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Tools = Theories = Data? On Some Circular Dynamics in Cognitive Science

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> *Where new auxiliary means become fruitful for research in a certain domain, it is a frequently occurring phenomenon that the auxiliary means are sometimes also confused with the subject matter.*
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> *Daß man, wo neue Hilfsmittel für die Forschung innerhalb eines bestimmten Gebietes fruchtbar werden, gelegentlich auch einmal das Hilfsmittel mit der Sache verwechselt, ist ja eine oft genug vorkommende Erscheinung.*
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> —(Wundt, 1921, Vol. I, p. 148; translated by T. Sturm)

Scientific inquiry is often divided into two great domains: (a) the context of discovery and (b) the context of justification. Philosophers, logicians, and mathematicians claimed justification as a part of their ter-

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ritory and dismissed the context of discovery as none of their business, or even as "irrelevant to the logical analysis of scientific knowledge" (Popper, 1935/1959, p. 31). Concerning discovery, there still remains a mystical darkness where imagination and intuition reigns, or so it is claimed. Popper, Braithwaite, and others ceded the task of an investigation of discovery to psychology and, perhaps, sociology, but few psychologists have fished in these waters. Most did not care or dare.

Inductivist accounts of science, from Bacon to Reichenbach and the Vienna School, often focus on the role of data but do not consider how the data are generated or processed. Neither do the anecdotes about discoveries, such as Newton watching an apple fall in his mother's orchard while pondering the mystery of gravitation; Galton taking shelter from a rainstorm during a country outing when discovering correlation and regression toward mediocrity; and the stories about Fechner, Kekulé, Poincaré, and others, which link discovery to beds, bicycles, and bathrooms. These anecdotes report the setting in which a discovery occurs, rather than analyzing the process of discovery.

The question "Is there a logic of discovery?" and Popper's (1935/1959) conjecture that there is none have misled many into assuming that the issue is whether there exists a logic of discovery or only idiosyncratic personal and accidental reasons that explain the "flash of insight" of a particular scientist. However, formal logic and individual personality are not the only alternatives (Nickles, 1980). The process of discovery can be shown to possess more structure than thunderbolt guesses but less definite structure than a monolithic logic of discovery of the sort for which Hanson (1958) searched. The present approach lies between these two extremes.

In this chapter, we argue that, in part, the generation of new theories can be understood by a *tools-to-theories* heuristic. This proposed heuristic (not logic) of theory development makes use of various tools of justification that have been used by scientific communities. By *tools* we mean both analytical and physical instruments that are used to evaluate given theories. Analytical tools can be either empirical or nonempirical. Examples of analytical methods of the empirical kind are tools for data processing, such as statistics; examples of the nonempirical kind are normative criteria for the evaluation of hypotheses, such as logical consistency. Examples of physical tools are measurement instruments, such as clocks.

The main goal of this chapter is to show that some tools can provide metaphors that become concepts for psychological theories. We will discuss the heuristic role, as well as the possibilities and problems, of two tools developed during, as it has been called retrospectively, the *cognitive revolution* in the American psychology of the 1960s: inferential statistics and the digital computer. The cognitive revolution was more than an overthrow of behaviorism by mental concepts. Mental concepts have been continuously part of scientific psychology, even coexisting with American behaviorism during its heyday (Lovie, 1983). The cognitive revolution did more than revive the mental; it changed its meaning. The two new classes of theories that emerged, and partially overlapped, pictured the mind as an "intuitive statistician" or a "computer program."

This chapter is structured as follows. First, we outline how tools inspire new theories, both on an individual level and on the level of a scientific community (Section I). Second, we sketch the possible value for the present explanatory approach for a critical evaluation of theories (Section II). After this, we analyze in greater detail the two examples of inferential statistics (Section III) and the digital computer (Section IV). We close with a reconsideration of the issue of the generation of psychological theories (Section V). In doing so, we aim to show how ongoing psychological research sometimes can, and should, integrate considerations concerning its history and philosophy, rather than outsourcing them to other disciplines.

I. TOOLS, METAPHORS, AND THEORY DEVELOPMENT

Conceiving the mind in terms of scientific tools may seem strange. However, understanding aspects of mental life in such ways might be rooted in our common-sense thinking or in our intellectual history.

For instance, before psychology was institutionalized as a discipline in the latter half of the 19th century, many investigations of our sensory capacities could be found in astronomical and optical writings. Investigations of human capacities were often driven by methodological needs of other sciences, and so the senses of human beings were viewed as instruments functioning more or less properly (Gundlach, 1997, 2007). The astronomer Tobias Mayer developed a series of what we would characterize as psychophysical experimental analyses of visual acuity, although his main goal was to develop a "science of errors" (Mayer, 1755; Scheerer, 1987). He aimed at an investigation of the weaknesses of our eyes, comparing their role with that of the instruments used in the observation of heavenly bodies. When Johann Heinrich Lambert tried to measure the intensity of light, he complained that there did not yet exist a photometer comparable to the thermometer in the theory of heat. Hence, the eye had to be used as the measuring device, despite its familiar limitations (Lambert, 1760/1892). Much talk of a "sensory apparatus" derives from such contexts; nowadays, this is ordinary, largely innocent talk, hardly recognizable in its metaphorical origins. Rhetoricians speak of "dead metaphors" here—a misleading metaphor itself, because the metaphors are better characterized as alive, although they are no longer noticed as such. These metaphors inform and shape the content of the terms we take to be as literally referring. The same can be, and often is, true in scientific theories. As W. V. O. Quine (1978) said, metaphors are "vital ... at the growing edges of science" (p. 159). It would be thus a mistake to ignore or prohibit the use of scientific tools for trying to conceive the mind in new ways.

To at least some extent, such a successful transfer of meaning is possible only if one does not understand the functioning of metaphors in traditional ways. It has often been claimed that metaphors work in one direction only, as when the metaphor "Achill is a lion" is teased out to give "Achill is like a lion in the following regards ..." This functioning of metaphor is didactical rather than heuristical; its goals are more understanding and teaching than research and discovery. However, metaphors frequently involve an interaction between the terms that are explained metaphorically and the metaphorical terms themselves, by which various meanings are picked out, emphasized, later on rejected, and remembered again. Both our understanding of what was originally referred to metaphorically and the metaphorical expressions are reshaped (Black, 1962; Draaisma, 2000, chap. 1). Such interaction is especially possible in the long-term developments of science and language. However one thinks of the functioning of metaphors in general, the interaction theory is adequate for the heuristically useful metaphors in science.

Not all scientific tools can play this heuristic role for science in general or for psychology in particular. The simple pieces of round white paper that were used in the Paris Academy in the 17th century to produce the impression of the blind spot in the visual field did never support the generation of new concepts of vision (Mariotte, 1668); neither did the early apparatuses used to experimentally present and measure the temporal persistence of visual sensations (D'Arcy, 1765; Sturm, in press), and so on, for many later psychological tools such as the simple weights used by E. H. Weber and G. T. Fechner in their psychophysiological experiments, the Hipp chronoscope in reaction time measurement, or, more recently, instruments for visual imaging such as positron emission tomography or functional magnetic resonance imaging.

But the tools-to-theories heuristic applies for various innovative theories within psychology (Gigerenzer, 1991). For instance, Smith (1986) argued that Tolman's use of the maze as an experimental apparatus transformed Tolman's conception of purpose and cognition into spatial characteristics, such as cognitive maps. Similarly, he argued that Clark L. Hull's fascination with conditioning machines shaped Hull's thinking of behavior as if it were machine design. The tools-to-theories heuristic also applies, as we will argue, in the cases of inferential statistics and digital computer programs.

The tools-to-theories heuristic is twofold:

1. *Generation of new theories*: The tools a scientist uses can suggest new metaphors, leading to new theoretical concepts and principles.
2. *Acceptance of new theories within scientific communities*: The new theoretical concepts and assumptions are more likely to be accepted by the scientific community if the members of the community are also users of the new tools.

This heuristic explains not the discovery but the generation or development of theories (theoretical concepts and claims). Talk of discovery tends to imply success (Arabatzis, 2002; Curd, 1980; Papineau, 2003; Sturm & Gigerenzer, 2006), but it should be treated as an open question whether theoretical notions and assumptions inspired by scientific tools might have led to good research programs or not. For a similar reason, we speak here not of *justification* but of *acceptance*. A scientific community might be justified from its own current point of view in accepting a theory, but such acceptance might later be found to be in need of further revision.

