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Chapters of Psychology

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An Interview with Wilhelm Wundt (1832–1920)

Sehr geehrter Herr Professor Wundt, what is your opinion about the place of psychology among the sciences of mind? [1]

As the science of the universal forms of direct experience, it is the foundation of the sciences of mind. Psychology is at the same time the most general science of mind and the foundation of all others, such as philology, history, economics, law, etc.[a]

Are these special sciences of the mind, then, "chapters of human psychology," just like, for instance, developmental psychology or general psychology?

The psychological analysis of the most general products of mind, such as language, mythical representations, moral norms, is psychology's due, in part as a necessary expansion of its territory over the phenomena of shared mental life, in part as an aid in grasping complex psychological phenomena at all.[b]

But is it realistic to expect that anthropologists (whom you call "Völkerpsychologen"), linguists, or historians who study these products of mental life will ever consider themselves psychologists?

It could appear that psychology, too, would be best served if the one who ventures to address the anthropological issues combines the qualifications of the historian and the philologist with those of the psychologist. But for two reasons I believe that there is little prospect, at least for the time being, that this wish will be fulfilled. First, given the current partitioning of scientific research, one can hardly expect the philologist or historian to approach the issues in a way that would satisfy the present standards of scientific psychology. Second, maybe one should not even blame him for this, because his task and the perspectives with which he necessarily approaches the issues are essentially different.[c] However, anthropology as such will remain part of psychology.[d]

You are called the father of experimental psychology. Will these other "parts" of psychology be experimental in nature?

Just like the natural sciences, psychology has two exact methods at its disposal: The first one, the experimental method, serves the analysis of the simpler

Figure 13.1. The author (left) interviewing Wundt (right).

psychological processes; the second one, the observation of the universal products of mind, serves the study of higher psychological processes and developments.[e]
Where, then, do you see the boundaries of the experimental method?

Its limits are reached only where specific mental phenomena and products arise from living in a human society; these are inaccessible to experimentation, such as is the case for language, mythology, and morals.[f] These mental products are the objects of observation.[g]

Finally, what is your conception of the mind–body relationship?

Such a relationship can be nothing else than two mutually connected causal chains, which, however, cannot affect easch other because of the incomparability of their parts. I have already called this the principle of psychophysical parallelism.[h] So, for instance, the elements that figure in a spatial or temporal mental image will also entertain a regular relation of coexistence or succession in their physiological substrates.[i]

Thank you, Professor Wundt, for these helpful comments.

From Wundt to Marr

Where are we now, almost a century later? In this section I will argue that, in spite of substantial progress in the study of mind, Wundt's double distinction is

by and large still valid. The first one concerns (simple) process versus (complex) product, the second one experiment versus observation; these distinctions are still ruling the relations between psychology and the other sciences of mind.

Process versus product: Marr's three levels of explanation

According to David Marr (1982), the "top level" of understanding any information-processing device is its abstract *computational theory*. This is a formal characterization of the input-to-output mapping of the device. Or, in Wundt's terms, it is a (formal) description of the system's *product*. One example of such a theory is a visual grammar, a formal characterization of three-dimensional interpretations for some domain of two-dimensional scenes. Another example is a generative grammar, a formal characterization of the well-formed sentences of a language. As we saw, language is Wundt's pet example of a complex psychological product. The goal of linguistics is to provide a computational theory of this product of mind. In Wundt's terms we are at this level dealing with anthropology, which is a proper part of psychology.

Marr's second level is the *algorithmic theory*. It deals with how the computation is done, how an input representation is transformed into an output representation.[2] There are myriad ways of generating a language's well-formed sentences, and myriad ways of parsing two-dimensional stereo patterns into three-dimensional configurations of objects. The psychological aim here is, of course, to come up with an algorithm that faithfully models how we, human beings, generate sentences or parse stereoscopic scenes. The algorithmic theory is a *process* theory. This is the traditional domain of psychology, dating back to Wundt and earlier. Time and again, Wundt stresses that the primary psychological phenomena are events, processes ("Vorgänge"), not objects.

Marr's third level deals with the *physical implementation* of these processes. How is a mental process realized in the nervous system? Wundt was still largely in the dark about these implementational issues (though his ruminations about the substrate of spatial and temporal images are certainly consonant with the later discoveries of retinotopic mapping in the visual cortex and the recent work by Georgopoulos et al., 1989 on the time course of mental rotation in the motor cortex).

How does the product/process distinction relate to psychology's place among the sciences of mind? My thesis is that, by and large, general psychology in the twentieth century has kept to its traditional role of studying processes, of seeking explanations at the algorithmic level. And it has been very good at that. But the corollary is that it has tended to ignore the universal products of mind; it has largely left computational theorizing to what I shall call the special sciences of mind, linguistics, logic, law, sociology, anthropology, and, as I shall argue below, economics.

Psychology has not been in the forefront asking questions such as: What is a *possible* human language? What are *possible* systems of human spatial orientation? What are *possible* kinship systems? What are *possible* musical systems? What are *possible* conceptions of other people's intentional states?" And so on. In short,

psychologists have tended to ignore the issue of universals of human cognition, our genetic cognitive endowment.[3]

Instead, and in the best of cases, psychologists have accepted one or another existing cognitive system as a given and studied how it functions. And here, they have been quite successful. We now have sophisticated process theories of visual scene analysis, of word and sentence parsing, of the perception of rhythm and tonality, of inference making, and so forth.

