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# **Communication, Mind, and Culture**

## **A Model of Conversation Meaning**

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# Abstract

This dissertation is a theoretical study of conversation meaning. The aim was to build a comprehensive theory of conversation meaning on the basis of the theories of cultural schemas, distributed cognition, and neural networks. The philosophical tradition of transcendental realism was introduced, and a metaphysical analysis of culture was presented. It was concluded that culture belongs to the realm of mind, and culture and society were strictly distinguished. By acknowledging the importance of culture in mind, various traditions of conversation research were critically reviewed. It was proposed that conversation is a distributed cognitive system, and the nature of conversation meaning was explored from that perspective. It was then concluded that conversation meaning was part of cultural meaning. The concept of conversation meaning as distributed and cultural is further investigated by situating it in the theory of neural networks. The theory advanced in this dissertation should be seen as the beginning of the formalization of conversation meaning. This theory has potential applications in second language education, one of the goals of which is enabling learners conduct conversation fluently in a second language. It was argued that cultural knowledge is a necessary resource in the process of language comprehension, and thus the theory supports educators' efforts to integrate culture into second language education.



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# Chapter 1

## Introduction

For second language learners living in an area where the language to be learned is spoken, everyday conversation is the most pervasive mode of language use. Especially for beginning learners, to understand meaning appropriately during the course of conversation in a second language is what they wish to accomplish someday. They study the target language by learning its pronunciation, vocabulary, and grammar. But even when they have learned the language quite well, there are occasions when although they feel that they understand what the other people *say*, they do not really understand what they really *mean*. At times learners even wonder: “Do these people think in the same way as I do?”

This problem has been recognized for a long time by second language teachers. And the teacher’s intuitive effort to solve this problem has been to teach something other than language in addition to the standard language curriculum. Teachers often call the extra component “culture.” Such efforts by teachers have resulted in the creation of different traditions in various countries under different names: for example, cultural studies in English-speaking countries, *Landeskunde* in German-speaking

ones, and *Nihonjijou* in Japanese-speaking ones.

Applied linguists working in the field of second language education are, by definition, supposed to do research on problems in second language education. Have they carried out research in this field? Partly yes. Partly, because in general such research have not exactly reflected the problem from the practice side. Most studies related to this problem have been conducted under the theoretical perspective of pragmatics, and it is called *interlanguage pragmatics* (e.g., Kasper, 1996; Kasper & Blum-Kulka, 1993). But interlanguage pragmatists continue to consider language to be the central object of study rather than what teachers call “culture.”

This study is an attempt to solve such learners’ and teachers’ problems through scientific research. I believe that there is some truth in the experiences of both learners and teachers despite the fact that the problem has been described rather vaguely. And I believe that it is the applied linguist’s job to clearly describe the problem and find out the cause of it.

To that end, I will take a bottom-up approach: If the problem is in the nature of meaning in conversation, the very mechanism of conversation meaning should be studied. Once it is possible to identify how learners, or more generally, any humans, process meaning in conversation, the most fundamental problem is solved. Naturally, it is also important to investigate the learning and teaching side, and studies of this side have already been conducted concerning the learning and teaching of various languages (e.g., Kaikkonen, 1994; Kramsch, 1993; Hinkel, 1999; Neustupný, 1995). However, I strongly believe that only after the cause of the problem, that is, the general mechanism of conversation meaning is discovered can teachers ever hope to think up a practical solution to the problem.

This is a theoretical study of conversation meaning from a multidisciplinary, applied linguistic perspective. Unfortunately, there is no ready-made discipline that studies the general nature of conversation meaning. Each discipline concerned with

conversation meaning studies only some particular aspect of it, and there is no theoretical framework that provides a holistic understanding of how conversation meaning takes place. What is urgently needed, then, is the creation of a unified theory that takes any available research results into consideration.

In what follows I will argue that conversation meaning is a kind of cultural meaning evoked in a distributed cognitive system. I will also show that the key to the successful processing of conversation meaning depends on the inference produced by cultural schemas in a distributed system of neural networks. Conversation is not a matter for one person, but a matter for all participants in a conversation. And the mechanism of conversation meaning is closely related to various aspects of culture.

From the point of view of practice, this study confirms that the intuition of second language teachers who consider “culture” as an important aspect of their work is correct. The teaching of the cultural schemas of target language speakers would rarely improve learners’ ability to construct grammatical sentences or pronounce words correctly. However, cultural schemas are the key factor in the interpretive process of conversation meaning. Thus it is important that the learning and teaching of cultural schemas be integrated into the learning and teaching of language; otherwise, learners are doomed to suffer from the problem described above forever.

Because this is a theoretical study that aims at providing a foundation for the exploration of conversation meaning, the chapters that constitute it follow in a logical order, the later chapters being dependent on the earlier ones. Chapter 2 lays out the philosophical foundations for the subsequent chapters, especially for Chapter 3. The mental and social sciences often suffer from metatheoretical disputes, and to avoid possible objections to the multidisciplinary theory presented in the later chapters, it is necessary to firmly ground this study in a theory of the philosophy of science. Chapter 3 extends the arguments of the previous chapter to the theory of culture and mind, and

I will show that culture is mental and part of what is mental is cultural. Chapter 4 provides an overview of the status quo of various kinds of conversation research. It will be concluded that at the moment there is no unified theory of conversation meaning, let alone a theory that incorporates the notion of culture. Chapter 5 is an attempt at the clarification of what conversation meaning is, and presents my theory of conversation meaning. Chapter 6 complements the previous chapter by further exploring the theory presented in Chapter 5, and summarizes the theoretical insights provided so far. Chapter 7 turns to the consideration of the significance of this study to the practice of second language education. Chapter 8 summarizes the contributions made in this study.

## Chapter 2

# Transcendental Realism Reconsidered

### 2.1 Introduction

In this chapter I will describe the main features of *transcendental realism*, which was introduced by the philosopher Roy Bhaskar in 1975. Transcendental realism is a school of thought belonging to the philosophy of science, and the arguments presented in the subsequent chapters will be based on it. Compared to other schools within the philosophy of science, transcendental realism has a peculiar characteristic, namely the fact that it has been developed not only by philosophers within philosophical communities but also by scientists, especially social scientists. Together with critical naturalism, which is an extension of transcendental realism to the mental and social sciences, the school of thought established by Bhaskar and his followers has come to be called *critical realism*.

It is often the case that theoretical disputes in the mental and social sciences are not scientific, but philosophical. They are

disputes in the realm of the philosophy of science, rather than in one of the disciplines of the mental and social sciences. Unfortunately, unlike in the time of Plato and Aristotle, scientists are no longer philosophers, and philosophers are no longer scientists. Although philosophers of science strongly argue for the importance of their discipline to scientists, scientists rarely engage in discussions about the philosophy of science. Even worse, scientists in one discipline do not usually realize that their theoretical problems are often discussed in other disciplines. The problem is not only that scientists are ignorant of the philosophy of science but also that they are ignorant of the theoretical problems of other disciplines.

The theoretical disputes occupying the mental and social sciences eventually go back to the debate about naturalism and antinaturalism. The problem appears in various forms in various disciplines, with naturalism dominant at one time and antinaturalism at another time. Mental and social scientists often think that theoretical disputes are problems that concern their own discipline and do not try to relate them to the general philosophy of science. But as a matter of fact, this is the same never-ending problem that has been discussed by philosophers of science for centuries. Take, for example, the processual vs. postprocessual debate in archaeology (e.g., Hodder, 2001; Johnson, 1999), the scientific vs. interpretive debate in cultural anthropology (e.g., Geertz, 1973, 1983; Lett, 1997), and the scientific status of psychology (e.g., Manicas & Secord, 1983) and psychoanalysis (e.g., Bhaskar, 1998). Despite their subject matters, the final question is always the same: which is correct, naturalism or antinaturalism? To avoid this unfortunate situation, I will start by discussing the philosophy of science first. The discussion in the chapter is heavily based on the concepts to be developed in this chapter.

## 2.2 Transitive and Intransitive Objects

The objects of scientific knowledge can be divided into two types, and it is critical that they are treated separately (Bhaskar, 1975). One consists of the *transitive objects of knowledge* and the other of the *intransitive objects of knowledge*.

Science is a social activity that produces knowledge. The social activity of science, like any other social activity, takes raw material as its input, turns it into a different form, and as a result produces new material. The product of the social activity or current scientific knowledge, then, is necessarily based on raw materials, that is, antecedently established scientific facts and theories. Such antecedent objects are what Bhaskar (1975) called the transitive objects of knowledge, and they are dependent on humans, who produced them and who transform them again and again.

If the transitive objects of knowledge are dependent on humans, there must exist something that we seek knowledge about. Because something exists independently of humans, we can seek and revise our knowledge of it. This is what Bhaskar (1975) calls the intransitive objects of knowledge, and they do not depend on humans for their existence. It is possible to imagine a world without science, as there was and can again be in the future, where there exist intransitive objects but no transitive objects. The intransitive side of knowledge is independent of humans and it exists even if we do not exist.

According to Bhaskar (1975), the transitive objects of knowledge are necessary for science to exist. We cannot imagine science without the transitive objects of knowledge, because knowledge depends on antecedent knowledge and scientific knowledge is no exception in this respect. Such antecedent knowledge is not necessarily scientific, and it can be non-scientific or pre-scientific, but in any case science cannot exist if antecedent knowledge of some kind does not exist. The intransitive objects of knowledge are also necessary for science to exist. Only

if there exists some entity whose existence is beyond the existence of humans can humans search for it. If no such entity exists, there is no rationale for doing science at all, because no entity which we seek to discover actually exists.

Of course such a truism still has room for a challenge. A philosophical theory called *idealism* in the philosophy of mind denies such a truism by claiming that there are no material objects or events that are opposed to mental objects or events (Heil, 1998). The transitive objects of knowledge are the kind of knowledge that cannot exist without humans, more specifically without human minds that process knowledge. So it is possible to interpret the transitive objects of knowledge as mental objects. The intransitive objects of knowledge are the objects that are external to human minds and exist independently of them. So the intransitive objects are equal to material objects, which are non-mental. According to idealism, the world consists exclusively of minds and their contents, and the material world that is believed to exist does not really exist. The regularity and orderliness of experience come from the intrinsic nature of minds or a benevolent God. Although idealism is consistent with all the evidence humans could ever have, as Heil puts it, most of us find it difficult to swallow. This dramatic theory goes against our intuition, and it is extremely hard for most of us to support it.

Bhaskar (1975) defended the existence of the intransitive objects of knowledge by the theory of perception and the intelligibility of scientific experiments. Such a defense, however, is a pragmatic one, and it does not follow that it is possible on the basis of how scientists do research to vindicate a metaphysical thesis. Such a lack of a metaphysical defense is a serious problem, because the problem at hand is metaphysical in nature. Of course believing that no intransitive objects exist is counterintuitive. But if it is important that metaphysical questions are seriously treated in science, it must be admitted that metaphysical arguments for the existence of intransitive objects are



unavoidable. In a sense the existence of intransitive objects is a pre-philosophical truism, but I suspect that the proof of their existence is such a problem that it goes back to the defense of metaphysical realism, which is, according to Loux (1998), one of the most fundamental questions since the beginning of Western philosophy. A defense of metaphysical realism is beyond the scope of this study, and even Bhaskar does not do it seriously. In brief, metaphysical realism involves the following views:

- (a) There are real objects (usually the view is concerned with spatiotemporal objects),
- (b) they exist independently of our experience or our knowledge of them, and
- (c) they have properties and enter into relations independently of the concepts with which we understand them or of the language with which we describe them. (Butchvarov, 1999, p. 562)

Thus it appears to be clear that Bhaskar based his arguments on the work of metaphysical realism, and that the opposing view of nominalism was out of his sight when he discussed the nature of science.

Even if it is supposed that both the transitive and intransitive objects of knowledge exist, there still remains a need to make their relationship clear. According to Bhaskar (1975), their relationship is such that the transitive objects are used to discover the intransitive objects. The aim of scientific endeavor is to find out and explain the nature of the intransitive objects. To do so, scientists create knowledge that is about the intransitive objects. But knowledge always comes from other knowledge, and the antecedent knowledge, which also consists of the transitive objects, is used to create such knowledge. So scientists use the transitive objects for the intransitive objects, but never the other way around.

According to transcendental realism, any adequate philosophy of science must be able to sustain and reconcile two aspects

of science (Bhaskar, 1975). First, the transitive aspect, that is, the fact that science is dependent on antecedent knowledge and the social activities of humans. Second, the intransitive aspect, that is, the fact that science is independent of both. Bhaskar characterizes these two aspects as:

- (1) the social character of science and
- (2) the independence from science of the objects of scientific thought. (p. 24)

He also turns these statements into more specific forms:

- (1)' a criterion of the non-spontaneous production of knowledge, viz. the production of knowledge from and by means of knowledge (in the transitive dimension), and
- (2)' a criterion of structural and essential realism, viz. the independent existence and activity of causal structures and things (in the intransitive dimension). (p. 24)

## 2.3 Transcendental and Empirical Realism

Transcendental realists claim that any philosophy of science that does not fulfill the two criteria mentioned above fails to capture the nature of science, and such schools of the philosophy of science include *classical empiricism* and *transcendental idealism* (Bhaskar, 1975). The distinction among those schools is a practical one, and some philosophers of science can be situated in more than one school. Transcendental realism can be historically presented as the third alternative among the schools of the philosophy of science (Bhaskar, 1975).

The first school is that of classical empiricism. The advocates of this position, namely David Hume and his heirs, claim

that science is a kind of behavioral response to the stimulus of given facts and their conjunctions. All that exists can be experienced, and the aim of science is to find out the invariance of the stimulus and response.

The second school is that of transcendental idealism proposed by Immanuel Kant. According to it, the objects of science are models of nature, and such models are only what humans create in their minds; they do not in fact exist in nature, but are imposed by humans on nature. Transcendental idealists consider classical empiricism to be insufficient, because it does not postulate the existence of such models, which should be the objects of science.

The third school is that of transcendental realism. It regards the objects of knowledge as structures and mechanisms that generate phenomena. The difference between transcendental idealism and transcendental realism is that whereas the former considers models to be human constructs imposed on phenomena, the latter considers the models to be real mechanisms that exist independently of humans.

In short, transcendental realism is in contrast to empirical realism, to which both classical empiricism and transcendental idealism subscribe (see Figure 2.1). For empirical realism, the entity that concerns science is a perceptible phenomenon. In other words, what science should discover is the nature of the phenomena that we, humans, can perceive and experience. In contrast, transcendental realism goes further than that by saying that the entity in question transcends our experience, and is independent of the existence of humans. The true entities to be concerned with are the mechanisms that cause the perceptible phenomena and that are often called laws or mechanisms. Transcendental realists agree with transcendental idealists in that what should be examined are not mere perceptible things, namely, conjunctions and invariances of phenomena. But transcendental realism differs significantly from transcendental idealism on the status of the existence of the mechanisms. For

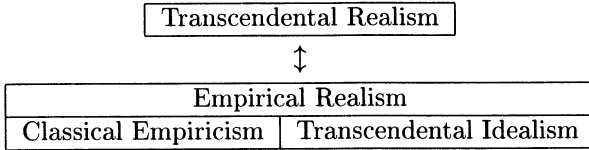


Figure 2.1: Transcendental Realism vs. Empirical Realism

transcendental realism, the mechanisms are real, that is, they exist independently of our existence.

From these arguments it follows easily that the problem of empirical realism is that it does not suppose the existence of the intransitive objects of knowledge, and that is why it is opposed fundamentally to transcendental realism, for which the existence of the intransitive objects is crucial. Although empirical realists do not consider the existence of the intransitive objects, there is a difference between classical empiricism and transcendental idealism as concerns the status of the transitive objects of knowledge.

As a matter of fact, classical empiricism ignores the existence of both the transitive objects and the intransitive objects of knowledge (Bhaskar, 1975). Even the idea that events are different from experiences cannot be sustained because there exists only what can be experienced, such as conjunctions and invariances. So in addition to criteria (2) and (2)', classical empiricism fails to satisfy criteria (1) and (1)'. On the other hand, transcendental idealism does satisfy criteria (1) and (1)', but it considers the transitive aspect to be the ultimate objective of science. Because the models that transcendental idealism considers to be the aim of science are entities that exist in human minds, and do not exist outside of human minds, transcendental idealism fails to satisfy criteria (2) and (2)'. Transcendental realism is able to satisfy criteria (1), (1)', (2), and (2)'.

Although Bhaskar's arguments (1975) on the existence of the intransitive objects of knowledge is sound, it is still incomplete. As I have pointed out above, his explanation for the existence of the intransitive objects is not metaphysical, which it makes his arguments less convincing. Because his attacks on empirical realism are based on the analysis of perception and experimental activity, let us now consider these analyses.

In criticizing empirical realism, which denies the existence of the intransitive objects of knowledge, Bhaskar (1975) presented his analysis of perception and noted that experience actually presupposes the existence of the intransitive objects. Scientific theories are based on our perception of, and experience with, the world. Perception presupposes that something exists intransitively beforehand. The fact that scientific changes occur shows that there exist objects to be studied. And the existence of scientific education presupposes the independence of the intransitive objects. If the intransitive objects do not exist, why do we educate people to discover them in the first place? It would be enough that we experience, if there were no intransitive objects. So the objects to be perceived should be distinguished from our experience of perceiving the objects. In other words, events and experiences must be distinguished.

Bhaskar (1975) also argued that scientific experiments, which are one of the hallmarks of science, are only intelligible if we assume the existence of the intransitive objects. He also claimed that transcendental realism is the only school of the philosophy of science that is able to explain why experiments exist and they are important in science. By conducting experiments we try to find out causal laws, and the events that appear during experiments are what these laws cause to happen. So during experiments we are a causal agent of the events, but not of the law that the events enable us to identify. This suggests that the real basis of these laws cannot be sequences of events, and that there must be a distinction between laws or mechanisms and events.

Obviously there exist no human experiences without humans. But even if experiences do not exist, events occur and exist, and laws that bring about the events exist. According to classical empiricism, if there are no human experiences, the objects of scientific inquiry do not exist either. So conjunctions of events, which are based on our experiences of events, do not exist, and the Humean *causal laws* do not exist either. For transcendental idealism, if events do not occur, laws or what they call models do not exist because such models reside in our minds and they are only imposed by humans. So if events do not exist, laws do not exist either. It follows from this that, as Bhaskar (1975) pointed out, empirical realism, which fails to suppose the existence of laws as the intransitive objects, is in fact based on *anthropocentricity*. But as I have suggested, we have strong reasons to suppose that the world exists even if we do not exist, and that empirical realism, which is built on top of anthropocentricity, is fundamentally flawed.

## 2.4 Three Domains

Bhaskar's analysis of perception (1975) thus shows that experiences are distinct from events, and his analysis of experimental activity shows that laws or mechanisms are distinct from events. This leads to one of the most important theses of transcendental realism, namely, the distinction of three domains (see Table 2.1). The problem with empirical realism, as can be seen from Table 2.1, is that it sustains the domains of the empirical and the actual, but not the domain of the real. If empirical realism fails to recognize the domain of the real, it means that it fails to acknowledge the existence of mechanisms, which are the central concern of transcendental realism. Transcendental realism recognizes the existence of the domains of the empirical and the actual as well. The problem of empirical realism is that it lacks the understanding of mechanisms because of its ignorance

Table 2.1: Three Domains

	Domain of Real	Domain of Actual	Domain of Empirical
Mechanisms	✓		
Events	✓	✓	
Experiences	✓	✓	✓

(From Bhaskar, 1975)

of the domain of the real.

In the original note to Table 2.1, there is an interesting account of the relationship between transcendental realism and empirical realism using the scheme of the three domains. Bhaskar (1975, p. 56) gives two equations that put transcendental realism and empirical realism in contrast to each other. And for transcendental realism the following equation holds:

$$d_r \geq d_a \geq d_e, \quad (2.1)$$

where  $d_r$ ,  $d_a$ , and  $d_e$  are the domains of the real, the actual, and the empirical respectively. For empirical realism, the equation is

$$d_r = d_a = d_e. \quad (2.2)$$

What is interesting is his claim that (2.1) is a special case of (2.2). He explains that:

- (a) for  $d_a = d_e$  the events are known under epistemically significant descriptions, which depends upon skilled perception (and thus a skilled perceiver);
- (b) for  $d_r = d_a$  an antecedent closure has been obtained, which depends upon skilled experimentation (and thus the planned disruption of nature). (p. 56)

So according to the argument, empirical realism is a special case, that is, a part of transcendental realism. There are at least three critical problems with this claim. First, this argument clearly contradicts the claim that transcendental realism is a doctrine which is in sharp contrast to empirical realism, and we have seen how Bhaskar (1975) argues for this. Second, even if the claim that (2.2) is a part of (2.1) and empirical realism is a part of transcendental realism is accepted, there is still room to wonder why in the world it is ever necessary to distinguish between these two doctrines. Third, for transcendental realism, it is too risky to think that from (2.1) it is possible that (2.2) ever holds. (2.2) is a purely theoretical construct, which has no practical counterpart, because in the real world it is too hard to imagine that such ideal perceivers as described in (a) and perfect experimentations as described in (b) could ever be possible.

The idea of a perfect perceiver is hard to accept. In reality there is no one who is a “perfect” perceiver, or if there were one, who decides so? As time goes by, better and better technology comes along that enables scientists to observe something that has never been observed before. So compared to the quality of experiments done before, it might seem that perfect experiments that make (2) possible can be carried out now. However, this is at odds with Bhaskar (1975) himself, who claimed:

Indeed, we know from the history of science that at any moment of time there are types of events never imagined, of which theoretical, and sometimes empirical, knowledge is eventually achieved. For in the transitive process of science the possibilities of perception, and of theoretical knowledge, are continually being extended. Thus unless it is dogmatically postulated that our present knowledge is complete or these possibilities exhausted, there are good grounds for holding that the class of unknowable events is non-empty, and unperceivable ones non-emptier; and no



grounds for supposing that this will ever not be so.  
(p. 32)

If there is always some possibility that scientists are not aware of some kind of event and cannot perceive such an event, there can be no such skilled perceiver who is able to perceive all events that exist in experiments. So any perceiver, even a highly skilled one, has some chance of not noticing and not experiencing some types of events. This falsifies (a). If there is always even a slight possibility that some events are not known, then there is always some chance that scientists are unable to repress some possibly intervening mechanisms to identify a given mechanism. Such a possibility can never be avoided no matter how good the experiments they set up because it is possible that unknown events are generated by some other mechanisms. As Bhaskar (1975) argued, events are caused by mechanisms, and unknown mechanisms always contain the possibility that scientists will be unable to identify the mechanism in question, and so (b) can never obtain.

It is obvious from the above argument that Bhaskar's (1975) idea that (2.2) is a special case of (2.1) is flawed, or, if it is true, Bhaskar himself is "dogmatically postulating" that a certain case is (2.2). (2.1) is also misleading since it makes the occurrence of (2.2) possible. I want to suggest a revision that for transcendental realism,

$$d_r > d_a > d_e. \quad (2.3)$$

(2.3) shows, in contrast to (2.1), that transcendental realism can never have a form equivalent to (2.2), and so it is always distinguishable from empirical realism. This solves the problem of the inconsistency in Bhaskar's arguments, and it is better suited to his central thesis that transcendental realism and empirical realism are two distinct theories.

To sum up the relationship between classical empiricism, transcendental idealism, and transcendental realism, classical

Table 2.2: Three Domains and Three Schools

	Domain of Real	Domain of Actual	Domain of Empirical
Transcendental Realism	✓	✓	✓
Transcendental Idealism		✓	✓
Classical Empiricism			✓

empiricism is concerned only with the domain of the empirical, but for transcendental idealism the existence of the domain of the actual is necessary. Transcendental realism is the only theory that, in addition to the domains of the empirical and the actual, recognizes the existence of the domain of the real (see Table 2.2). It is clear from the fact that for transcendental realism the domain of the real is critical that “the aim of science is the production of the knowledge of the mechanisms of the production of phenomena in nature that combine to generate the actual flux of phenomena of the world” (Bhaskar 1975, p. 17). It is not enough that scientists study our experiences of events or events generated by mechanisms. Science should strive to identify the mechanisms that actually cause the events that enable the existence of our experiences.

## 2.5 Laws and Theories

Before continuing, I want to clarify some terms so that they can be used consistently. Bhaskar (1975) uses the words *causal law* or *law* and *mechanisms* almost interchangeably. Usually they both mean what exists in the domain of the real that has the power to cause events, as in “causal laws discovered in nature do not depend on thought” (Bhaskar, 1975, p. 27). However, the word *law* seems to have two different meanings even within

the literature of transcendental realism, and thus its use is not always consistent. On the one hand, it refers to “that feature of nature which makes such a formulation true” (Collier, 1994, p. 43), which exists in the domain of the real. On the other hand, it refers to “formulations in words or symbols, which constitute part of the discourse of a science” (Collier, 1994, p. 43). Indeed, Bhaskar (1975) also sometimes uses the term in this second sense, as in “they [laws] are *statements* [italics added] about the ways things act in the world . . . and would act in a world without men” (p. 17).

From now on, I will use the term *law* only in the sense that it exists in the universe independently of humans and will use the term *theory* to refer to statements about laws or what Salmon (1992) named *law-statements*. So a theory is the description by humans of a law, and as such it may be susceptible to change. It is the transitive object of knowledge. But a law itself does not change in the way a theory changes, because it is the intransitive object of knowledge. This is in sharp contrast to the way Collier (1994) uses the terms. He uses the terms *law* and *theory* to refer to the same entity and the term *mechanism* for what I mean by the term *law*. I prefer to use the term *law* in this way because whenever it is necessary to refer to a law-statement, I can call it a *theory*. This distinction should be carefully sustained, because confusion over the use of the term *law* can easily ruin the distinction between the transitive and intransitive objects of knowledge.