A highly difficult question is that of how, as it is claimed in Step 1, tools can begin to be used as new metaphors. How is a new theoretical concept, as inspired by a tool, originally generated in a scientist? We think that it is important to note here that it is not tools *simpliciter* that suggest new concepts, but the way a tool is used. When tools of justification are used metaphorically to conceptualize the mind, a new, deviant use of the tools comes into play. Such a deviant use becomes possible if the scientist has a practical familiarity with a tool. A sophisticated understanding of the tool is not necessary. A scientist who knows how to successfully apply a given method to analyze his data may start to compare other systems with the functioning of his tool and then to interpret these systems in terms of the tool. Some such psychological processes should play a role, and they are themselves in need of a better explanation: Are there highly general principles or mechanisms that guide all such processes of theory generation? Or is the nature of these processes more strongly constrained by the specific tools that are used as metaphors, and the psychological phenomena that are conceptualized thereby? Surely such an explanatory task is too complex to be fully addressed here. We wish to emphasize that, first, it is the practical familiarity with the tool that can inspire a new metaphor. Second, it is important that even the ordinary use of a tool—its use for the justification of empirical claims or for the evaluation of a general hypothesis—is not always one and the same. Methods of statistical inference, for instance, have been used for various purposes and in various ways: For example, one might use methods of statistical inference to test hypotheses, or to check the data. It is important to clarify which of these options have also entered the cognitive theories of human thought and behavior that were developed on the background of the metaphor of the mind as an intuitive statistician. This general point applies to the case of the metaphor of mind as computer as well. We return to this later.

II. THE CRITICAL VALUE OF AN EXPLANATION OF THEORY GENERATION

Within the class of tools that can play a metaphorical role, some are better suited for this than others, much as some metaphors in general can be better than others. Once this is recognized, it becomes clear that the tools-to-theories heuristic may be of interest not only for an a posteriori understanding of theory development, or for a psychology of scientific creativity (e.g., Gardner, 1988; Gruber, 1981; Tweney, Dotherty, & Mynatt, 1981). It may also be useful for a critical understanding of present-day theories and for the development of new alternatives. We shall illustrate this by three closely related topics: the justification of these theories; the realistic interpretation of these theories; and the complex relation between theory, data, and tools.

First, let us go back to the distinction between *discovery* and *justification*. It is important here not to view it as a distinction between different processes, let alone processes of a specific temporal order: First comes discovery, then justification. We should rather emphasize that there are different types of questions we can ask with regard to scientific propositions. For any given claim *p*, we can always ask "Is *p* justified?" This question differs in principle from the question "How did someone come to accept that *p*?" (Hoyningen-Huene, 1987; Reichenbach, 1938; Sturm & Gigerenzer, 2006).

Hans Reichenbach and other adherents of the discovery–justification distinction often assume that the critical task of evaluating a scientific claim can be pursued quite independently of knowledge about the origins of that claim. This is why defenders of the distinction hardly found it necessary to pursue research about what brings about new discoveries. Here we disagree. It seems plausible that sometimes a good criticism of a theoretical assumption will profit from such knowledge, if not be impossible without it. The reason for this claim is the following: The heuristic function of tools in theory generation involves a metaphorical transfer of meaning. Metaphorical transfer of meaning from one context to another is often advantageous, but it can also include losses. S. Freud famously compared the relation between the two systems of perception–consciousness and memory to the *Wunderblock* or "mystic writing pad." On such a pad, consisting of a wax layer, a wax sheet, and a transparent celluloid paper, one can erase text by simply pulling the paper free of the wax layer. When one pulls the paper, however, at a deeper level a trace of what had been written is stored. Freud also pointed out that, unlike our capacity of memory, the pad cannot reproduce the erased text from inside (Draaisma, 2000). Metaphors emphasize some aspects and leave others out. Especially in cases of the more successful metaphors in science, such partiality can easily be forgotten. The more aware we become that there has been, and continues to be, a use of tools in the development of theoretical concepts or assumptions, the better we can take care of the pitfalls contained in influential theoretical concepts and assumptions.

Second, the tools-to-theories explanation of theory generation has caused some worries among realistically inclined philosophers. Thus, it has been maintained that tools have been merely necessary conditions of the generation and the factual acceptance of the theories that we will discuss:

> How can cognitive scientists possibly be tracking the truth, if they can be persuaded to believe given theories by institutional developments which have no apparent connection with the subject matter of those theories? ... It would indeed be damning if the institutional developments in question were *sufficient* to determine theory acceptance. But their being necessary leaves it open that other factors might also have been necessary, and in particular proper empirical support might have been necessary too. (Papineau, 2003, pp. 146–147)

Such worries are inspired by debates about realism and antirealism in the philosophy of science (see Hacking, 1983; Kitcher, 1993, chap. 5; Papineau, 1996). Here it is important to see, first, that we keep up the traditional distinction between discovery and justification in a certain sense. From the fact that the generation of the new theories is to be (in part) explained on the basis of the tools-to-theories heuristic, it does not follow that the theories are correct. A main goal of this chapter is to make psychologists aware of where crucial new ideas of the cognitive revolution came from and that these origins are by no means innocent. Second, the view that theoretical models were inspired by certain methodological tools by no means implies that the models must be incorrect either. The explanation of the development of new theories leaves open the question of whether they "map" an independent reality or whether the claims of the theory are true or correct.

This reply leads to the crucial worry. The debates between scientific realism and antirealism mainly concern the meaning of *theoretical* terms and statements. Can terms such as "electron" or "DNA" be interpreted realistically? That is, do they refer to mind-independent objects and properties? And can the statements in which such terms occur be true or false in the same way in which more mundane observational statements, such as "The cat is on the mat," can be, or are they simply efficient instruments of prediction and explanation of the phenomena?

There are no simple answers to such questions. We should hardly be surprised if it is an open question whether current theories of cognitive science can be understood realistically. We should also resist the assumption that one either has to be a scientific realist *tout court* or one has to accept antirealism. One may defend a realistic interpretation of, say, *electron* without thereby being a realist with regard to all theoretical parts of the various sciences. The difficult task is to identify criteria for a realistic interpretation and to show that these criteria apply.

As cited earlier, Papineau (2003) suggested that the relevant cognitive theories might have been accepted not only because scientists were fond of their tools but also because there was proper empirical support. However, that is much too simplistic. We argue later in this chapter that some types of empirical evidence were possible only because the theories were already assumed to be correct, and so the reference to empirical evidence needs additional qualifications at least. Also, some alternative theories of cognitive processing (e.g., different statistical models) can make some data virtually disappear. Stated generally, talk of proper empirical support cannot do the real job. It might also lead to a merely instrumentalist, antirealistic interpretation of the theoretical concepts and claims.

In fact, the defense of a realistic interpretation of any particular theory depends on more complex arguments and is itself a matter of piecemeal, long-term research. Typical kinds of arguments that support realism about a given theory involve *extrapolation*, as when microscopic phenomena are legitimately understood in terms of macroscopic phenomena; or *circumstantial evidence*, which may be illustrated by the case of the quite heterogeneous discoveries in support of an atomistic theory of matter. In physical and chemical theories of matter of the 18th and 19th centuries it was found out independently that substances react in fixed numerical proportions; that solid bodies must be viewed as structures of elements that do not allow for arbitrary combinations, a fact that excluded theories of matter as a continuous entity; that the number of particles in a chemical substance could be determined by Avogadro's number, and so on. Such heterogeneous discoveries supported a realistic understanding of the term "atom," but this was a hard-fought-for achievement (Krüger, 1981). Knowing the origins of some theoretical concepts better might help us to think critically about such issues and reflect whether such criteria apply: Is it, or is it not, a legitimate extrapolation to view some aspects of thought and behavior in terms of information processing or statistics? Is there any circumstantial evidence for this theoretical vocabulary?