In the worst of cases, though, psychologists have continued to stay away from the naturally given systems. In the tradition of Ebbinghaus (who was the first to study verbal memory experimentally, but by means of nonsense syllables), they have so much "purified" their experimental materials that the ecologically given system (of memory, of object recognition, of spatial attention, or what have you) is immunized. The resulting theories of processing are often mathematically sophisticated, but of questionable generalizability (i.e., ecological validity). To conclude this point, psychologists have been more than happy to accept Wilhelm Wundt's experimental psychology, but they have been far less inspired by him as the father of *Völkerpsychologie*. Although products of mind are often the object of study in developmental psychology (e.g., What is the structure of children's number system, their lexicon, or their conception of time?), they seldom are in Wundt's core area, general psychology. By and large, general psychology (i.e., the study of general principles of mental life) has taken the complex products of mind for granted, leaving their analysis to the special sciences of mind, such as linguistics, anthropology, law, logic, sociology, musicology, and what have you. And there has never been much prospect indeed for Wundt's wish in this respect to be fulfilled—that students of these products of mind are themselves sophisticated in psychology. In other words, psychology has left some of its most precious gems to the care of other disciplines.

Experiment Versus Observation

Wundt's position here was straightforward. Simple psychological processes should be studied by the experimental method. Complex mental processes, however, are inaccessible to experimentation and should, therefore, be approached indirectly—namely, by analyzing their products or outcomes. It is society that stabilizes such products (a language, a system of norms, etc.). They are inaccessible to experimental study; observation is the only appropriate method here.

At this point, one should say that what Wundt established as the father of experimental psychology flourished substantially—better than he could have foreseen. Already in Wundt's own time, there were remarkable efforts to study experimentally higher-order processes such as reading, thinking, sentence comprehension; this in spite of Wundt's castigations (Wundt, 1907, 1908). But it is especially since the so-called cognitive revolution of the 1950s and 1960s that the experimental method became successfully applied to complex mental processes as well. One of the main forces here has been psycholinguistics. The experimental work by George Miller and his associates on syntactic processing in language comprehension still stands as a landmark of experimental innovation and creativ-

ity. Now, thirty years later, we have at our disposal a wide gamut of experimental methods that trace processes of lexical access, syntactic parsing, and discourse interpretation "on line." A very similar development is currently under way in the study of our skill of speaking (Levelt, 1989, 1993). For Wundt, the creation of sentences—one of the key issues in his psychology of language—was entirely in the domain of *Völkerpsychologie*—that is, inaccessible to experimentation.

Not only has the experimental method conquered the domain of complex mental processing, it also came to be used in the study of outcomes or products of complex processing. We now ask our subjects to judge the well-formedness of sentences; we ask them which bet they prefer, or which of two visual patterns they find more pleasing.

Although these developments have shown Wundt's assignment of methods to be untenable, it still governs the relations between psychology and its neighboring sciences of mind. Psychology is the only science of mind where the dominant methodology is experimental. One does see an occasional experiment in anthropology and a few more in economics, but that is about it. The major method for the other sciences is systematic observation: questionnaires, statistics, participant observation, introspection, diary studies, or whatever the local art may be. After almost a century, Wundt's assignment of research methodologies to disciplines (whether or not "parts of psychology") is still in force. Although there can be good reasons to opt for one methodology over another (for instance, it would be plainly impossible to approach historical issues by means of experiment), these methodological predilections often lack intrinsic motivation. It is, in fact, remarkable to what extent psychology and experimental psychology have become associated in twentieth-century psychology—often to the degree of near synonymy. It is somewhat like defining history as the science of archive searching.

Linguistics and Microeconomics: Two Computational Chapters of Psychology

In this section I elaborate on the sketched relationship between psychology and the other sciences of mind in light of two examples—linguistics and microeconomics. I argue that, in spite of their huge differences in subject matter, they entertain the same formal relationship to psychology. And that relationship is probably quite similar for other sciences of the mind.

Linguistics

Generative theories of grammar, Marr (1982) argued, are computational theories. They delineate the well-formed linguistic representations (syntactic, phonological, etc.) that the mind is capable of computing. Psycholinguistic theories, on the other hand, are algorithmic in nature. They deal with the mental processes that generate or parse the structural representations postulated in the computational theory.

Marr gave the advice to proceed from top to bottom in cognitive science. First produce a formal account of what the device is intended to compute, then start

bothering about how it does the computing. The physical implementation will rarely, if ever, be a major constraint on the algorithmic theory, according to Marr (1982). Chomsky's position has been similar:

> There seems to be little reason to question the traditional view that investigation of performance will proceed only so far as understanding of underlying competence permits. (Chomsky, 1965, p. 10)

In other words, it makes little sense to create theories of human language parsing (theories of performance, algorithmic theories) without a solid understanding of the mental grammar in the language user (a theory of competence, a computational theory).