The reason that I prefer the term *law* to *mechanism* when discussing transcendental realism in this chapter is that the term *law* is analyzed more than the term *mechanism* in the philosophy of science. Philosophers of science have provided formal definitions for the term *law*, and despite its sometimes inconsistent use by philosophers, it is analyzed much more than the term *mechanism*. For example, Salmon (1992) argues that a

statement that a law exists or a *law-statement* has the form

$$\forall x(P(x) \rightarrow Q(x)). \quad (2.4)$$

For example,  $P(x)$  is “ $x$  is a gas kept in closed containers of fixed size” and  $Q(x)$  is “ $x$  exerts greater pressure when heated.” But Armstrong (1983) criticizes this theory of a law saying that many laws are not of this form, and he presents an alternative theory of a law. I will not discuss the various theories of what a law is here, but want to point out that contrary to the concept of a law, which has been analyzed by philosophers of science for ages, the concept of mechanism is hardly even analyzed, and so it is much more vague in its use. However, some recent studies have been done on this problem, the results of which ultimately suggest that laws and mechanisms are of a similar kind.

Machamer, Darden, and Craver (2000) presented a provocative analysis of the concept of a mechanism using examples from neurobiology and molecular biology. Although they found that *mechanism* is a pervasive term in philosophy and science, “there is no adequate analysis of what mechanisms are and how they work in science” (p. 2). Their analysis encompasses only neurobiology and molecular biology, but they believe that such an approach may well apply to other sciences, and it may be even applicable to mental and social mechanisms. Although the concept of a mechanism had been studied before, their survey of the literature shows that it has not been analyzed in detail. They have found that, for example, Schaffner (1993) claimed that *mechanism* is an “unanalyzed term” (p. 287) to be avoided. It is an important discovery by Machamer et al., however, that Schaffner considered mechanisms to be dependent on laws, from which it is possible to infer that he thought that mechanisms and laws are basically of the same kind.

Basically Machamer et al. (2000) provides a similar line of argument by saying that “laws are taken to be determinate regularities” that “describe something that acts in the same way under the same conditions,” and “this is the same way we talk

about mechanisms and their activities” (p. 7). Although the point of their paper was to show how mechanisms work in neurobiology and molecular biology, and not to examine the relationship between the concepts of mechanism and law, they still draw the important conclusion that mechanism and law work in similar ways in science. This supports my argument that mechanism and law can be used interchangeably as Bhaskar (1975) does, and there is no fundamental problem in using only one of them to avoid any unnecessary terminological confusion.

## 2.6 Open and Closed Systems

Bhaskar (1975) does not regard laws as regularities that hold over space and time. From his analysis of experimental activity, Bhaskar distinguishes *open* and *closed systems*. He argues that if empirical realism were right, it would not be possible to understand why scientists conduct experiments and consider them to be so significant in scientific activities. Thus to understand the existence of scientific experiments, two kinds of world, the open world and the closed world, should be presupposed.

Open systems are the normal conditions under which laws operate, and under such conditions no constant conjunctions or regular sequences of events are coming. For empirical realists, laws are conjunctions of sequences of events, but for transcendental realists, laws are real entities that are able to produce sequences of events. Closed systems are systems in which constant conjunctions occur, and such systems do not exist in nature, but must be set up by humans by means of experiments. So empirical realists fail either to explain the reasons for scientific experiments or to prove the existence of laws in open systems.

Suppose that laws are merely conjunctions or regularities of events that do not exist in reality, but only in human minds. Then there is apparently no point in establishing closed systems,

because regular sequences of events can occur in any system, and there is no reason to suppose that experiments let scientists discover laws more easily than in natural conditions. Or if more regular sequences of events can be found in closed systems, it might be the case that certain laws exist in that condition. For example, if it is found that whenever P takes place, Q follows. Because laws are conjunctions of events, they obviously follow (2.4). However, in open systems, such laws do not necessary obtain. So it is necessary to conclude that laws do not exist in natural conditions at all, and they exist only in the scientist's mind during experiments.

For transcendental realism, laws do exist in reality, but there are so many laws in reality that it is too hard for scientists to identify them in natural settings, that is, in open systems. That is why it is necessary for scientists to establish experimental conditions or closed systems to identify laws. Otherwise the task of identifying laws becomes an impossible task. So for transcendental realism, "the intelligibility of experimental activity presupposes that a constant conjunction is no more a necessary than a sufficient condition for a causal law" (Bhaskar 1975, p. 33). This view is radically different from the one held by empirical realists for whom a constant conjunction is the very necessary and sufficient condition for the existence of laws.

The ideas of open and closed systems proposed by Bhaskar (1975) is closely related to his theory of the three domains. For transcendental realism, events and laws exist in separate domains, the domains of the actual and the real, respectively. For empirical realism, they exist in the same domain; laws are what humans experience, and so they exist in the domain of the actual. For transcendental realism, conjunctions of events are not necessary because science searches for laws, not conjunctions. For empirical realism, laws are the same as conjunctions of events.

The crucial idea that follows from the distinction between open and closed systems is that the absence of regularity of

sequences of events does not mean the absence of laws governing such events. This suggests that events that at first sight do not show any kind of regularity can possibly be still governed by laws. Bhaskar (1998) developed *critical naturalism* on the basis of his transcendental realism, and according to critical naturalism, difficulties in finding regularities in the mental and social sciences do not mean that it is impossible to find any laws in the realm of mind and society. It is virtually impossible to create closed systems in the mental and social sciences, which makes it difficult for scientists to identify laws, but there is still a chance to find mental and social laws. McIntyre (1996) identified the problem vividly as follows:

It is usually antecedently believed that social science cannot use laws, and the nature of the debate about them has largely — in recent years — consisted of disagreement over the adequacy of each attempted inference to the best explanation for why there are in fact no social scientific laws. The issue, therefore, doesn't seem to be whether there are or could be laws. Rather, the debate is over finding the right peg upon which to hang the failure of social scientists to produce them. . . . And no matter what the outcome of this debate, many believe the result will be the same: Laws will not be forthcoming. (p. viii)

Transcendental realism gives an alternative view of the problem, according to which it is not that laws do not exist, but that the conception of laws is flawed.

Bhaskar's suggestion of separating the idea of the regularity of events from that of laws was published in 1975. I see interesting correlations between his idea and the recent developments in what Kauffman (1995) calls the *sciences of complexity*. The results of the sciences of complexity seem to support Bhaskar's conception of laws, and combined with these recent developments, transcendental realism becomes a powerful alternative

to the current antinaturalist trends of the postmodern era.

## 2.7 Nonlinearity and the Status of a Law

I suppose that Bhaskar was not aware of the existence of the theory of nonlinear systems when he wrote *A Realist Theory of Science* in 1975. According to Waldrop (1992), it was at the beginning of the 1980s that physicists began to realize that complicated systems could be described by the theory known as nonlinear dynamics. But what is now called *chaos* was already known around 1975 (Alligood, Sauer, & Yorke, 1996), and its origin goes back as far as the nineteenth century. So it is reasonable to conclude from the fact that Bhaskar (1975) never mentioned such ideas in mathematics and physics that he did not think that his theory was closely related to what is nowadays called nonlinear dynamics and the sciences of complexity. I want to show that Bhaskar's idea of the existence of laws in events that look irregular seems to work in science. Unfortunately, he stayed within the pure world of philosophy and never showed real examples from science. So he did not analyze such sciences or predict that such sciences would emerge in the future. This trend in transcendental realism seems to continue today. In a recent textbook on transcendental realism, the philosopher Collier (1994) did not provide any analysis of the cutting-edge sciences of complexity. Similarly, in a recent collection of writings by transcendental realists and critical naturalists from diverse disciplines (Archer et al., 1998), discussions of complexity, let alone the relationship between Bhaskar's idea and the recent physical sciences, cannot be found.

To show why I think Bhaskar (1975) is right about laws in open systems, it is necessary to show how events that seem to have no conjunctions can be actually governed by laws. And because nonlinear science appears to be a good example of how



it really is the case, it is necessary to present how linear systems differ from nonlinear systems. As Holland (1995) notes, “it is little known outside the world of mathematics that most of our mathematical tools, from simple arithmetic through differential calculus to algebraic topology, rely on the assumption of linearity” (p. 15). It seems to me that the arguments put forward by empirical realists correlate with the studies of linear systems, whereas the arguments made by transcendental realists that conjunctions of events are not necessary or sufficient correlate with the studies of nonlinear systems.

Linear systems are systems that can be described by linear functions. Linear functions are defined as follows:

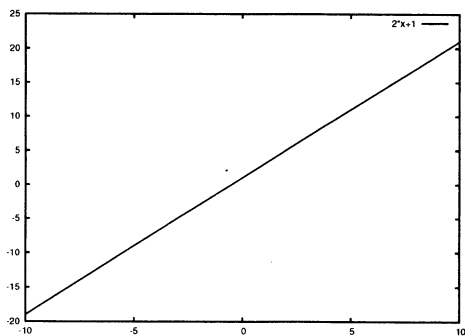
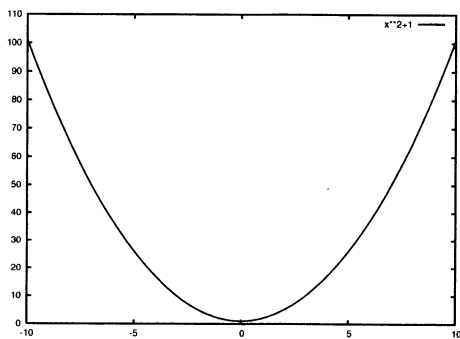
A function  $g$  defined for all real  $x$  by a formula of the form

$$g(x) = ax + b$$

is called a linear function because its graph is a straight line. (Apostol, 1967, p. 54)

In linear systems, the value of a function is the sum of variables. Let us assume that  $a = 2$ ,  $b = 1$ , and so  $g(x) = 2x + 1$ . If  $x = -1$ ,  $g(x) = 2$ , and if  $x = 2$ ,  $g(x) = 5$ . Because the graph of  $g(x)$  is a straight line, as the value of  $x$  becomes smaller, the value of  $g(x)$  becomes smaller, and as the value of  $x$  becomes bigger, the value of  $g(x)$  becomes bigger (see Figure 2.2).

In nonlinear systems, the value of a function is not the sum of variables, but it is a result of the interaction of variables. A simple example of a nonlinear function is  $y = x^2 + 1$  (see Figure 2.3). Now let us consider a real example of a nonlinear system, the populations of predator and prey, described by the Lotka-Volterra equation (Lotka 1956). As an example, let us assume that the predators are wolves ( $W$ ) and the prey are rabbits ( $R$ ). When there are enough rabbits for the wolves to eat, the number of wolves grows. But as the wolves eat the rabbits, the number of rabbits decreases, and food gets scarce

Figure 2.2:  $y = 2x + 1$ Figure 2.3:  $y = x^2 + 1$

for the wolves. So the number of wolves decreases too. But as the number of wolves decreases, there are fewer wolves to eat the rabbits, and the number of rabbits starts to grow. And the wolves have enough rabbits to eat again, and on and on. It is also important to note here that the variables interact with each other; in this example, wolves and rabbits interact with each other (and wolves eat rabbits, as the story goes).

This system can be formulated in the Lotka-Volterra equation. Let  $R(t)$  be the number of prey and  $W(t)$  be the number of predators at time  $t$ . Then the system of wolves and rabbits is as follows:

$$\frac{dR}{dt} = kR - aRW \quad \frac{dW}{dt} = -rW + bRW, \quad (2.5)$$

where  $k$ ,  $r$ ,  $a$ , and  $b$  are positive constants. It is important to notice in (2.5) that there is an interaction between rabbits and wolves  $RW$ . As is clear from the above description, the growth rate of rabbits and wolves are not linear. It is not always the case that when the number of rabbits increases, the number of wolves decreases. At certain time  $t$ , the growth rates of rabbits and wolves change from positive to negative.

Let us consider another example, which is more interesting in terms of the discussion of open systems. Suppose, as empirical realists claim, that there must be regular sequences of events; regularity here is equivalent to linearity. If so, then, it is impossible to find any mechanism in systems of the following type. Here is an example of a discrete dynamical system described in Alligood et al. (1996):

$$f(x) = 4x(1 - x). \quad (2.6)$$

Let us assume that the parameter  $n$  is either zero or a positive

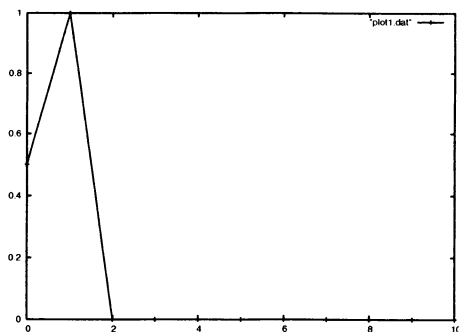
integer and  $x_n$  is obtained by calculating  $f(x_{n-1})$  as follows:

$$\begin{aligned}x_1 &= f(x_0) \\x_2 &= f(x_1) \\x_3 &= f(x_2) \\&\vdots \\x_n &= f(x_{n-1})\end{aligned}$$

For example, if  $x_0 = 0.5$ , then

$$\begin{aligned}x_0 &= 0.5 \\x_1 &= 1 \\x_2 &= 0 \\x_3 &= 0 \\&\vdots \\x_n &= 0.\end{aligned}$$

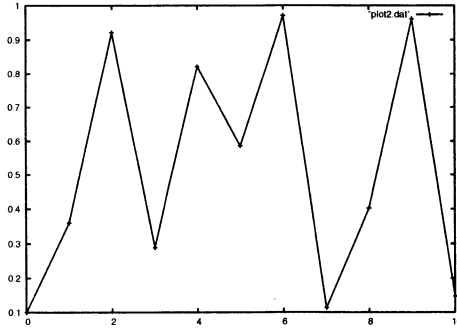
In the above example, the value of  $f(x)$  is eventually fixed to 0, and it is easy to see a regularity in it (see Figure 2.4). But it is not always the case that there is a fixed point. Let us assume

Figure 2.4:  $x_0 = 0.5$ 

that  $x_0$  is 0.1. Then the following can be observed:

$$\begin{aligned}x_0 &= 0.1 \\x_1 &= 0.36000000000000004 \\x_2 &= 0.9216 \\x_3 &= 0.28901376000000006 \\x_4 &= 0.8219392261226498 \\x_5 &= 0.5854205387341974 \\x_6 &= 0.970813326249438 \\x_7 &= 0.11333924730376121 \\x_8 &= 0.4019738492975123 \\x_9 &= 0.9615634951138128 \\x_{10} &= 0.1478365599132853 \\&\vdots\end{aligned}$$

This is an example of *chaos*. By only looking at the numbers it is virtually impossible to tell if there is any kind of regularity

Figure 2.5:  $x_0 = 0.1$ 

(see Figure 2.5).

Let us go back to the transcendental realist theory of laws (Bhaskar, 1975): the regularity of sequences of events is neither necessary nor sufficient. The mathematical example above clearly supports the transcendental realist idea. If the variable  $n$  is interpreted as discrete time and  $f(x)$  as any other variable, there is no apparent regularity between  $n$  and  $f(x)$ . For example, let us interpret that when  $f(x) > 0.5$  there is an event  $A$  and when  $(x) \leq 0.5$  there is an event  $B$  at a given time  $n$ . Then there is no “regularity” between  $n$  and  $f(x)$ , and it is virtually impossible to predict the occurrence of event  $A$  and the occurrence of event  $B$  over a long period time because of the chaotic nature of the phenomenon. But nonetheless, the relationship between  $n$  and  $f(x)$  is governed by the iteration of (2.6), and thus there is a mechanism behind the numbers produced. This mathematical examination of the debate of the status of laws clearly favors transcendental realism and disfavors empirical realism.

Nonlinear systems cannot be described by linear functions, and their analysis is much more difficult than that of linear systems. However, as I have pointed out, the transcendental

empiricist conception of laws is not defensible. Recent studies in nonlinear systems indicate that regularity and a law are two different things, which opens the way for a new line of research based on whether laws exist in the realm of the mind and society or not.

## 2.8 Stratification

The final idea in transcendental realism to be considered is *stratification*. This idea is related to the above discussions of domains, laws, and open and closed systems. And stratification is the idea on the basis of which my argument against the commonplace confusion of mind, society, and culture will be structured.

Bhaskar (1975) maintained that the universe is not arbitrarily organized, but it is stratified. So there are strata in the universe, and such strata are ordered. Because transcendental realists claim that the things to be studied exist before science exists, it follows logically that the division of science reflects such stratification of the universe.

The notion of stratification suggests that higher-level entities can be studied by lower-level sciences. So plants can be studied through physics, chemistry, and biology, but not through psychology, because the level of plants is lower than the level be studied by psychology. Instead, animals can be studied also through psychology, because they belong to that level of stratification.

One important point of the concept of stratification proposed by Bhaskar (1975) is that it is not events, but laws that are stratified. Each layer of laws in the stratified universe affects the laws belonging to a higher level. And higher level laws are explained by lower level laws. Bhaskar gave an example of a chemical theory, which in essence is as follows: the chemical theory of  $2\text{Na} + 2\text{HCl} = 2\text{NaCl} + \text{H}_2$  is explained by the theory of atomic number and valency, which in turn is explained by the

theory of electrons and atomic structure, which in turn should be explained by some theory of subatomic structure (p. 169). He called this type of explanation *vertical explanation*.

Vertical explanation is commonplace in natural science, as in the above example, where a chemical law is explained by physical theories. Note that the scheme of vertical explanation should not be confused with reductionist explanation. The property of  $2\text{Na} + 2\text{HCl} = 2\text{NaCl} + \text{H}_2$  is not the same as, say, the property of the theory of subatomic structure. It is only that laws at higher levels can be explained by laws at lower levels, but it is not that events appropriately explained by higher-level laws should be explained by lower-level laws.

Another type of explanation besides vertical explanation is *horizontal explanation*. In horizontal explanations, events are explained with reference to laws. To explain events, laws should be used at an appropriate level, because otherwise the explanation misses the point.

As Collier (1994) noted, there are many different ways of distinguishing levels, and the manner of distinction is a matter of debate. Following Ruben (1985), I want to divide entities in the universe into three groups, the physical, the mental, and the social. Ruben actually uses the word *material* instead of *physical*, but the meaning here is ultimately the same. In the literature on transcendental realism, there is a general agreement on the point that the highest level is the social level and the mental level is below it. Below the mental level, there are disagreements, and another possible distinction would be to divide the physical level further into the physical and the biological levels. I have no objection to such a distinction, but the reason why I group all that is not the mental or the social into the physical is that the focus of my argument will be on the relationship between the mental and the social. Also, it is worth noting that this is the line separating what is conventionally called “natural” science and the other sciences.

Bhaskar (1975) considers events in the open systems to be



the result of the power of laws at different levels. In experiments, scientists try to create a closed system such that only one law at one level exists. And because in open systems diverse laws from diverse strata interact with each other, the events that laws suggest may happen do not necessarily follow. Even the laws of physics are not able to predict what happens in an open system where other laws have potential possibility to intervene. So the prediction of events in open systems, according to transcendental realism, is virtually impossible, and it is not necessary for a theory to fulfill such a criterion. Prediction and explanation are strictly distinguished by transcendental realism.

Scientists can be sure that a certain event takes place on the basis of our knowledge of laws only if they let it happen in a closed system. And that is the reason why experiments are so important in science. However, for most of the mental and social sciences, experiments are practically impossible, and researchers have to proceed with open systems. This is certainly a difficulty that mental and social scientists face, but as I mentioned previously, this does not yet mean that it is impossible to identify laws in the mental and social sciences. As Bhaskar (1975) noted, even for the physical sciences (such as astronomy), establishing closed systems and conducting experiments is sometimes impossible, but yet scientists in these fields of research have developed good scientific theories.

## **2.9 Three Elements in One Universe**

Above I have presented an overview of transcendental realism and an outline of how it can be related to modern science, including the mental and social sciences. Transcendental realism is a powerful theory, and it will become the basis of the arguments in the following chapters. However, I find two major problems in this theory, which have especial relevance to the mental and social sciences. In this last section, I want to sug-

gest an alternative theory of the relation between laws, events and experiences that will overcome these problems.

The problem that concerns me is the number of the domains in Bhaskar's (1975) theory. As a reaction to transcendental empiricism's failure to acknowledge the existence of laws as *real*, Bhaskar creates the domain of the real. However, it brings about a new problem of the spatial status of laws. Experiences that are in the domain of the empirical reside in the mind, and there is no problem with this because all three schools (i.e., transcendental realism, transcendental idealism, and classical empiricism) are in agreement on this point. But where do events and the domain of the actual, and laws and the domain of the real exist? Do they exist in the universe as humans know it or somewhere else? But before going on with this problem, let us examine another problem, namely the nature of experiences and its place in the universe.

Events occur in the world, in nature, or in the universe. According to Bhaskar (1975), events do not cease to exist even if human minds do not exist (see section 2.3). However, it is possible to study others' experiences as mental events, as mental scientists do. Others can see our experiences as mental events, and we can see their experiences as mental events. The distinction between events and experiences made by Bhaskar, and for that matter, also by transcendental idealism, come from Descartes's controversial theory of dualism.

According to Descartes, any mental phenomenon such as a sensation of pain or an intentional state of believing is essentially private and subjective, and only a single subject to whom it occurs has direct cognitive access to it (Kim 1998). However, it is possible to study mental events scientifically, as research in psychology and cognitive science suggests. I will not discuss the problem of dualism here, because it has been done elsewhere by philosophers of mind, and want to just note that there are good reasons for avoiding Cartesian dualism in the study of the mind.

From the anti-dualist point of view, mental events and ex-

periences are one and the same thing except for one's point of view. Humans can observe them objectively and experience them subjectively at the same time. So mental events exist in the universe in the same way as other types of events, and their relationship is not one to one, but part and whole.

Bhaskar (1975) did not deal with the spatial properties of the domains of the real, the actual, and the empirical, and subsequently of laws, events, and experiences. But it is impossible to avoid the argument of their metaphysical characteristics, and without such an argument, it is impossible to vindicate that transcendental realism is a sound metaphysical theory. Let  $U$ ,  $L$ ,  $E$ , and  $M$  be the universe, laws, events, and mental events respectively, interpreted as sets. One possible interpretation of transcendental realism is

$$(U = L \cup E) \wedge (L \cap E = \emptyset). \quad (2.7)$$

Here  $M$  is not part of  $U$ , because it does not exist in the domain of the real or the actual.  $M$  is an entity without any spatial property. In contrast,  $L$  and  $E$  have spatial properties and exist in the universe.

Another possible interpretation is

$$U = E, \quad (2.8)$$

and  $L$  and  $M$  exist somewhere else. Here the status of laws is problematic. Bhaskar maintains that laws do not exist in the domain of the actual, and he posits the domain of the real. But then it needs to be asked why it is that humans can experience entities in the domain of the actual, but not in the domain of the real. Is the domain of the real not in the universe as we know it? If so, there is no other choice than to think that laws do not exist in the three-dimensional world where we live. However, if it is possible to posit worlds with more than three dimensions, the fundamental agreement of science collapses; now it is possible to explain the phenomena in the three-dimensional world by citing

any forces that we do not know or will never be able to know. For example, a coin falls down from a tower because divine forces are working. Such a view will allow idealism (see p. 8), but as I showed there are good reasons for not supporting it. (2.8) should be avoided also on the basis of the main line of the transcendental realist arguments. If the universe is stratified, and the stratification of the universe does not mean that events are stratified, but that laws are stratified, as Bhaskar (1975) claims, then laws must exist in the universe. So (2.8) must be wrong.

Now there is only one possible interpretation of transcendental realism left, that is (2.7). (2.7) means that the universe consists of laws and events, but not experiences. In other words, it means that the domain of the real and the actual belong to the universe, but not the domain of the empirical. Note here that  $L$  and  $E$  are disjoint, and there are no elements that belong to both  $L$  and  $E$ . This follows from the discussion in section 2.4 that (2.1) cannot be true, but the revised version (2.3) can be true following the main arguments of Bhaskar (1975).

Because (2.7) is the only one available, according to transcendental realism, it must be true. Yet there are problems even with (2.7) concerning the relationship between  $E$  and  $M$ , because it does not say anything about this relationship. As I have mentioned, transcendental realism seems to be based on Cartesian dualism as concerns the relationship between non-mental events and mental events. From the point of view of dualism, there is no problem with (2.7) even if it does not say anything about the relationship between  $E$  and  $M$ , because  $M$  does not exist in  $U$ .

I have noted that (2.7) must be true because it is the only one left. But it is for Bhaskar (1975) that it is the only one, which does not guarantee that there is no other choice. I want to show that there is another choice and that it overcomes the problems inherent in Bhaskar's theory. To put it another way, current transcendental realism cannot solve the problem of the

existence of mental events.

Let us leave Cartesian dualism now, on which transcendental realism rests, and assume that mental events are part of events in general, that is,  $M$  is a subset of  $E$ . Then we have the following:

$$(U = L \cup E) \wedge (M \subset E). \quad (2.9)$$

All sets that exist according to transcendental realism are included in (2.9), and their relationship is clarified. This is clearly more accurate than (2.7), which has no  $M$  component.

Yet the first problem that I noted at the beginning of this section is not solved in 2.9. The status of laws is still not clear in relation to the universe here. Since events are actual, they are the entities that clearly belong to the universe. But Bhaskar (1975) did not give further arguments as to how it is possible that laws and events exist in the universe, but in different “domains.” What does a “domain” mean after all? I find it too problematic to divide the universe into domains that can be experienced (events) and that cannot be experienced (laws). Does it mean that they somehow exist in different ways in the universe, or is it that humans cannot experience laws but perhaps some other creatures can? Bhaskar’s theory does not touch this problem, and such an artificial division of domains within the universe is difficult to imagine.