One might try to avoid such difficult problems by biting the antirealistic bullet. Is it not better to view the theoretical concepts and claims of cognitive psychology as "mere" constructs or as "as-if" models? One may do so, but there is a price to be paid here. For instance, we mentioned earlier that behaviorists did use a mentalistic vocabulary. However, for them mentalistic terms did not really refer to intervening variables that are crucial for a cognitivist approach to the explanation of psychological phenomena. Only the latter approach takes seriously the view that mental states play real causal roles. Empiricists within current psychology who wish to treat talk of information processing or of the mind as a computer as merely a model or as merely metaphorical face a similar problem. Their explanations remain on a purely empirical level of generalization, at the risk of being mere redescriptions instead of real explanations. One takes a step back if one does not try to substantiate the pretensions of the cognitive revolution. Again, however, even a moderate realism about cognitive theories cannot be hoped for if one has not critically reflected where theoretical concepts came from, how they have spread over the scientific community, and what their possible problems and limitations are.

Third, the generation of theories through tools leads to possibly problematic relations between theory, data, and tools which should be highlighted in advance. The familiar theory-laden ness of data and instruments already questions simple views about the relation between theory and data (Figures 14–1 and 14–2). Now, the fact that certain theories are inspired by the tools scientists favor makes things even more difficult, because scientists are rarely aware of the metaphorical origins, and possible pitfalls, of their theories. Neither are they always clear that their favorite tools, theories, and data might be supporting one another, in ways that leave other, and perhaps more fruitful, research directions out of sight (see Fig. 14–3).

We do not claim that the circularity indicated in Figure 14–3 must always occur, or that its problems cannot be avoided. On the other hand,

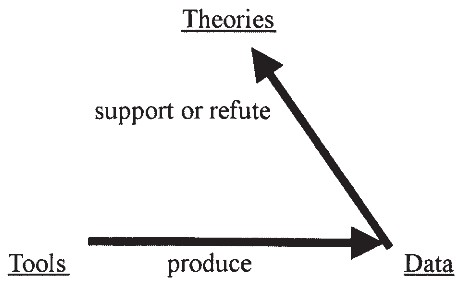

Figure 14–1. The standard view of the relation among tools, data, and theories. According to this view, scientific instruments can be used to produce data, which are then used to support or refute theories, in a neutral or unbiased way.

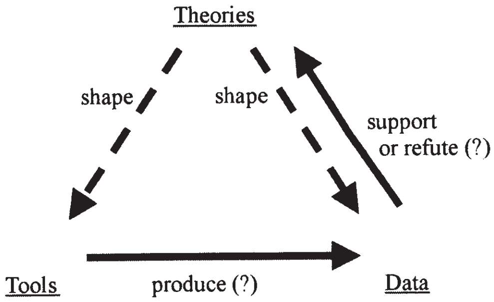

Figure 14–2. The well-known theory-ladenness of data and instruments questions the standard view. Are data produced in theory-neutral ways? If not, can they be used to support or refute theories? How can theories then be understood realistically?

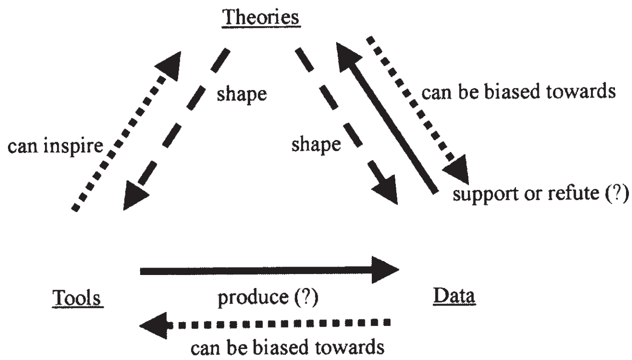

Figure 14–3. The possibly circularity among tools, theories, and data. Theoretical concepts and claims are often inspired by the scientists' favorite research tool, theories are not supported or tested by theory-neutral data, and tools tend to favor certain data and to leave others out. If that is so, do then tools, theories, and data justify one another in a circular or self-vindicating way?

we do not see any general procedure for solving the problems. The best thing seems to be to learn from historical case studies and to make scientists aware of the potentially circular relation among tools, theories, and data. This said, we turn to the two tools that have turned into psychological theories: (a) inferential statistics and (b) the digital computer.

III. COGNITION AS INTUITIVE STATISTICS

In American psychology, the study of cognitive processes was suppressed in the early 20th century by the allied forces of operationalism and behaviorism. The operationalism and the inductivism of the Vienna School, *inter alia*, paved the way for the institutionalization of inferential statistics in American experimental psychology between 1940 and 1955 (Gigerenzer, 1987a; Toulmin & Leary, 1985). In experimental psychology, inferential statistics became almost synonymous with the scientific method. Inferential statistics, in turn, provided a large part of the new concepts for mental processes that have fueled the cognitive revolution since the 1960s. Theories of cognition were cleansed of terms such as *restructuring* and *insight*, and the new mind has come to be portrayed as drawing random samples from nervous fibers, computing probabilities, calculating analyses of variance, setting decision criteria, and performing utility analyses.

After the institutionalization of inferential statistics, a broad range of cognitive processes were reinterpreted as involving *intuitive statistics*. For instance, W. P. Tanner and his coworkers assumed in their theory of signal detectibility that the mind "decides" whether there is a stimulus or only noise, just as a statistician of the Neyman–Pearson school decides between two hypotheses (Tanner & Swets, 1954). In his causal attribution theory, Harold H. Kelley (1967) postulated that the mind attributes a cause to an effect in the same way as behavioral scientists have come to do, namely, by performing an analysis of variance (ANOVA) and testing null hypotheses. These influential theories show the breadth of the new conception of the "mind as an intuitive statistician" (Gigerenzer & Murray, 1987).

Three points need to be argued for in closer detail here. First, the development of theories based on the conception of the mind as an intuitive statistician caused discontinuity in theory rather than being merely a new, fashionable language. Second, there was an inability of researchers to accept the conception of the mind as an intuitive statistician before they became familiar with inferential statistics as part of their daily routine. Third, we will show how the tools-to-theories heuristic can help us to see the limits and possibilities of current cognitive theories that investigate the mind as an intuitive statistician.

Discontinuity in Cognitive Theory Development

The spectrum of theories that model cognition after statistical inference ranges from auditory and visual perception to recognition in memory, and from speech perception to thinking and reasoning. The discontinuity within cognitive theories can be shown in two areas: (a) stimulus detection and discrimination and (b) causal attribution.

What intensity must a 440-Hz tone have to be perceived? How much heavier than a standard stimulus of 100 gm must a comparison stimulus be in order for a perceiver to notice a difference? How does one understand the elementary cognitive processes involved in those tasks, known today as ***stimulus detection*** and ***stimulus discrimination***? Since Herbart (1816), such processes have been explained by using a threshold metaphor: Detection occurs only if the effect an object has on our nervous system exceeds an absolute threshold, and discrimination between two objects occurs if the excitation from one exceeds that from another by an amount greater than a differential threshold. Weber's and Fechner's laws refer to the concept of fixed thresholds, Titchener (1896) saw in differential thresholds the long-sought-after elements of mind (he counted approximately 44,000), and classic textbooks such as Brown and Thomson's (1921) and Guilford's (1954) document methods and research.

Around 1955, the psychophysics of absolute and differential thresholds was revolutionized by the new analogy between the mind and the statistician. W. P. Tanner and others proposed a ***theory of signal detectability*** (TSD), which assumes that the Neyman–Pearson technique of hypothesis testing describes the processes involved in detection and discrimination. Recall that in Neyman–Pearson statistics two sampling distributions (hypotheses H_0 and H_1) and a decision criterion (which is a likelihood ratio) are defined, and then the data observed are transformed into a likelihood ratio and compared with the decision criterion. Depending on which side of the criterion the data fall, the decision "reject H_0 and accept H_1" or "accept H_0 and reject H_1" is made. In straight analogy, the TSD assumes that the mind calculates two sampling distributions, for "noise" and "signal plus noise" (in the detection situation), and sets a decision criterion after weighing the cost of the two possible decision errors (Type I and Type II errors in Neyman–Pearson theory, now called ***false alarms*** and ***misses***). The sensory input is transduced into a form that allows the brain to calculate its likelihood ratio and, depending on whether this ratio is smaller or larger than the criterion, the participant says "No, there is no signal" or "Yes, there is a signal." Tanner (1965) explicitly referred to his new model of the mind as a ***Neyman–Pearson detector*** and, in unpublished work, his flow charts included a drawing of a homunculus statistician performing the unconscious statistics in the brain (Gigerenzer & Murray, 1987, pp. 43–53).

