mastery that are independent of features of particular languages (Smith and Miller, 1966) and that might at some future time help to elucidate the question of language universals. Fundamentally important as these issues are, we will not explore them further in this discussion because they revolve around a somewhat

different question from that concerning us. Chomsky, Miller, and others are asking the question: What are the cognitive operations underlying the acquisition and use of language? In other words, what capacities do we need in order to speak? The problem tackled in this chapter has been that of specifying the interrelations *between* language processes and other cognitive operations: How are speech and thinking related to each other?

Summary

Our review of the research evidence bearing on the Whorfian hypothesis certainly makes untenable any strong version of linguistic relativity. It is probable that the majority of scholars would agree in rejecting those of Whorf's formulations that stress the *arbitrary* character of the language–experience relationship and the inescapable and rigid constraints imposed on cognitive processes by language. Yet in spite of the patchiness of the evidence, few would be likely to allow linguistic relativity no role whatsoever. Here are some of the reasons we would give for keeping the question open.

1. First we would want to stress the limited nature of the experimental operations that have been brought to bear on the hypothesis. While there were good reasons for choosing to investigate linguistic relativity through color terminology, the superior intelligence of hindsight suggests this may have been something short of an ideal strategy. It is very likely that the expression of perceptual experience is most constrained by certain salient and stable stimulus attributes and is less responsive to the variability introduced by language. It may very well be that the "filtering effect" of language is greatest in respect to domains of phenomena that are definable, not in terms of physical properties, but in terms of attributes that are culturally specified. One thinks of such domains as social roles, for example; attributes defining categories of people (unlike those defining colors) are assigned by culture not nature. Or consider the area of ideology or theoretical work in general, where concepts largely acquire their meanings through their being embedded in explanatory verbal networks. It is here that language may play the greatest role in shaping the person's view of reality, in influencing his memory and thinking processes, and in contributing to his understanding or misunderstanding of other cultures. But such a proposition brings us around full circle to the difficulty with which we started: Can this hypothesis be tested empirically, and how?

2. A second point that should be made is that the demonstration of universal relations between aspects of language and cognition does not automatically make moot the question of culturally relative differences. Nor is it necessarily paradoxical that there should be both universals and differences in any domain of human experience. By now it is clear that the relations between language and cognition are not likely to be exhausted in a few general propositions. The growing body of research dealing with language and thought is uncovering multiple and complex interrelations between them. Our understanding will grow as theoretical work and cross-cultural research succeed in elaborating these multiple relations in both their universal *and* their particular aspects.

3. Finally, while Whorf's view of the particular characteristics of language important for cognition has not been disproved, there appear to be more fruitful ways today of investigating the classic questions. In our analysis of the Brown-Lenneberg experiments on color codability and memory (pp. 45–46) we pointed out that the hypothesized effects of language operated only through some assumed *verbal activity* on the part of the subject. None of the experimenters suggested that the vocabulary item as a static piece of information was responsible for the accuracy of recognition; all stressed what the subject did with it. These observations led us to point out the possible important implications of different *uses* of language for cognition. This indeed has been a subject of intensive research in the last few years, although not in the area traditionally designated as cross-cultural; this research has been stimulated by social class and subcultural comparisons within one society (principally the United States and England). A new field of *sociolinguistics* is showing rapid growth, its credo being that language cannot be understood except in its use functions—as human communication sensitive to the social contexts in which it is carried out. One of the seminal thinkers in this field, Basil Bernstein, has delineated different forms of speech codes that he considers characteristic of the English working class and English middle class, respectively, and that he feels significantly affect their learning experiences (1972). Bernstein has been concerned with how members of certain social strata develop characteristic ways of using speech to communicate with one another. This is seemingly a far cry from the characteristics of language that interested Whorf, but Bernstein specifically acknowledges his debt to Whorf for alerting him to the "selective effect of culture (acting through its patterning of social relationships) upon the *patterning* of grammar together with the pattern's semantic and thus cognitive significance" (1972, p. 224). In this young and potentially rich field of investigation of how individuals use their language not only for social communication but as a tool for thought, Whorf still lives.

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chapter 4 Culture and Perception

"Primitives perceive nothing in the same way as we do," said Levy-Bruhl (1910, p. 30) and while it is clear that his controversial conclusion was based on inadequate evidence, more recent efforts to get adequate evidence about what people in other cultures "see" have continued to be dogged with difficulties and disputation.