Transcendental empiricists suppose that there are in fact two types of universe: One is the objective one, where events occur, and the other is the subjective one, where our experiences (or mental events) occur. Let the objective universe and the subjective universe be  $U_o$  and  $U_s$  respectively. Then the transcendental empiricists’ claim is as follows:

$$(U_o = E) \wedge (U_s = M) \wedge (L \subset M). \quad (2.10)$$

Transcendental realists would object to the part of  $L \subset M$  in (2.10), and instead of placing  $L$  in  $M$ , they posit a third

universe, the universe of the real  $U_r$ .

$$(U_r = L) \wedge (U_o = E) \wedge (U_s = M). \quad (2.11)$$

However, this does not solve the problem at all, because it would necessitate a detailed discussion of the status of  $U_r$ , which Bhaskar (1975) does not provide. Even if the Cartesian dualism inherent in (2.10) and (2.11) can be overcome, the problem of the status of the real universe and laws does not disappear.

Because making a new universe to overcome transcendental empiricism does not seem to succeed, I want to approach the problem by staying within transcendental empiricism but interpreting the whole thing differently. So I present a non-Cartesian version of (2.10) as follows,

$$(U = E) \wedge (M \subset E). \quad (2.12)$$

(2.12) advances from (2.10) by incorporating  $M$  into  $E$ .

Although  $L$  is not included in (2.12), it does not mean that laws do not exist in the universe. I still maintain the claim by transcendental realists that laws do exist in reality, and they are not what humans imagine in the mind as transcendental empiricists claim. What I want to show is that the attempt to put  $L$  into (2.12) as well as (2.11) is misleading.

All kinds of  $U$  that have been considered so far are about *entities*. All the events including mental ones that occur in the three-dimensional world are entities. But what if laws are interpreted not as entities but as *properties*? Bhaskar (1975) maintains that laws are entities, and that is why it was necessary for him to posit the domain of the real in addition to the domain of the actual. But the whole problem of the existence of the domain of the real and of laws can be avoided if entities and properties are distinguished and events are considered to be entities and laws to be properties. This is indeed the argument put forward by some theories of laws. For example, Armstrong (1983) argued that laws are *universals*, that is, kinds of properties. Considering laws to be properties is completely different

from considering them to be non-real. Even though laws are real, they can still be properties, and the next task is then to show that properties are real. So I argue that the controversial features of transcendental realism can be solved by changing the point of view, and moving from the sole argument about entities to entities and properties. So from my point of view the universe can be expressed as

$$(U = E) \wedge (E \supset M) \quad (2.13)$$

where the entity  $E$  has the property  $L$

This avoids the problematic theory of the “domains” and is still able to maintain the realist claim that laws do exist in the universe. In (2.13) all elements of transcendental realism, experiences, events, and laws are included. Unlike transcendental realism, however, it is not necessary to posit any extra universes, and all three elements can coexist in the single universe. This idea overcomes the difficulty of presupposing various universes and also shows how all the elements can exist in the universe in relation to each other.

## 2.10 Conclusions

I have presented a critical review of the essence of transcendental realism established by Bhaskar (1975) in addition to my own revision of its theory of experiences, events, and laws. Bhaskar (1998) later developed *critical naturalism*, which has significantly influenced the social sciences and to some extent the mental sciences. Transcendental realism and critical realism (often combined as critical realism) have been developed by both philosophers and scientists (see Archer et al., 1998) and seem to have formed a school of their own. Although critical realism has had a great impact on the social sciences, especially

on sociology and economics, its arguments in relation to the mental sciences are rather weak compared to those of the social sciences. But the concept of reality as well as society that follows from it is very useful in conceptualizing the relationship between the mind and society. It is this aspect of critical realism that I will extend in the next chapter.



## Chapter 3

# The Metaphysics of Culture and Its Applications

### 3.1 Introduction

In this chapter I will explore the metaphysics of culture based on the arguments developed in the previous chapter. Culture is one of the most confusing concepts in science as well as outside science, and I want to clarify it through philosophical arguments. Studies of the nature of the concept of culture carried out in anthropology and other disciplines for more than a century were not very successful if the current ambiguity of the use of the word *culture* is examined. Following the analytical scheme of Bhaskar (1975) I will start from metaphysical arguments of what culture is and then move on to consider the epistemological questions of how culture should be studied and what the science of culture should look like. It seems that most of the debates conducted in anthropology and related disciplines on the question of culture

center on the epistemological questions, and researchers have rarely faced the metaphysical problem of what culture truly is. Or there was a pretence of solving such a metaphysical problem on the basis of epistemological solutions, which Bhaskar (1975) criticizes as the reduction of metaphysics to epistemology. My intention here is to separate these problems and confront the metaphysical one directly.

### **3.2 Matter, Mind, and Society**

The notion of the stratification of the universe discussed in chapter 2 has important consequences for the practice of science. If the view that the universe is stratified in an orderly way is accepted, then the sciences should be divided according to the stratification of the universe. Thus the proper procedure would be to first consider what exists for scientific research and then consider how scientists can possibly know about it. As Bhaskar (1998) notes, for transcendental realism things to be studied exist before scientists study them, and so it is necessary to conduct a metaphysical analysis before an epistemological one.

Critical realists maintain that mind and society should be separated as distinct strata of the universe (Bhaskar, 1998; Collier, 1994). There can be many different strata below the stratum of the mind, but I will include all of them in a single stratum, the stratum of matter. The discussion of strata in this chapter centers around the strata of the mind and society, and I will not discuss the nature of any strata below these. Thus there are three strata in the universe that scientists study, and according to the stratum in question I will divide the sciences into the physical sciences (matter), the mental sciences (mind), and the social sciences (society).

To continue the discussion of the relations between mind and society and among the sciences of them, it is necessary to categorize them in a purely philosophical way rather than a socially

accepted way. In the real world the sciences do not have to be practiced in this way by means of, for instance, institutional organization. For the actual implementation of scientific research there are many factors that cause the sciences to be divided in the ways they are in the real world. It is for the purpose of discussing the different strata of the universe and their sciences that I use the terms above. They do not have to have an actual counterpart in institutions, but the sciences divided in the above way exist in the logical world.

The disciplinary boundaries of different sciences vary across time. They also vary geographically, from country to country and from institution to institution. Let us examine the widely known term *social science*. This is perhaps the most problematic term to be examined here, because the distinction between the strata of mind and society is not often made clear in the conventional use of the term. For example, it is quite usual to find an institution that includes, say, a psychology department in a “social science” division. “Social science” is also used as something opposed to “natural science” and the humanities. So the conventional use of the term *social science* does not function as something referring to the sciences of the stratum of society. A similar problem arises with other names. For example, the term *cognitive science* does not usually include psychiatry, which definitely has something to do with the mind but is traditionally categorized as part of medicine. In addition, perhaps not all psychologists would be happy to be called cognitive scientists. As a matter of fact, the utopia of the comprehensive science of mind proposed at the dawn of cognitive science seems to be far from reality; as a recent survey (von Eckardt, 2001) of the disciplines that contribute to cognitive scientific journals showed, most studies in cognitive science are conducted by psychologists (more precisely, cognitive psychologists) and computer scientists (more precisely, artificial intelligence researchers).

A question inevitably arises here: How is each scientific discipline defined as independent? It might seem that each disci-

pline defines itself according to the object of study, but actually it is not clear at all how it is in fact defined. There are several ways to define a discipline. For example, some want to define it according to its methods. For example, though both are concerned with language, linguistic anthropology is considered to be separate from linguistics partly by its fieldwork methods, though those who call themselves linguists also conduct fieldwork nowadays. Others want to define a discipline according to its object of study. For example, political science is concerned with politics whereas economics is concerned with the economy. Perhaps it is best to think that each discipline has come to be defined by its history and tradition. Only a handful of scientists who are philosophically oriented and concerned with metatheories of their own discipline care about such disciplinary problems, and in most cases disciplinary boundaries are drawn as former practitioners of the discipline have drawn them (Manicas, 1987). What makes the problem of the boundaries of scientific disciplines more difficult is the expansion of interdisciplinary or multidisciplinary research in recent years. As Bechtel (1986) shows the analysis of disciplinary boundaries is a highly complex and yet important problem that needs to be studied along with the history of different disciplines.

So to discuss disciplinary boundaries at the purely philosophical level, it is necessary to use terms that reflect the stratification of the universe in a one-to-one relationship. Following the recognition of the three strata, there should be, then, three types of science: the physical sciences, the mental sciences, and the social sciences. On the basis of transcendental realism, Bhaskar (1998) argues for an absolute distinction to be made of the mental and social sciences, which are often confused:

I want to *distinguish sharply* [italics added], then, between the genesis of human actions, lying in the reasons, intentions and plans of people, on the one hand, and the structures governing the reproduction

and transformation of social activities, on the other;  
and hence between the *domains of the psychological  
and the social sciences* [italics added]. (p. 35)

How does such a sharp distinction help scientists? The most important consequence of such an analysis is that they are able to realize otherwise unknown characteristics of the sciences and find out possibilities for co-operative research. For example, psychiatry institutionally belongs to medicine. But as far as it is the science of *mental* disease, it belongs to the mental sciences. What is more interesting is that a discipline that is thought to be one can be revealed to be in fact a discipline spanning more than two strata. Medicine is often attached to biology, and it is thus part of the physical sciences. But it also includes elements of the mental sciences as well as the social sciences, especially when it deals with public health. Indeed, Engel (1981) proposed a biopsychosocial model of medicine, and from such discussions it is easy to see that medicine spans over three different strata of the universe. Whatever their goals might be, some sociologists are actually engaged in the mental sciences when they are studying the life history of a person. Artificial intelligence is often associated with cognitive psychology and thus the mental sciences, but in recent years, in the field of social simulation (see Gilbert & Troitzsch, 1999), artificial intelligence researchers are actively participating in the study of society. In short, what I will call *vertical separation* is the key to the philosophical analysis of the relation among the sciences in different strata.

### 3.3 Vertical and Horizontal Separation

By dividing the universe and the sciences that explore it into strata, I am conducting a vertical separation. The opposite of vertical separation is what evolutionary psychologists call *vertical integration*: Its main thesis is that in the same way as the “natural sciences” successfully integrate “vertically,” so

should the “social sciences” (Cosmides, Tooby, & Barkow, 1992; Barkow, 1994). Cosmides et al. explain that vertical integration

refers to the principle that the various disciplines within the behavioral and social sciences should make themselves mutually consistent, and consistent with what is known in the natural sciences as well. . . . The laws of chemistry are compatible with the laws of physics. . . . The natural sciences are understood to be continuous. . . . Such is not the case in the behavioral and social sciences. Evolutionary biology, psychology, psychiatry, anthropology, sociology, history, and economics largely live in inglorious isolation from one another. . . . In these fields, paying attention to conceptual integration and multidisciplinary compatibility, while not entirely unknown, is unusual. (p. 4)

I agree with the basic idea of vertical integration, and it is actually one of the main concepts that Bhaskar (1975) advanced in his theory of transcendental realism. However, when the arguments of evolutionary psychologists are analyzed in greater detail, I cannot help wondering if they have a grasp of vertical separation, which needs to be carried out *before* vertical integration is done. If an attempt is made to integrate things, it can be done because separate things exist. But it seems that evolutionary psychologists are integrating things that are not separate in the first place. Barkow (1994) showed how the field of evolutionary psychological anthropology can be advanced by vertically integrating evolutionary psychology and psychological anthropology. He argues that evolutionary psychological studies of psychological mechanisms should be integrated with psychological anthropology to study social institutions. The characteristics of psychological anthropology will be discussed in more detail in section 3.7, and it is enough to note here that psychological anthropology should be conceived as a mental sci-

ence. Thus Barkow's argument does not integrate disciplines, but misunderstands the meaning of disciplinary boundaries and messes them up.

Although evolutionary psychologists argue for vertical integration, and it is a positive way of thinking, because of their lack of the understanding of vertical separation, scientific integration similar to that done in the physical sciences cannot be achieved according to their scheme. In particular, the confusion of the mental sciences and the social sciences is the commonest and worst mistake for scientists to make: If scientists go against or ignore the stratified structure of the universe, they can never understand the universe.

So even though vertical integration as done mostly in the physical sciences is elegant, in the case of the mental and the social sciences, the first thing to do is vertical separation because unlike the physical sciences, the vertical division of the disciplines is not yet clear. In the physical sciences, vertical separation has already been done and that is why vertical integration is successful; for example, theories of chemistry are in harmony with those of physics, and yet they describe mechanisms at different strata. Only after vertical separation is done, does it become possible to move on to vertical integration. Unfortunately, the problem — and the difficulty of solving it<sup>1</sup> — of the strata of the mind and society is so huge that it is impossible to proceed straight to vertical integration as in the physical sciences.

In addition to vertical separation and integration, *horizontal separation* and *integration* should also be considered. By horizontal separation and integration I mean the analysis of the various sciences that study the same stratum of the universe. For example, the debate about the relationship between linguistics and psychology or sociology and economics pops up from

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<sup>1</sup>Consider the difficulty of the mind-body problem, which is a problem of the relation between the stratum of mind and that of matter.

time to time, and this sort of debate is actually on horizontal separation and integration. For example, when it is discussed how the separate disciplines of the sociology of economy and economics differ and whether they can be fruitfully integrated, the problem is about horizontal integration. In this case, both disciplines study the same stratum of society and the same topic within that stratum. So if we imagine a three-dimensional space represented by the  $x$ -axis,  $y$ -axis, and  $z$ -axis, vertical separation and integration are about the identification of points of the  $z$ -axis, and horizontal separation and integration is about the relation of the points on the  $xy$ -plane.

The focus of the following arguments is on the strata of mind and society, and consequently on the relation between the mental sciences and the social sciences. My aim is to show that culture in fact belongs to the stratum of mind despite the pervasive conflation of culture and society, as it often appears in a phrase such as “Finnish culture and society.” To begin with, it is necessary to analyze the concept of society in greater detail to fully understand the relation between culture and society.

### 3.4 Individualism

There are many theories of what society is and consequently what social scientists should study. However, after all, there are only two views of the nature of society: (*methodological*) *individualism* and *anti-individualism*. There are several versions of anti-individualism, and the claims here are not as coherent as those of individualism. Briefly speaking, individualists claim that society is reducible to individual behavior, and facts about society can and should be explained as facts about individual behavior. In this view, economy, for example, is equal to people’s behavior of buying, selling, and so forth.

There are at least two kinds of anti-individualism: *holism* and *relationalism*. Bhaskar (1998) distinguished them with the



adjectives *collectivist* and *relational*, but because the debate is often called the individualism–holism debate in the literature of the philosophy of science, I prefer to use the term *holism* instead of *collectivism*. Archer (1995) developed a social theory based on transcendental realism and uses the term *morphogenesis* for a concept similar to Bhaskar’s relationalism. Even though the theory of morphogenesis is fascinating, it can be seen as a version of relationalism, and I will not count it as a separate theory here.

Holists argue that the whole is not reducible to parts or that society is not reducible to individual behavior. For example, Kincaid (1996) defined holism as follows:

- (1) there can be good social science that explains in terms of social entities—such as classes, institutions, and so on—and their characteristics;
- (2) the social sciences must explain partly in terms of social entities and their characteristics. (p. 6)

So holists believe that there are social entities that are not reducible to individual behavior. I do not feel comfortable with the common use of the term individual behavior because, as a matter of fact, this debate is about whether the stratum of society is differentiable from the stratum of mind, and so I will turn the phrase “society versus individual behavior” into “society versus mind.”

As it may be clear from the previous chapter, I argue that there is society apart from, and not reducible to, mind. Bhaskar (1998) proposed an alternative version of anti-individualism, relationalism, which differs from holism significantly. As Collier (1994) notes, the relational concept of society advanced by Bhaskar has its roots in the materialism of Karl Marx. Bhaskar identified Marx’s theory as relationalist and Émile Durkheim’s theory to be collectivist or holist about the object of social scientific research. So he in fact groups the theories of the objects of social science into three as we can see in the following statement:

The *relational* conception of the subject-matter of sociology [read social science] may be contrasted not only with the *individualist* conception . . . but with what I shall call “collectivist” conception, best exemplified perhaps by Durkheim’s work. (Bhaskar, 1998, p. 30)

Though Bhaskar (1998) makes three groups to be analyzed on the conception of society, I believe that such an argument can be taken up in connection with the individualism–anti-individualism debate after all. I admit that relationalism and holism are two distinct theories, but I do not think that the nature of their difference is unlike the one between individualism and anti-individualism. So I will treat Bhaskar’s theory of society not as one opposed to both individualism and holism, but one that is opposed to individualism but revises holism.

In any case it is necessary to explore the nature of society up to the point where it is possible to identify it to some extent before going on to social scientific discussions. Some social scientists might think that this kind of work is worthless. An exact answer to the question of what society is can never be found, and so it is just a waste of our time to think about the question. But for transcendental realists or those who believe in the existence of real mechanisms, it is a necessary question to be asked for the social sciences:

I shall concentrate first on the ontological question of the properties that societies possess, before shifting to the epistemological question of how these properties make them possible objects of knowledge for us. This is not an arbitrary order of development. It reflects the condition that, for transcendental realism, it is the nature of objects that determines their cognitive possibilities for us. (Bhaskar, 1998, p. 25)

Because the objects of study, in the current case society, exist *before* scientists study them, it would be a waste of time if the

question was not explored. How is it possible to study an object  $O$  when it is not identified what  $O$  is? Naturally, during the course of the investigation of  $O$  scientists will know more and more about it. But for transcendental realists because  $O$  is not created by humans but it exists independently of our experiences, it is necessary to start from the examination of the nature of  $O$  to produce the knowledge of it, and the examination of the nature of  $O$  should proceed side by side along with the production of the knowledge of it.

### 3.5 Relationalism

The dichotomy between individualism as originated in Max Weber and anti-individualism as originated in Émile Durkheim is so fundamental in the philosophy of the social sciences and in the social sciences that “the various schools of social thought — phenomenology, existentialism, functionalism, structuralism, etc. — can then be seen as instances of one or other of these positions. And the varieties of Marxism can then also be neatly classified” (Bhaskar, 1998, p. 31–32). Relationalism is no exception, and it is a variant of anti-individualism.

According to relationalism, society is not created by people, but it pre-exists people, and pre-existent society is reproduced or transformed by people. The properties that society possesses are different from the properties that individuals possess. For relationalism, society is not reducible to individual behavior as individualists claim, and the nature of society and individual behavior should be differentiated. Because society exists in reality, people learn to reproduce or transform it, and such a process is often called *socialization*. Society is often reproduced by individual behavior, but it is sometimes transformed because of the change in individual behavior. For example, revolution is a mode of the transformation of society, where people behave differently from the way they used to. But often society is re-

produced or transformed by people unconsciously. For example, teachers usually do not teach consciously to reproduce an educational system, but it is as a result of such teaching that the educational system is reproduced. When we feel thirsty and buy a bottle of juice, we do not buy it because we want to reproduce our economic system, but because we feel thirsty. But it is through such an action as buying that society is reproduced.

If society is conceived as the relations of people, a strict distinction between society and group as aggregations of people can be made. But as Bhaskar (1998) puts it, "it turns out that most individualists regard 'the social' as a synonym for 'the group'" (p. 28). For relationalism, society does not consist of behavior of groups, but of "the persistent relations between individuals (and groups), and with the relations between these relations (and such relations and nature and the products of such relations)" (pp. 28–29).

The distinction between society and an aggregation of people such as a group or a population is not usually taken seriously among mental scientists and social scientists. It is often taken for granted that the opposite of "social" is "individual," and whenever an event involves more than two people, that is, whenever the event cannot be characterized as "individual," it is automatically labeled as "social" without question. However, from the relationalist perspective, this line of thought is wrong. Even if an event cannot be characterized as individual, it does not necessarily follow that it is social because for an event to be social it must involve relations among people. So I will strictly distinguish society and a population. In the stratification of the universe, an individual (a person) and a population of them (people) belong to the same stratum. But society and people belong to different strata. A population can have social properties if social relations exist in it. But it is still necessary to conceptually distinguish a population (the stratum of mind) from society (the stratum of society). The distinction between people and society is hard to make in reality; even though what can

be seen is the same collection of people, there are two different strata existing in it.

The examination of the relationship between different strata may help understand why the line of argument followed by individualists cannot be true. Individualists maintain that society is equal to a population of people. That is, the stratum of society emerges *necessarily* if a collection of entities of that stratum exist. Let us examine some other stratum. Take, for example, the stratum of life and its science biology. And let us suppose that the stratum higher than life is mind, and thus that the science at the level higher than biology is psychology. This would be a conventional way of categorizing the strata in question. Using this example, individualists' confusion between people and society should be easy to see. Biologists study living organisms. They study individual living organisms, but they also study populations of living organisms, and the branch of biology that studies these is called *population biology*. If individualists were right, the science of a population of living organisms necessarily becomes the science of the higher stratum, the stratum of mind. So in that case, population biology should not be part of biology, but is must be psychology. But population biology is not psychology; it is still biology and deals with the stratum of life. Thus even though population biology studies a population of living organisms, it does not deal with a higher level of stratification such as the mind or society.

According to relationalism, scientists do not have to deal with mass behavior to study society as some holists believe they should. Society is equal to neither people nor mass behavior of people. Mass behavior is not subject matter for the social sciences, but it is "an interesting social-psychological phenomenon" (Bhaskar, 1998, p. 29). Contrary to the common belief that a mass of people is necessary for society to exist, only two people can have social relations, and layers of such microrelations create macrorelations such as the world economy. The relationalist conception is thus positive to the microanalysis of

social phenomena if it is not individualist; for example, conversation analysis, which originated out of sociology, is a good example of a sociological study that discovers relations in a small number of people. So when Bhaskar (1998) notes that society consists of relations and relations between such relations, he is arguing that the stratum of society is stratified going from microrelations to macrorelations, which consist of microrelations. So the science of society should also be stratified, and the traditional division of microeconomics and macroeconomics can be vindicated by relationalism.

The above discussion of relationalism conceptually leads to the following next step:

On this [relationalist] conception, not only is sociology [read the social sciences] not essentially concerned with the group, it is not even essentially concerned with *behaviour* [italics added]. (Bhaskar, 1998, p. 30)

This is a potentially controversial argument, because for individualists behavior *is* the subject-matter of the social sciences. Also a great number of social scientists believe that their object of study is behavior (Manicas, 1997). There have been attempts among philosophers to revise or augment this radical idea of Bhaskar's. Manicas (1997), for example, argued that social scientists are misled if they think that the object of social scientific explanations is the concrete behavior of individuals. However, he did not abandon the involvement of behavior in social scientific explanations altogether, but instead developed the concept of *typical individuals* who act. So social scientific explanations do not seek to explain what individuals do in a certain place and time, but how typical individuals do in a rather idealized world.

This kind of explanation is consistent with relationalism if typical individuals are interpreted as certain social roles. Here what people actually do is not explained, but how social roles are

related to each other is explained. I agree with Bhaskar's idea that the social sciences are not concerned with behavior, and want to add another reason for this. Human behavior, that is, the movement of the human body, is caused by laws belonging to multiple strata, from physical to mental. So humans can never run faster than the speed of light because we are governed by physical laws. We are also constrained by biological laws, and we cannot (at least for the moment) live longer than, say, 150 years. In other words, behavior is the outcome of multiple laws, and it is misleading to think that behavior is governed by laws of only one stratum. Behavior is, in short, an event in an open system, and it is not a certain stratum of the universe. Thus defining the relevant stratum of the social sciences in terms of behavior is misleading.

By extending the theory of transcendental realism, it is possible to reach the conclusion that the social sciences are not concerned with people, a population of individuals, or behavior. Instead, society consists of social relations, which are not reducible to any one of those other concepts that are advocates of theories other than relationalism argues for.

### 3.6 The Problem of Critical Realism

Now that I have analyzed and clarified to some extent what society is from the point of view of relationalism, let us move on to examine the concept of *culture*, which may be a more ambiguous concept than *society*. The aim of my argument is to show that society and culture are two distinct concepts on the basis of the relational concept of society and to show that they in fact belong to different strata. The consequences of this argument should be enormous because I will deny that the study of culture has any "direct" relation to the study of society, though it does have an "indirect" relation to it through emergence. Instead of including the study of culture in the study of society, I maintain

that the science of culture is part of the science of mind. The misunderstanding that the study of culture is a study of society is a mistake that has been made for more than a hundred years.

It would be ideal if at least some critical realists, who developed philosophical arguments on the metaphysics of mind and society, their sciences following Bhaskar, had ever done serious research on the relationship between society and culture. Unfortunately, critical realists have rarely taken this problem seriously, and it has almost never been discussed seriously in the literature of critical realism. Whenever critical realists consider culture, they simply conflate culture and society. For example, Manicas (1987) argues that there is no useful distinction between society and culture, and an elaborate study of culture by Archer (1996) showed in the end how culture, the ideational structure, is related to the social structure, the material structure, and she does not deal with the concept of culture on its own.

It is no wonder that researchers are reluctant to examine the place of culture in the stratification of the universe, for at least two reasons. First, the idea of culture has its roots in the humanist thought of the humans rather than in scientific thinking. Because critical realists' arguments are centered on the possibility of scientific research of various strata, arguments that are strongly humanistic in nature are simply ignored. Second, although the definition of culture has been one of the central concerns of anthropology for a long time, it was not the central concern of anthropologists to expand the argument to the general discussion about the stratification of the universe and the place of culture in it.

Unfortunately, critical realism does not help much in an attempt to clarify the concept of culture. Let us have a look at some examples of how critical realism becomes powerless when it comes to the discussion of culture. Critical realists' favorite example of social structure is language. Manicas (1987) notes that language is an example of a social structure, but in the same



book, he says that language is a psychological entity; these are clearly contradictory statements. Also, to explain the nature of society or social structure, Collier (1994) notes as follows:

Language is a good example. Unless we have learned a pre-existent language with rules that exist independently of us we could not talk at all (structure as condition). We talk not as a rule to reproduce or transform the language but for personal ends of which we are conscious (practice as production). But our language only continues to exist because of talk, for it has no existence apart from people talking (structure as outcome). So our acts of talking do reproduce and transform the language, without our for the most part intending it to do so. (p. 146)

But how can any reader take his argument seriously if he writes in the same book that “linguistic structures should not be used as a model for the understanding of social structures” (p. 134)? Bhaskar (1998) also presented a contradictory argument of the same kind. He maintains that linguistics is a social science as he refers to “social sciences (such as linguistics, economics, etc.)” (p. 41). But he also writes that “the rules of grammar . . . are, or must be conceived as, relations” (p. 40).