This is a rather ascetic position. Linguists are, not surprisingly, not too pressed to complete their chapter of psychology. And psycholinguists, I fear, don't have the patience to wait for that glorious moment. Are they, then, meanwhile messing about in gloom? Not in my experience. Instead, the two enterprises are not entirely independent. In fact, one of the attractions of Marr's distinction between computational and algorithmic levels of explanation is that one has to bother about the correct *assignment* of an explanatory factor. Is it computational or algorithmic? Chomsky and Halle pointed out this problem a quarter of a century ago, in a less ascetic, more balanced mood:

> It must, incidentally, be borne in mind that the specific competence–performance delimitation provided by a grammar represents a hypothesis that might prove to be in error when other factors that play a role in performance and the interrelation of these various factors come under investigation. . . . When a theory of performance ultimately emerges, we may find that some of the facts we are attempting to explain do not really belong to grammar but instead fall under the theory of performance, and that certain facts that we neglect, believing them to be features of performance, should really have been incorporated in the system of grammatical rules. (Chomsky & Halle, 1968, p. 111)

This implies the possibility that algorithmic research may affect the computational theory. Hence, the two enterprises should go hand-in-hand, as has been Professor Wundt's good advice all along.

Any computational theory involves an idealization, an abstraction from process and implementation. Chomsky's rigorous idealization for linguistics goes like this:

> Linguistic theory is concerned primarily with an ideal speaker-listener, in a completely homogeneous speech-community, who knows its language perfectly and is unaffected by such grammatically irrelevant conditions as memory limitations, distractions, shifts of attention and interest, and errors (random or characteristic) in applying his knowledge of the language in actual performance. (Chomsky, 1965, p. 3)

During the almost three decades following this demarcation, we have learned to ask two questions. The first one is: Are the computational assumptions correct,— that is, are they psychologically valid? And the second one is: Are we making the psychologically correct assignments to the computational and the algorithmic levels of explanation?

Storms have raged over these issues. We have seen dramatic changes and diversification in grammatical theorizing. Some, such as Gazdar et al. (1985), continued Chomsky's generative program—that is, to write grammars (of minimal generative capacity) that generate all and only the sentences of a natural language. These theorists maintain the psychological assumption that the generative rule-based grammar is a correct representation of the language user's linguistic knowledge.

Others, including Chomsky himself, came to entirely dismiss the notion that a language can be formally generated: *"Further formalization is pointless"* (Chomsky, 1986, p. 91). The user's linguistic knowledge is not a system of generative rules: *"There are no rules at all, hence no necessity to learn rules"* (Chomsky, 1987, p. 68). Instead, according to Chomsky, our innate language capacity (or "universal grammar") is a network of modules, each of which is based on principles that are invariant among languages. For instance, there are only a finite number of possible phrase structures within the syntactic module. They differ in terms of only a few parameters. Learning a language is setting those parameters. (Why this should make further formalization pointless is, however, less than obvious.)

The psychological validity of these, and many other computational proposals, is the subject of empirical research. Are Gazdar's rules reflected in the way we parse sentences when we listen or read (Fodor, 1989)? Are young children really "setting parameters" when they acquire their native language (Weissenborn, Goodluck, & Roeper, 1992)? And so on. Each computational theory carries its own research agenda.

And with respect to the second question, what to assign to the computational and what to the algorithmic level (to "competence" or to "performance" in the linguistic jargon), the fights have been fierce as well. Initially (during the early 1960s), the relation was considered to be quite transparent: Each (computational) rule of grammar would correspond to an (algorithmic) operation in perception or production. As a consequence, the more complex a sentence's syntax, the harder it would be to process. But counterexamples soon emerged. A sentence like *The horse raced past the barn fell,* for instance, is much harder to parse than its simple syntax justifies. And this, Fodor et al. (1974) argued, is the result of our perceptual strategies or heuristics. When a sentence begins with a noun phrase followed by a verb, our first guess is that the noun phrase and verb relate as actor and action, and that is usually correct. But the heuristic doesn't work for the example sentence (the horse *was* raced past the barn before it fell). This heuristics approach solved a range of enigmas in sentence processing. It also limited the role of the computational theory. The latter was merely there to characterize the well-formed output of parsing—that is, the ideal structural target of processing. But whether or how the target was reached became a problem *sui generis.* As a consequence, the algorithmic theory became much less dependent on the theory of syntax than it used to be. In fact, modern processing theories of language are compatible with almost any sophisticated grammar. The primacy of the computational theory over the algorithmic theory, as proclaimed by Chomsky and by Marr, has disappeared; the two enterprises are developing concurrently with surprisingly little interaction. Too little, to my taste. But the abandonment of primacy claims, one way or another, is a major step ahead.

Let us now move to an entirely different discipline, microeconomics, and notice that it entertains the same formal relationship to psychology.

Microeconomics

Microeconomics deals, in part, with human decision-making—in particular, with the consumer's choice behavior. That part of microeconomics is a theory of choosing between means that are in short supply. And since most commodities in life (such as food, jobs, spouses, education, or fresh air) are in short supply, this part of microeconomics is *ipso facto* a quite general theory of human choice behavior. It should be noticed that other, and highly successful, parts of microeconomics don't have individual choice behavior as their object. Operations research, for instance, deals with the efficiency of production or transportation processes, not with consumers' choices. The following discussion, however, concerns only the aspect of microeconomics interested in consumer choices.