The new analogy between mind and statistician replaced the old concept of a fixed threshold by the twin notions of observer's attitudes and observer's sensitivity. Just as the Neyman–Pearson technique distinguishes between a subjective part (e.g., selection of a criterion dependent on cost–benefit considerations) and a mathematical part, detection and discrimination became understood as involving both subjective processes, such as attitudes and cost–benefit considerations, and sensory processes. Swets, Tanner, and Birdsall (1964, p. 52) considered this link between attitudes and sensory processes to be the main thrust of their theory. The analogy between technique and mind made new research questions thinkable, such as "How can the mind's decision criterion be manipulated?" A new kind of data even emerged: Two types of errors—false alarms and misses—were generated in the experiments, just as the statistical theory distinguishes two types of error. The development of TSD was not motivated by new data; instead, the new theory motivated a new kind of data. In fact, in their seminal article, Tanner and Swets (1954) admitted that their theory "appears to be inconsistent with the large quantity of existing data on this subject," and they proceeded to criticize the "form of these data" (p. 401).

The Neyman–Pearsonian technique of hypothesis testing was subsequently transformed into a theory of a broad range of cognitive processes, ranging from recognition in memory (e.g., Murdock, 1982; Wickelgreen & Norman, 1966) to eyewitness testimony (e.g., Birnbaum, 1983) and discrimination between random and nonrandom patterns (e.g., Lopes, 1982).

The second example concerns theories of causal reasoning. Albert Michotte (1946/1963), Jean Piaget (1930), the Gestalt psychologists, and others had investigated how temporal–spatial relationships between two or more visual objects, such as moving dots, produced phenomenal causality. For instance, research participants were made to "perceive" that one dot launches, pushes, or chases another. After the institutionalization of inferential statistics, Harold H. Kelley (1967) proposed in his ***attribution theory*** that the long-sought laws of causal reasoning are in fact the tools of the behavioral scientist: R. A. Fisher's ANOVA. Just as the experimenter has come to infer a causal relationship between two variables from calculating an ANOVA and performing an F test, the man in the street infers the cause of an effect by unconsciously doing the same calculations. By the time Kelley developed the new metaphor for causal inference, about 70% of all experimental articles already used ANOVA (Edgington, 1974).

The theory was quickly accepted in social psychology; Kelley and Michaela (1980) reported more than 900 references in 10 years. The vision of the Fisherian mind radically changed the understanding of causal reasoning, the problems posed to the participants, and the explanations looked for. Here are a few discontinuities that reveal the fingerprints of the tool.

1. ANOVA needs repetitions or numbers as data to estimate variances and covariances. Consequently, the information presented to the participants in studies of causal attribution consists of information about the frequency of events (e.g., McArthur, 1972), which played no role in either Michotte's or Piaget's work.

2. Whereas Michotte's work still reflects the broad Aristotelian conception of four causes (see Gavin, 1972), and Piaget (1930) distinguished 17 kinds of causality in children's minds, the Fisherian mind concentrates on the one kind of causes for which ANOVA is used as a tool (similar to Aristotle's "efficient cause").

3. In Michotte's view, causal perception is direct and spontaneous and needs no inference, as a consequence of largely innate laws that determine the organization of the perceptual field. ANOVA, in contrast, is used in psychology as a technique for inductive inferences from data to hypotheses, and the focus in Kelley's attribution theory is consequently on the data-driven, inductive side of causal perception.

The last point illustrates that the specific *use* of a tool, that is, its practical context, rather than merely its mathematical structure, can also shape theoretical conceptions of mind. What if Harold Kelley had lived 150 years earlier than he did? In the early 19th century, significance tests (similar to those in ANOVA) were already being used by astronomers (Swijtink, 1987). However, they used their tests to reject data, so-called "outliers," and not to reject hypotheses. At least provisionally, the astronomers assumed that the theory was correct and mistrusted the data, whereas the ANOVA mind, following the current statistical textbooks, assumes the data to be correct and mistrusts the theories. So, to our 19th-century Kelley, the mind's causal attribution would have seemed expectation driven rather than data driven: The statistician homunculus in the mind would have tested the data and not the hypothesis.

Before the Institutionalization of Inferential Statistics

There is an important test case for the present hypothesis: (a) that familiarity with the statistical tool is crucial to the generation of corresponding theories of mind and (b) that the institutionalization of the tool within a scientific community can strongly further the broad acceptance of those theories. That test case is the era before the institutionalization of inferential statistics. Theories that conceive of the mind as an intuitive statistician should have a very small likelihood of being discovered and even less likelihood of being accepted. The two strongest tests are cases where (a) someone proposed a similar conceptual analogy and (b) someone proposed a similar probabilistic (formal) model. We know of only one case each, which we will analyze after defining first what is meant by the term ***institutionalization of inferential statistics***.

Statistical inference has been known for a long time. In 1710, John Arbuthnot proved the existence of God using a kind of significance test; as mentioned earlier, astronomers used significance tests in the 19th century; G. T. Fechner's statistical text *Kollektivmasslehre* (1897) included tests of hypotheses; W. S. Gosset (using the pseudonym "Student") published the *t* test in 1908; and Fisher's significance testing techniques, such as ANOVA, as well as Neyman–Pearsonian hypothesis testing methods, have been available since the 1920s (see Gigerenzer et al., 1989). Bayes's theorem was known since 1763. Nonetheless, there was little interest in these techniques in experimental psychology before 1940 (Rucci & Tweney, 1980).

By 1942, Maurice Kendall (1942) could comment on the statisticians' expansion: "They have already overrun every branch of science with a rapidity of conquest rivaled only by Attila, Mohammed, and the Colorado beetle" (p. 69). By the early 1950s, half of the psychology departments in leading American universities offered courses on Fisherian methods and had made inferential statistics a graduate program requirement. By 1955, more than 80% of the experimental articles in leading journals used inferential statistics to justify conclusions from the data (Sterling, 1959), and editors of major journals made significance testing a requirement for the acceptance of articles submitted (e.g., Melton, 1962).

The year 1955 can be used as a rough date for the institutionalization of the tool in curricula, textbooks, and editorials. What became institutionalized as the logic of statistical inference was a mixture of ideas from two opposing camps, those of R. A. Fisher, on the one hand, and Jerzy Neyman and Egon S. Pearson (the son of Karl Pearson) on the other.

Genesis and Early Rejection of the Analogy

The analogy between the mind and the statistician was first proposed before the institutionalization of inferential statistics, in the early 1940s, by Egon Brunswik at Berkeley (e.g., Brunswik, 1943). As Leary (1987) showed, Brunswik's probabilistic functionalism was based on a very unusual blending of scientific traditions, including the probabilistic worldview of Hans Reichenbach and members of the Vienna School, and Karl Pearson's correlational statistics.

The important point here is that in the late 1930s Brunswik changed his techniques for measuring perceptual constancies, from calculating (nonstatistical) *Brunswik ratios* to calculating Pearson correlations, such as *functional* and *ecological validities*. In the 1940s, he also began to think of the organism as "an intuitive statistician," but it took him several years to spell out the analogy in a clear and consistent way (Gigerenzer, 1987b).

The analogy is this: The perceptual system makes inferences from its environment from uncertain cues by (unconsciously) calculating correlation and regression statistics, just as the Brunswikian researcher does when (consciously) calculating the degree of adaptation of a perceptual system to a given environment. Brunswik's "intuitive statistician" was a statistician of the Karl Pearson school, like the Brunswikian researcher. Brunswik's intuitive statistician was not well adapted to the psychological science of the time, however, and the analogy was poorly understood and generally rejected (Leary, 1987).

Brunswik's analogy came too early to be accepted by his colleagues of the experimental discipline; it came before the institutionalization of statistics as the method of scientific inference, and it came with the "wrong" statistical model: correlational statistics. Correlation was an indispensable method not in experimental psychology but in its rival discipline, known as the *Galton–Pearson program* or, as Cronbach (1957) put it, the "Holy Roman Empire" of correlational psychology. The schism between the two disciplines had been repeatedly taken up in presidential addresses before the American Psychological Association (Dashiell, 1939; Cronbach, 1957) and had deeply affected the values and the mutual esteem of psychologists (Thorndike, 1954). Brunswik could not succeed in persuading his colleagues from the experimental discipline to consider the statistical tool of the competing discipline as a model of how the mind works. Ernest Hilgard (1955), in his rejection of Brunswik's perspective, did not mince words: "Correlation is an instrument of the devil" (p. 228).