A problem we face at the outset is the varied usage of the term *perception* in the psychological and anthropological literature. Chapter 3 should serve to make us cautious about how we interpret certain words, such as *blue,* which may have different referents in two languages. But the problems relating to the term *perception* are much deeper than the question of language referents.

When psychologists use the word *perception,* they generally refer to processes by which people organize and experience information that is primarily of sensory origin. They also commonly emphasize that perception involves active operations on in-

formation and is not a passively received "direct copy" of the external world. Anthropologists and laymen, however, tend to use the word *perception* in a much broader sense, to refer not only to the organization of sensory data but to such phenomena as outlook on life, world view, interpretation of events, and the like. In the work to be reviewed in this chapter, we will be speaking of perception in the narrower, psychological sense. We will deal with some of the issues involved in the broader definition in later chapters where we discuss classification and conception.

But perception in the narrow sense has also been a source of theoretical conflict within psychology from its inception. One of the earliest major controversies pitting the titans of psychology against one another concerned the nature of perceptual experience (Boring, 1950): well-trained observers from different laboratories reported different perceptual experiences under identical stimulus conditions. The disillusionment that followed years of unresolved arguments was a major impetus to a shift in research strategy among psychologists. Instead of attempting to make inferences about perceptual experience on the basis of introspective reports, they tried to get at experience through the use of behavioral indicators. As we shall see, this methodological reform did not dispose of the problems of inference and interpretation of data from perception experiments.

However, even in the heyday of early psychological research on perception, cross-cultural research seemed an attractive possibility to some investigators. While recognizing the new difficulties introduced by language and custom, studies of different cultural groups, especially non-European peoples, seemed to offer a means of resolving several perennial debates among psychologists and philosophers. Primary among these was the nativist–empiricist issue: Are the basic perceptual catgories (constancies, figure-ground perception, and the like) innate or the result of experience? Of equal interest in the post-Darwinian era was the possibility of discovering something about the evolution of mental capacities. This was one of the motivations leading to Rivers's perceptual investigations in the Torres Straits in 1901, mentioned in Chapter 2.

In the tradition of Spencer and others, Rivers and his colleagues speculated that certain senses might be more highly developed among primitive people than among industrialized people. They

put these speculations to the test by carrying the instruments and techniques developed in the new experimental psychology laboratories to anthropological field stations on the Papua Coast and in southern India. Here they collected a large number of systematic observations on vision, hearing, and other sense modalities, using diversified tasks and measurement procedures. Reviewing the data on visual acuity, Rivers concluded that there was little basis for prior beliefs that sensory acuity was better among non-Western people. He did not, however, draw the additional conclusion that the observations on which these beliefs were based were erroneous. Rather, he argued that it *appeared* as though primitive people had exceptional sensory powers because they devoted their attention predominantly to "objects of sense," making minute discriminations among the details of landscape, vegetation, and animal life, which are made in our society only by zoologists and botanists. He also clung to Spencer's hypothesis that highly refined and discriminatory powers of observation were attained at the cost of the higher mental faculties. "If too much energy is expended on the sensory foundations, it is natural that the intellectual superstructure should suffer" (Rivers, 1901, pp. 44, 45).

Rivers's data, though not his extrapolations from it, were largely accepted by psychologists, and little systematic work has been done since that time on comparative sensory capacities. Attention has shifted to more sophisticated questions about the influence of cultural and environmental experiences on perception. In reviewing this work, though, we are struck by the fact that only a small set of the problems investigated in European and American laboratories during the past hundred years has been submitted to cross-cultural analysis. The specific questions that have been popular research topics during the past two decades of heavy activity have emphasized cultural differences in very restricted domains of perceptual experience. Before we look at these questions in detail, we need to point out that all cross-cultural research on cultural differences in perception rests on the assumption that commonalities in perceptual processes among peoples of the world far outweigh whatever differences may be found. This simple fact tends to be forgotten in the search for variation. But its truth is evident—it would be impossible to test for *differences* if there were not a commonly shared perceptual foundation to use

as a starting point. It is taken for granted, for example, that everyone possesses form and depth perception in the real visual world, no matter how they perform on perceptual tests making use of special stimuli (whether they show 3-D perception in viewing photographs, for example).