For Adam Smith (economics' eighteenth-century founding father), economic theory should take the *individual* consumer's behavior as its starting point. According to Smith, the behavior of aggregates derives in regular ways from individual choice behavior, variable as this may be. What would Wilhelm Wundt's view be on this matter?

Professor Wundt, I forgot to ask you this: How does an economical system emerge? Is economics also a chapter of psychology?

Every attempt to understand economical history in causal terms leads to a psychological analysis. What else are supply-demand relationships, the spur of competition, and the other leverages of labor and trade than psychological motives?[j]

Thank you, professor. I won't intrude again.

This nicely parallels Wundt's view on language. Language, according to Wundt, is in the final analysis a process in the individual speaker. But in an aggregate, a language becomes a more or less stabilized product.

As a theory of the individual consumer's choice behavior, microeconomics is another chapter of human psychology. But it is a computational chapter. Modern microeconomic theories are typically axiomatic-deductive formal systems that generate sets of well-formed or "rational" choices. A rational choice is one that, given a limited set of resources, allocates these in such a way that the decision-maker's own satisfaction is maximized. Although economic theories differ in important details with respect to their definition of rational choice, the rational choice *paradigm* is at the basis of almost all present-day consumer theory.

The parallels to generative linguistics are ubiquitous. Both are deductive theories, involving similar idealizations. In economics one is dealing with an ideal decision-maker, just as generative linguistics postulates an ideal speaker-listener. An ideal decision maker is one who is fully informed about his or her own needs and preferences, and possesses all relevant information about the choice alternatives and their utilities—just as the ideal speaker-listener "knows his language perfectly." Moreover, the ideal decision maker is not subject to limitations of attention or memory; all relevant information is always available. This is, again, precisely the same idealization as was made in generative grammar. Finally, the

market community is homogeneous; all consumers are alike in terms of informational state and subjective utility functions. This parallels the "completely homogeneous speech-community" of linguistics.

As a computational theory, microeconomics is a theory of rational outcomes or choices—of *products* of behavior. The rational choice parallels the well-formed sentence in linguistics. In neither case are we dealing with *actual* products of behavior, but with *virtual* or *possible* products. In that sense both kinds of theory are normative (see Massaro, 1991); they tell you what product is all right and what product is not.

And neither of the theories are *process* theories. The rational choice paradigm is as much ignorant of how a choice comes about, as is the generative grammar paradigm about how a sentence is produced.

Above we discussed two questions that were raised with respect to the demarcation between computational and algorithmic theories in linguistics. Exactly the same issues have been hotly debated in economics.

The first one was, Are the computational assumptions correct; are they psychologically valid? In other words, are consumers rational agents? In economics, there has grown a kind of monstrous alliance to deny the validity of the assumptions. Arrow (1986), for instance, argues that the assumptions are incoherent. If all individuals are alike in utility function and information state (the homogeneity assumption), and rational decision implicates complete exploitation of information, then there would be no trading at all. Trading results from economic agents being *different* in their state of knowledge or utility function.[4]

Others argue that the paradigm is well-nigh vacuous. Hogarth and Reder (1986) write:

> However, to apply the rational choice paradigm, few—if any—psychological assumptions are needed. The economic implications of the paradigm are compatible with virtually any account of the decision-making process so long as this generates appropriately sloped supply and demand curves.

And essentially the same is argued by Simon (1986), when he remarks that *"neoclassical economics becomes, as has been observed more than once, essentially tautological and irrefutable."*

Tversky and Kahneman (1979, 1986), psychological intruders in the economic playground, take another tack. They turn to the axioms of expected utility theory—that is, to the foundations of the rationality paradigm—and test their psychological validity *by means of experiments*. The results are shocking; there is substantial and systematic violation of all axiomatic assumptions. This is like the systematic and substantial violations of linguists' grammaticality predictions that Levelt (1972) found in an experiment where (other) trained linguists judged the well-formedness of sentences.

The second question was, Are we making the psychologically correct assignments to the computational and the algorithmic levels of explanation? This issue has been actively pursued in economics with equal force. For decades, Herbert Simon has been in the forefront here. He is the preeminent *algorithmic* economist. According to him, the only thing of real interest is *how* people make their deci-

sions. Decisions are made in a context of limited information about cost and supply functions, a particular framing of the choice situation, under severely limited attentional conditions, guided by particular beliefs and expectations. Within these limitations, the consumer will still have good reasons for each step in the process. In other words, economic agents have "procedural rationality." But there is not the slightest hope that procedural rationality will have "substantive rationality" (i.e., computational rationality) as an emergent property. Says Simon (1986, p. 39):

> I would recommend that we stop debating whether a theory of substantive rationality and the assumptions of utility maximization provide a sufficient base for explaining and predicting economic behavior. The evidence is overwhelming that they do not.

Arrow (1986, p. 201) is as rabid in undermining the computational underpinnings of the rational choice paradigm. The rationality assumptions

> certainly imply an ability at information processing and calculation that is far beyond the feasible and that cannot well be justified as the result of learning and adaptation.