It should be clear from these examples that critical realist accounts of language are self-contradictory. In the same vein, critical realist accounts of culture are, though rarely provided, largely unsound. Critical realists are good at explaining the nature of society in relation to the mind, but on the nature of mind and culture, their arguments suddenly become weak. It is thus fair to say that the main concern of critical realists is on the nature of society and its science, but not the nature of the mind and its science.

### 3.7 Culture in the Stratified Universe

Because critical realists and philosophers of science in general do not seem to consider the nature of the science of culture seriously, the problem is left to anthropologists. The philosophy of science is often said to “arrive late,” meaning that a study of a problem in science only gets started when the problem has become a serious one. Otherwise, given that there are so many scientific disciplines, and that in the history of the philosophy of science most of the research has been done on physics and other physical sciences, it is no wonder that philosophers of science are not ready to tackle problems in the science of culture. Although an ideal situation would be one in which philosophers of science discuss the problem of culture and its science in great detail, in reality the problem is left to the practitioners of science. Philosophers of science have not arrived on the scene yet.

The first attempt to define culture in the history of anthropology was made by Edward Burnett Tylor in 1871: “that complex whole which includes knowledge, belief, art, law, morals, custom and any other capabilities and habits acquired by man as a member of society.” It is notable in Tylor’s definition that culture is considered to be something *acquired*. The answer to the question of “where do the capabilities and habits acquired exist?” should be “in the mind,” because the mind is the place where acquired knowledge is stored. So, as a matter of fact, from the very beginning of the formal definition of culture, it has a mental characteristic. Tylor did include the word *society* in the definition, but culture is not thought to be equal to society, but to be what humans acquire in society.

So even though cultural anthropology is often included in the “social sciences” in a vague sense, if its subject matter is culture, it is not a social science but a mental science. Cultural anthropologists often object to the idea that their subject matter is the mind calling such idea “reductionism.” But this idea is completely different from reductionism in its real sense. If

objects in a higher stratum are treated as if they are objects in a lower stratum, then it is reductionism. But if the object of study is seen as belonging to a lower stratum despite the common belief that it belongs to a higher stratum, then it is not reductionism. On the other hand, arguing that culture belongs to the stratum of society and that it is about the mind is a case of reductionism. As I have shown in the discussion on relationalism, methodological individualism is a typical reductionist approach. But I argue that culture is not in the stratum of society in the first place, and so this argument should be distinguished from reductionism.

There is a school called psychological anthropology in the history of cultural anthropology. Although it has not been a mainstream school, especially since interpretive anthropology has come to the fore, it is a tradition that has a long history and is still in rigorous development. Psychological anthropology is often considered to be only a branch of many kinds of schools of cultural anthropology rather than a distinct philosophical orientation toward anthropology. But in his historical study of the nature of anthropology Stocking (1992) noted that the founder of U.S. anthropology, Franz Boas, was a psychological anthropologist. He argued that "before Franz Boas became an anthropologist, he was a psychologist . . . what is arguably his single most important piece, . . . as well as most of his major generalizing anthropological statements, may be regarded as essays in comparative psychology" (p. 311). Psychological anthropology is not a branch of cultural anthropology in a strict sense, but a theoretical orientation of anthropology.

As Stocking (1992) pointed out, there are interesting facts in Boas's writings on psychological anthropology. Boas (1904/1974b) states that "the subject-matter of anthropology is partly a branch of biology, partly *a branch of the mental sciences* [italics added]. Among the mental phenomena, language, invention, art, religion, social organization, and law have received particular attention" (p. 23). It is possible to assume that the mental sci-

ences in Boas's sense are more or less equivalent to the mental sciences as the sciences of the stratum of the mind. Boas was talking about vertical separation and was saying that anthropology spreads over two strata of the universe, the stratum of matter and the stratum of the mind. Boas argued that the generalized study of the evolution of culture, which was a common research problem in anthropology at the beginning of the twentieth century, "may be subjected to a further analysis regarding the *psychic causes* [italics added] which bring about the regular sequence of the stages of culture" (p. 27).

There is a significant change in Boas's thought in his paper "The History of Anthropology" (1904/1974b) if it is compared to his earlier paper "Aims of Ethnology" (1889/1974a), in which he remained within the geographical-historical school. Boas (1909/1974c) further advanced the argument for the importance of the mental aspect of anthropology in addition to the geographical-historical questions:

We are also trying to determine the *psychological laws*<sup>2</sup> [italics added] which control the mind of man everywhere, and that may differ in various racial and social groups. In so far as our inquiries relate to the last-named subject, their problems are *problems of psychology* [italics added], though based upon anthropological material" (p. 243).

Boas's work was significant not only in anthropology but also in the history and philosophy of anthropology. He reflected on what anthropology is, and his description was illuminating. Think about the confusing situation one hundred years after Boas made the above statements. Do cultural anthropologists have any clear understanding of what they study? Since the

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<sup>2</sup>This phrase shows that Boas was a naturalist. I will not deal with this problem here, but for naturalism and cultural anthropology, see Sperber (1996).

“revolution” of postmodernism it seems that cultural anthropologists are no longer sure about anything. One may think of this as a progress, but I believe that it is a retreat because cultural anthropologists are nowadays confused about the stratification of the universe. There are two possible explanations of why cultural anthropology is nowadays so different from Boas’s view of it. One explanation is that anthropology was a newly established discipline in Boas’s time, and it was clear what it was. As time goes by, the number of anthropologists grows, anthropological research becomes more and more diverse, and anthropology becomes so complex that anthropologists cannot give an accurate description of what it is. Another explanation, to which I adhere, is that anthropologists are simply losing sight of what they are doing as things get more complex. However, it does not mean that anthropologists cannot be clear about what they do. It is true that anthropology is more complex than in Boas’s time, but it does not follow from this that anthropologists cannot reflect on what they do. Some anthropologists still reflect on what cultural anthropology is and what it should be (e.g., Carrithers, 1990; Sperber, 1996). What is strongly needed is a philosophical analysis of culture and its science.

For Boas (1904/1974b, p. 32) anthropology was a science of human biological and psychological characteristics, and he divided anthropology into two sub-disciplines, *biological anthropology* and *psychological anthropology*. And Boas further divided psychological anthropology into ethnology, archaeology, and linguistics. It is interesting to see, that even though he is often thought to be the father of anthropology as a “holistic” discipline, he noted as follows:

the biological, linguistic, and ethnologic-archaeological methods are so distinct, that on the whole the same man will not be equally proficient in all of them. The time is rapidly drawing near when the biological branch of anthropology will be finally separated

from the rest, and become a part of biology. *This seems necessary* [italics added], since all the problems relating to the effect of geographical and social environment and those relating to heredity are primarily of a biological character" (p. 35).

Boas also argued that for a cogent reason the separation of linguistics from ethnology was inevitable.

Psychological anthropology was thus for Boas a mental science that deals with the mind rather than society, and its branches are ethnology, archaeology, and linguistics. So it logically follows that ethnology, archaeology, and linguistics are sciences of the mind. Indeed, the idea of the anthropological study of the human mind has never disappeared, and as I have noted above, it is nowadays called "psychological anthropology." Note that "psychological anthropology" here is used in a narrower sense than what Boas meant with the term. It means a school of cultural anthropology that studies the relationship between mind and culture. Thus psychologically oriented archaeology is not included in "psychological anthropology," and it often goes under another name of *cognitive archaeology* (Mithen, 1996; Nowell, 2001; Renfrew, 1983; Renfrew & Zubrow, 1994). Interestingly, although the history of cognitive archaeology is quite short compared to the history of "psychological anthropology," it is one of the fastest growing fields in archaeology due to the influence of the recent interest in the ancient mind by cognitive scientists and evolutionary psychologists.

The arguments above are made from the point of view of cultural anthropology, but there are also analyses of the relation of culture and society made by sociologists that lead to the same conclusion: Culture is not in the stratum of society, but in the stratum of the mind. Porpora (1993) sought the possibility of unifying the Winchian and the Marxian traditions in sociology. He came to the conclusion that social relations emerge from cultural rules, which in turn exist at the level of agency. Although

agency is not equal to the mind, the point of his argument in the current context is that he identified the level of culture as something lower than the level of society. So this suggests that his claim was that culture belongs to the stratum of mind and not to the stratum of society, which emerges out of the stratum of the mind.

Ruben (1985) also pointed out that a social property and a social rule are quite distinct ideas. What he meant by a social rule seems to be equivalent to culture, and he gave an example of drinking tea at breakfast, which is a custom shared by some people. Such shared patterns of behavior are what is called culture, and Ruben's analysis of social properties showed that such a social rule is not a social property, and so it does not belong to the stratum of society.

On the nature of culture and especially its relation to society, Kuper (1999) conducted a detailed historical as well as philosophical analysis. He traced the historical roots of the separation of psychology, sociology, and anthropology back to the idea of Talcott Parsons. According to Kuper, Parsons thought that the study of the mind is the job of psychology, society of sociology, and culture of anthropology. After a grand analysis of Parsons's scheme, Kuper came to the following conclusion:

It is a poor strategy to separate out cultural sphere, and to treat it in its own terms. Parsons attempted a synthesis among cultural theory, social theory, and psychology. He failed, however grandly, but unless we separate out the various processes that are lumped together under the heading of culture, and then look beyond the field of culture to other processes we will not get far in understanding any of it.  
(p. 247)

Note here that Kuper was not saying whether culture belongs to society or the mind. He was saying that the concept of culture, in the end, has no analytical power. It is a pity that only at

the end of the book did he cite an alternative approach, namely, the psychological conception of culture proposed by D'Andrade (1995). He only noted briefly that D'Andrade's conception is psychological without his doing any further analysis on the possibility of considering culture as mental. For the current concern, it is enough to conclude that Kuper's detailed analysis of culture, comprising a whole book, shows that culture is distinct from society, and the study of culture should not be confused with the study of society.

I have presented various pieces of evidence for rejecting the widely held view that culture is something like society, culture is an epiphenomenon of society, and the study of culture is about society. On a serious analysis of the concept of culture, it can be concluded that the nature of culture has been mental since the beginning of its definition. I suspect that the reason why culture is such a misunderstood term though it is so commonly used not only in the academic world but also outside of it is that we feel uncomfortable thinking about mind as if it contains culture. To put it in another way, it is the conception of the mind as an individual *tabula rasa* that is wrong (Plotkin, 1998), and that is why we cannot find any reasonable place for culture in the mind. And because we feel that culture does not fit into our everyday conception of mind, we mistakingly conflate culture and society.

Kuper (1999) pointed out that Tylor's definition of culture "included too many elements in culture however, and in particular he did not distinguish between culture and social organization. It was preferable to narrow down the definition, to distinguish culture from society" (p. 57). On the other hand, if social organization is excluded from Tylor's definition, the other elements are mental. So it is possible to conclude from the analysis of Tylor's definition that culture is mental, and from Boas's ideas that the science of culture is a mental science. Does this mean that cultural anthropology is a branch of psychology? I believe that there are still differences between cultural anthro-



pology and psychology even though both deal with the stratum of the mind. In other words, there is a horizontal separation between cultural anthropology and psychology

### **3.8 Cultural Anthropology and Psychology**

From a historical point of view, it is a fact that psychological schools in cultural anthropology have continued to exist, and there are no signs of their disappearing and being incorporated into psychology soon. From a philosophical point of view, cultural anthropology and psychology aim at explaining different kinds of mental events, and so they are not competing research schemes but complementary ones. So the division of cultural anthropology conceived as a mental science and psychology is not just a historical accident, but it exists in the way it should.

First, I will examine why it is that mentally oriented cultural anthropology has never become a branch of psychology. Second, I will consider the possibilities that exist for thinking about the relationship between the mind and culture. If cultural anthropology considers itself to be a science of the mind and to be a science working vertically at the same level as psychology, it must demonstrate its own territory horizontally (see sec. 3.3).

To understand why psychological anthropology and psychology do not share the same subject matter, let us examine what Boas said about their relationship. If he argued that psychological anthropology belongs to the mental sciences and yet established a new discipline of anthropology in the United States, he must have had good reasons for distinguishing psychological anthropology from psychology. He explained it as follows:

It might appear that the domain of anthropology, of "the science of man," is preoccupied by a whole array of sciences. The anthropologist who studies

bodily form is confronted by the anatomist who has spent centuries in researches on the gross form and minute structure of the human body. The physiologist and the psychologist devote themselves to inquiries into the functioning of body and mind. Is there, then, any justification for the anthropologist to claim that he can add to our fund of knowledge?

There is a difference between the work of the anthropologist and that of the anatomist, physiologist, and psychologist. . . . The interest centers always in the individual as a type, and in the significance of his appearance and functions from a morphological, physiological or psychological point of view.

*To the anthropologist, on the contrary, the individual appears important only as a member of a racial or a social group* [italics added]. (Boas, 1928/1962, p. 12)

When Boas (1928/1962) stated that the anthropological interest lies in “a member of a racial or a social group,” he essentially meant that in anthropology an individual is seen as part of a collection of people. As I have explained in section 3.5, a collection of people does not mean society. The study of society is the study of the relations between people and relations between their relations. So the study of an individual as a member of a certain population should be differentiated from the study of society. Boas’s statement suggests that anthropologists are not concerned with an individual person, but a population of people. As Boas rightly noted, psychology is concerned with an individual as an individual, and psychologists do not consider the populational characteristics of individuals. In contrast to psychology, Boas argued that psychological anthropology is concerned with the characteristics of a population of individuals rather than that of an individual.

Because I have shown how the subject matters of cultural

anthropology and psychology differ, I will move on to the current state of research into mind and culture that often goes under the name *cognitive anthropology*. Modern cognitive anthropology considers itself to be part of cognitive science, and as such it is closely related to other branches of cognitive science, especially cognitive psychology. But unlike other branches of cognitive science such as cognitive psychology, artificial intelligence, and cognitive neuroscience, the concept of culture and the relationship between culture and mind are central in cognitive anthropology. Cognitive anthropologists consider culture to be something residing in the mind, and they have characterized culture as a mental object. There have been attempts to consider culture as schemata, models, or representation, all of which are concepts borrowed from cognitive psychology (D'Andrade, 1995). Even though each concept differs slightly, the fundamental point is the same: culture is a kind of mental representation. Sperber (1996) argued that cultural representation is a kind of mental representation that is shared with others whereas individual representation is a kind of mental representation that is not shared with others. According to this view, there is no need to posit anything special about a kind of mental representation that is cultural; it is just that cultural representation is entertained in a population rather than only in an individual.

On the basis of the discussion about the relationship between mind and culture presented above, Shore (1996) vividly expressed the difference between cognitive psychology and cognitive anthropology. According to him, psychologists treat mental representation as *subjective* representation, but anthropologists treat it as *intersubjective* representation. This point is crucial because it shows how fundamentally the conceptions of mind in psychology and anthropology differ. It seems that Shore considered anthropological and psychological approaches to the mind to be competing enterprises, but I maintain that they are mutual approaches because anthropologists and psychologists do research on horizontally separate areas. Anthropologists and

psychologists can co-operate well once they appreciate the differences and similarities of their enterprises (Schwartz, 1992), and moreover, anthropologists cannot ignore the studies of psychological processes in their efforts to understand culture (Strauss & Quinn, 1997).

By developing Boas's and recent cognitive anthropologists' ideas, cultural anthropology can be conceived as the mental science of human populations. Then it is natural to think that cultural anthropology is concerned with how the mind of one organism affects other minds and is affected by them. Sperber (1996) maintained that the science of culture should be the epidemiology of representations. The epidemiology of representations, as the epidemiology of diseases, tries to explain how a certain type of mental representation comes to be shared within a population. Such an endeavor seeks the mental cause of the spread of representation. Boyer (1999) advances this concept and shows how anthropologists can conduct psychological experiments to better answer old questions in anthropology. Cultural anthropology and psychology have been connected in various forms, and they should look at each other's research to achieve their goals.

### **3.9 Process and Content**

Let us now consider the dichotomy between process and content pervasive in the mental sciences. According to the dichotomy, culture is considered to be content, not process, and the mind is the processor that processes such content. Psychology is based on the premise that there is a central processing mechanism universal to humans, and the things that the mechanism processes can be neglected in the search of it (Shweder, 1991). So while psychology studies the mind as a processor, cultural anthropology studies the mind as a place in which the content resides.

As Boyer (1999) puts it, such a traditional view of the mind

as a receptacle of culture is now untenable. The mind has been traditionally seen by psychologists as a *tabula rasa* that processes chunks of symbols, and mainstream cognitive science still takes the view that culture consists of such chunks of symbols. But for anthropology the mind is far from a *tabula rasa*, but it is full of information that humans have learned since birth. To view culture only as a static object that resides in the mind is also problematic, because information stored in our mind changes all the time. Thus it is not correct to think of culture as if it is a thing (Strauss & Quinn, 1997).

If culture is a dynamic object that resides in the mind, does it affect our cognitive processes? As D'Andrade (1995) argued, culture has significant effects on perception, memory, and reasoning. Studying the way culture affects cognition is an important question for both psychology and anthropology. Here the emphasis is on process rather than content. Putting forth the view that culture is a process, Hutchins (1995) argued as follows:

Culture is not any collection of things, whether tangible or abstract. Rather, it is a process. It is a human cognitive process that takes place both inside and outside the minds of people. . . . I am proposing an integrated view of human cognition in which a major component of culture is a cognitive process . . . and cognition is a cultural process.

Actually the phrase "culture is a cognitive process and cognition is a cultural process" does not help much to understand what he meant because it is merely a tautology. I suppose that the point is that he rejected the separation of culture and cognition as two different things. The mind consists of the psyche (processor) and stuff (content), and it is not possible to separate them as if they are two distinct entities (Shweder, 1991).

For example, Hutchins (1980) showed that Trobriand Islanders not only make logical inferences but also reason about

complex things by means of schemas. D'Andrade (1989) showed that U.S. undergraduates were able to conduct *modus tollens*: (a) if *p* then *q*; (b) not *q*; and (c) therefore not *p*, within the domain of well-formed cultural schemas. But about half of the informants were not able to correctly infer according to this rule when the content being processed was not culturally relevant for them. There have been other studies that suggest how cultural knowledge affects human cognitive processes, and it seems unavoidable that culture has to be taken into consideration in the study of the mind.

To understand the stratum of the mind fully, it should be studied both in an individual and in a population. When it comes to the study of the mind, too much emphasis has been traditionally put on an individual mind. But as I will show in the discussions to follow, such an approach does not help us explain some types of cognitive events, including conversation meaning, and it is necessary to tackle a problem from the point of view of the study of the mind in a population.

### **3.10 Conclusions**

In this chapter I applied the theory of transcendental realism presented in the previous chapter and argued that culture belongs to the stratum of the mind. I showed a metaphysical analysis of society based on transcendent realism, and from such an analysis it became clear that culture should not be located vertically in the same place as society. This suggests that the closeness of various disciplines, especially cultural anthropology and psychology, is often neglected. The concept of the mind assumed in psychology is inaccurate, and for that matter, most theories of the philosophy of mind fail to treat culture as a mental entity powerfully engaged in the processes of human cognition. By keeping the effect of culture in cognition seriously in mind, I will review the various disciplines that study conversa-

tion in the next chapter.





## Chapter 4

# Approaches to Conversation

### 4.1 Introduction

If culture is, simply put, a system of shared mental representations, communication should be central to the study of culture. Communication is the process of the sharing of information, and thus it is part of cultural process. Humans use language for communication, and this peculiar feature differentiates them from other animals. As Sperber and Wilson (1995) speculated, communication might be the oldest way of information sharing, and language has appeared only lately in human evolution. It should be the appearance of language that greatly changed the course of human evolution, and what is more clear is the fact that the appearance of language has changed the way of human communication. So linguistic communication or communication involving language is central to our very existence, and culture, communication, and language are interrelated to the extent that they should not be investigated in isolation.

There are diverse modes of linguistic communication in human life. But through the examination of how language has been used in the evolutionary history of humans, it is obvious that face-to-face conversation is the commonest setting of language use (Clark, 1996). Because most of the typical characteristics of human communication are exhibited by conversation, researchers can accumulate considerable knowledge of the fundamental features of human communication by studying conversation. Naturally genres of communication other than conversation include their own sets of standards. In written communication the visual aspect of communication is crucial. Public speeches given by politicians contain certain institutionalized ways of accomplishing it. Technology has influenced the way of communication: the advent of the Internet, for example, has definitely changed the mode of human communication. However, no matter when in evolutionary history and where on the earth a population of humans live, one thing is sure: they have talked and will very likely talk to each other face-to-face, and it is this mundane face-to-face communication that is one of the most central activities in their everyday life. Conversation is the most basic form of human communication, and perhaps even it is the most pervasive form of human behavior.

In this chapter, various traditions of, and approaches to, the study of conversation will be critically examined. The emphasis will be on the meaning of conversation rather than the form it takes. This chapter serves as the basis for the discussion presented in the next chapter, in which I will attempt a synthesis of various approaches and the creation of a new perspective toward conversation meaning. Conversation has been studied by researchers from many different disciplines, who unfortunately have not usually held interdisciplinary discussions with each other. However, the aim of this chapter is not to exhaust all the different kinds of studies relating to conversation that have been conducted. Instead, I will provide a critical overview of some of the key disciplines that have particular rel-

evance to the meaning of conversation. Because of the diversity of the disciplines that study conversation explicitly or implicitly, my goal here is to introduce each discipline separately and to identify what it is as well as what it is not.

## **4.2 Form/Meaning and Mental/Social**

Before presenting the characteristics of each discipline, I want to point out that there are two distinct objects in the study of conversation: form and meaning. This will allow the features of conversation that each discipline particularly looks at to become clear. The purpose of this distinction is not to argue that each discipline belongs to either one of the groups categorized by its object of study. A single discipline might study only one of the two features or both, and whether they aim at knowing only one feature or two will depend on the particular discipline. Still, such an examination will clarify what each discipline seeks to describe and explain in the study of conversation.

The first object of the study of conversation is the form of conversation. The form of conversation is analogous to what grammar is for language. Thanks to the work of conversation analysts, it is now well-known that conversation is structurally a highly organized phenomenon. And it is from such organization that conversationalists are able to process the meaning of an utterance embodied in conversational sequences. The function of the structure of conversation is similar to that of syntax when the semantic meaning of a sentence is processed according to grammatical rules.

The second object of the study of conversation is the meaning of conversation. In the next chapter I will show an elaborate definition of what the meaning of conversation is, but here let us briefly assume that it is analogous to the domain of what semantics and pragmatics is for language. It is the meaning that conversationalists look for in conversation, and it is what

matters to participants in conversation the most. In contrast to form, which does not usually attract the attention of participants, meaning is central to the conversational activities of participants.

The relationship between these two objects of study is sometimes controversial even within a single discipline. For example, in a paper written by a linguistic anthropologist and a conversation analyst (Goodwin & Heritage, 1990) it has been noted that, despite some theoretical controversies, there are arenas for cooperative research. On the other hand, the linguistic anthropologist Alessandro Duranti (1997) argued that the aims of linguistic anthropology and conversation analysis are incompatible, and though there can be positive influences in either directions, the theoretical gap between them remains the same. What is problematic in this argument is that one party does not recognize another party's objective of research as such, but denies the importance of their research program altogether. Duranti used the metaphor of conversation analysis as the *syntax* of conversation, while the linguistic anthropological study of conversation aims at understanding the meaning of conversation. This suggests that linguistic anthropology and conversation analysis are concerned with two distinctive objects, the former with meaning and the latter with form. The problem is the lack of recognition that researchers in these two disciplines are working on different objects.

In linguistics no one would find any rationality in semanticists' arguing that grammarians are not right because they do not study meaning. Similarly, no one would find any rationality in grammarians' arguing that semanticists are not right because they do not study the form of language. Both semanticists and grammarians study language, and the different modes of their work are merely a division of labor within linguistics. Perhaps it is because they are all linguists that they are aware of this division of labor.

However, in conversation research the situation is different.

There is no single discipline like linguistics that studies both the form and meaning of conversation, and the fields that study them are scattered across disciplines. If what drives conversation analysts to study conversation is an interest in the logic of a series of structures such as recurring patterns of certain types of acts and moves, and if linguistic anthropologists study conversation as a means to understand types of phenomena other than conversational structures as Duranti (1997) argued, then naturally there cannot be any compatibility between these disciplines; what they look for in the study of conversation is different in the first place. What researchers ultimately need is not to argue how disciplines differ or which one is right, but to examine how they can be related to each other, as Goodwin and Heritage (1990) showed in their argument that it is possible to synthesize linguistic anthropology and conversation analysis for a more comprehensive understanding of conversation.

It is unfortunate that the current situation of conversation research is not like that of linguistics in the way just described. There are so many disciplines and approaches in the study of conversation, and consequently it is often the case that researchers in each discipline consider themselves not as collaborators with researchers in other disciplines, but as unrelated or intellectually opposed researchers. Collaboration across disciplines in the study of conversation is often thought to be unnecessary or not worth doing because each discipline has different objectives in the study of conversation. Naturally, as far as researchers are only concerned with their own discipline and its goal, there is nothing wrong with this. However, when conversation turns to be the research objective itself, a crucial problem arises: no single discipline has a comprehensive picture of the nature of conversation. To understand conversation holistically, its form, its meaning, and their interface should be studied, and so it is necessary that researchers cross disciplinary boundaries and at the same time attempt to produce a synthesis of various disciplines.