The research questions most actively pursued today include the following:

1. Are there experiences that influence the perception of artificial visual representations (like photographs and drawings)?
2. Do different experiences lead to alternative ways of organizing ambiguous or deceptive stimuli?
3. Does growing up in a particular cultural environment predispose a person to select specific features of his environment for special attention so that they are seen more clearly or quickly than others?

In looking at psychologists' answers to these and related problems, we will concentrate on a few techniques that have received the most attention and that therefore present the most solid cases for study.

Pictorial Depth Perception

From time to time one reads the reports of travelers indicating that native informants fail to recognize the contents of photographs, even when the pictures are taken in the informant's locale and even if the pictures are of the informant or members of his family.

Such is the case in central Liberia, where nonliterate, traditional Kpelle rice farmers were shown the pictures in Figure 4–1. Looking at the photographs we can think of various reasons why naive subjects who had not previously seen such pictorial representations of real objects might be confused. The perspective of the mat upon which the objects rest is represented by the up-down dimension of the picture, a convention that we take for granted, but which is by no means inborn. We need only recall that it was not until the fifteenth century that such conventions became a part of Western artistic work; the use of perspective to represent distance became a full-blown feature of the Western tradition with Leonardo da Vinci. (For pre-da Vinci distance

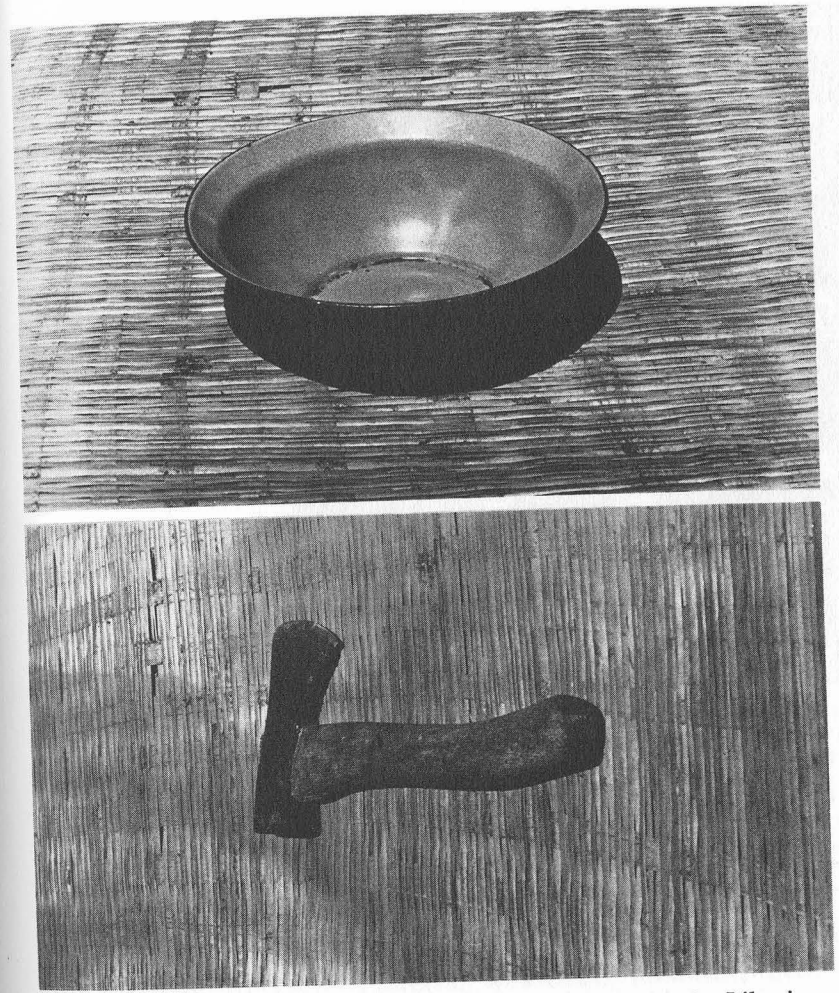

Figure 4-1. Photographs shown to Kpelle rice farmers in Liberia, Africa.

representation, see Figure 4–2.) Even today the use of perspective is by no means worldwide, and some modern Western painters deliberately violate the conventions to obtain particular artistic effects. The various cues in paintings and photographs, which we take for granted, took centuries to develop; it requires some measure of experience on the part of an individual before three-dimensional perceptions of pictures becomes natural.