This agrees with Simon's position. The obvious untenability of the computational assumptions on which subjective utility theory is based led to various adaptations that can be interpreted as "assignment shifts." The computational theory started "importing" factors that had been previously assigned to the algorithmic level.

One example is the move to drop the assumption that the economic agent is fully informed. Information is among the scarcities that a decision maker has to cope with. In Search Theory (Stigler, 1961) the state of information is a variable, whose cost is a factor at the level of the computational theory. Smith (1985) similarly attaches a price tag to computational effort—that is, agent's costs of thinking.

Another example of shifting boundaries between computational and algorithmic theory is provided by the Rational Expectations model (Lucas, 1981) in which agents behave fully rationally, given their state of information. But process factors may create systematic distortions in that informational state. For instance, managers systematically err in ascribing price movements to general versus industry-specific changes. In other words, agents are subject to illusions—which is an assignment to the algorithmic level. Tversky and Kahneman were among the first to stress the irrational force of such illusions.[5]

Whereas the latter two examples are still adaptations of the rational choice paradigm—essentially preserving the computational theory—Kahneman's and Tversky's Prospect Theory (1979) shifts most of the explanatory work to the algorithmic level. Making a choice is a two-phase process. During the first phase the decision problem is "framed" in terms of potential acts, contingencies, and outcomes. This framing process is subject to norms, habits, expectancies, and so on. During the second phase the prospects resulting from the first phase are evaluated, and the best one is selected.

It is an empirical issue how framing and evaluation are achieved by the subject. According to the authors, the decision maker uses a set of powerful heuristics to arrive at a representation of the problem. These heuristics do not derive from the axioms of rational choice, but they are "procedurally rational" (Simon's term) given the limited information on which the consumer has to act.

What is left, then, for a computational theory? Or in Marr's terms: What is the agent trying to achieve? According to Prospect Theory it is, first, to avert losses and, second, something like living by the adage "a bird in the hand is worth two in the bush." Consumers are certainly not maximizing expected utility. These aims are formally specified by means of an evaluation function, which is the computational part of Prospect Theory.

These developments in microeconomics are highly similar to those in linguistics. The algorithmic theory has become largely independent of the computational theory. Heuristic procedures that are still reasonably effective under conditions of limited information and limited temporal resources replace foolproof rational procedures that require omniscience and unlimited computational resources. The computational theory has become more realistic; at the same time, it has ceased to dictate the structure of the algorithmic theory.

The present section exemplified psychology's formal relationship to computational sciences of the mind by considering linguistics and economics in some more detail. In both cases, the situation evolved from one in which the computational theory dictated the structure of the algorithmic theory to one in which the algorithmic theory became independently motivated. Is there reason to expect that this more balanced relation will also extend to the implementational theory? I shall return to that question after a few remarks on the cultural relations among the sciences of mind.

Science Culture

The sciences of mind developed from a common core; many of them emancipated from philosophy no more than a few hundred years ago. The easy way of interpreting the resulting partitioning is that it naturally follows the "joints of nature." Linguistics deals with one faculty of mind, economics with another one, and so on. But this is obviously false. Which faculty of mind is the subject of anthropology? Certainly, it must include the abilities to talk, to trade, to exercise moral judgment, etc. And how is anthropology different from sociology? In that it studies "non-Western" people? Are there "Western" versus "non-Western" faculties of mind?

Clearly, the present partitioning of the sciences is a rather arbitrary result of our cultural history. Capitalism grew economics, colonialism grew anthropology, and so on. And each science of mind cultivated its own local culture, its own pet topics, its own cherished methods.

But in spite of their ever growing divergence, the "computational" sciences of mind have still kept commonalities in scientific culture. These, however, are just as arbitrary as are their differences. I have already mentioned the tendency, canon-

ized by Wundt, for these sciences to evade experimentation as a method. Economists do study "experimental markets" (see Smith, 1962, for a pioneering study), but this methodology is as marginal as the systematic experimental elicitation of sentences by linguists or the controlled field experiment in anthropology.

Another cultural commonality among the computational sciences of mind is to capitalize on intuitive judgment. The linguist's or native speaker's intuition that "this is a grammatical sentence" still counts as critical evidence in the evaluation of a theory of grammar. This in spite of obvious problems of measurement and interpretation (Levelt, 1974, Vol. 3). Similarly, the economist's intuition that "this choice is rational" is still an important guide in constructing theories of choice, or at least in selling them: *"To add credibility to the story, appeal is often made to everyday intuition concerning individual behavior"* (Hogarth & Reder, 1986, p. 3).

Such examples can easily be multiplied. But the point can already be made: Neither the partitioning of the sciences of mind nor their differences and commonalities of method are deeply principled in nature. Instead, we are all subject to an arbitrary legacy of history. But in the next section I argue that there is hope for the next century. The new generation of scientists of mind is increasingly dressed in blue jeans wearing the label "cognitive science." That term is as ill-defined as the traditional ones, but it is at least nondivisive and nondogmatic.