Brunswik, who coined the metaphor of "man as intuitive statistician," did not survive to see the success of his analogy. It was accepted only after statistical inference became institutionalized in experimental psychology and with the new institutionalized tools rather than (Karl) Pearsonian statistics serving as models of mind. Only in the mid-1960s, however, did interest in Brunswikian models of mind emerge (e.g., Brehmer & Joyce, 1988; Hammond, Stewart, Brehmer, & Steinmann, 1975).

Probabilistic Models Without the "Intuitive Statistician"

Although some probabilistic models of cognitive processes were advanced before the institutionalization of inferential statistics, they were not interpreted using the metaphor of the mind as intuitive statistician. This is illustrated by models that use probability distributions for perceptual judgment, assuming that variability is caused by lack of experimental control, measurement error, or other factors that can be summarized as experimenter ignorance. Ideally, if the experimenter had complete control and knowledge (e.g., Laplace's superintelligence), all probabilistic terms could be eliminated from the theory (Laplace 1814-1951, p. 1325). This does not hold for a probabilistic model that is based on the metaphor. Here, the probabilistic terms model the ignorance of the mind rather than that of the experimenter; that is, they model how the "homunculus statistician" in the brain comes to terms with a fundamental uncertain world. Even if the experimenter had complete knowledge, the theories would remain probabilistic, because it is the mind that is ignorant and needs statistics.

The key example is L. L. Thurstone, who in 1927 formulated a model for perceptual judgment that was formally equivalent to the present-day TSD. However, neither Thurstone nor his followers recognized the possibility of interpreting the formal structure of their model in terms of the intuitive statistician. Like TSD, Thurstone's model had two overlapping normal distributions, which represented the internal values of two stimuli and which specified the corresponding likelihood ratios, but it never occurred to Thurstone to include the conscious activities of a statistician, such as the weighing of the costs of the two errors and the setting of a decision criterion, in his model. Thus, neither Thurstone nor his followers took—with hindsight—the small step to develop the "law of comparative judgment" into TSD. When Duncan Luce (1977) reviewed Thurstone's model 50 years later, he found it hard to believe that nothing in Thurstone's writings showed the least awareness of this small but crucial step. Thurstone's perceptual model remained a mechanical, albeit probabilistic, stimulus–response theory without a homunculus statistician in the brain. The small conceptual step was never taken, and TSD entered psychology by an independent route.

To summarize: There are several kinds of evidence for a close link between the institutionalization of inferential statistics in the 1950s and the subsequent broad acceptance of the metaphor of the mind as an intuitive statistician: (a) the general failure to accept, and even to understand, Brunswik's intuitive statistician before the institutionalization of the tool, and (b) the case of Thurstone, who proposed a probabilistic model that was formally equivalent to one important present-day theory of intuitive statistics but was never interpreted in this way.

Limitations and Possibilities of Current Research Programs

How can the preceding analysis be of interest for the evaluation of current cognitive theories? One has to recognize that tools like statistics are not theoretically inert. They come with a set of assumptions and interpretations that may be smuggled, in Trojan horse fashion, into the new theories and research programs. Tools may have the advantage of opening new conceptual perspectives or making us see new data, but they may also make us blind in various ways.

There are several assumptions that became associated with the statistical tool in the course of its institutionalization in psychology, none of them being part of the mathematics or statistical theory proper. The first assumption can be called "There is only one statistics." Textbooks on statistics for psychologists (usually written by nonmathematicians) generally teach statistical inference as if there existed only one logic of inference. Since the 1950s and 1960s, almost all texts teach a mishmash of R. A. Fisher's ideas tangled with those of Jerzy Neyman and Egon S. Pearson, but without acknowledgment. The fact that Fisherians and Neyman–Pearsonians could never agree on a logic of statistical inference is not mentioned in the textbooks; neither are the controversial issues that divide them. Even alternative statistical logics for scientific inference are rarely discussed (Gigerenzer, 1993). For instance, Fisher (1955) argued that concepts such as Type II error, power, the setting of a level of significance before the experiment, and its interpretation as a long-run frequency of errors in repeated experiments, are concepts inappropriate for scientific inference—at best, they could be applied to technology (his pejorative example was Stalin's). Neyman, for his part, declared that some of Fisher's significance tests are "worse than useless" (because their power is less than their size; see Hacking, 1965, p. 99). Textbooks written by psychologists for psychologists usually present an intellectually incoherent mix of Fisherian and Neyman–Pearsonian ideas, but a mix presented as a seamless, uncontroversial whole (Gigerenzer et al., 1989, chaps. 3 and 6).

This assumption that "statistics is statistics is statistics" reemerges at the theoretical level in current psychology (Gigerenzer, 2000). For instance, research on so-called "cognitive illusions" assumes that there is one and only one correct answer to statistical reasoning problems. As a consequence, other answers are considered to reflect reasoning fallacies. Some of the most prominent reasoning problems, however, such as the cab problem (Tversky & Kahneman, 1980, p. 62), do not have just one answer; the answer depends on the theory of statistical inference and the assumptions applied. Birnbaum (1983), for example, showed that the "only correct answer" to the cab problem claimed by Tversky and Kahneman, based on Bayes's rule, is in fact only one of several reasonable answers—different ones are obtained, for instance, if one applies the Neyman–Pearson theory (Gigerenzer & Murray 1987, chap. 5).

A second assumption that became associated with the tool during its institutionalization is that "there is only one meaning of probability." For instance, Fisher and Neyman–Pearson had different interpretations of what a level of significance means. Fisher's was an epistemic interpretation; that is, that the level of significance tells us about the confidence we can have in the particular hypothesis under test, whereas Neyman's was a strictly frequentist and behavioristic interpretation that claimed that a level of significance tells us nothing about a particular hypothesis but about the long-run relative frequency of wrongly rejecting the null hypothesis if it is true. In textbooks, these alternative views of what a probability (e.g., level of significance) could mean are generally neglected—not to speak of the other meanings, subjective and objective, that have been proposed for the formal concept of probability (Hacking, 1965).

Many of the so-called cognitive illusions were demonstrated using a subjective interpretation of probability, specifically, asking people about the probability they assign to a single event. When instead researchers began to ask people for judgments of frequencies, these apparently stable reasoning errors—the conjunction fallacy and the overconfidence bias, for example—largely or completely disappeared (Gigerenzer, 2000, chap. 12; 2001). Untutored intuition seems to be capable of making conceptual distinctions of the sort statisticians and philosophers make, such as between judgments of subjective probability and those of frequency (e.g., Cohen, 1986; Lopes, 1981; Teigen, 1983). These results suggest that the important research questions to be investigated are "How are different meanings of 'probability' cued in every-day language?" and "How does this affect judgment?" rather than "How can we explain the alleged bias of 'overconfidence' by some general deficits in memory, cognition, or personality?"

To summarize: Assumptions entrenched in the practical use of statistical tools—which are not part of the mathematics—can re-emerge in research programs on cognition, resulting in severe limitations in these programs. This could be avoided by pointing out these assumptions, and this may even lead to new research questions.

IV. MIND AS COMPUTER

Prehistory

The relation between conceptions of the mind and the computer has had a long history, involving an interaction among social, economical, mental, and technological contexts (see Gigerenzer, 2003). Here, we concentrate on the period of time since the cognitive revolution of the 1960s when the computer, after becoming a standard laboratory tool in this century, was proposed and, with some delay, accepted, as a model of mind. In particular, we focus on the development and (delayed) acceptance of Herbert Simon and Allen Newell's brand of information processing psychology (Newell & Simon, 1972).

The invention of the first modern computers, such as the ENIAC and the EDVAC at the University of Pennsylvania during and after the second world war, did not lead immediately to a view of the mind as a computer. There were two groups drawing a parallel between the human and the computer, but neither used the computer as a theory of mind. One group, which tentatively compared the nervous system and the computer, is represented by the mathematician John von Neumann (1903–1957). The other group, which investigated the idea that machines might be capable of thought, is represented by the mathematician and logician Alan Turing (1912–1954). Von Neumann, known as the father of the modern computer, wrote about the possibility of an analogy between the computer and the human nervous system. He thus drew the comparison on the level of the hardware. Turing (1950), in contrast, thought the observation that both the digital computer and the human nervous system are electrical, is based on a "very superficial similarity" (p. 439). He pointed out that the first digital computer, Charles Babbage's Analytical Engine, was purely mechanical (as opposed to electrical) and that the important similarities to the mind are in function, or in software.