In addition to the distinction between form and meaning, the distinction between the mental and social aspects of conversation should be made. As I showed in chapter 3, the mental and the social level of analysis should be distinguished, and the same applies to the study of conversation. Some disciplines look for social mechanisms that govern conversational organization, while others search for mental mechanisms of conversation comprehension and production. In this respect conversation analysis and psycholinguistics are good contrasts. Conversation analysts as sociologists see conversation as a type of social organization, and their unit of analysis is social. On the other hand psycholinguists aim at understanding the mental processes of conversationalists, such as how they process language and information during conversation, and their unit of analysis is mental. The division of these two categories between the mental and the social is useful when reviewing the various disciplines of conversation.

In what follows I will critically examine various disciplines that I think are highly relevant for the comprehensive understanding of conversation, especially the meaning of conversation. Thus the purpose of the examination is not to exhaust all the studies of conversation that have been carried out until now. In addition the review is biased by the fact that my research interest is mainly on the meaning rather than the form of conversation. I am aware of the fact that the following overview is far from complete. However, a complete overview, if ever possible, is an immensely difficult task to conduct because conversation is one of the most basic modes of verbal behavior and virtually any study dealing with verbal behavior should be included. What I want to point out is how various disciplines and research programs that explicitly deal with conversation differ from each other and in what way. The goal is the vertical and horizontal separation (see chap. 3) of conversation studies. It is only after the diverse traditions are separated vertically and horizontally that it is possible to start synthesizing them by considering their various goals, strengths and weaknesses.

### 4.3 Semantics and Pragmatics

The branches of linguistics that study the meaning of language are semantics and pragmatics. Even though the distinction between them is a matter of debated, there are some consensuses in linguistics that while semantics studies the context-free meaning of language, pragmatics studies the meaning of utterances in context. The dichotomy between them may go back to the idea of Ferdinand de Saussure and Noam Chomsky that *langue* and *parole* or competence and performance should be distinguished in the study of language. Even though this dichotomy has been the tradition of linguistics since its inception, it is not an *a priori* fact but at best a theory. Thus it is a theory to be investigated further, and there are linguists who try to overcome the dichotomy and show where they are blurred (see Kempson, 1996).

The units of analysis that have been investigated in semantics are mostly words, sentences, and recently discourses. The study of the meaning of discourse often concentrates on written discourse or text, and traditionally conversation has not been a target of study in semantics. On the other hand, pragmatics is often concerned with spoken rather than written discourse, and conversation has been an actively investigated topic of study. It is rather difficult to define pragmatics compared to semantics, and various definitions of it have been proposed. One common approach is to define pragmatics in terms of “language use.” For example, Mey (2001) noted that “pragmatics studies the use of language in human communication as determined by the condition of society” (p. 6). In this sort of definition, pragmatics is not always explicitly about the meaning of language because eventually the “use of language” is not equal to meaning, and indeed some areas that pragmatics cover are not explicitly concerned with meaning. Also the border between pragmatics and other disciplines is not always clear; for example, speech act theory from the philosophy of language and conversation anal-

ysis from sociology are often cited in textbooks of pragmatics (e.g., Levinson, 1983; Mey, 2001). Some pragmatists have even departed from the position of pragmatics as a branch of linguistics and taken it as a branch of cognitive psychology (Sperber & Wilson, 1995).

Although the disciplinary boundaries surrounding pragmatics are rather unclear, pragmatics has traditionally been regarded as a branch of linguistics, and it seems to share the basic premise with semantics that the meaning of language should be studied in relation to the structure of language. Compared to the relationship between pragmatics and syntax, the relationship between semantics and syntax is clearer, but still a relationship to syntax is maintained in the research program of pragmatics. As the syntax-semantics interface is important in linguistics, the syntax-pragmatics interface is important as well. This aspect of pragmatics differentiates itself from conversation analysis, which also examines language use, but as Hutchby and Wooffitt (1998) argued, it is not concerned with the structure of language or even language itself.

Pragmatics also shares some basic assumptions with other branches of linguistics, especially in its central concern with utterances that are produced by a single speaker. As with other branches of linguistics, the unit of analysis in pragmatics is that of a single speaker and a single hearer, which has been criticized by researchers from other disciplines, especially by ethnographers of communication (Hymes, 1974). The typical pragmatic approach is to study monologic utterances in relation to the context in which they are produced, but not in the sequences of conversation as conversation analysts do or in the embodied, physical and social context as ethnographers of communication do. When pragmatists deal with dialogue, they still divide utterances into pieces, and examine them out of the holistic context, thus not in their truly dialogic contexts.

From the point of view of conversation research, the fundamental view of pragmatics toward language use, that is, lan-



guage use as a collection of acts of an individual produces a fatal problem. Conversation should be seen in its dynamics, participants interacting with each other, rather than speakers producing utterances. However, pragmatics is one of the central blocks out of which conversation research should be built. The origin of the pragmatic idea of conversation goes back to Paul Grice (1989), who first developed the *co-operative principle*, which can be treated as a theory of conversation. Grice's work has guided pragmatists' view toward conversation, and new theories based on his framework are still being developed today; for example, relevance theory by Dan Sperber and Deidre Wilson (1995) and the theory of generalized conversational implicature by Stephan Levinson (2000). Because of their expansion in scope, these recent theoretical developments have had more impact on the study of conversation meaning from a more naturalistic and interactive perspective than traditional pragmatic theories.

#### 4.4 The Ethnography of Communication

If pragmatics studies conversation in an idealized context, the ethnography of communication studies conversation in a "real" context. In 1962 the anthropologist Dell Hymes inaugurated the ethnography of speaking in his paper "The Ethnography of Speaking," which was revised in 1968. He and John J. Gumperz renamed the field as the ethnography of communication in the special publication of *American Anthropologist* that they edited in 1964. In the early phase of the ethnography of communication, it is clear that Hymes wanted to create a new ethnographic field that studies language in the same way as ethnographers study other parts of culture through fieldwork. However, Hymes's idea of creating a field that studies language within the theoretical framework of anthropology was not new; the founder of U.S. anthropology Franz Boas had already argued for the

importance of studying language as part of anthropological research. Under the heading "Language a Part of Ethnological Phenomena in General," he noted:

If ethnology is understood as the science dealing with the mental phenomena of the life of the peoples of the world, human language, one of the most important manifestations of mental life, would seem to belong naturally to the field of work of ethnology, unless special reasons can be adduced why it should not be so considered. (Boas, 1911/1966, p. 59)

Indeed, linguistics or philology as it was called by the time already existed in the academic scene in the USA and could be argued to be just such a "special reason." But Boas argues that it does not hinder the prospect of anthropologists studying language:

It is true that a practical reason of this kind exists, namely, the specialization which has taken place in the methods of philological research, which has progressed to such an extent that philology and comparative linguistics are sciences which require the utmost attention, and do not allow the student to devote much of his time to other fields that require different methods of study. This, however, is no reason for believing that the results of linguistic inquiry are unimportant to the ethnologist. There are other fields of ethnological investigation which have come to be more or less specialized, and which require for their successful treatment peculiar specialization. This is true, for instance, of the study of primitive music, of primitive art, and to a certain extent, of primitive law. Nevertheless, these subjects continue to form an important part of ethnological science. (Boas, 1911/1966, 59)

Boas was aware of the complexity of language and the difficulty of specializing in the study of language within the holistic study of culture. However, he considered the difficulty as a practical problem and argued that, theoretically speaking, language should be studied by anthropologists as part of culture as they study music, art, or law. Incidentally, the examples Boas gave, that is, the studies of music, art, and law still continue to be major areas of anthropological research about one hundred years later.

But it is difficult to say that the same can be said about language. Until the re-emergence of the anthropology of language with Hymes in the 1960s, there was no systematic line of anthropological research on language. Of course, such researchers as Edward Sapir and Benjamin Lee Whorf continued research on language. It would be fair to say, however, that research on language within anthropology was not a central concern of anthropology as a whole. What is significant in Hymes's work is that he re-established a field that is not linguistics practiced by anthropologists, which had been practiced until the 1960s and even today to some extent (Duranti, 1997), but an anthropological study of language of the kind that Boas argued for at the beginning of the twentieth century.

For anthropologists and anthropologically-minded investigators from other disciplines, ethnography of communication seems best to indicate the necessary scope, and to convey and encourage the fundamental communication in the scope and kind of patterned complexity with which they deal with. (Hymes, 1964, p. 2)

It is not linguistics, but ethnography, not language, but communication, which must provide the frame of reference within which the place of language in culture and society is to be assessed. (Hymes, 1974, p. 4)

As Duranti (2001) pointed out, it is around this time that linguistic anthropology as it is known today was established in opposition to linguistics in an anthropology department that had been practiced since the end of the nineteenth century in the USA. So there are two waves in the anthropological study of language. The first wave was started by Franz Boas at the turn of the twentieth century. The second wave came from Dell Hymes and his associates, especially John J. Gumperz. In short, there have been at least one hundred years of tradition in the anthropological perspective toward language, and the ethnography of communication as it arose in the 1960s was one moment in such a continuum.

The ethnography of communication aims at the description of communication in all parts of the world just in the same way as ethnographers describe other parts of culture such as religion, and marriage. Ethnographers of communication realized that communication, despite its universality and centrality in human life, had been usually neglected by ethnographers. In fact there were some ethnographers who described communication before Hymes's inauguration of the discipline. For example, Malinowski (1923), the father of modern ethnography, described some aspects of communication as part of his ethnographic fieldwork. He was in this respect also an ethnographer of communication. But what was new and unique in Hymes's idea, and what made the ethnography of communication so attractive to ethnographers in general, is the fact that he created an interdisciplinary field that at times he called *sociolinguistics*,<sup>1</sup> which was devoted solely to the ethnographic study of communication. He not only argued for the importance of language and communication in anthropology, but also advanced a theoretical framework and posed various concepts for describing communication ethnographically. As a matter of fact it should

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<sup>1</sup>For an overview of the historical development of the concept of sociolinguistics proposed by Dell Hymes, see Kuniyoshi (1999).

not be a coincidence that Harvey Sacks, the founder of conversation analysis (see sec. 4.6), started to create his discipline during the same time. Hymes's view of communication also stimulated researchers outside of anthropology, and some sociologists and folklorists engaged in the agenda actively, as can be seen from the diversity of contributors to the book edited by Gumperz and Hymes (1986), which is one of the major collections of articles in the field of ethnography of communication.

Dell Hymes (1972, 1974) developed many concepts, including *communicative competence*, that are also well-known in applied linguistics. For the present purpose I want to highlight three aspects of the ethnography of communication that especially contribute to conversation research. The first contribution is the invention of the SPEAKING model. "SPEAKING" in the SPEAKING model stands for *situation, participants, ends, act sequence, key, instrumentalities, norms, and genres* (Hymes, 1974, 1986). Each of these eight categories is then divided into subcategories, and the number of the subcategories varies from researcher to researcher; for example, in addition to the original proposal of Hymes (1986), Duranti (1988) suggested subcategories for some of the subcategories. Ethnographers are supposed to collect data on the basis of this model, which functions as a catalog of items to be observed and documented. But the model served only as a helpful outline rather than topics that must be covered. It is also not intended that every component of the model must be collected in every session of ethnographic fieldwork. Some ethnographers concentrate only on one or some components of the SPEAKING model, while others describe nearly all the components in a given speech community. Thanks to the clearness and usefulness of the SPEAKING model, this research scheme was widely accepted and pursued by ethnographers. For example, *Explorations in the Ethnography of Speaking* (Bauman and Sherzer, 1989) first published in 1974, is one of the most significant contributions to the ethnography of communication in its heyday. It shows that many researchers

have conducted ethnographic fieldwork all over the world and documented communicative activities in the framework of the ethnography of communication initiated by Hymes.

The second contribution is the separation of the levels of description and analysis not practiced in other disciplines, especially in linguistics. For ethnographers of communication, the largest unit of analysis is not language, but a speech community. This is a theoretical stance that vividly exhibits the difference between the ethnography of communication and linguistics. A speech community is the largest category that has other units as its subcategories (Hymes, 1974). Within a speech community, there are speech situations. Within a speech situation, there are speech events. And within a speech event, there are speech acts. The idea of a speech act in the ethnography of communication is similar to that of a speech act in speech act theory (Searle, 1969), but still they are not the same. The crucial difference is that while for speech act theory a speech act is a unit to be fully studied on its own, for the ethnography of communication it is a unit that cannot be completely grasped without reference to the analysis of the three larger categories, namely, speech event, situation, and community.

Although the boundaries between a speech community, speech situation, speech event, and speech act are not always clear, and although it depends on the interpretation of ethnographers as to how and where to draw the boundaries, they are yet powerful tools of analysis. Let us take an example of a speech community a student association, and let us assume that the student association organizes different kinds of activities. More often than not, they have parties in the dormitory; these are speech situations. They might have some other speech situations, such as meetings to discuss the quality of teaching at the university where they study. At their party or meeting, there are many kinds of activities going on: they chat together, they dance together, or they just drink together. Each one of these is a speech event, and it happens within a single speech situation. The speech event of

chat in turn consists of single speech acts such as "How are you?" The merit of the ethnographic approach to communication lies in the fact that a single speech act such as "how are you?" is analyzed not in isolation, but in relation to the larger contexts of a speech event, situation, and community. Hymes (1974) emphasized this point by criticizing the tendency of linguistics to conflate language and speech community and by arguing that they are concepts that should be treated as distinct.

The third contribution is the view that speech acts do not take place as acts by individual speakers, but as acts by a collection of individuals participating in speech event and situation. This idea leads to another important feature of the ethnography of communication that distinguishes it from linguistics. In linguistics, the agent who produces language is a single speaker and a single hearer, both of whom are conceptualized in a rather idealized and detached context. For the ethnography of communication, it is not a single speaker or a hearer, but *participants* who engage in speech events: "serious ethnographic work shows that there is one general, or universal, dimension to be postulated, that of *participant*" (Hymes, 1974, p. 54). To holistically see communication, which consists of various participants' activities, it is more useful to approach communication as action by participants than as that by a single speaker and hearer. The concept of participation that has been developed by extending Hymes's concept of participant is one of the central themes besides performance and indexicality in today's linguistic anthropology (Duranti, 1997).

The analytical power of four incremental units of speech is enormous compared to the approach that only takes speech acts as the unit of analysis if researchers want to study communication in the widest possible context. For the study of conversation meaning, the four units of analysis established by the ethnography of communication is an indispensable tool. Conversation is a phenomenon that cannot be studied in neat and idealized isolation, and its meaning is affected by various fac-

tors in the physical and cultural environments. Although conversation and the ethnography of communication would make a perfect match, as a matter of fact conversation as one of the genres of communication has not been a major object of study by ethnographers of communication. Researchers in other disciplines as well as within the ethnography of communication have pointed out that most studies conducted within the tradition of the ethnography of communication deal with monologue, and dialogue is ignored. Poetic and ceremonial genres have received special attention, and the spontaneous genre of conversation has not been studied systematically.

However, recently more and more research has been done into conversation from the point of view of the ethnography of communication, often using insights from conversation analysis as well (e.g., Moerman, 1988; Goodwin, 1990). Nowadays the ethnography of communication seems to have been replaced by linguistic anthropology, and today it is rather difficult to find a study whose approach derives purely from that of the ethnography of communication in the form created in the 1960s. However, the ethnography of communication was obviously the beginning of the systematic anthropological study of communication, and it seems that Hymes (1986) intended to develop the descriptive science of the ethnography of communication into an explanatory science of the anthropology of communication. Indeed, in 1967 Hymes published a paper titled *The Anthropology of Communication*, which he begins by saying "an anthropology of communication does not exist" (p. 1). Thus any researcher studying communication in an anthropological framework owes his or her work to the tradition of the ethnography of communication. I believe that the fundamental point of view of Dell Hymes is the most appropriate one for the study of conversation meaning.

I have presented the difference between the linguistic and the anthropological approach to the study of language, and it should be apparent from this that linguistics and anthropol-



ogy are based on different theoretical frameworks when they study conversation meaning. Although the units of analysis differ significantly between the linguistic (i.e., semantic and pragmatic) and the anthropological study of conversation meaning, they still have one feature in common: both take the problem of meaning as their research agenda. Among the various disciplines, to the extent that the ethnography of communication usually centers on the problem of meaning, it is closer to linguistics, especially to semantics and pragmatics, than conversation analysis whose main subject matter is the structure, rather than the meaning, of conversational sequences.

## 4.5 Interactional Sociolinguistics

Before moving on to conversation analysis, it would be useful to take a look at another discipline that originated from an anthropological perspective: interactional sociolinguistics. The ethnography of communication came out of the tradition of anthropology, but it is not the only field that did so. Although it is at times difficult to separate them, the work of the anthropologist John Gumperz proceed from a different point of view compared to that of Dell Hymes. Since the 1960s Gumperz has greatly contributed to the the development of what is today called *interactional sociolinguistics*. But what I will discuss in the following should not be taken as an argument on interactional sociolinguistics in general because interactional sociolinguistics as a discipline is potentially much wider in scope than the overview presented here, and should be interpreted as a presentation of the approach of John Gumperz.

Although they often collaborated at the beginning of the development of the ethnography of communication, Gumperz has paid more explicit attention to language for understanding communication compared to Hymes. Gumperz (1982, 1992) coined the well-known term *contextualization cues*, and his research

areas (1996, 2001) have often lain in the process of conversational inference that humans make during their interpretation of speech. What Gumperz shares with Hymes is the anthropological point of view, which takes cultural factors into consideration in the investigation of the interpretation of speech. Because of his orientation toward the quest for the mechanism of inferential processing, Gumperz's approach is closer to cognitive science than Hymes's. While Hymes (1962, 1968) saw his research as partly related to the study of cognition, the ethnography of communication eventually came to concentrate more on the social aspect of communication rather than the cognitive mechanism that produces inference in communication. Gumperz's research has had from its beginning a cognitive scientific orientation and thus has more relevance to the problem of language and culture from a cognitive point of view. Because of his cognitive rather than social interest he has collaborated with other researchers who are also cognitively oriented, as his recent contribution to the problem of linguistic relativity (Gumperz, 1996) shows.

What also differentiates Gumperz's research from the ethnography of communication is that he has often studied conversation as a genre of communication (e.g., 1982), even when ethnographers of communication were concentrating their research on poetic and ceremonial speech. But in contrast to the shift in the ethnography of communication in its later period, where the insights of conversation analysis, especially its methods of recording and transcription, were slowly imported, Gumperz rarely borrowed insights from conversation analysis and continued along his own path. His study of conversational inference seems to share the concern of conversation analysts regarding inferential order, but it seems that he has never seriously attempted to integrate his own studies with studies done in conversation analysis.

The ethnography of communication has had a different relationship with conversation analysis: historically speaking, they were close, at least in the beginning. In the early days of both

disciplines during the 1960s and 1970s, the key developers of conversation analysis, such as Harvey Sacks and Emanuel A. Schegloff, published articles in some collections of ethnographies of communication (Sacks, 1986, 1989; Schegloff, 1986). However, this did not happen the other way around: Ethnographers of communication remained reluctant to do research into the dialogic and spontaneous genre of conversation, and subsequently the distance from conversation analysis became so long that they finally turned into separate and independent disciplines with a minimal relationship.

In the climate of the 1960s and 1970s Gumperz's approach was rather isolated compared to the fact that the ethnography of communication and conversation analysis developed in close relationship. However, this also suggests that Gumperz's research on conversational inference was very original. So far Gumperz is the only researcher who has developed a theoretical insight into the nature of conversation inference from an anthropological point of view, arguing for the importance of culture in the inferential process. But in relation to conversation meaning the most important theoretical contribution of Gumperz is the invention of the idea of contextualized cues, and it is important that this concept be incorporated into other studies of conversation meaning.

## **4.6 Conversation Analysis**

The disciplines considered so far are chiefly concerned with the meaning of conversation. But there are disciplines whose interest lies largely in the structure, rather than the meaning, of conversation. Conversation analysis is one of those disciplines and has a leading role as a structural study of conversation. In addition, conversation analysis has greatly influenced other disciplines that examine conversation precisely because conversation is its target. In the next chapter I will also touch on the

nature of conversation analysis; here I will only briefly examine the general characteristics of conversation analysis to contrast it with the other disciplines presented in this chapter.

Conversation analysis was founded by the sociologist Harvey Sacks in the 1960s. Its most important characteristic is that its objective has been social scientific from the beginning. Conversation analysts see conversation as a social phenomenon where structure is reproduced or transformed by conversation-ists. The reason why Sacks started to study conversation was not because he was interested in conversation *per se*, but because he thought that conversation can be recorded, and such recorded data enable researchers to do objective research on society (Sacks, 1992). Thus it is possible to interpret the rise of conversation analysis not as due to the lack of the study of conversation in sociology before Sacks's time, but as a reaction to other theoretical schools of sociology. Without such a context, it is simply impossible to understand the reason why Sacks vividly differentiates his research not only from "non-observational" schools of sociology, but also from another "observational" school (to which, according to Sacks, conversation analysis belongs), namely, the ethnographically oriented school of sociology. He uses biology as an example of how a science of society should be: he argues that sociologists should present their data to be examined by other researchers in the same way as biologists present their data in their research papers. For Sacks only then can researchers in biology or sociology criticize each others' results because they have the data at hand from which such results are derived.

In Sacks's arguments (1992) it is important to notice the relationship between data and research; for him the study of perceivable social activities rather than of an unobservable social structure is central to sociology, and conversation is a good phenomenon because it is easy to observe.

But social activities are observable; you can see them

all around you, and you can write them down. The tape recorder is important, but a lot of this could be done without a tape recorder. If you think you can see it, that means we can build an observational study, and we can build a natural study. (p. 28)

On the basis of the argument presented in chapter 2, it is possible to conclude that Sacks was committed to empirical realism. So for him the world consists only of perceivable events, and such events should be the objective of sociologists. The motivation behind Sacks's study of conversation has the nature of empirical realism, and indeed the empirical realist tradition seems to continue to exist even today within the circle of conversation analysts. This point is often neglected in the current scene of conversation research as conversation analysis gets more and more interdisciplinary involving not only sociologists but also linguists, psychologists, and anthropologists. The fact that conversation analysis has its origin in theoretical disputes in sociology is no longer recognized. However, it is crucial to grasp this point to avoid invalid theoretical criticisms of conversation analysis from the standpoints of other disciplines. Note that I am not arguing that the empirical realist research program of conversation analysis is right; as I showed in chapter 2, empirical realism is fundamentally flawed. What I do want to argue here is that it is important to understand that conversation analysis is a research program aiming at goals similar to those other empirical realists aim at.

If the nature of conversation analysis is understood from the point of view of the philosophy of science, it is fairly easy to notice how the discussion of the place of theory is misguided in conversation analysis. For example, as Levinson (1983) already showed about 20 years ago, conversation analysis is often contrasted with discourse analysis in relation to the treatment of theory and how research should proceed in general. According to the conventional argument, discourse analysis proceeds de-

ductively, first by setting out some kind of theoretical constructs and then by explaining examples (which are often detached from the context in which they are produced) using such theoretical constructs.

The major critiques made by conversation analysts toward discourse analysis center on this deductive versus inductive nature of research. Conversation analysts say that they proceed empirically by avoiding theory construction, and that their approach is inductive. It sometimes seems as if conversation analysts even deny the possibility of any theory construction, which is the business of discourse analysts. What is flawed in this argument is the notion that conversation analysts do empirical research without any theoretical construction. No empirical research can be theory-free; in fact, as philosophers of science have taught us, it is inevitable that observations are theory-laden. So the contrast between discourse analysis and conversation analysis does not lie in the deductive or the inductive nature of their research, but in the fact that, due to their empirical realist orientation, conversation analysts do not aim at constructing theories that are about unobservable mechanisms. Sacks (1992) used the word *mechanism* often in his writings, but because he is an empirical realist, a mechanism is not something that exists in the unobservable world. For him a mechanism is what researchers see in observable social activities, and that is different from the attitude of discourse analysts who consider their business to be the description of real mechanisms, which researchers cannot observe but that they expect to exist on the basis of the analysis of empirical data.

The obvious consequences of the negative attitude taken by conversation analysts toward theoretical research — “theoretical” in the transcendental realist sense — appear in their very lack of theories of conversation. So far a tremendous amount of recordings of conversation have been studied by conversation analysts around the world, and that is the proper objective of the study of conversation for conversation analysts. But when

it comes to theory development, which is the central activity of science from the point of view of transcendental realism, there has not been much progress since the time of Sacks. Curiously, conversation analysis in its early phase was in fact explicitly concerned with theoretical matters. The rules of turn-taking that were developed in the 1970s (Sacks, Schegloff, & Jefferson, 1974) is a good example. Indeed, conversation analysts like any other empirical realists would argue that such rules are not “theories,” but merely descriptions of sequences of events. For transcendental realists the description of turn-taking is clearly a theory developed on the basis of recordings of conversation, and there is no problem calling the turn-taking rules a theory.

The theories that conversation analysts have developed are explanations of the sequential organization of conversation. They are theories of the social properties of the structure of conversational sequences. Conversation analysts think that conversational sequences are governed by social causes. For example, in their presentation of the conversation analytic perspective for the study of *preference*, Hutchby and Wooffitt (1998) noted that preference as a conversation analytic term does not mean psychological motivation, but the structural property of conversation. What is problematic in this sort of argument is the way conversation analysts neglect the psychological study of utterance production and comprehension. Surely, the way an individual makes a preference within sequences of conversation is a cognitive matter. From the point of view of transcendental realism, structure is not something that guides the way people behave, but it is something that emerges from people’s behavior. Thus because of the making of personal preferences such conversational structure as *preference* comes into being, but not the other way around.