Marr's Three Levels According to Escher, Exemplified by the Theory of Speaking

Wilhelm Wundt was right: Psychology is the foundation of the sciences of mind. Its task is to disentangle how the mind and all of its faculties function. And Marr was right too: To study the mind's operations, one must consider what it tries to achieve, what computational problem it tries to solve. Ever since Wundt's time, the latter kind of question has been largely left to the "special" sciences of mind. And they have considerably grown apart, both among them and away from psychology.

This has been to the detriment of both psychology and of the special sciences. The most remarkable effect on psychology has been the morbid growth of *processitis*, the tendency to study processes irrespective of their functions and of the representations that are relevant to those functions. Behaviorism, built on the ultimate stimulus–response process, was killed by this disease (and its heir connectionism is a vulnerable next candidate). But processitis has been a lingering condition of psychology since the cognitive revolution. For instance, many psychologists still consider it to be an art to clean away from their experimental materials everything that could be of any ecological validity to the subject (the Ebbinghaus syndrome). The special sciences of mind likewise suffered by naively relying on outdated psychology (such as rational choice theory, behaviorism, or psychoanalytic theory).

But there can be well-founded hope that these seemingly irrevocable developments are coming to an end. As I have already indicated, the twentieth century is
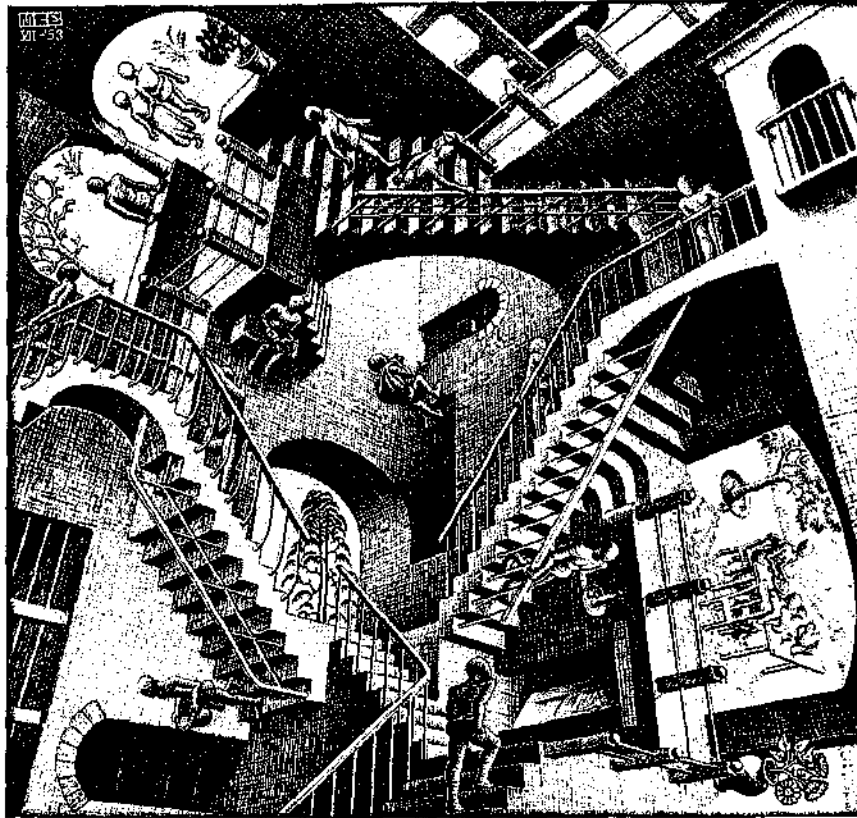
Figure 13.2. Marr's three levels according to Escher. Copyright © 1953 by M.C. Escher/
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closing its books with an ill-defined item called "cognitive science." It is not a
coherent science in terms of object, methodology, or education, but it certainly is
a gigantic melting pot where disciplinary boundaries no longer hold. This is the
right climate for growing irreverent offspring, for whom Marr's three levels are
like Escher's litho *Relativity,* where climbing is decending and descending is
climbing, without any preestablished priority or hierarchy among levels.

Let me exemplify this new state of affairs by referring to our most complex
and species-specific skill, the ability to talk. Returning to his ascetic position,
Chomsky (1988) argued that the scientific study of how we express our thoughts,
the ordinary use of language in everyday life, is beyond reach, if not principally
then at least factually for the time being. Why? Because there is no prospect of
solving "Descartes' problem"—namely, how it is possible that we can act in a
free and undetermined way. Talking is free, undetermined action in that sense.
Hence, there is no hope for a theory of speaking.

This is both logically and factually false. Logically, because even if we don't
know where thoughts to be expressed come from, we can study how, given such

a thought, it becomes expressed in language (and why wouldn't we be able to investigate where thoughts come from?). It is factually false, because since the 1960s there has been substantial theoretical and empirical progress in the study of how we speak (for a review, see Levelt, 1989). This progress concerns both the issue of how speakers generate thoughts to be expressed, and the machinery of giving these thoughts syntactic, phonological, and articulatory shape. We have a clear case here where the (or, rather, one) computational perspective has been deeply misleading with respect to the feasibility of an algorithmic theory (let alone its physical implementation).

As a matter of fact, if anywhere, it has been in the study of language and speech production that Escher's democratic relation between levels became a living reality. Let us first consider the computational and algorithmic levels.