Turing discussed the question of whether machines can think rather than the question of whether the mind is like a computer. Thus, he was looking in the opposite direction than psychologists were going the cognitive revolution and, consequently, he did not propose any theories of mind. He argued that it would be impossible for a human to imitate a computer, as evidenced by humans' inability to perform complex numerical calculations quickly. He also discussed the question of whether a computer could be said to have a free will, a property of humans (many years later, cognitive psychologists, under the assumptions that the mind is a computer and that computers lack free will, pondered the question of whether humans could be said to have one). And, most famously, the famous Turing test is about whether a machine can imitate a human mind, but not vice versa.

Turing (1969) anticipated much of the new conceptual language and even the very problems Newell and Simon were to attempt, as we will see. With amazing prophecy, Turing suggested that many intellectual issues can be translated into the form "find a number *n* such that ..."; that is, that "search" is the key concept for problem solving, and that Whitehead and Russell's (1935) *Principia Mathematica* might be a good start for demonstrating the power of the machine (McCorduck, 1979, p. 57). Still, Turing's work had practically no influence on artificial intelligence in Britain until the mid-1960s (McCorduck, 1979, p. 68).

Newell's and Simon's New Conception: Meaning and Genesis

Babbage's mechanical computer was preceded by human computers performing highly limited tasks of calculation. Similarly, Newell and

Simon's first computer program, the Logic Theorist (LT), was preceded by a human computer. Before the LT was up and running, Newell and Simon reconstructed their computer program out of human components (namely, Simon's wife, children, and several graduate students), to see if it would work. Newell wrote up the subroutines of the LT program on index cards:

> To each member of the group, we gave one of the cards, so that each person became, in effect, a component of the LT computer program—a subroutine—that performed some special function, or a component of its memory. It was the task of each participant to execute his or her subroutine, or to provide the contents of his or her memory, whenever called by the routine at the next level above that was then in control.
>
> So we were able to simulate the behavior of the LT with a computer consisting of human components ... The actors were no more responsible than the slave boy in Plato's Meno, but they were successful in proving the theorems given them. (Simon, 1991, p. 207)

As in Babbage's engine, the essence of the functioning of the LT is a division of labor—each human actor requiring little skill and repeating the same routine again and again. Complex processes are achieved by an army of workers who never see but a little piece of the larger picture.

However, there is an important difference between Babbage's mechanical computer and Simon's LT (and their human precursors). Babbage's engine performed numerical calculations; Simon's computer matched symbols, applied rules to symbols, and searched through lists of symbols—what is now generally known as *symbol manipulation*.

An important precondition for the view of mind as a computer is the realization that computers are symbol manipulation devices, in addition to being numerical calculators: As long as computers are viewed as being restricted to the latter, and as long as mental activities are seen as more complex than numerical calculation, it is hardly surprising that computers are not proposed as a metaphor for the mind. Newell and Simon were among the first to realize this. In interviews with Pamela McCorduck (1979, p. 129), Allen Newell recalled, "I've never used a computer to do any numerical processing in my life." Newell's first use of the computer at RAND corporation—a prehistoric card-programmed calculator hooked up to a line printer—was calculating and printing out symbols representing airplanes for each sweep of a radar antenna.

The symbol-manipulating nature of the computer was important to Simon because it corresponded to some of his earlier views on the nature of intelligence:

> The metaphor I'd been using, of a mind as something that took some premises and ground them up and processed them into conclusions, began to transform itself into a notion that a mind was something which took some program inputs and data and had some processes which operated on the data and produced output. (cited in McCorduck, 1979, p. 127)

It is interesting to note that 20 years after seeing the computer as a symbol manipulating device, Newell and Simon came forth with the explicit hypothesis that a physical symbol system is necessary and sufficient for intelligence.

The LT generated proofs for theorems in symbolic logic, specifically, the first 25 or so theorems in Whitehead and Russell's (1935) *Principia Mathematica*. It even managed to find a proof more elegant than the corresponding one in the *Principia.*

In the summer of 1958, psychology was given a double dose of the new school of information-processing psychology. One was the publication of the *Psychological Review* article "Elements of a Theory of Human Problem Solving" (Newell, Shaw, & Simon, 1958). The other was the Research Training Institute on the Simulation of Cognitive Processes at the RAND institute, which we discuss later.

The *Psychological Review* article is an interesting document of the transition between the view that the LT is a tool for proving theorems in logic (the artificial intelligence view) and an emerging view that the LT is a model of human reasoning (the information-processing view). The authors go back and forth between both views, explaining that "the program of LT was not fashioned directly as a theory of human behavior; it was constructed in order to get a program that would prove theorems in logic" (Newell, Shaw, & Simon, 1958, p. 154) but later that LT "provides an explanation for the processes used by humans to solve problems in symbolic logic" (Newell et al., 1958, p. 163). The evidence provided for projecting the machine into the mind is mainly rhetorical. For instance, the authors spend several pages arguing for the resemblance between the methods of LT and concepts such as set, insight, and hierarchy, described in the earlier psychological literature on human problem solving.

In all fairness, despite the authors' claim, the resemblance to these earlier concepts as they were used in the work of Karl Duncker, Wolfgang Köhler, and others, is slight. It is often a useful strategy to hide the amount of novelty and claim historical continuity. When Tanner and Swets, 4 years earlier, also in *Psychological Review*, proposed that another scientific tool, Neyman–Pearsonian techniques of hypothesis testing, would model the cognitive processes of stimulus detection and discrimination, their signal detection model also clashed with earlier notions, such as the notion of a sensory threshold. Tanner and Swets (1954, p. 401), however, chose not to conceal this schism, explicitly stating that their new theory "appears to be inconsistent with the large quantity of existing data on this subject." There is a different historical continuity in which Simon and Newell's ideas stand: the earlier Enlightenment view of intelligence as a combinatorial calculus. What was later called the "new mental chemistry" pictured the mind as a computer program:

> The atoms of this mental chemistry are symbols, which are combinable into larger and more complex associational structures called *lists* and *list structures*. The fundamental "reactions" of the mental chemistry use elementary information processes that operate upon symbols and symbol structures: copying symbols, storing symbols, retrieving symbols, inputting and outputting symbols, and comparing symbols. (Simon, 1979, p. 63)

This atomic view is certainly a major conceptual change in the views about problem solving compared with the theories of Köhler, Wertheimer, and Duncker. But it bears much resemblance to the combinatorial view of intelligence of the Enlightenment philosophers.[1]

The different physical levels of a computer led to Newell's cognitive hierarchy, which separates the knowledge-level, symbol-level, and register-transfer levels of cognition. As Arbib (1993) pointed out, the seriality of 1971-style computers is actually embedded in Newell's cognitive theory.

One of the major concepts in computer programming that made its way into the new models of the mind is the decomposition of complexity into simpler units, such as the decomposition of a program into a hierarchy of simpler subroutines, or into a set of production rules. On this analogy, the most complex processes in psychology, and even scientific discovery, can be explained through simple subprocesses (Langley, Simon, Bradshaw, & Zytkow, 1987).

The first general statement of Newell and Simon's new vision of mind appeared in their 1972 book, *Human Problem Solving*. In this book, the authors argue for the idea that higher level cognition proceeds much like the behavior of a production system, a formalism from computer

[1]The new view was directly inspired by the 19th-century mathematician George Boole who, in the spirit of the Enlightenment mathematicians such as Bernoullis and Laplace, set out to derive the laws of logic, algebra, and probability from what he believed to be the laws of human thought (Boole, 1854/1958). Boole's algebra culminated in Whitehead and Russell's (1935) *Principia Mathematica*, describing the relationship between mathematics and logic, and in Claude E. Shannon's seminal work (1938), which used Boolean algebra to describe the behavior of relay and switching circuits (McCorduck, 1979, p. 41).

science (and before that, from symbolic logic) that had never been used in psychological modeling before:

> Throughout the book we have made use of a wide range of organizational techniques known to the programming world: explicit flow control, subroutines, recursion, iteration statements, local naming, production systems, interpreters, and so on .... We confess to a strong premonition that the actual organization of human programs closely resembles the production system organization. (Newell & Simon, 1972, p. 803)

We will not attempt to probe the depths of how Newell and Simon's ideas of information processing changed theories of mind; the commonplace usage of computer terminology in the cognitive psychological literature since 1972 is a reflection of this. It seems natural for present-day psychologists to speak of cognition in terms of encoding, storage, retrieval, executive processes, algorithms, and computational cost.