The items shown in Figure 4–1 are also presented apart from

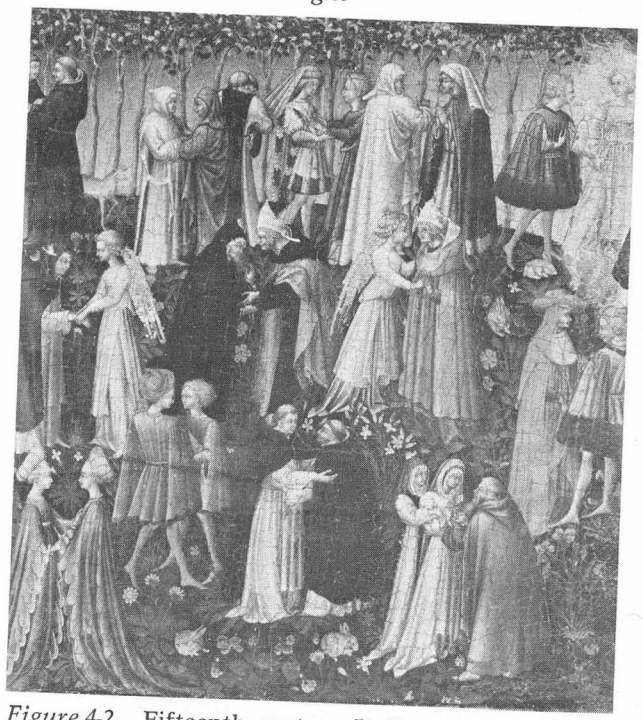

Figure 4-2. Fifteenth century Italian artist Di Paolo shows people in the distance larger than those in foreground.

their habitual contexts, and the colors are not perfectly true to life. Considerable systematic research is needed to disentangle the many possibilities that could account for the difficulties of interpretation experienced by naive observers of photographs or other two-dimensional pictures.

On certain questions of interpretation, research has been considerable but not systematic. On the most elementary level, one would want to know what factors are involved in grasping the notion that lines, colors, or black, gray and white shadings on paper represent anything at all. Such a notion must underlie the identification of what is represented. An oft-quoted observation by Herskovits describes a bush woman's confusion when presented with a photograph of her son, how she turned the piece of paper this way and that, not knowing what to do with it. When Herskovits pointed out the details of the photograph, however, she perceived the subject (cited in Segall, Campbell, and Herskovits, 1966). There is impressive evidence that those who have the notion that known things may be represented on paper can

identify the things under conditions of representation that do not involve the use of conventionalized cues (we will discuss these later). For example, Brimble (1963) presented Bantu villagers with 40 simple line drawings of familiar things and secured correct identifications in over 90 percent of the cases. Deregowski (1968a) found that Zambian adults and children, when shown extremely simplified photographs of animal models against a neutral background, could select the correct match from an array of the actual models with an above-chance level. What is not clear in these studies, however, is whether the subjects had prior exposure—even on a minimal basis—to pictorial material, and evidence of this nature is crucial for an understanding of how the ability to process information presented in these modalities develops. (See R. J. Miller, in press, for a thorough review of this line of work.)

While there may be some amount of object recognition in simplified presentations, there is no doubt that the kind of pictorial material that is common in modern nations—such as the photographs with which we introduced this section—presents great difficulties to many traditional peoples. An ingenious line of research initiated several years ago by Hudson has helped us to understand some of the factors at work, particularly the role of Western conventions of perspective in pictorial representation.

Hudson (1962b) was concerned with a practical problem: how to train largely nonliterate Bantu workers employed in South African mines and factories. He found that training films and safety posters often failed to have the desired effect, and an investigation indicated that the problem was one of interpretation—the visually presented material was being misinterpreted or not interpreted at all.

In order to make a systematic investigation of the factors involved, Hudson employed a set of cards, some of which are shown in Figure 4–3.

All of the pictures contain figures of an elephant, an antelope, and a man pointing a spear. In each picture the spear is aligned with both the elephant and the antelope. The subject is asked various questions designed to elicit what he sees in the picture. Most important for assessing the use of depth cues is a question like "What is the man doing with the spear?" and if this fails to yield a response, "Which animal is the man aiming the spear at?"