The speech-producing mechanism appears to have a highly modular organization. Among the various component modules, there is one that controls grammatical encoding—that is, the selection of appropriate words from the mental lexicon and the incremental production of syntactic structure. Another module controls phonological encoding—that is, it computes the phonological shape of the utterance. For each module, its scientific analysis consisted of determining its characteristic input and output representations (semantic, syntactic, phonological) and the operations that mediate between them.

For instance, the grammatical encoding module takes conceptual or semantic structures as input and generates syntactic surface structures as output. The phonological encoding component takes surface structures as input and generates phonological plans (both segmental and suprasegmental) as output. There is no way in which the computed representations are logically "prior to" the operations. There are quite restrictive operational requirements on the theory. For example, one central property of any psychologically sophisticated model of speech production is that production is "incremental"; both syntax and phonology are generated "from left to right" without much look-ahead. This, in turn, restricts the character of the input and output representations (semantic, syntactic, phonological) that can figure in such a theory. There is no primacy either for the computational theory (the theory of representations), or for the algorithmic theory (the process must run on relevant representations).

And what about Marr's implementational level? Historically, the theory of speaking has been the neurologists' playground since Broca discovered the speech–motor center in the left brain. The smarting shortage of interested psychologists gave the neurologists a free hand. And we should be grateful for what they accomplished. The careful delineation of aphasic syndromes, initially as a means to accomplish in vivo anatomical localization of cerebral disorders, led to the first *functional* models of language production. And these models were mostly modular in nature. Each module subserved a particular function in the process of speaking (such as activating word meanings or activating the articulatory shapes of words) and could ideally be localized on one of the lobes of the left hemisphere. This paradigm of negotiating between functional and cerebral modeling has fruitfully continued to the present day.

And equally active is the direct negotiation between cerebral modeling and

representational or computational theory. A substantial part of present-day cognitive neuropsychology is concerned with the types of representations that are accessible or computable under different kinds of brain damage. Grodzinsky (1990, p. 17) argues that these breakdown patterns are as criterial for a theory of grammar—that is, the computational theory—as is its compatibility with an algorithmic or functional theory:

> The internal structure of the theoretical account of a domain, then, effectively dictates which patterns of impairments are possible, and which are impossible. An examination of deficit descriptions can be used to evaluate the theory. If the predictions it makes are correct, and if it is found to be compatible with breakdown patterns, we can conclude that it meets the neuropsychological constraint of breakdown-compatibility. This will be added to two other proposed constraints on the theory of grammar: those of *learnability* and *parsability*.

And the search for the brain's modular specialization for different types of linguistic representation continues at increased speed. The aphasiological evidence is being complemented by two further sources.

There is, first, the evidence stemming from single-cell recordings during open brain surgery. Creutzfeldt, Ojemann, and Lettich (1989), for instance, found neurons in the left superior temporal gyrus that specifically responded to compound words such as *horseshoe* (as opposed to monomorphemic words, such as *spaghetti*). Cellular and cell assembly models of linguistic units (such as phonological features, syllables, phrases, and clauses) are beginning to be developed (Braitenberg & Pulvermüller, 1992). Critical here is the realistic modeling of cortical circuits, such as is the case for the "canonical microcircuits" that Douglas and Martin (1990) proposed on the basis of their extensive anatomical and physiological studies of brain tissue. The so-called "neural" networks of connectionism have very little to do with these real cortical circuits. Rather, the make-believe "neural" network modeling of connectionism is the latest excuse for behavioral scientists to stay away from issues of implementation, continuing the tradition of behaviorism.

There is, second, the explosive development of noninvasive (or almost noninvasive) brain imaging technology. The brain's metabolic activity during the execution of linguistic tasks can now to some extent (and in different ways) be traced by positron emission tomography (PET) and through functional nuclear magnetic resonance imaging (MRI). The first PET studies of speech production have recently appeared (Peterson et al., 1988; Wise et al., 1991). They show that different aspects of word retrieval (semantic, phonological/phonetic) in speech production involve different areas of the brain, and thus are beginning to reveal the relations between representation and implementation at the brain's macro scale.[6] Functional MRI is the greatest promise here. There is the expectation that its superb spatial resolution will soon be matched by a temporal resolution of one second or even less. The new imaging technologies will be material in redressing the still existing imbalance between computational and algorithmic theory, on the one hand, and implementational theory, on the other.

The successful dissection of our faculty of speech sets an example for the study of mind beyond the year 2000. Major leaps are to be expected if representa-

tion, process, and implementation are studied in close interdisciplinary cooperation. Any claims to priority among these three will be counterproductive. And don't believe that Escher's picture is an impossible one.