Although the central concern of conversation analysis is the structure or *sequential order* of conversation, Hutchby and Wooffitt (1998) argued that it is equally important to study inference or *inferential order* in conversation analysis:

CA has an equally important interest in what we will call the *inferential order* of talk: the kinds of cultural and interpretive resources participants rely on in order to understand one another in appropriate ways. (p. 39)

By avoiding to deal explicitly with the mental properties of conversation, conversation analysts have created a naive psychology of utterance comprehension. They take it for granted that participants understand each other in a way that is observable to researchers through the data. They do not ask the question of how participants understand each other and do not rely on psychological research on utterance comprehension either because they believe that social structure produces conversational sequences. Let us examine the following sequence presented and discussed by Hutchby and Wooffitt (1998, p. 16):

- 1 Mother: Do you know who's going to that meeting?
- 2 Russ: Who?
- 3 Mother: I don't know!
- 4 Russ: Oh, probably Mr Murphy and Dad said Mrs
- 5 Timpte an' some of the teachers.

The interest of conversation analysts lies in the sequential organization of this segment, and the method they use to analyze data is called the *next-turn proof procedure*:

Speakers display in their sequentially "next" turns an understanding of what the "prior" turn was about. That understanding may turn out to be what the prior speaker intended, or not; whichever it is, that itself is something which gets displayed in the next turn in the sequence. (Hutchby & Wooffitt, 1998, p. 15)

Conversation analysts assume that researchers can actually observe the way participants understand by means of the analysis



of conversational sequences. In the above example it can be observed in line 2 that Russ understood Mother's speech act in line 1 as a pre-announcement rather than a question, and it can be also seen in line 3 that Mother was in fact asking for information. So the question is not of the mental processing of Russ and Mother, and such processing is not thought to be a valuable question for conversation analysts because for them it is the structural features that determine the sequential organization of conversation.

The official subject matter of conversation analysis is the structural properties of conversation, but it has nonetheless provided a unique perspective on the meaning of conversation even though it seems to be a by-product of structural analysis. It is important to grasp the great theoretical difference between conversation analysis and linguistics in the concept of meaning. For conversation analysis, meaning is not what a single speaker intends, but what participants produce intersubjectively. This is an enormous theoretical departure from other disciplines' notion of meaning because conversation analysis considers meaning to be an intersubjective phenomenon.

Although the concept of meaning for conversation analysis is unique, strictly speaking, it does not deal with meaning in itself. Recall the next-turn proof procedure used in the above example. There, conversation analysts do not explain *how* participants' understanding takes place. The process of understanding is taken for granted, and it is seen only as a result or product from which structural analysis is built. So for conversation analysis what happens in participants' minds while they participate in conversation is not a research problem, but merely a fact on the basis of which their analysis is conducted. And this is why I argue that conversation analysis does not study meaning even though it necessarily has to deal with the inferential processes that are mental rather than social in nature. In short, conversation analysts are right when they say that their object of analysis is sequential organization rather than cognitive

processes and when they base their research on participants' understanding displayed in conversation; but they are wrong when they also claim to study the inference processes that go on in participants' minds, because they do not in fact study those processes, but take it for granted that participants understand conversational sequences in the way displayed in the products of cognitive processing. The study of how participants understand each other in conversation to be presented in the next chapter is different from conversation analysis, and it is a study at the level of the mind rather than society because "understanding" is a mental property.

## 4.7 Psycholinguistics

In contrast to conversation analysis, which is a social science, psycholinguistics is a mental science. Its goal is to understand how humans process language in the mind. Psycholinguists have traditionally studied three different aspects of language processing: comprehension, production, and acquisition. The unit of analysis that they have traditionally used follows the path taken by linguists: word, sentence, and recently discourse. Their interest usually parallels the various branches of linguistics such as phonology, morphology, and syntax, but they are especially concerned with the cognitive processing of such linguistic features.

Because psycholinguistics has taken a view toward meaning similar to that of linguistics, its contribution to the understanding of conversation meaning is rather small. However, psycholinguistic research on conversation does exist, and it should not be neglected. Recently, there has been a growing interest in conversation in psycholinguistics, and some psycholinguists have taken a new perspective on language different from the traditional approach.

Within psycholinguistics the difference between those who

do research on conversation and those who do not is whether they treat monologue or dialogue as their unit of research. Because most psycholinguists follow the linguistic tradition of the division of labor in their study of language processing, the largest unit of research in psycholinguistics has been discourse. However, in addition to studies of sentences or discourses that are monologic, there have been psycholinguistic studies that explicitly study language processing in dialogue. For example, Clark (1992, 1996) did pioneering work in this field by investigating conversation from a psychological point of view, and he has also kept a close relationship with other disciplines studying conversation.

Still, it is only within the last decade or so that psycholinguistics as a discipline has started to pay serious attention to conversation and dialogue in general. In a recent review of the relationship between psycholinguistics and dialogue, Garrod (1999) noted the following:

It [dialogue] is commonly viewed as marginally grammatical, contaminated by complexities beyond theoretical consideration and outside the scope of rigorous psycholinguistic investigation. However, in recent years there has been an increasing interest in the psycholinguistic study of dialogue and in what it can contribute to theories of processing" (p. 389).

It is interesting to see Garrod saying that dialogue is commonly seen as a *theoretically* unimportant topic. It is clear also from this that psycholinguistics has such a close relationship to linguistics, which strictly divides competence and performance and considers the former to be the proper area of investigation. What this new trend in psycholinguistics shows is that some psycholinguists are departing from this traditional view of language and starting to investigate the messy area of performance as a potentially rich area for the theorization of language processing.

Because of the tradition in psycholinguistics of concentrating on the processing of linguistic structure rather than meaning, it seems that psycholinguists interested in dialogue processing are not particularly interested in the problem of conversation meaning. So at least at the moment the contribution of psycholinguistics to the study of conversation meaning is marginal. But it is important to get rid of the traditional image of psycholinguistics and look at recent developments in dialogue processing to find out if it has any relevance to conversation meaning.

## 4.8 Natural Language Processing

Natural language processing (NLP) is a discipline closely related to linguistics and psycholinguistics. As the alternative name *computational linguistics*<sup>2</sup> suggests, it is the marriage of computer science and linguistics. In the similar way as psycholinguistics, NLP is concerned mostly with the levels of word, sentence, and discourse. However, perhaps much more rapidly than in psycholinguistics, NLP researchers' interest in dialogue has been expanding in recent years. A field called *spoken dialogue systems* (e.g., Allen, 1995, chap. 17; Jurafsky & Martin, 2000, chap. 19) seems to have established itself as a branch of NLP or a discipline of its own, and a textbook (Ishizaki & Den, 2001) dedicated to discourse and dialogue from a computational perspective has also been recently published.

Compared to the disciplines that have been presented so far, spoken dialogue systems is concerned with the technology of computational systems capable of communicating with humans in natural language. So it is often the case that researchers do not develop theories of dialogue on their own, but apply existing

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<sup>2</sup>Some use the term *computational linguistics* as an alternative term for natural language processing, and others distinguish them as separate disciplines. I will use the term *natural language processing* as the cover term for both disciplines.

theories to develop a computational system. It sounds as no contribution has been made by spoken dialogue systems to the theoretical understanding of conversation meaning. Yet, spoken dialogue systems is potentially one of the most important fields in the study of conversation meaning.

To build a spoken dialogue system, what one needs is not a knowledge of dialogue scattered across disciplines, but an integrated knowledge of it. As Ishizaki and Den (2001) noted, the field spans philosophy, linguistics, sociology, psychology, artificial intelligence, and computer science, but it has a specific point of view, that is, a computational perspective. Each field presented so far is a branch of a traditional discipline that distinguishes itself from others by its theories and methods. For example, semantics is a branch of linguistics which has a different theoretical interest from sociology to which conversation analysis belongs, and the ethnography of communication is part of anthropology, which has a different theoretical interest from psychology to which psycholinguistics belongs, and so on.

To build a spoken dialogue system, it is not enough to accumulate all the knowledge produced in diverse disciplines. Such an approach would eventually fail because some of their theoretical concepts are not compatible with each other. As I have shown, for instance, conversation analysis and linguistic anthropology are not theoretically compatible because of their difference in focus. Spoken dialogue systems overcomes this dilemma by methodologically grounding its work on computation. For the computational approach, designing a system and implementing it by building exploratory computer programs are the central activities. But it is not the case that every kind of theory is straightforwardly computable: to be able to *compute* an idea, the idea has to be in a form that can be computed. Unfortunately, theories of mental and social science often do not meet this requirement. In physics, for example, many theories are expressed in mathematics, and it is rather easy to compute such theories for experimentation. But theories of mental and so-

cial science are usually not expressed in mathematics or logic. This means that the researchers into spoken dialogue systems themselves have to do the work of making existing theories so explicit that they can possibly be computed. Thus the computability of existing theories is a crucial aspect of the research activities of spoken dialogue systems; for example, in the emerging field of computational semantics (Bunt & Muskens, 1999; Bunt, Muskens, & Thijsse, 2001) this computational problem is claimed to be one of the central problems.

I want to point out the pervasive misunderstanding and neglect by non-NLP researchers of spoken dialogue systems. Such an ignorance is clear from the fact that even though NLP makes considerable use of theories and methods from other disciplines, this does not happen the other way around; research results of spoken dialogue systems are rarely referred to in the literature of other disciplines. It seems often to be thought that spoken dialogue systems is just computer programming, that is, making software by directly applying theories developed elsewhere. Such an attitude will prevent us from seeing some of the significant contributions of spoken dialogue systems to the study of conversation meaning.

In fact, the building of a system capable of showing human characteristics of conversation should be a necessary component of conversation research. Clark (1997) argued that if the properties of a system are fully understood, it should be possible to build such a system. He did not mean that such a system should be actually created; for practical reasons it is not always possible to do so. For example, even though researchers know a great deal about the nature of volcanic activity, they cannot build a real volcano. But human cognition is something that seems to be built, or that is what artificial intelligence is all about. And if the criteria of *buildability* is necessary for our full understanding of a system, spoken dialogue systems is precisely a field that aims at building a human-like conversation system, and should therefore be regarded as one of the most important

aspects of conversation research.

Indeed, for conversation researchers it is possible to interpret spoken dialogue systems as the computer simulation of human conversation. A spoken dialogue system can be used for the computer simulation of human conversation in which researchers are able to test their ideas by modifying and observing the behavior of the system. Thus the function of a spoken dialogue system implemented as a computer program is similar to that of an artificial intelligence program that is used as a model for experimentation in cognitive science. If spoken dialogue systems employs theories of human conversation research, it should be possible to see it as a test case of such theories; through the models of spoken dialogue systems researchers can observe how a theory produces events, either successfully or unsuccessfully.

Although NLP has had a tendency to study solely the structural properties of language in the same way as linguistics and psycholinguistics, the situation has been changing recently. Nowadays researchers talk not only about computational semantics (Bunt & Muskens, 1999; Bunt et al., 2001) and computational pragmatics (Bunt & Black, 2000), but also about the semantics and pragmatics of spoken dialogue. Such a transition is crucial to the understanding of conversation meaning, and it is also necessary to allow information to flow from spoken dialogue systems to the study of human conversation.

## **4.9 Conclusions**

I have provided an overview of the arena of conversation research today. It was by no means complete; I believe that there are other disciplines or approaches that are relevant to conversation research, and the choice of the disciplines presented here is an eclectic one. The choice is based on my own subjective interest in, and point of view toward, conversation research. As I have pointed out, however, the problem in the first place is that there

is no mutual ground on which to discuss various disciplinary points of view in today's conversation research. Of course there have been attempts at comparing at or even synthesizing a couple of disciplines. For example, conversation analysis and linguistic anthropology (e.g., Goodwin & Heritage, 1990) and psycholinguistics and conversation analysis (e.g., Clark, 1996). Still it is out of most researchers' interest to take multiple disciplines into consideration, compare them, and identify the differences and similarities between them because eventually each one is satisfied with their own discipline and sees no urgent need for integrating disciplines. However, to solve a practical problem scientifically it is necessary for applied linguists to accumulate insights from various disciplines and tackle the problem at hand in the best manner possible.



## Chapter 5

# Conceptualizing Conversation Meaning

### 5.1 Introduction

The previous chapter showed that there are diverse disciplines concerned with, or having the potential for, the study of conversation meaning. It was also shown that the study of conversation meaning is scattered among various disciplines, and at the moment there is no interdisciplinary discourse, let alone a single unified discipline, that is devoted to the study of the mechanism of conversation meaning. In what follows I will suggest the outline of a unified theory of conversation meaning. To achieve that goal, it is necessary first to explore the very possibility of developing such a theory, and then, assuming it is possible, develop a theory on which further empirical research can follow. Thus, strictly speaking, what will be presented is more like a hypothesis or a theoretical framework than a theory. However, because the difference between a theory and hypothesis is not

so clear in mental and social science as in physical science,<sup>1</sup> and because my theory is based on theoretical as well as empirical insights from various disciplines, I will simply call it a theory.

## 5.2 A Need for Theory and Metatheory

It is difficult to approach the problem of conversation meaning from the theoretical stance of already existing disciplines. And it is not an easy task either to synthesize those disciplines and create a new one on top of such a synthesis because of the diversity in their theoretical assumptions. So it is important to have a strategy to overcome these difficulties in creating a unified view toward conversation meaning. On the one hand, it does not make any sense simply to “combine” existing disciplines because such an attempt would result in a theory full of internal conflicts, and as a result the new theory would very likely be contradictory in itself. On the other hand, it would be just a waste of resources to create a completely new discipline that has no connection with currently available studies of conversation. Therefore, the best strategy would be to create a new theoretical perspective to avoid possible theoretical conflicts, and at the same time integrate available insights from existing studies into this theoretical perspective.

The new perspective I want to propose is inherently *cultural*. As recent studies conducted by anthropologists and psychologists in the relationship between cognition and culture have shown, human cognition is essentially cultural (see Boyer, 1999, 2001; D’Andrade, 1981, 1989, 1995; Holland & Quinn, 1987; Hutchins, 1980, 1995a; Plotkin, 1997, 2001; Shore, 1996; Sperber, 1996; Strauss & Quinn, 1994, 1997; Tomasello, 1999). And

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<sup>1</sup>For example, Sperber and Wilson (1995) noted that what they developed is actually a hypothesis, but they still called such a hypothesis “relevance theory.”

it follows from such a view on cognition that the cognitive processing of conversation meaning also shows a cultural characteristic. But to successfully locate culture and cognition in a close relationship, it is necessary to extend the traditional view of cognition as something internal and individual to something external and populational.

Because I will build a theory of conversation meaning in this chapter and the next, the nature of the discussion to follow is highly theoretical and at times even metatheoretical. Looking at the history of conversation research begun in the 1960s, I believe that it is necessary to develop a unified metatheoretical background on which conversation research in various disciplines can be assessed. The study of conversation has proceeded for more than 40 years with little metatheoretical discussion, and it is high time that someone started to do it.

This study is based on philosophical realism according to which real unobservable mechanisms exist and the aim of science is to find out what those mechanisms are (see chap. 2). Unfortunately, realism is not common on the current conversation research scene, and I suspect that theoretical and metatheoretical debate by conversation researchers has been suspended mainly because of this trend in conversation research. The implicit philosophy of science behind such disciplines as conversation analysis and linguistic anthropology seems to be empiricism, as discussed in chapter 2, and their view of theory is *instrumentalist*. Many conversation researchers seem to adhere to the view that theory in science is nothing more than an instrument to be used in the organization of empirical data. They do not suspect that there are hidden mechanisms behind what can be experienced, and thus for them the empirical rather than the invisible mechanism is assumed to be the object of science.

I agree with Devitt and Sterelny (1999), who noted that the most speculative and conceptually difficult parts of science rely on philosophy. And it is this most speculative and conceptually difficult part of the science of conversation meaning that

this study now attempts. Because the discussions to follow are at the borderline of theoretical science and philosophy, it is unavoidable that they will move back and forth at a purely abstract and conceptual level rather than at an empirical level; it is not a question of how a theory can be proved to be correct through data, but how a theory can be developed from other theories and how such a theory should be theorized. In the physical sciences, especially in physics, in which the division of labor into theoretical and empirical research is a normal practice, often resulting in the division of theoreticians and experimentalists, theoretical research has recognized significance. But in the mental and social sciences, such a division of labor is not common. Still, from the point of view of realism and indeed naturalism, there is no reason why the separation of theoretical and empirical research should be impossible in sciences other than physical science. On the contrary, such a practice should be followed to enhance the scientific rigor in other sciences too, if it seems to be a powerful strategy.

To avoid a misunderstanding, I want to make it clear that I am not refuting the importance of empirical research at all; on the contrary, I believe that empirical research following a theoretical and metatheoretical conceptualization is necessary. Empirical research is as important as theoretical research. But without good theoretical frameworks, empirical research becomes a mere data collection activity that throws no new light on the development of theory. And as far as conversation research is concerned, it is the theoretical side that is weak compared to the enormous amount of empirical studies. I hope that the theory to be presented below will contribute to remedying the current imbalance in theoretical and empirical research in the study of conversation meaning.

### 5.3 Distributed Cognition

It has been assumed for a long time that cognition takes place inside individual organisms. However, this alleged fact has recently been questioned by a number of philosophers and cognitive scientists. Clark (1997) as well as Clark and Chalmers (1998) argue that the mind is embodied, and they have developed a deliberate thesis of the extended concept of the mind in which the mind is investigated within the framework of the coupling of an organism and an environment. The psychological concept of *affordance* developed by Gibson (1986) and the research program of ecological psychology (see Reed, 1996) are also based on an awareness that environment plays a crucial role in the understanding of cognition.

The idea of extended and embodied cognition, whose unit of analysis goes beyond a single organism and to the coupling of an organism and an environment, has recently been developed even further. If the cognition of an organism should be analyzed in relation to its environment, it naturally follows that organisms other than the one in question as an environment should be taken into consideration as well. Hutchins (1995a) went beyond the coupling of an organism and an environment, and proposed that researchers take a group of organisms and their surrounding environments as their unit of analysis. A cognitive system larger than an individual organism has properties that cannot be reduced to the cognition of individual organisms (Hutchins, 1995b; Hutchins & Klausen, 1996). The view that extends the realm of what is traditionally considered cognition to include a population of organisms and their physical environments is called *distributed cognition* (Hutchins, 1995a; Hollan, Hutchins, & Kirsh, 2000). The concept of distributed cognition has the potential of dramatically changing the conception of cognition in general.

A common objection to this approach to cognition would be the question "Can a unit larger than an individual have cogni-

tive and more generally mental properties?" Advocates of distributed cognition would straightforwardly answer "yes," and show empirical data, for instance, how a cockpit "remembers" its speeds (Hutchins, 1995b; Hutchins & Klausen, 1996). As a matter of fact, however, the real nature of this question is not whether the mental properties of such units can be shown empirically or not; rather, it is a metaphysical question of whether other units than that of the individual, which is often supposed to be the seat of the mind, can ever have mental properties. If the argument of the advocates of distributed cognition that a group of organisms can exhibit their own cognitive characteristics is correct, what is the difference between a social system and a distributed cognitive system?

The philosopher Ruben (1985) suggested a metaphysical theory according to which social entities can have social properties, but they cannot have mental properties; on the other hand, mental entities can have both mental and social properties. So according to Ruben, if it is possible to prove that a distributed cognitive system is a mental entity, the puzzling metaphysical problem is solved because as a mental entity rather than a social entity, a distributed cognitive system is entitled to possess mental properties. But if it is not possible to prove this, the very idea of distributed cognition must be false. Assuming that Ruben is right, this is a question that must be asked.

The advocates of distributed cognition seem to argue that a distributed cognitive system indeed is a mental entity, not a social one. For example, in the cockpit studies conducted by Hutchins (1995b) and Hutchins and Klausen (1996), a cockpit is seen as one cognitive system that includes pilots and a collection of technological artifacts, and is considered to be a mental entity that has computational features as an individual organism has. So it has been claimed that a cockpit *remembers*, which is a mental property assigned to a mental entity. In the study of distributed cognition, a collection of individual organisms and their surrounding artifacts are not seen as a social

entity. Indeed, such collection should be capable of exhibiting social properties. In any case, it is not interpreted as a social entity, but a mental entity larger than a single individual. So on the basis of the arguments of Ruben (1985), it should be possible to consider the theory of distributed cognition to be valid because as a mental entity it is capable of showing both mental and social properties.

A similar idea to that of distributed cognition, but nonetheless a distinctive one, has been developed in artificial intelligence recently. The traditional approach to computation in artificial intelligence also concerns itself mainly with the computation of an individual agent. This is analogous to the traditional approach of cognitive science in which an individual organism is considered to be the unit of analysis. However, within the last decade or so there has been a growing interest in what is called *multiagent systems* (MAS) or *distributed artificial intelligence* (DAI) (see Ferber, 1999; Weiss, 1999; Wooldridge, 2002). As the name suggests, its perspective is similar to that of distributed cognition; computation is not carried out within a single agent but carried out as a result of the interaction between a number of agents. There is one difference between distributed cognition and DAI that is empirical rather than theoretical: While the former is concerned with the nature of human cognition in a distributed system, the latter is concerned with the agent technology that employs multiagents to solve problems.

Although distributed cognition and DAI are distinct fields, it is notable that both have started to grow during roughly the same period of time, and there is a clear resemblance between distributed cognition and DAI. What DAI suggests about cognition, for example, is that there are cases in which the use of multiagents provides possibilities to solve problems that cannot be solved by a single agent (Durfee, 1999). This parallels the perspective of distributed cognition in which a problem is solved by means of the interaction of a collection of people and the artifacts surrounding them. Because of the rapid growth

of agent technology in recent years, researchers in human distributed cognition should also keep an eye on research into DAI.

## **5.4 Conversation as a Distributed Cognitive System**

Is the notion of distributed cognition relevant in any way to the study of conversation? I believe that without the notion of distributed cognition it is virtually impossible to capture the true nature of conversation. Studies in artificial intelligence (see Allen, 1995) have shown that to develop a “conversational agent,” that is, a computational agent capable of exhibiting human conversational features, language should be seen as a multiagent activity. Such a view is not pervasive in linguistics and psycholinguistics, but in some other disciplines such as linguistic anthropology and conversation analysis, it is a common view. And this is the view that should be embraced in pursuit of the development of a theory of conversation meaning.

The idea of seeing conversation as a distributed cognitive system itself is not new. As early as in the 1960s, when Hymes (1962) stressed the importance of participation in the study of communication, he suggested that researchers take a speech community rather than language as their unit of analysis. This call by Hymes, departing from his predecessors for whom language was still the primary analytical unit, was in fact a call to view communication as a distributed cognitive system. What is new in viewing conversation or communication in general from the point of view of distributed cognition is that for the first time researchers can be fully aware of the fact that the very same technique used in the study of individual cognition can also be applied to the study of distributed cognition. And as Hutchins (1995a) described, in a distributed cognitive system, researchers can step inside the system, which cannot be accomplished when an individual cognitive system is studied. So in addition to see-



ing conversation as a non-individual phenomenon, the perspective of distributed cognition enables researchers to see conversation as a unified, single cognitive phenomenon rather than a mere collection of cognitive phenomena taking place in different individuals.

Although researchers interested in language as an interactive phenomenon (Duranti & Goodwin, 1992) have been advancing the argument that language should not be studied in isolation from context and participants, their work has not been connected to other kinds of investigations of cognition. Their claim is roughly that their approach is better than other approaches that are individualistic. This is a mistake Dell Hymes also made when he argued against Chomskian linguistics in the 1960s and 1970s. If there are competing scientific theories on the same topic, it is legitimate to struggle with other theories. However, when it comes to the study of language and conversation, the problem at hand is not one of competition between scientific theories, but merely that of to what aspect of research the highest preference should be placed.

By looking at different disciplines of conversation research through the theory of distributed cognition, it is easy to notice that Dell Hymes has no necessity to compete with Noam Chomsky, though he often does, because they are doing research on different problems. Chomskian linguistics is aimed at understanding language as individual cognition or language as something working within the cognitive system of an individual human. Hymes is concerned with language within a larger cognitive system, that is, a system that comprises individuals and their surrounding environment. The concepts of speech community, situation, and events are good examples. They are not about an individual, but about a group of individuals and their surrounding environments. So the question "Which is the right one?" is itself ill-formed because it is impossible to compare things that are not comparable in the first place.

On the basis of the premise that language is a cognitive phe-

nomenon in addition to the other premise that cognition can be distributed, it can be concluded that there are units in which language exists that are larger than an individual. In linguistics, which usually studies language within the unit of an individual, language is divided into various units that can be produced by an individual such as words and sentences. To study language in a unit larger than an individual, it is necessary to postulate units of language different from those of linguistics. From this point of view, conversation is one such linguistic unit. Conversation is a cognitive system in the same way as words and sentences are. If it is too problematic to say that a sentence is a cognitive system, it can be rephrased that a sentence is an instance of a cognitive system called language. But departing from the traditional view of language as a system only within an individual, there should be no problem in saying that conversation is an instance of language, or that conversation is a cognitive system and language is the cover term for various cognitive systems such as sentences and conversations. I believe that this is the view that has been implicitly supported by ethnographers of communication, interactional sociolinguists, and other anthropologists for decades. Conversation, however, is not only a linguistic system. It includes non-linguistic features such as gestures, facial expressions, and so forth. Thus as a cognitive system, conversation is a combination of a linguistic system and a non-linguistic system. In any case, the most important point is to embrace a point of view according to which conversation is a distributed cognitive system.

Unfortunately this is the point that is often misunderstood by linguistic anthropologists and conversation analysts who view language as an interactive phenomenon. I agree that language, understood as a distributed cognitive system, is an interactive phenomenon; a distributed cognitive system exists because individuals and their surrounding environments are in constant interaction. But I do not agree with the dichotomy between language as an interactive phenomenon and language as a cognitive

(interpreted as individual) phenomenon. For example, Hutchby (2001) contrasted two views of communication: the perspective of computation, which is roughly the same as an individual cognition described above, and that of talk-in-interaction. He concluded that the computational perspective is flawed, and the perspective of talk-in-interaction is the right paradigm to be followed in the study of conversation. I admit that conversation should be viewed from the perspective of talk-in-interaction, but I see no reason to abandon the computational perspective to follow the perspective of talk-in-interaction because these two are not competing ideas and because both can be seen as cognitive approaches differing only in that one deals with an organism-environment while the other deals with a collection of organisms in an environment. Thus the concept of distributed cognition suggests that the interactive view of conversation should be seen as an extension of the cognitive or computational views based in turn on the internal working of an individual's mind. In other words, under the guidance of the theory of distributed cognition, it can be realized that interactive and cognitive or computational perspectives can coexist.