The pictures differ with respect to the cues available for interpreting the picture. Cards 1 and 2 contain the depth cues of object

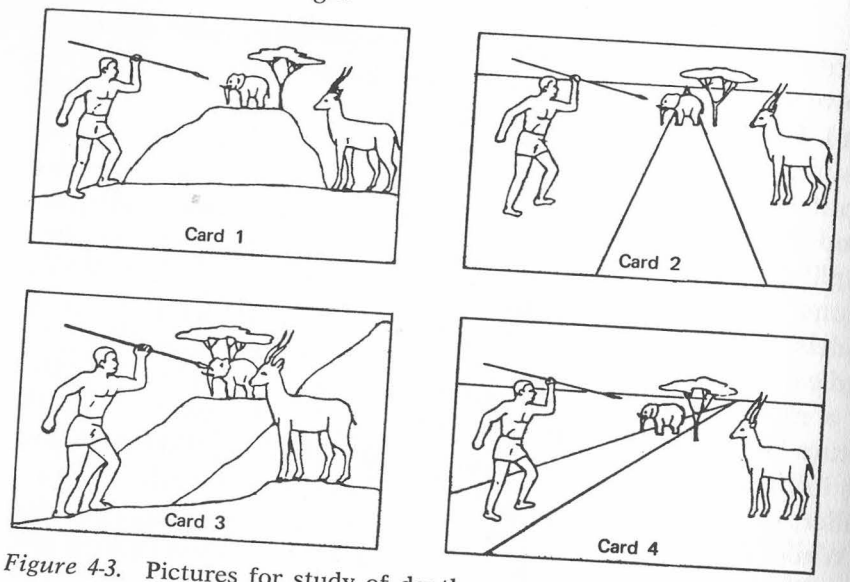

Figure 4-3. Pictures for study of depth perception in Africa.

size and superposition; cards 3 and 4 contain perspective cues as well.

Materials such as these were used with several different groups of people in South Africa (Hudson, 1962a, b) and Ghana (Mundy-Castle, 1966). Among the groups studied were European and Bantu primary school children of various ages, Bantu secondary school children, nonliterate European and Bantu workers, and Indian school children. Results showed that at the beginning of primary school, the European children had a great deal of trouble perceiving the pictures as three-dimensional—that is, they would say that the man was aiming his spear at the elephant. By the end of primary school, virtually all these children responded to the stimuli as three-dimensional. Not so the Bantu children (in Hudson's sample) or the Ghanian children (in Mundy-Castle's sample). These children all tended to interpret the pictures in a two-dimensional fashion. Hudson also found that the nonliterate laborers, both Bantu and European, and Indian children responded to the pictures as flat, rather than three-dimensional. He concludes from this set of studies that

> formal schooling in the normal course is not the principal determinant in pictorial perception. Informal instruction in the home and habitual exposure to pictures play a much larger role (1967, p. 95).

This conclusion is seconded by Mundy-Castle, who also conducted surveys in the communities and homes of the children who participated in his experiment. He reports that he found

> no evidence of activities such as reading, drawing, painting, looking at pictures, pattern-making, or playing with constructional toys, and it was exceptional for a child to have used a pencil prior to going to school. . . . The opportunity for informal pictorial experience was therefore negligible (1966, p. 298).

These results seem quite convincing for Hudson's task. But how representative is the task itself? Is it the case that people who respond inappropriately to questions about Hudson's pictures simply can't perceive pictures three-dimensionally? Or are there other ways of evaluating what people see, perhaps with different kinds of stimuli, that would reveal three-dimensional perceptions?

This is the question asked by Deregowski (1968b), who has carried out many studies on the relation between culture and perception. His work was conducted with 7- to 16-year-old schoolboys (average years in school, 3.9) and adult domestic workers in the city of Lusaka, Zambia.

To each subject Deregowski gave a version of Hudson's test, using pictures like those in Figure 4–3. Then he presented subjects with quite different kinds of pictures, shown in Figure 4–4. Instead of asking subjects questions about these pictures, Deregowski asked them to construct a model of the picture, using sticks that could be stuck together easily. Of course, he made sure that each person knew how to make a model with the sticks in a practice session.

The major question was, would people who respond two-dimensionally when asked about Hudson's pictures also respond two-dimensionally when asked to make models of abstract line drawings? In general, Deregowski's answer to this question was *no.*

Consistent with previous results, Deregowski found that verbal statements about the object relationships portrayed in Hudson's pictures overwhelmingly indicated two-dimensional perception: 100 percent of the domestic workers and 80 percent of the schoolboys responded in this manner. *But more than half of these same subjects constructed three-dimensional models of the figures in*