the nature of intention and purpose, gain new urgency because of inventions like the computer.

I see the invention of cognitive science as a wonderful stimulus for philosophy, on the one hand, and philosophy as an indispensable handmaiden for the empirical scientists, on the other. Philosophy enables us to define fundamental cognitive scientific questions in a coherent way, and assures the proper integration of work in disparate fields. But, by the same token, philosophy must attend assiduously to empirical findings in order to avoid becoming a barren discipline or one irrelevant to scientific work. It is thus fitting that the field of philosophy, whose initial agenda helped to stimulate the rise of cognitive science, has been fueled by that new discipline, even as philosophy can, in turn, help to inform and interpret work spawned by its recent intellectual offspring.

Hilary Putnam, a veteran of many of these discussions, has reflected on the role of philosophy in the contemporary scientific scene. His prudent comments take seriously the various critiques of "grand" philosophy, while recognizing the important role philosophy should continue to play in discussions of new scientific endeavors:

> I have not attempted . . . to put forward any grand view of the nature of philosophy; nor do I have any such grand view to put forth if I would. It will be obvious that I do not agree with those who see philosophy as the history of "howlers" and progress in philosophy as the debunking of howlers. It will also be obvious that I do not agree with those who see philosophy as the enterprise of putting forward *a priori* truths about the real world. . . . I see philosophy as a field which has certain central questions, for example, the relation between thought and reality. . . . It seems obvious that in dealing with these questions philosophers have formulated rival research programs, that they have put forward general hypotheses, and that philosophers within each major research program have modified their hypotheses by trial and error, even if they sometimes refuse to admit that that is what they are doing. To that extent philosophy is a "science." To argue about whether philosophy is a science in any more serious sense seems to me to be hardly a useful occupation. . . . It does not seem to me important to decide whether science is philosophy or philosophy is science as long as one has a conception of both that makes both essential to a responsible view of the real world and of man's place in it. (Putnam 1975*a*, p. *xvii*)

Gardner, The Mind's New Science: A history of the cognitive revolution

5

Psychology: The Wedding of Methods to Substance

Three Pivotal Lines of Research from the 1950s

George Miller's Magic Number 7

In 1956, George Miller published, in the *Psychological Review*, an artfully presented essay—"The Magical Number Seven, Plus or Minus Two: Some Limits on Our Capacity for Processing Information." More of a synthesis than a report of a crucial experiment or a presentation of a formal theory, Miller's opening statement was backed by considerable empirical evidence: "My problem is that I have been persecuted by an integer. For seven years this number has followed me around, has intruded in my most private data, and has assaulted me from the pages of our most public journals" (p. 81). Miller showed that the individual's ability to make absolute distinctions among stimuli, to distinguish phonemes from one another, to estimate numbers accurately, and to remember a number of discrete items all seemed to undergo a crucial change at about the level of seven items. Below that number, individuals could readily handle such tasks: above it, individuals were likely to fail. Nor did this discontinuity seem accidental. In Miller's view:

> There seems to be some limitation built into us either by learning or by the design of our nervous systems, a limit that keeps our channel capacities in this general range. On the basis of the present evidence it seems safe to say that we possess a finite and rather small capacity for making such . . . [judgments] and that this capacity does not vary a great deal from one simple sensory attribute to another. (P. 86)

Humans, however—as Miller reassured his audience—have ways of getting around these constraints. Processing or coding entities in terms of their various dimensions can enlarge the number of elements that can be distinguished from one another. One can chunk, or group together, a number of elements (for example, a set of numbers or letters) and then treat the assemblage as a unit. One can make relative rather than absolute judgments. The capacity to recode information into language and to remember this more abstract symbolism is especially important. As Miller notes, "This kind of linguistic recoding that people do, seems to me to be the very lifeblood of the thought processes" (1956, p. 95). Indeed, he once described the potential efficiency of this recoding process in this manner, "To use a rather farfetched analogy, it is as if we had to carry all our money in a purse that could contain only seven coins. It doesn't matter to the purse, however, whether these coins are pennies or silver dollars" (Miller, Galanter, and Pribram 1960, p. 132).

Why did this apparently simple point have a decidedly major impact within cognitively oriented communities? First Miller's essay brought together a large amount of hitherto dispersed data and suggested that they pointed to a common conclusion. A valuable synthesis, to begin with. Second, it suggested that the number 7 was no mere accident: it designated genuine limitations in human information-processing capacities. While such "built-in" limitations might be anathema to radical empiricists, they helped signal a shift toward exploring the nature and structure of a central cognitive processing mechanism. The point about strict processing limits, not coincidentally, was made in terms of the theory of information, which Miller explained early in the paper: thus, he introduced a method whereby researchers could examine other sensory modalities or tasks and ascertain whether this apparent limitation in fact obtains. (Much ensuing controversy centered on how to make such translations and whether, when made, they did in fact yield the magic number 7.) Third, as indicated, the message in the paper was not without hope, for Miller indicated ways by which humans ingeniously transcend this limitation.