## **5.5 Meaning in a Distributed Cognitive System**

Now that I have shown how the concept of distributed cognition can be of great help for the investigation of conversation, let us move on to consider the problem of what conversation meaning is. As I have explained in the previous section, I treat conversation partly as one unit of language in the same way as a word or a sentence is a unit of linguistic analysis. Of course, conversation not only includes a linguistic system but also includes other non-linguistic systems. Linguistics makes a division between the study of the form and that of the meaning of language, and its branches are divided accordingly. Semantics is the branch of lin-

guistics that studies linguistic meaning, and its objects of study are usually divided according to units of analysis such as word meaning and sentence meaning. Traditionally the largest possible unit of study in semantics is a discourse, which is usually defined as a unit larger than a sentence. Although a discourse is larger than a sentence, it is often a monologic unit, like any other unit. So a dialogic genre such as conversation should be distinguished from discourse. This practice is often followed by researchers in natural language processing as well (see Ishizaki & Den, 2001).

Because conversation is also a cognitive system that includes a linguistic system, it should be possible to do semantic research in it in a way similar to that in which the semantics of a word or sentence. But then it becomes problematic as to how to distinguish semantics from pragmatics in the study of conversation meaning. Or, to put it in another way, is the study of conversation meaning in the realm of semantics or pragmatics?

I do not intend to enter into the debate concerning the distinction between semantics and pragmatics in linguistics here. The problem is not how to define each field in relation to another, but whether it is possible to separate them in the study of distributed cognition to begin with. Still, I want to point out that by looking at conversation from the point of view of distributed cognition, it does not make any sense to distinguish semantics from pragmatics in the traditional and conventional way. When it comes to the level of conversation or generally dialogue instead of monologue, the distinction no longer has analytical power.

First of all, it is impossible to avoid the question of whether the study of the meaning of conversation belongs to either semantics or pragmatics. According to the conventional differentiation between these fields, the difference is roughly that while semantics studies the meaning of language in isolation from external, non-linguistic contexts, pragmatics studies an individual speaker's meaning in a social and physical context. So while the

sentence “I feel thirsty” means semantically that the physiological state of the speaker is such that he or she wants water, it means pragmatically that, for example, he or she wants someone else to bring some water.

Is it possible to make this kind of semantic-pragmatic distinction in the study of conversation meaning? Let us consider the example presented in chapter 4 again.

- 1 Mother: Do you know who’s going to that meeting?
  - 2 Russ: Who?
  - 3 Mother: I don’t know!
  - 4 Russ: Oh, probably Mr Murphy and Dad said Mrs
  - 5 Timpte an’ some of the teachers.
- (Hutchby & Wooffitt, 1998, p. 16)

It is important to consider this segment as *one* system, not as a collection of separate sentences. Then the question is not about sentence meaning (semantics) or an individual speaker’s utterance meaning (pragmatics), but about a meaning produced in a single cognitive system, that is, a meaning that is produced by all participants. To quote the conversation analysts Hutchby and Wooffitt (1998) that:

Taken in the abstract, Mother’s utterance is ambiguous, and on a purely analytical level it would be problematic to assign a meaning to it. However, for CA [conversation analysis], the issue is how the *participants* understand, or make sense of, any given utterance. (p. 16)

The point made above, originally developed by Sacks on the basis of the influence of ethnomethodology (Silverman, 1998), is similar to the difference between what anthropologists call the *emic* and *etic* perspectives. Roughly speaking, the emic perspective corresponds to the native’s or “insider’s” point of view, while the etic perspective corresponds to a researcher’s or “outsider’s” point of view. As can be seen from the above

quotation, conversation analysts argue that meaning and understanding should be studied from the emic perspective. And this emic view of conversation analysis is also shared by anthropologists studying conversation, as can be seen in the following:

The analysis of everyday conversation, after all, is an attempt to get into people's life world and to understand how society and culture is "lived" in the depths. Such a goal precisely fits the way of thought of anthropology. (Sugawara, 1998, p. 30, my translation)

To illustrate the difference between conversation analysis and the distributed cognition approach, I want to point out how their research questions differ in terms of the above example. For conversation analysis the question is how the sequential organization of this segment is produced. For example, it asks why it is that Russ asks "who?" in line 2. And the explanation follows that it is because he interpreted line 1 as an pre-announcement, which according to Terasaki (as cited in Hutchby & Wooffitt, 1998) has a structure of pre-announcement-opportunity-to-be-announced. Even though conversation analysts consider meaning to be something that participants understand, it does not matter to them how such understanding itself takes place. They take it for granted that researchers can access participants' meaning by means of the next-turn proof procedure, and on the basis of participants' meaning, they analyze how sequences of conversation are organized.

For the distributed cognition approach, the question is what conversation analysts take for granted; how participants come to interpret speech acts in a certain way, and how mutual interpretation takes place. What is sought for in this approach is not how the sequential organization of conversation is structured, but how participants understand each other and produce *mutual, intersubjective* meaning. For conversation analysis, too, intersubjectivity is an important question. But for conversation

analysts, being intersubjective means that participants are able to produce sequences that are intelligible within the sequential context. As the method of the next-turn proof procedure shows, conversation analysts rarely bother themselves about what is going on in the participants' minds.

It can be said that the conversation analysts' approach is grounded in *psychological behaviorism*. Psychological behaviorism denies the postulation of internal states of mind, and instead it only examines the relationship between stimulus and response (Heil, 1998). However, this is a naive critique that confuses the stratification of the universe and the aim of sciences that follow such stratification (see chap. 2). After all, conversation analysis is not concerned with the mind, but society. The pre-announcement structure shown above is not the structure of a mental entity, but a social entity. So the conversation analysts' approach, which assumes that it is possible to perceive the interpretation of participants is not the same thing as psychological behaviorism.

Conversation analysis is thus doing appropriate things for its own purposes, and I have no reason to criticize it on those grounds. What I want to emphasize is that the problem of conversation meaning itself is not an area studied by conversation analysis, semantics, pragmatics, or any other fields. And the concept of conversation meaning in these fields is not accurate enough for a serious study of it. Duranti (1997) noted that conversation analysis can be described as the "syntax" of conversation. One possible objection to this description is that conversation analysis is a social, not a mental science, but syntax is a mental science. As I have previously shown, this critique is not reasonable because conversation analysis is not concerned with the mind. But the point of that metaphor, and the critique of conversation analysis by linguistic anthropologists, is that conversation analysis is concerned with structure, but not with meaning. And linguistic anthropology, which is currently greatly influenced by interpretive anthropology, postmodernism,

and other kinds of antinaturalist dogmas, in turn has not concerned itself with the theorization of conversation meaning at all, let alone the establishment of the semantics of conversation in contrast to the syntax of conversation allegedly practiced in conversation analysis.

Conversation meaning, from the perspective of distributed cognition, is thus not what individual speakers mean during the course of talk. It is the meaning that participants in conversation come to share as a result of interaction. In this sense, conversation meaning is inherently distributed. And the distributed meaning is cultural. The concept of responsibility in discourse discussed by Hill and Irvine (1992) is close to that of distributed cognition in that both do not take a single speaker's mind as the locus of meaning. Still, the attitude of these researchers differs from that of the advocates of distributed cognition because the former attribute the locus to social relationship rather than to a distributed cognitive system.

## **5.6 Conversation Meaning as Cultural Meaning**

The discussion presented in chapter 3 becomes relevant and important once conversation meaning is identified as both cultural and cognitive. I have shown that from the cognitive point of view culture is seen as knowledge, meaning, representation or any other learned mental property that is shared in a given human population. Cultural meaning is distinguished from idiosyncratic meaning by the fact that it is loosely shared among people. The difference between idiosyncratic representation and cultural representation is not qualitative, but they are simply two ends of a continuum called representation (Sperber, 1996). And conversation meaning is a specific type of cultural meaning that participants in the conversation come to share as a result of their conversational interaction. So the explanation of conver-



sation meaning is part of the explanation of cultural meaning.

Thus studying conversation meaning necessitates a discussion of theoretical problems in cognitive anthropology. The nature of the mechanism of how conversation meaning takes place cannot be fully captured by psychological theories alone because, as I have shown in chapter 3, it is a phenomenon that occurs in a population, in this case, in a population of conversationalists. Although approaching the problem from the point of view of anthropology should be more appropriate than from a psychological one, it is not the case that there is a ready-made anthropological theory that is able to explain conversation meaning. The reason is that in cognitive anthropology — the study of culture from the cognitive point of view — conversation has not been an object of study, and in linguistic anthropology a perspective which unites culture and cognition has not been employed in its study of conversation. Even within the framework of culture, there is currently no single perspective which is suitable for the study of the problem of conversation meaning, and so it is necessary to build a new cultural perspective which unites cognitive anthropology and linguistic anthropology.

When conversation meaning is treated as part of cultural meaning, it is necessary to define in some form what cultural meaning is. Because I have already done the “culture” part of this task in chapter 3, I will provide the definition of the second part, “meaning,” here. Cognitive anthropologists have proposed diverse answers to this problem, and the one I want to choose is the definition given by Strauss and Quinn (1997):

The meaning we will give to “meaning” here is the interpretation evoked in a person by an object or event at a given time. . . . For the time being, however, we should note that a person’s interpretation of an object or event includes an identification of it and expectations regarding it, and often, a feeling about it and motivation to respond to it. . . . In

other words, we are saying that what something (a word, an object, an event) means to somebody depends on exactly what they are experiencing at the moment and the interpretive framework they bring to the moment as a result of their past experiences. (p. 6)

This is not a very precise definition, but there are two crucial points to be drawn from this passage. First, meaning is something interpreted or experienced. Second, such interpretation or experience depends on a framework which is a result of past interpretations and experiences. I explained in chapter 3 that the concept of the mind as a *tabula rasa* that only processes information is misleading, and that the mind should be conceptualized as a combination of processors and contents. What Strauss and Quinn (1997) called a *framework* is equivalent to a schema. Briefly speaking, schemas are “the procedural devices one uses to make an interpretation; they are not any one interpretation, even the typical or most common” (D’Andrade, 1992).

The points made above give a significant insight into the identification of what conversation meaning is. Conversation meaning, as cultural meaning, consists of interpretations and experiences. This is intuitively easy to accept or is even something difficult to deny. What is important is the next point: such interpretations and experiences depend on schemas. Thus meaning in conversation that results in interpretations and experiences by conversationalists depend on the schemas that they possess. As Shore (1996) noted, meaning making is a *psychocultural* process; it is a process that needs both the psychological insight of (non-semantic) cognitive processing and the anthropological insight of meaning. This line of thought already departs from the perspective of conversation analysis, which views the mind as something empty that only responds to social structure, and also from that of linguistic anthropology, which admits the

centrality of meaning but still neglects the interplay of schemas and cognitive processing that are at work in the creation of cultural meaning.

Putting the definitions of culture and meaning together, Strauss and Quinn (1997) defined cultural meaning as follows:

*A cultural meaning* is the typical (frequently recurring and widely shared aspects of the) interpretation of some type of object or event evoked in people as a result of their similar life experiences. (p. 6)

Following from this and from Strauss's and Quinn's definition of meaning, it can be concluded that the existence of a typical interpretation is only possible if shared or similar frameworks exist in people. So cultural meaning is possible only if those who experience it have shared schemas, that is, cultural schemas. This suggests that conversation meaning as part of cultural meaning is only possible if conversationalists have cultural schemas. For the explanation of conversation meaning, then, researchers should first identify what such cultural schemas are, and then show how cultural schemas work to produce conversation meaning.

It should now become apparent that when studying conversation meaning, culture is the central concept, and thus any theory of conversation meaning that avoids the issue of culture or that does not fully embrace the idea of culture is fundamentally flawed. Some of the leading theories of language and communication suffer from this problem even though at first sight they resemble the theory I am advancing here. In addition to the conversation analytic and linguistic anthropological theories already presented, such theories include the notion of joint action (Clark, 1992, 1996), conversational implicature (Grice, 1989; Levinson, 2000), and speech act theory and the theory of intentionality (Searle, 1969, 1983, 1990).

At the fundamental level, the idea of distributed cognition seems to be related to Clark's joint action theory (1992, 1996).

However, it differs from the current theory of conversation meaning precisely in that it does not incorporate the notion of culture at all. The theories of conversational implicature (Grice, 1989; Levinson, 2000) and speech act theory (Searle, 1969) have the same problem; in their theories meaning and culture are not integrated. Also, these theories do not consider a unit larger than an individual, and they are constrained on the cognition of a single organism.

The recent theory of collective intentions advanced by the philosopher John R. Searle (1990) looks as if it shares the same thesis with the theory of distributed cognition, that is, the thesis that collective cognition has a property not reducible to individual cognition. He proposes five theses of collective intentions, and what is especially interesting in the current context are the first three:

*Thesis 1*

There really is such a thing as collective intentional behavior that is not the same as the summation of individual intentional behavior.

*Thesis 2*

We-intentions cannot be analyzed into sets of I-intentions, even I-intentions supplemented with beliefs, including mutual beliefs, about the intentions of other members of a group.

*Thesis 3*

The thesis that we-intentions are a primitive form of intentionality, not reducible to I-intentions plus mutual beliefs, is consistent with these two constraints.

The theory of collective intentions is thus based on two premises: first, there is such a thing as intention (we-intention and I-intention), and second, there is such a thing as mutual belief.

This theory would be in a serious trouble if these two premises are shown to be false. And in fact, both premises seem to be false according to researchers who are critical of Searle's philosophical theory.

On the first premise, the linguistic anthropologists Duranti (1992, 1993, 1997) and Du Bois (1992) showed how the existence of intention itself is suspect. Their ethnographic data suggest that the concept of intention does not necessarily exist in the non-Western world, and thus it is not a universally existent notion. Duranti (1992) argued that "the relevance assigned to the speaker's intentions in the interpretation of speech may vary across societies and social contexts" (p. 26), and the assumption of a straightforward relation of intention to meaning "appears too limited or overtly ethnocentric to anthropologists and linguists who have been looking at non-Western or (within the US) non-mainstream modes of communication" (p. 25). In a similar line of argument, Du Bois even argued that intention is not a necessary criterion for meaning at all. What these studies have shown is that the premise that intention is involved in, or is central for, the interpretation of meaning is highly problematic when it is investigated empirically.

On the second premise, the theory of mutual belief has been criticized by pragmatists (Sperber & Wilson, 1995) as well as a sociologist (Nishizaka, 1997). Sperber and Wilson pointed out that what they call the mutual knowledge hypothesis exemplified by Lewis (1969) and Schiffer (1972) has a serious problem as a model of human cognition because of the paradoxical nature of nth-order beliefs. In the same vein, any theory developed from the mutual knowledge hypothesis has to provide an answer to this critique. Clark and Marshal (1981/1992) and Clark and Schaeffer (1989/1992) developed a theory called *grounding* by modifying previous models based on the mutual knowledge hypothesis. According to Sperber and Wilson, the fundamental notion of mutual knowledge itself is flawed, and thus it is a problem that cannot be remedied by being extended or modi-

fied. Interestingly, the notion of grounding has received a significant amount of attention from natural language processing researchers whose primary concern is the computational theory rather than the theory of human cognition, but it rarely appears in the literature of pragmatics, conversation analysis and linguistic anthropology whose primary concern lies in humans.

The philosophically oriented theory of collective intentions or mutual beliefs is of a different kind from the empirically oriented theory of distributed cognition. Unlike the theory of collective intentions or mutual beliefs, that of distributed cognition does not suffer at all from attacks on the notion of intentions and the mutual knowledge hypothesis because it does not rely on such empirically unrealistic ideas.

## 5.7 Cultural Schemas Versus Relevance

The present line of argument has come to a point where the explanatory scheme of conversation meaning has become clear: To explain conversation meaning as part of cultural meaning requires that the concept of cultural schemas be applied to it. In the case of conversation, the process of applying cultural schemas to a given interpretation is called “inference.” However, my theory is not the only one that tries to explain meaning through the process of inference. Of the various theories that employ a similar scheme, *relevance theory* of Sperber and Wilson (1995) is perhaps the most widely accepted one. In what follows I want to show how the explanation of inference in my theory differs from relevance theory, and to explore the origin of the difference.

Relevance theory is a beautiful theory that can be expressed in two sentences. The most recently revised version of the theory, also called the *principles of relevance*, looks like this:

- (1) Human cognition tends to be geared to the maximisation of relevance.

- (2) Every act of ostensive communication communicates a presumption of its own optimal relevance.  
(Sperber & Wilson, 1995, p. 260)

Because relevance theory is packed so neatly, it also needs deliberate unpacking to become fully understandable. Such unpacking is done in Sperber and Wilson (1995), and a thorough presentation of it is beyond the scope of the current study. So I will only consider some of the main points that are relevant to the distributed and cultural view of conversation meaning.

As can be seen from the definition, relevance theory consists of two parts. It is a theory of communication in itself, but is built on top of a theory of cognition. And the theory of cognition on which it depends says that biological cognition has evolved in such a way so as to gain maximal benefit with minimal effort. According to relevance theory, relevance is virtually the only concept needed to explain ostensive communication. And the maximization of relevance can be calculated as the amount of processing effort and contextual effects. It is ironic that relevance theory is based on the amount of maximization and optimality despite its strict rejection of the method of quantification. After all, what relevance theory says about communication is that a biological organism wants maximal effects with minimal effort, and the same principle applies to communication: People interpret an utterance in a way that requires least processing effort and gains most contextual effects.

My theory of conversation meaning shares with relevance theory the idea that ostensive communication is about inference, and the key to the understanding of meaning is the search for the mechanism of such inference. They also have a common ground in that it is not only process but also content and their relationship that should be studied. However, they differ significantly in that while my theory aims at explaining meaning in a distributed cognitive system, relevance theory, as a pragmatic and psychological theory, deals with meaning at the level

of an individual. Moreover, the theories of cognition on which my theory and relevance theory depend are completely different. As a result, though both theories concentrate on the mechanism of inference, they draw very different conclusions on the nature of it.

In relevance theory, inference is thought to be a deductive process that works on knowledge. And the only deductive mechanism used in inference is *elimination rules*.

The only deductive rules available for use in the spontaneous processing of information — the only rules which in any interesting sense form part of the basic deductive equipment of humans — are elimination rules. (Sperber & Wilson, p. 96)

Thus relevance theory is committed to the traditional, symbolic paradigm of computation as a model of cognitive explanation. The deductive rules function as what is called an algorithm in computer science. And knowledge that consists of current as well as new information functions as what is known in computer science a data structure in computer science. It is because of this symbolic perspective that pseudocodes are occasionally presented (e.g., p. 111) for the explanation of relevance theory.

As I have argued in chapter 3, the concept of the mind on which relevance theory is based, that is, the idea that process and content residing in the mind are separable, is flawed. And from the discussion of cultural meaning, it should be clear that relevance theory neglects, again like the disciplines presented in chapter 4, the fact that algorithms that process data structures are not completely separable from the data structures themselves in biological cognition. Relevance theory claims that deductive rules or algorithms are separable from knowledge or data structures. But according to the theory of cultural meaning, schemas on which experiences depend not only cause experiences, but also are modified as a result of experiences. In this model, the two components of a cognitive system, things that



process and things that are processed, are integrative.

In short, what relevance theory suggests is the mechanism of inference and what I think is that mechanism is are different as a result of different conceptions of the mind. For relevance theory, it is deductive rules that explain how humans make inference. For the theory I am developing here, it is cultural schemas that explain how humans make inference and, as a result, produce meaning. So while relevance theory considers algorithms operating in the mind to be the mechanism of inference, I argue that it is rather schemas, or what relevance theorists call knowledge, that explain the working of the mechanism.

The difference between relevance theory and the theory presented here might seem as if it is simply a matter of a preference for either algorithms (deductive rules) or data structures (cultural schemas) to explain the interpretation of meaning. But the difference is not so simple; it is not about preference, but about the very concept of cognition. The concept of cultural schemas is based on a specific model of cognition, or a philosophical theory of the mind, different from the model on which relevance theory is based. In contrast to the symbolic approach of relevance theory, cultural schema theory is based on *connectionism*. Thus to explain how cultural schemas explain conversation meaning, it is necessary to discuss connectionism, which is the topic of the next chapter.

## 5.8 Conclusions

This chapter discussed the nature of conversation meaning and showed several steps by which it can be theorized. Conversation was defined as a distributed cognitive system, and this in turn led to the idea that conversation meaning is cultural. As a part of cultural meaning, it is possible to apply the notion of schemas to conversation meaning, and the search for the mechanism of interpretation called inference by means of the

application of cultural schema theory then becomes the central task of the study of conversation meaning. Compared to other theories of conversational inference, the theoretical frameworks of distributed cognition and cultural schemas, both neglected by them, distinguish the present theory. On the basis of these findings, I will further explore and develop this theory by situating it in the theoretical framework of connectionism in the next chapter.

# Chapter 6

## Toward a Connectionist Theory

### 6.1 Introduction

In the previous chapter I argued that conversation is a distributed cognitive system, and that as a property of such a system the meaning of conversation is cultural. The ideas that conversation is cognitively distributed and that conversation meaning is cultural are the building blocks of the theory of conversation meaning developed in the present study. However, merely saying that conversation meaning is cultural does not explain anything. It only identifies and highlights the characteristics of conversation meaning. The next step would be to describe the way conversation meaning is processed in the human mind.

Here I will place the theoretical perspective presented in the previous chapter in a theory called *connectionism*. Connectionism is a theoretical view of cognition that is based on the structure of the brain. According to connectionism, because the brain is the seat of the mind and because the brain consists of neural

networks, models of cognition should resemble the organization of neural networks. In practice this means that models of cognition would be created by means of artificial neural networks often implemented as computer programs. If cultural schemas are cognitive entities, it should be possible to situate them in artificial neural networks, and this would provide further insights into the nature of conversation meaning.

## **6.2 Connectionist Cultural Anthropology**

For the theory of conversation meaning to be a cognitive and cultural theory, it is impossible to bypass the problem of symbolism and connectionism. Nowadays nearly all theories of cognition can be categorized as symbolic, connectionist, or a mixture of both (often called a hybrid theory). As a matter of fact, in addition to these two approaches, there is an emerging alternative that applies the mathematical theory of dynamical systems to the study of cognition. The relationship between the dynamical systems approach and connectionism, however, is not yet clear at the moment because the application of dynamical systems is still very new. Bechtel and Abrahamsen (2002) showed that there are two views of the relationship between the dynamical systems approach and connectionism. According to one view, presented by van Gelder and Port (1995), the dynamical systems approach is a new paradigm that replaces both the symbolic and connectionist approaches. According to another view, presented by Horgan and Tienson (1996), the dynamical systems approach only complements symbolism and connectionism, and thus it is not a new third approach.

In the history of cognitive anthropology, most studies are based on symbolism, as can be seen from the historical account provided by D'Andrade (1995). Recently, however, a few anthropologists have investigated the possibility of applying the

connectionist approach to the study of culture. Shore (1996) described the relevance of connectionism to the problem of cultural meaning as follows:

Connectionist models . . . provide a promising avenue for making the case for culture in mind because they have a number of characteristics of great significance for a theory of cultural cognition. . . . They provide a way to understand how models [schemas] can be both shared within a community and idiosyncratically modified through experience. (pp. 349–350)

Still, Shore's discussion continues to remain merely as a prospect. Like him, some anthropologists have realized that connectionism, which has been developed outside of anthropology, has great relevance to their research problems. However, anthropologists have not fully developed connectionist theories of culture yet. The point of view of connectionism has been incorporated into anthropological studies, but actual models of parts of culture have rarely been created. This is perhaps because of the short length of the history of connectionism in anthropology. Schema theory was incorporated into anthropology in the 1980s, and it is only since the 1990s that connectionism has become known in the anthropological community (D'Andrade, 1995). Thus Shore's attempt was one of the first lengthy studies to firmly incorporate schema theory and extend the discussion to connectionism.

In psychology the situation is quite different: Nowadays the use of connectionist models seems to be well established in cognitive psychology (see Ellis & Humphreys, 1999). In cognitive anthropology, which often borrows concepts from cognitive psychology, however, it seems that connectionist models have just arrived on the scene. In 1998 Strauss and Quinn published *A Cognitive Theory of Cultural Meaning*, which is dedicated to the account of the possibility of understanding cultural mean-

ing by means of schema theory and connectionism. For Strauss and Quinn the problem was largely about shared meaning and understanding, and they explain how and why connectionism may help understand such phenomena better than symbolism. The book was the beginning of *connectionist cultural anthropology*, but Strauss and Quinn did not develop actual connectionist models in their research. Rather, they used connectionism as a new theoretical guide to investigate various case studies. Still, it is a new way of approaching culture, and the context of connectionist cultural anthropology in addition to connectionist psychology provides a necessary environment for the theory of conversation meaning.

### 6.3 Neural Networks

Artificial neural networks (hereafter simply neural networks) provide an alternative model of cognition to the symbol-based, good-old-fashioned-artificial-intelligence (GOFAI) model of cognition. It is also called the *connectionist* or *parallel distributed processing* model. To avoid confusion, I will use the term *connectionism* when comparing those approaches to that of symbolism as a paradigm of the study of cognition and will use the term neural networks to refer to the actual model. Thus when the connectionist and symbolist paradigms are discussed, it is assumed that this discussion is a theoretical discussion of the nature of cognition.