Notes

1. Wundt's responses are free but faithful translations of the following German texts:

a Als Wissenschaft von den allgemeingültigen Formen unmittelbarer Erfahrung . . . ist sie die Grundlage der Geisteswissenschaften . . . Psychologie . . . ist [sie] *selbst die allgemeinste Geisteswissenschaft und zugleich die Grundlage aller einzelnen, wie der Philologie, Geschichte, Nationalökonomie, Rechtswissenschaft usw* (Wundt, 1914, p. 18).

b daß . . . die psychologische Analyse der allgemeinsten geistigen Erzeugnisse, wie der Sprache, der mythologischen Vorstellungen, der Normen der Sitte, der Psychologie teils als eine notwendige Ausdehnung ihres Gebiets auf die Vorgänge des gemeinsamen seelischen Lebens, teils als ein Hilfsmittel für das Verständnis der verwickelteren psychischen Vorgänge überhaupt zufällt. (Wundt, 1914, p. 10).

c Nun könnte es scheinen, als wenn auch der Psychologie dann am besten gedient wäre, wenn derjenige, der sich an die völkerpsychologischen Probleme heranwagt, die Eigenschaften des Philologen und des Historikers mit denen des Psychologen verbände. Aus zwei Gründen glaube ich jedoch, daß dieser Wunsch, vorläufig wenigstens, kaum Aussicht hat, verwirklicht zu werden. Erstens wird man bei der gegenwärtigen Teilung der wissenschaftlichen Arbeit schwerlich erwarten dürfen, daß der Philologe oder Historiker die Sache in einer den heutigen Forderungen der psychologischen Wissenschaft genügenden Weise in Angriff nehmen werde; und vielleicht wird man ihm dies nicht einmal verdenken können, da die Aufgaben und, was damit unvermeidlich verbunden ist, die Gesichtspunkte, mit denen er an die Probleme herantritt, wesentlich abweichend sind (Wundt 1904, p. v).

d Gleichwohl wird die Völkerpsychologie als solche ein Teil der Psychologie bleiben (Wundt, 1904, p.vi).

e Demnach verfügt die Psychologie, ähnlich der Naturwissenschaft, über zwei exakte Methoden: die erste, die experimentelle Methode, dient der Analyse der einfacheren psychischen Vorgänge; die zweite, die Beobachtung der allgemeingültigen Geisteserzeugnisse, dient der Untersuchung der höheren psychischen Vorgänge und Entwicklungen (Wundt, 1914, p. 30).

f Grenzen sind ihr erst da gesetzt, wo durch das Zusammenleben der Menschen geistige Vorgänge und Erzeugnisse eigener Art entstehen, die, wie die Sprache, die mythologischen Vorstellungen, die Sitten, der experimentellen Einwirkung unzugänglich sind (Wundt 1919, p. 11).

g . . . daß in diesem Fall geistige Erzeugnisse die Objekte der Beobachtung sind (Wundt, 1914, p. 29).

h Eine solche Beziehung kann nicht anders denn als ein Parallelgehen *zweier miteinander verbundener, aber vermöge der Unvergleichbarkeit ihrer Glieder niemals direkt ineinander eingreifender Kausalreihen angesehen werden. Ich habe dieses Prinzip . . . bereits als das des* psychophysischen Parallelismus *bezeichnet.* (Wundt, 1919, p. 550).

i So werden z.B. die Elemente, die eine räumliche oder zeitliche Vorstellung konstituieren, auch in ihren physiologischen Substraten in einem regelmäßigen Verhältnis der Koexistenz oder Sukzession stehen (Wundt, 1914, p. 396).

¹ . . . daß jeder Versuch, die Erscheinungen der Wirtschaftsgeschichte ursächlich zu begreifen, auf eine psychologische Analyse hinausführt. Was sind in der Tat die Verhältnisse von Angebot und Nachfrage, der Sporn der Konkurrenz und die anderen Hebel des Arbeits—und Handelsverkehr anderes als psychologische Motive . . . ? (Wundt, 1908b, p. 397).

2. There is a potential source of confusion here. On the one hand, Marr calls the computational theory a representation. A grammar, for instance, is a representation of the language. On the other hand, he also calls the input and output of an algorithm a representation. I use "representation" only in this latter sense. However, this does not affect my use of Marr's tripartition. The theory of representations—that is, their ultimate explanation—is the computational theory. The algorithmic theory takes representations for granted; it is explanatory only for the ways in which they are created or transformed.

3. Remarkable exceptions in the history of psychology are, among others, to be found in the Gestalt school of psychology, Heider's social psychology, Piaget's genetic psychology, Michotte's "Kantian" psychology, Gibson's ecological psychology, and Rosch's prototype theory. All deal, in different ways, with outlining well-formed or "possible" products of mind.

4. The corresponding argument has never been made within the other chapter of psychology, linguistics. If the linguistic community is completely homogeneous, all language users knowing their language perfectly and being without limitations in terms of their interests or states of attention, would there be any talking? At any rate, there will be no language *learning*, because learning presupposes the existence of an incompletely informed state, for which there is no place in the idealization. There can be only a magical switch from the intitial state to the fully informed state.

5. Tversky and Kahneman's theory of cognitive illusions, and more specifically their experimental approach to testing the rationality axioms, has not survived without criticism. See, for instance, Gigerenzer (1993).

6. We shall also have to live with dramatic overinterpretations. The beautifully colored, symmetrical PET-scan images are becoming the Rorschach pictures of popular brain science: Every interpretation is accepted. The recent "Mind and Brain" issue of *Scientific American*, for instance, depicts verbs as being located in the prefrontal lobe and nouns in the temporal lobe of the left hemisphere, momentarily ignoring the speaker's quite productive ability to use denominal verbs and deverbal nouns.

Acknowledgement

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