New Experiments, New Data

New tools can transform the kinds of experiments performed and the data collected. This happened when statistical tools turned into theories of mind, and a similar story is to be told with the conceptual change brought about by Newell and Simon—it mandated a new type of experiment, which in turn involved new kinds of subjects, data, and justification. In academic psychology of the day, the standard experimental design, modeled after the statistical methods of Ronald A. Fisher, involved many subjects and randomized treatment groups. The 1958 *Psychological Review* article uses the same terminology of *design of the experiment* and *subject* but radically changes their meanings. There are no longer groups of human or animal subjects. There is only one subject: an inanimate being named LT. There is no longer an experiment in which data are generated by either observation or measurement. Experiment takes on the meaning of simulation.

In this new kind of experiment, the data are of an unforeseen type: computer printouts of the program's intermediate results. These new data, in turn, require new methods of hypothesis testing. How did Newell and Simon determine whether their program was doing what minds do? There were two methods. For Newell and Simon, simulation was a form of justification itself: a theory that is coded up as a working computer program shows that the processes it describes are, at the very least, sufficient to perform the task, or, in the more succinct words of Simon (1992), "A running program is the moment of truth" (p. 155). Furthermore, a stronger test of the model is made by comparing the computer's output to the think-aloud protocols of human participants. Newell and Simon put their subject, the LT, as a coauthor of a paper submitted to the *Journal of Symbolic Logic*. Regrettably, the paper was rejected (as it contained no new results from modern logic's point of view), and the LT never tried to publish again.

The second dose of information processing (after the *Psychological Review* article) administered to psychology was the Research Training Institute on the Simulation of Cognitive Processes at the RAND institute, organized by Newell and Simon. The institute held lectures and seminars, taught IPL-IV (Information Processing Language-IV) programming, and demonstrated LT, the General Problem Solver, and the EPAM (Elementary Perceiver and Memorizer) model of memory on the RAND computer. In attendance were some figures who would eventually develop computer simulation methods of their own, including George Miller, Robert Abelson, Bert Green, and Roger Shepard.

An early, but deceptive, harbinger of acceptance for the new information-processing theory was the publication, right after the summer institute, of *Plans and the Structure of Behavior* (Miller, Galanter, & Pribram, 1960), written mostly by George Miller. This book was so near to Newell and Simon's ideas that it was at first considered a form of theft, although the version of the book that did see the presses is filled with citations recognizing Newell, Shaw, and Simon. Despite the 1959 dispute with Newell and Simon over the ownership and validity of the ideas within, this book drew a good deal of attention from all of psychology.

It would seem the table was set for the new information-processing psychology; however, it did not take hold. Simon complained of the psychological community who took only a cautious interest in their ideas. Computers were not yet entrenched in the daily routine of psychologists.[2]

[2]Another evidence for this view is that a similar development within the philosophy of mind of the 1960s did not support the acceptance of the computer metaphor within the psychological community either. Hilary Putnam's articles on the status of psychological predicates, and on the relevance of Turing's work for a better understanding of the relation between the mind and the brain, became quickly influential among philosophers. In particular, Putnam's work explained a crucial weakness of mind–brain identity theories that had been quite widespread during the 1950s. Putnam argued for a distinction between mind and brain in terms of the difference between software and hardware, thus showing that mental states can be realized in quite different physical systems (Putnam, 1960, 1967a, 1967b, all reprinted in Putnam, 1975). Such an abstract argument could influence the philosophical debate, because it was restricted to a principled, ontological understanding of the mind–body relation. The new (computer) functionalism was also quickly seen as a good basis for the autonomy of psychology in relation to other sciences such as biology or neurophysiology. However, even this did not help the computer metaphor to become more popular within psychology. Although the metaphor was available, and although it had started to do some fruitful work within a different community, the psychological community remained reluctant or ignorant.

No Familiar Tools, No Acceptance

We take two institutions as case studies to demonstrate the part of the tools-to-theories heuristic which concerns acceptance: (a) the Center for Cognitive Studies at Harvard, and (b) Carnegie Mellon University. The former never came to fully embrace the new information-processing psychology. The latter did, but after a considerable delay.

George Miller, the cofounder of the Center at Harvard, was certainly a proponent of the new information-processing psychology. Given Miller's enthusiasm, one might expect the center, partially under Miller's leadership, to blossom into information-processing research. It never did. Looking at the *Annual Reports* of the center from 1963–1969, we found only a few symposia or papers dealing with computer simulation.

Although the center had a PDP—4C computer, and the reports anticipated the possibility of using it for cognitive simulation, as far as 1969 it never happened. The reports mention that the computer served to run experiments, to demonstrate the feasibility of computer research, and to draw visitors to the laboratory. However, difficulties involved with using the tool were considerable. The PDP saw 83 hours of use, on an average week in 1965–1966, but 56 of these were spent on debugging and maintenance. In the annual reports are several remarks of the type "It is difficult to program computers ... Getting a program to work may take months." They even turned out a 1966 technical report called "Programmanship, Or How to Be One-Up On a Computer Without Actually Ripping Out Its Wires."

What might have kept the Harvard computer from becoming a metaphor of the mind was that the researchers could not integrate this tool into their everyday laboratory routine. The tool turned out to be a steady source of frustration. Simon (1979) took notice of this:

> Perhaps the most important factors that impeded the diffusion of the new ideas, however, were the unfamiliarity of psychologists with computers and the unavailability on most campuses of machines and associated software (list processing programming languages) that were well adapted to cognitive simulation. The 1958 RAND Summer Workshop, mentioned earlier, and similar workshops held in 1962 and 1963, did a good deal to solve the first problem for the 50 or 60 psychologists who participated in them; but workshop members often returned to their home campuses to find their local computing facilities ill-adapted to their needs. (p. 365)

At Carnegie Mellon, Newell, Simon, a new information processing-enthusiastic department head, and a very large National Institute of

Mental Health grant were pushing "the new [information-processing] religion" (H. A. Simon, personal communication, June 22, 1994). Even this concerted effort failed to proselytize the majority of researchers within their own department. This again indicates that entrenchment of the new tool into the everyday practice was an important precondition for the spread of the metaphor of the mind as a computer.

Acceptance of Theory Follows Familiarity With Tool

In the late 1950s, at Carnegie Mellon, the first doctoral theses involving computer simulation of cognitive processes were being written (H. A. Simon, personal communication, June 22, 1994). However, this was not representative of the national state of affairs. In the mid-1960s, a small number of psychological laboratories were built around computers, including Carnegie Mellon, Harvard, Michigan, Indiana, MIT, and Stanford (Aaronson, Grupsmith, & Aaronson, 1976, p. 130). As indicated by the funding history of National Institute of Mental Health grants for cognitive research, the amount of computer-using research tripled over the next decade: In 1967, only 15% of the grants being funded had budget items related to computers (e.g., programmer salaries, hardware, supplies); by 1975, this figure had increased to 46%. The late 1960s saw a turn toward mainframe computers, which lasted until the late 1970s, when the microcomputer started its invasion of the laboratory. In the 1978 Behavioral Research Methods & Instrumentation conference, microcomputers were the issue of the day (Castellan, 1981, p. 93). By 1984, the journal *Behavioral Research Methods & Instrumentation* appended the word *Computers* to its title to reflect the broad interest in the new tool. By 1980, the cost of computers had dropped an order of magnitude from what it was in 1970 (Castellan, 1981, 1991). During the last 20 years, computers have become the indispensable research tool of the psychologist.

Once the tool became entrenched into everyday laboratory routine, a broad acceptance of the view of the mind as a computer followed. In the early 1970s, information-processing psychology finally caught on at Carnegie Mellon University. Every Carnegie Mellon authored article in the 1973 edition of the Carnegie Symposium on Cognition mentions some sort of computer simulation. For the rest of the psychological community, who were not as familiar with the tool, the date of broad acceptance was years later. In 1979, Simon estimated that, from about 1973 to 1979, the number of active research scientists working in the information processing vein had "probably doubled or tripled."