A thorough presentation of neural networks is, of course, neither the aim of this section nor a necessary condition for the current purpose. However, to understand the nature of the connectionist model of cultural schemas, it is necessary to show the basic features of neural networks, and especially the way they differ from the symbolic paradigm. Because there are some advantages in connectionism that cannot be found in symbolism, connectionism has become popular in cognitive psychology es-

pecially since the 1980s. The features of neural networks are mostly mathematical, and there is no other way to understand connectionism other than to grasp its mathematical nature. The main characteristics of neural networks are as follows:

1. Information processing occurs at many simple elements called neurons.
2. Signals are passed between neurons over connection links.
3. Each connection link has an associated weight, which, in a typical neural net, multiplies the signal transmitted.
4. Each neuron applies an activation function (usually nonlinear) to its net input (sum of weighted input signals) to determine its output signal.

(Fausett, 1994, p. 3)

The way the connections between neurons are structured is called *architecture*, and there are three different kinds of network architectures: single-layer feedforward networks, multilayered feedforward networks, and recurrent networks (Haykin, 1999). I will provide a simplified version of the explanation given by Haykin using a neural network consisting of one layer of *inputs* and an *output* (see Figure 6.1). *Signals* denoted by  $x_j$  function as inputs of neuron  $k$ . Usually *bias* denoted by  $b_k$  is also included as an input signal. Between the neurons there are *connection links* that are characterized by certain *weights* denoted by  $w_{kj}$ . A neural network is a system that calculates what it receives as inputs and produces a certain output. And there is a certain kind of algorithm that is in use during the calculation. The output denoted by  $y_k$  is calculated as follows. First, signals  $x_j$  and weights  $w_{kj}$  are multiplied. And then the results are

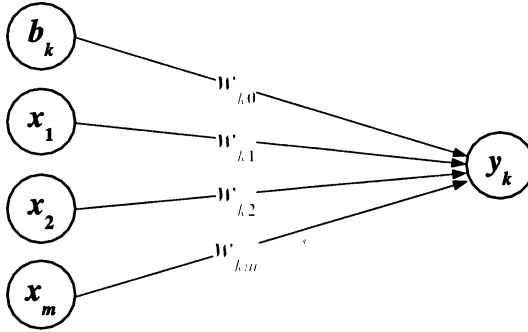


Figure 6.1: A Simple Neural Network

summed. Let the sum be  $u_k$ . Then,

$$u_k = \sum_{j=1}^m w_{kj} x_j. \quad (6.1)$$

The weight of the bias is often supposed to be +1. When the bias is added to  $u_k$ , the result is called the *induced local field* denoted by  $v_k$

$$v_k = \sum_{j=1}^m w_{kj} x_j + b_k. \quad (6.2)$$

The output  $y_k$  can be obtained by applying an *activation function* denoted by  $\varphi$  to the induced local field, thus

$$y_k = \varphi(v_k). \quad (6.3)$$

There are varieties of activation functions, both linear and non-linear. One of the most often used activation functions is the *sigmoid function*, and an example of the sigmoid function is the



*logistic function*

$$\varphi(v_k) = \frac{1}{1 + e^{-av_k}} \quad (6.4)$$

where  $a$  is called the *slope parameter*. Depending on the amount of the induced local field, the output neuron is either activated or not activated, and the value of such a dividing line is called the *threshold*.

Let us look at the way symbolism and connectionism differ in representing information by examining a very simple case described by Fausett (1994), the logical operator *AND*. In the symbolic paradigm, the *AND* operator can be represented as a series of rules. When T is true and F is false, the rule says

```

if T and T
  then T
else if T and F
  then F
else if F and T
  then F
else if F and F
  then F

```

This rule is applied to the input data, and depending on the value of the input the result is obtained. Let us do the same thing using the connectionist paradigm this time. Suppose that there are two input neurons and one output neuron, and there is no bias (see Figure 6.2). Two signals  $x_1$  and  $x_2$  can have either 1 or 0 as their values. The weights  $w_{k1}$  and  $w_{k2}$  are both 1, and the threshold is 2. Then, only when both  $x_1$  and  $x_2$  have the value 1, does it satisfy the threshold 2, and the output  $y_k$  is activated. In the other cases,  $v_k$  is smaller than the threshold 2, and  $y_k$  remains unactivated. In other words, this function as the *AND* operator. What is important here is that there are no rule-like representations for four different cases. There needs to be four different rules in the symbolic version, but the neural

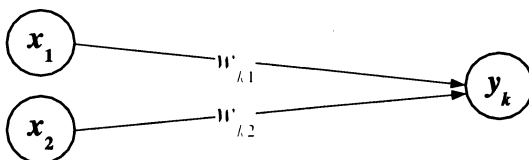


Figure 6.2: *AND* Operator in a Neural Network

network only calculates what comes in and outputs the result. Still, it works as and can be interpreted as the *AND* operator.

This is only one aspect of the difference between the symbolic and connectionist paradigms. One of the greatest differences between these two is that because neural networks do not represent information explicitly by means of symbols, they are capable of representing *fuzzy* cases. Human cognition is not something that always yields either True or False. There are cases in which we think that something is more likely to be True than to be False, but we are not quite sure. Such fuzzy concepts cannot be well represented by the symbolic paradigm, and it is one of the strengths of connectionism. Compared to the symbolic systems, then, neural networks provide more flexibility.

## 6.4 Schemas in Neural Networks

Having seen the basic features of neural networks, it is now possible to consider the relationship between them and cultural

schemas. Within the connectionist framework, “a schema is not a set of sentences but rather a pattern of interaction among strongly interconnected units” (Strauss & Quinn, 1997, p. 52). For example, the famous restaurant schema provided by Bower, Black, and Turner (1979) is symbolic in that each slot (particular information within a schema) is represented symbolically in order. Part of the schema is as follows:

Open door  
Wait to be seated  
Go to table  
Be seated.

If cognition is modeled using this type of representation, there is no way to avoid what is called the frame problem. Humans following this schema cannot do anything or repeat the same action forever if, for example, the door cannot be opened for some reason (e.g., it is broken). But it is hard to believe that real humans would act like that because human cognition is flexible. Interpreted in the connectionist way, the restaurant schema is not represented as above; instead, it is represented as the weights of connection links between the neurons representing, for example, “open door” and “wait to be seated.” If the door cannot be opened, there are many other possible actions to choose from because the “open door” neuron is connected to neurons other than “wait to be seated.” It is only that the weights between these two neurons are the strongest that “wait to be seated” is usually followed by “open door.”

Culture is often seen to consist of the rules shared by a certain population. The restaurant schema presented above is also a good example of a cultural rule shared by those who were the subjects of the experiments conducted by Bower et al. (1979). The restaurant schema might differ, for example, in Finland and in Japan. It is a cultural schema, and such a schema is often said to be a rule to be followed in a certain place.

As D'Andrade (1995) noted, connectionism provides a new way of thinking about culture as a collection of rules. According to symbolism, rules are rigid, stable, and logic-like entities. In connectionism, on the contrary, they are soft, unstable, and neuron-like entities. They are represented only as weights of connection links between neurons; no sentence-like propositions are needed any more. They are dynamic, that is, they are constantly changing. For the cognitive theory of culture, the connectionist view seems to fit what culture is envisaged as much better than the symbolic one. Thus the difficult problem of the flexible nature of culture that is encountered when culture is conceptualized as mental representations or schemas can be solved by thinking in a connectionist way because it provides an alternative way of looking at culture as a mental representation.

There are some problems in considering culture as schemas implemented in neural networks. First of all, there are difficulties in the representation itself. How do researchers represent cultural schemas in neural networks? In other words, the gap between culture as a meaningful representation and the numerical characteristics of neural networks is so wide that it makes the implementation of actual cultural schemas in this way difficult. This seems to be the reason why so little research that actually attempts implementing cultural schemas in neural networks has been done. Second, because any cultural schema is virtually connected if culture is seen as integrated and holistic as anthropologists often claim, it is in practice difficult to model a certain part of culture. These are the problems that needs be overcome in the future if researchers truly want to consider culture as schemas implemented in neural networks, and it remains to be seen how such problems can be solved.

## **6.5 Distributed Cognition and Connectionism**

To create a theory of conversation meaning that satisfies the premises of distributed cognition and connectionism, it is necessary to explore the compatibility of these two approaches. For the theory of distributed cognition, the unit of analysis is the combination of individual cognitive systems and their environment. So in the case of conversation this means that the unit of analysis is a combination of the participants to conversation and their environment. In contrast, connectionism suggests that the information processor consists of a collection of neuron-like units. Does this mean that each participant should be considered to be a neural network surrounded by an environment or that the system of participants and their environment should be considered to be a neural network?

In the former case, existing connectionist models can simply be applied to the study of how participants process information. Although the task at the level of each participant is rather easy, there remains a difficult task of conceptualizing how these various participants interact with each other through their environment. In the latter case, the combination of individual participants and their environment is seen as one neural network. Such a neural network produces meaning as one system, and the way participants reach a shared meaning is studied within a single network. The problem in this approach is that it seems too simplistic to consider each participant to be just a binary operator. It is intuitively not appealing to think of human participants as neuron-like beings, and it is empirically doubtful if such a model can ever successfully produce complex meaning.

In social scientific simulations in which the functioning of each agent is kept at a minimal level, as in classical economics, the latter approach is more appealing, and indeed it has been empirically successful (see Gilbert & Troitzsch, 1999). But the

problem is a cognitive one, not a social one. And so the method used in social simulation does not necessarily apply to the study of distributed cognition. In distributed cognition, it cannot be ignored that each individual organism possesses a high cognitive ability. Also, researchers cannot pretend that the environment is just something existing between organisms because an environment plays a crucial role in cognition (Clark, 1997).

To overcome the constraints of both approaches, I suggest that conversation meaning can be conceived as a phenomenon taking place in a *neural network of neural networks*. It has already been argued by others (Shore, 1996; Strauss & Quinn, 1998) that to explain cultural phenomena, it is advantageous to use connectionist models rather than symbolic ones. But it is not enough to conceptualize cultural meaning as something taking place in neural networks. A further step should be taken, and cultural meaning should be conceived as existing in a neural network of neural networks because it is a phenomenon that can only exist if there are more than two people. So the problem is how to think about the nature of relationships consisting of neural networks. If distributed cognition is a kind of cognition, and connectionism as a model of cognition obtains, distributed cognition should also consist of neural networks. Then it follows inevitably that each unit of distributed cognition consists of units that are also neural networks, because each unit is also a cognitive system. The theory of conversation meaning as occurring in a neural network of neural networks also explains why conversation meaning is such a complex phenomenon that it cannot be explained by existing theories of monologic utterance meaning.

## 6.6 A Theory of Conversation Meaning

I have shown how to utilize the theory of neural networks in conversation research. By putting this and the arguments discussed in the previous chapter together, I will present the final stage of my theory of conversation meaning grounded in the theories of cultural schemas and neural networks.

Humans experience meaning to be a result of the interaction between schemas and the environment. The environment is constantly changing; the outer world in which humans live is in flux. Experiences are phenomena that take place in such a dynamic environment, and it is only with the interaction through the environment or the internalized environment constituted as a result of prior interaction that meaning arises.

Schemas are general knowledge structures by which knowledge is organized. Mental representations that humans have are not arbitrarily stored in the mind; they are organized with the help of schemas. From the symbolic point of view, schemas are structured in a similar way as logic is structured, and they consist of language-like propositions that are ordered in a certain way. The limits of this model are clear as I have presented in the previous discussions. In a word, the symbolic model cannot adapt to the ever-changing environment in any other way but by putting more and more propositions into the memory. It is psychologically not realistic to suppose that humans memorize more and more propositions expressed in language-like representations. If this were true, an unlimited number of schemas would be needed *before* humans would be able to act rationally in the real-world environment.

By putting forth the alternative perspective of connectionism, schemas are not only seen as structured, but also as connected to each other by means of connection links. Moreover, each connection link has a specific weight, which suggests that

there are infinite variations of relationships between schemas. This produces flexibility to human cognition because there is always an infinite number of choices that humans can make in reaction to the dynamic environment. Complete knowledge of the environment is not needed before we act, but necessary knowledge is learned as a result of the interaction of the body and the environment.

Cultural meaning is thus a dynamic phenomenon happening in time. It is not what exists before or after the interaction of schemas and the environment, but what happens during the interaction itself. Meaning, knowledge, and schemas are all dynamic contrary to the conventional view that they can be studied as static objects as if they were "things." The same applies to culture; culture consists of schemas shared in a given population, and cultural schemas are dynamic like any other schemas are.

The processing mechanism and knowledge stored in memory become closer and closer when considered from the connectionist point of view. It is not that a central processing mechanism works on static knowledge to produce meaning, but both the algorithm and the knowledge are dynamic and they engage in the meaning production process together. As the concept of the mind as a *tabula rasa* is misleading, the concept of the process of meaning as the result of independent processing algorithm only is misleading too. Although a certain algorithm is employed in neural networks, the algorithm alone does not determine the value of the output; the algorithm together with the nature of knowledge stored in a neural network determines the output.

Cultural meaning, then, takes place as a result of the interaction of cultural schemas and the environment. Basically if two people share a cultural schema in relation to what happens in a given environment, they should share a similar cultural meaning. At the lower level, a cultural schema is represented in neural networks, and cultural meaning takes place because a similar state of weights is achieved as a result of the interac-



tion of cultural schemas and the environment. In principle, it is possible that different meanings are achieved as a result of the interference from other schemas even though a relevant cultural schema is shared in a population.

But humans do not produce meaning in such a passive way. They try to reach a similar state of weights even if they have difficulty in doing so. Conversation meaning, as part of cultural meaning, is such an active process. So it is not only that conversationalists share some cultural schemas with which they are able to understand each other or produce meanings similar to other conversationalists but that they also try to create cultural meaning. Still, some cultural schemas seem to be necessary to achieve that goal: language, the rules of turn-taking, knowledge of the topic discussed, shared understanding of the physical context, and so on. As a matter of fact, part of the research program of the ethnography of communication is to find out such cultural schemas as are needed to produce conversation meaning.

Indeed, by extending this view the rules that conversation analysts have found can be seen as cultural schemas. Porpora (1993) argued that cultural rules do not exist in the realm of society but in that of the mind. For example, the turn-taking rules discovered by Sacks, Schegloff, and Jefferson (1974) are from this point of view cultural schemas. Their original research interest lay in the organization of conversational sequences, which are produced by such rules. But the rules that they discovered are shared by participants, and by following such rules they are able to produce intersubjective meaning. As cultural schemas, the rules of turn-taking can vary from place to place and time to time (see Moerman, 1988; Sugawara, 1998a, 1998b). Thus turn-taking rules are one of the most important cultural schemas that are employed during the process of conversation meaning. When someone says "Hello," it is by virtue of the cultural schema of a greeting-greeting adjacency pair that it is possible to interpret the meaning of the returning "Hello" or the absence of it.

But virtually any kind of cultural schemas can participate in the process of conversation meaning; humans employ a vast amount of knowledge during conversation, and any knowledge can affect the inference made in the creation of conversation meaning. So it is not important to think about what cultural schemas are employed in conversation meaning; instead, it would be much more fruitful to study how cultural schemas engage in the process of conversation meaning.

It should now be clear that cultural schemas are necessary for humans to conduct conversation. However, in the study of the process of meaning called inference cultural schemas are rarely considered to play a necessary role. Conversational inference has been studied by researchers in various disciplines as I showed in chapter 4, but most of them ignore the fact that inference takes place within a population. However, inference during conversation should not be conceived as merely what an individual does. Instead, such inference should be conceived as a mutual process of individuals involved in conversation.

I pointed out above that conversation meaning is implemented in a neural network of neural networks by extending insights from the theory of distributed cognition. Conventional theories of conversation inference thus take into account only the aspect of a lower-level neural network, and they ignore the higher-level neural network that should be the place in which conversation meaning resides. Conversation meaning is achieved by attempts at producing meaning that is as close as possible to the meaning of the interlocutor. In terms of the theory of neural networks, conversation meaning is a process in which a neural network tries to achieve the same state as the other neural networks by using the others' outputs as inputs. So there is a recurrent process between the networks until they have some certainty of achieving a similar state.

Naturally there is the practical problem of how to determine if networks have achieved similar states or not. But it should be clear that the aim of inference during conversation is to achieve

a situation in which each network is to some extent certain that it shares a similar state with others. Conversationalists usually make some kind of response to the utterances of interlocutors. For example, as conversation analysts have shown, even story telling, which at first sight looks like a monologue at first sight is accomplished with interlocutors nodding, laughing, saying “ah uh” and so on (e.g., Sacks 1989). It seems that the reason why we behave in that way during conversation is that we need to ensure the degree of sharedness of meaning in the course of it.

As a result of the successful process of conversation meaning, the amount of shared knowledge increases, which in turn further promotes the possibility of cultural meaning. Because the process of cultural meaning is a kind of learning process by which humans share with others new information, conversation meaning is also a learning process. It is a learning process that takes place in lower-level neural networks as well as in higher-level neural networks. The meaning obtaining within lower-level networks is close to the concepts of conversational inference and utterance meaning. But conversation meaning exists at a higher level than such individual processing because it involves more than two people and it is the meaning shared by such people.

The process of inference involved in conversation, thus, cannot be investigated by isolating a conversationalist and by studying his or her inferential process. Only within the context of conversationalists as a group does it become possible to uncover the real mechanism of inference that conversationalists employ. So the mechanism of inference cannot be the deductive rules that relevance theorists (Sperber & Wilson, 1995) propose.

The inferential process that produces conversation meaning is, after all, the learning process taking place within lower-level networks adjusting to other neural networks as well as the learning process of the higher-level network as a whole. Thus I want to propose that the inferencing mechanism is the learning process that takes place in an individual’s neural network consisting of cultural schemas and in a higher level neural network as a re-

sult of the learning process of the lower-level neural networks. This idea of the mechanism of inference producing conversation meaning is in harmony with the synchronic view of culture as shared schemas and the diachronic view of culture as a learning process.

## **6.7 Conclusions**

The theory of neural networks is a good complement to cultural schema theory. They share the fundamental view of cognition as flexible and “soft.” And the theory of conversation meaning presented here acquires its explanatory power when it is situated in the context of connectionism. The creation of conversation meaning is a cultural process in which participants come to share similar meaning by applying schemas that they have. What this theory suggests is that conversation meaning cannot be explained by some simple set of rules that the participants have, but it can be explained by considering the participants and their environment as the unit of study. Another reason why existing theories of meaning cannot explain conversation meaning well boils down to the misleading view of the mind as a tabula rasa containing independent rules like algorithms and knowledge-like data structures. By looking at human cognition as cultural, it becomes possible to overcome the difficulties that the other theories possess.

The theory of conversation meaning presented here is a first step toward the comprehensive understanding of it. It is necessary that artificial neural networks are actually built and compared to human data to test this theory. And it is likely that unexpected practical problems will arise in the course of such empirical inquiry. However, at the moment the real value of this theory does not lie in its possible empirical counterpart even though it might strengthen the theory. Its real value lies, as is the case with any theoretical research, in the fact that it

provides a new way of looking at the world. As I have repeatedly shown, conversation should be *seen* as a distributed connectionist cognitive system in which cultural schemas operate to infer meaning. This is the way I see conversation meaning, and this is what I believe is the best way of seeing it. In current conversation research, there is lack of debate over how conversation should be seen, or at best there are disciplinary struggles to dominate the field. For the multidisciplinary and comprehensive study of conversation to be scientific and rigorous, it is necessary that researchers argue seriously about how to see conversation in the first place. The theory presented here has been such an attempt.



## Chapter 7

# From Theory to Practice

In this chapter I proceed from theory to practice. I will consider the practical implications of the theory of conversation meaning developed above. There are implications that might be relevant to several areas of practice. In addition to second language education, such areas as intercultural communication, translation, and language technology would seem to profit from the theory of conversation meaning. However, I will restrict my attention only to second language education here.

First and foremost, the theory implies that *culture matters* in second language education. On the basis of the theory, it is now possible to scientifically claim that culture is needed in second language education, and even more, that culture should be placed at the center of it. At the level of linguistic meaning that is produced by individual utterances, most of the content of teaching in current second language education suffices. Of course, in addition to vocabulary, grammar, and pronunciation, pragmatic skills are needed. But pragmatic skills have already

received attention in second language education, and the use of dialogues in different kinds of situations is an example of such pragmatic awareness.

The present study showed that such linguistically based content teaching is not enough for learners to appropriately understand meaning in conversation. Put in another way, there is a limit to the application of linguistic knowledge to the comprehension of conversation meaning. Conversation meaning is created by cultural schemas in which linguistic schemas are included, but linguistic schemas are merely part of cultural schemas. The point is that varieties of cultural schemas other than linguistic ones are also actively used during the processing of conversation meaning, and non-linguistic cultural schemas can affect meaning even more powerfully than linguistic schemas.

What should be done, then, to enhance learners' ability to comprehend and produce conversation meaning? One thing that can be done is the use of ethnography in second language education. I do not mean that applied linguists should employ the ethnographic method, but I mean that ethnographic knowledge should be blended into language teaching materials. In fact teachers have been making such an attempt for decades, and it is known by various names in different countries: for example, cultural studies in the English-speaking world, *Landeskunde* in the German-speaking world, and *Nihonjijou* in the Japanese-speaking world. However, attempts at "knowing the people who speak the target language" seem to have often remained at the level of ad hoc ethnography. Such ad hoc ethnographies simply present people in a stereotypical way and says, for instance, that Finns are such and such people. They are often based on language material writers' own experiences with the people, and little systematic endeavor, let alone a serious anthropological attitude, can be found.

The problem is as practical as it is scientific. It would be felt to be ridiculous if teaching materials were created on the basis of the personal linguistic experiences of the writers. Writ-



ers try to base their description of the language as closely and accurately as possible on current trends in linguistics. Do writers base their writings on cutting-edge trends in anthropology when they describe the culture of people speaking the target language? I strongly doubt that this is the case, and the reason does not seem to be that material writers and language teachers are unaware of the problem. There also seems to be a problem on the researchers' side.

The relationship between linguistics and applied linguistics has been very close, has also the relationship between psycholinguistics and applied linguistics. The relationship between anthropology and applied linguistics has, however, been limited. Furthermore, the distance between anthropology or anthropologically oriented applied linguistics and the world of second language education has been so great that it is even hard to find any real contact between them in the history of both enterprises.

This fact is also reflected in the training of second language teachers. Usually trainees are educated in languages, linguistics, education, and various areas of applied linguistics such as second language acquisition research. But as far as I know, anthropology is not included in their syllabuses. The need of cultural knowledge only becomes apparent when those trainees become teachers and engage in cultural activities such as inviting visitors who speak the target language or traveling with pupils in a country where it is spoken.

The theory of conversation meaning suggests that knowledge of anthropology should be seriously considered in second language education. Also, the help of anthropologists is necessary in the development of teaching materials, curriculum design, and so on just as linguists are often involved in such activities. By incorporating anthropology, the quality of non-linguistic cultural materials in second language education can be greatly improved, and by integrating cultural schemas into the content of language teaching, learners would be better equipped for the

comprehension and production of conversation meaning.

If cultural schemas are a necessary component of conversation meaning, then the place of anthropology in applied linguistics and second language education should equally be recognized, and that seems to be the only way to achieve one of the goals of second language education, namely, that learners are able to comprehend and produce conversation meaning in a second language.

## Chapter 8

# Conclusion

The present study investigated conversation meaning from a theoretical point of view. A theory of conversation meaning grounded in the theories of cultural schemas, distributed cognition, and neural networks was advanced throughout the chapters. Chapter 1 introduced the practical problem at hand and a possible way to solve this problem. Chapter 2 described the philosophical background of the study. The basic features of transcendental realism were presented, and some of its problems were pointed out. In the end, a possible way to remedy the problem of transcendental realism was proposed. Chapter 3 extended the discussions of Chapter 2 by applying transcendental realism to the concept of culture, society, and mind. The notion of culture has not been studied from the transcendental realist point of view, and I made an attempt to extend the perspective to the nature of culture and its science, cultural anthropology. In particular, the concept of the mind as an empty central processing unit was rejected, and instead that of the mind as consisting full of cultural knowledge engaging actively in the cognitive processes was brought up. Chapter 4 introduced diverse traditions of research on conversation on the basis of the

discussions in Chapter 3. It was pointed out that there are both compatible and incompatible assumptions in the fields of conversation research. By comparing various disciplines, the covert theoretical assumptions of each as well as its scientific goal was uncovered. Chapter 5 presented the concept of conversation meaning in detail. Theories of distributed cognition and cultural schemas were introduced as the foundations of the theory of conversation meaning. Chapter 6 described the basic features of neural networks and placed the discussions of Chapter 5 in the framework of connectionism. It was shown that connectionism is not only compatible with the theories of cultural schemas and distributed cognition but it also enhances the rigor of these theories. A summary of the theory of conversation meaning advanced in this study was also provided. Chapter 7 turned to the realm of practice by looking at second language education from the point of view of the theory of conversation meaning developed in the previous chapters.

The theory advanced throughout this study should be seen as the beginning of the formalization of conversation meaning. As a first step toward the understanding of conversation meaning, the most important and urgent task is to lay down a firm framework within which conversation meaning can be conceptualized. I showed why and how conversation meaning cannot be accurately captured by means of existing theoretical frameworks. It would be misleading researchers if we were to take the study of conversation meaning just as an extension of the study of linguistic meaning. There would be an inherent difficulty if we were to study conversation meaning using conventional theoretical frameworks and methods because its nature greatly differs from other linguistic phenomena. What researchers need to do to understand how conversation meaning takes place is to see conversation as conversation, to develop a theoretical framework that truly fits the nature of conversation, and to study conversation as conversation within such a framework. This study was an attempt to provide a theory on the basis of which researchers

would be able to study conversation meaning in its totality.

The theory of conversation meaning would significantly contribute to the solutions of the problems in numerous areas of linguistic practice. One of the most important contribution would be to the debate of the place of culture in second language education. According to the theory, culture in second language education should be considered much more seriously than it is currently considered by practitioners of second language education. And the integration of cultural schemas to the curriculum would be necessary for second language learners to be able to comprehend and produce conversation meaning. In other words, the theory shows that, for second language learners, learning the way of life or world view of the people who speak a target language is as important as learning the target language.



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