This does not mean that the associated methodology became accepted as well. It clashed too strongly with the methodological ritual that was institutionalized during the 1940s and 1950s in experimental psychology. We use the term *ritual* here for the mechanical practice of a curious mishmash between Fisher's and Neyman–Pearson's statistical techniques that was taught to psychologists as the sine qua non of scientific method. Most psychologists assumed, as the textbooks have told them, that there is only one way to do good science. However, their own heroes—Fechner, Wundt, Pavlov, Köhler, Bartlett, Piaget, Skinner, and Luce, to name a few—never had used this ritual, but some had used experimental practices that resembled the newly proposed methods used to study the mind as computer.

Pragmatics

Some have objected to this analysis of how tools turned into theories of mind. They argue that the tool-to-theories examples are merely illustrations of psychologists being quick to realize that the mathematical structure of a tool (e.g., ANOVA, or the digital computer) is precisely that of the mind.

This repeats a simplistic version of realism we have already criticized (see section II). Now, we can add that the assumption that new theories just happen to mirror the mathematical structure of the tool overlooks the important pragmatics of a tool's use (which is independent of the mathematical structure). The same process of projecting pragmatic aspects of a tool's use into a theory can be shown for the view of the mind as a computer. One example is Levelt's (1989) model of speaking. The basic unit in Levelt's model, which he calls the *processing component*, corresponds to the computer programmer's concept of a subroutine. The model borrowed not only the subroutine as a tool but also the pragmatics of how subroutines are constructed.

A subroutine (or *subprocess*) is a group of computer instructions, usually serving a specific function, which is separated from the main routine of a computer program. It is common for subroutines to perform often-needed functions, such as extracting a cube root or rounding a number. There is a major pragmatic issue involved in writing subroutines that centers around what is called the *principle of isolation* (Simon & Newell, 1986). The issue is whether subroutines should be black boxes. According to the principle of isolation, the internal workings of the subroutine should remain a mystery to the main program, and the outside program should remain a mystery to the subroutine. Subroutines built without respect to the principle of isolation are *clear boxes* that can be penetrated from the outside and escaped from the inside. To the computer, of course, it makes no difference whether the subroutines are isolated or not. Subroutines that are not isolated work just as well as those that are. The only difference is a psychological one. Subroutines that violate the principle of isolation are, from a person's point of view, harder to read, write, and debug. For this reason, introductory texts on computer programming stress the principle of isolation as the essence of good programming style.

The principle of isolation—a pragmatic rule of using subroutines—has a central place in Levelt's model, where the processing components are "black boxes" and constitute what Levelt considers to be a definition of Fodor's notion of *informational encapsulation* (Levelt, 1989, p. 15). In this way, Levelt's psychological model embodies a maxim of good computer programming methodology: the principle of isolation. That this pragmatic feature of the tool shaped a theory of speaking is not an evaluation of the quality of the theory. In fact, this pragmatic feature of the subroutine has not always served the model well: Kita (1993) and Levinson (1992) have attacked Levelt's model at its Achilles heel—its insistence on isolation.

Limitations and Possibilities of Current Research Programs

The computer metaphor has been so successful that many find it hard to see how the mind could be anything else: to quote Philip Johnson-Laird (1983), "The computer is the last metaphor; it need never be supplanted" (p. 10). Such a stunningly realistic attitude interpretation overlooks that the computer metaphor, as every metaphor, has some important limitations. They can be inferred from two main discrepancies: First, human minds are much better at certain tasks than even the most developed computer programs and robots; second, digital computers are much better at certain tasks than human minds. Although human minds are still much better in, say, pattern recognition, the understanding of emotion and expressions, or in the learning of fast intentional bodily movement (as in sports), computer programs succeed in complex arithmetical calculations (e.g., Churchland, 1995, chap. 9). The important task is to understand why the differences obtain.

Alan Turing predicted in 1945 that computers will one day play very good chess; and others have hoped that chess programming would contribute to the understanding of how humans think. Turin's prediction turned out to be correct, as shown by the famous defeat of world chess champion Garry Kasparov against the IBM computer program Deep Blue in 1997. The other hopes did not turn out to be correct, and this signals one of the limitations of the computer metaphor.

Consider the different heuristics chess computers and human beings use. Both have to use heuristics, because there is no way to fully compute all possible moves in order to figure out the best strategy to win a given game. Both need to pursue intermediate goals that offer some probability of leading to success if repeatedly achieved. The heuristics computers and human beings use are different because they work on different capacities. Deep Blue has the enormous power to go through 200 million operations every second and uses a relatively simple heuristic to compute how good each of these moves is. Human chess experts do not generate all these possible moves but use the capacity of spatial pattern recognition, which is unmatched by any existing computer program. Kasparov once said that he thinks only 4 or 5 moves ahead, whereas Deep Blue can look ahead about 14 turns. Also, Herbert Simon has tried to take the opposite direction of Turing's suggestion, that is, Simon and his colleagues interviewed human chess experts in order to extract their heuristics and then implement them on chess computers. These programs did not play very well, however. The heuristics used in computer programs and in human minds are not identical.

The current alternative to the digital computer is *connectionism*, or models of parallel distributed processing. These have various advantages over traditional computer models, and they have important applications within artificial intelligence research. However, as models of the mind they are not without limitations either. For instance, connectionist researchers have been unable to replicate so far the nervous system of the simplest living things, such as the worm *Caenorhabdis elegans*, which has 302 neurons, even though the patterns of interconnections are perfectly well known (Thomas & Lockery, 2000; White, Southgate, Thomson, & Brenner, 1986). We should not adopt, certainly not by now, a realistic interpretation of the computer and connectionist models of the mind.

Other objections have been advanced against the program of artificial intelligence, but we are skeptical about these. For instance, John Searle has advanced the argument that computer programs do not, and cannot, realize true mentality. His argument is that they merely perform syntactical operations upon symbols, whereas real minds additionally possess a semantic understanding of symbols and symbolic operations (Searle, 1984). Most critical in this argument is the unquestioned assumption of a certain theory of meaning or intentionality. And there are other skeptical arguments. They concern the question of whether, say, computer algorithms can ever reveal the full mathematical capacities of human beings, or whether computers or artificial neural networks possess the phenomenal or qualitative features that accompany many mental states, such as perceptions or feelings. What connects these objections is that they are based on unquestioned intuitions about human minds and computers or artificial neural networks

(Churchland, 1995, chap. 9). To find out real differences between about minds and computers (or artificial neural networks), we should not rely on mere intuitions but instead try to empirically identify the various heuristics used by them. Moreover, we must show how human (and other living) minds differ not only in their functional architecture but also in their physical architecture from computers and connectionistic networks. We must work out how the software depends on the hardware.

V. THE GENERATION OF THEORIES RECONSIDERED

The tools-to-theories heuristic is about scientists' practice, that is, the analytical and physical tools used in the conduct of empirical research. This practice has a long tradition of neglect. The very philosophers who called themselves logical empiricists had, ironically, no interest in the empirical practice of scientists. Against their reduction of observation to pointer reading, Kuhn (1970) has emphasized the theory-ladenness of observation. Referring to perceptual experiments and Gestalt switches, he wrote, "scientists see new and different things when looking with familiar instruments in places they have looked before" (p. 111). Both the logical empiricists and Kuhn were highly influential on psychology (see Toulmin & Leary, 1985), but neither view has emphasized the role of tools and experimental conduct. Only recently have they been scrutinized more closely, both in the history of psychology and generally in the history and philosophy of science as well (Danziger, 1985, 1987, 1990; Galison, 1987; Hacking, 1983; Lenoir, 1986, 1988). Without being able to discuss such analyses here, it can be pointed out that they have made it highly plausible that theory is often inseparable from instrumental practices.

Should we go on telling our students that new theories originate from new data? If only because "little is known about how theories come to be created," as Anderson introduces reader to his *Cognitive Psychology* (1980, p. 17)? On one widespread view, theories are simply "guesses guided by the unscientific" (Popper, 1935/1959, p. 278). Against this, we wish to emphasize that in order to understand the generation of theories appropriately, the familiar theory–data relation should be supplemented by a third factor: the use(s) of tools. Moreover, it cannot be overemphasized that some guesses are better than others from the very beginning. Even when rational evaluation of theories has not been achieved, the question of which theories are plausible and serious candidates must have its own rationale. The tools-to-theories heuristic is one possible answer, even if the metaphorical use of tools requires continuous critical reflection.

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