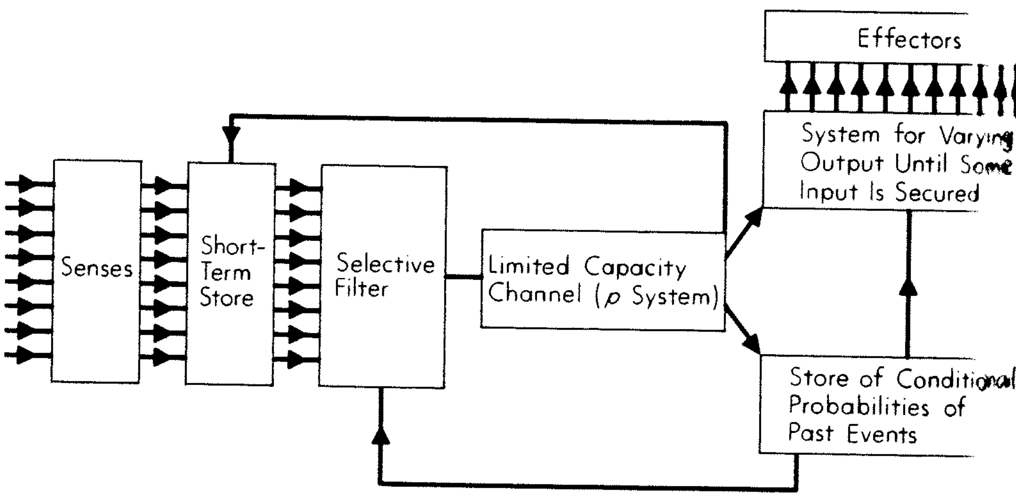
There may have been another reason for the impact of this essay. Psychologists had been trying for approximately a century to discover the basic laws of the human mental system. Many promising avenues had been launched, but most of them—including, most recently, the behaviorist approach—had eventually foundered. In recent years, the most exciting new work in the human sciences had come from two connected areas: information theory, which posited principles of transmission applicable to any kind of channel; and computer science, which now featured machines engaged in symbol manipulation. Miller was holding out hope of marriage between the quantities of data collected by psychologists over the years and the rigorous new approaches of the engineering-oriented scientists. The result might be a genuine science of psychology with its own set of immutable laws. No one thought to question whether all contents, or bits, can in fact be treated (and then counted) as equivalent.

The British Approach to the Processing of Information

Just as Miller and his colleagues were applying concepts from communication science to psychology, a parallel movement was getting under way in Great Britain. This movement grew directly out of the applied psychological work that had been carried out in Britain during the Second World War when psychologists joined other scientists in efforts to crack enemy codes, understand night vision, plan air-raid alerts, assist in spotting of enemy aircraft, and meet sundry other needs of war. Two men who had been involved in this applied effort were Colin Cherry and Donald Broadbent; their studies in the 1950s inspired the British approach to information-processing psychology.

A follower of information theory, Cherry (1953) focused on the capacities of individuals to attend to and obtain information from noisy channels. He instructed subjects to follow a message, delivered to one ear, by the method of shadowing: that is, by repeating each word as soon after its initial presentation as possible. Cherry found subjects unable to report much of what had come into the opposite (unattended) ear. More precisely, they could report gross characteristics of the signal, such as whether it was music or speech, but not shifts of content or tongue. Broadbent (1954) refined this procedure by presenting sets of digits simultaneously, in strings of three, to both ears. He found that subjects had the easiest time and achieved the highest score when they reported all the digits presented to one ear first, and then all the digits presented (at the same time) to the other ear.

For our purposes, the important part of the Broadbent-Cherry work is the model of human thought processes to which it gave rise. The model conformed to the tradition of British empiricism. It began with information taken in from the senses, but focused on a new and important feature: that the individual has a limited capacity for the intake *and* storage of information. (In this stress on limits of information processing, the model was intimately linked to George Miller's studies of the magic number 7.) There was an important added twist: rather than simply speaking of structural limits in a static way, the British researchers sought to determine precisely what *happens* to this information from the moment one first apprehends it. Given this "engineering" approach, it became a natural step to draw a flow chart of what happens, as the perceptual system operates upon new information. In fact, according to a recent textbook, Broadbent was the first psychologist in modern times to describe cognitive functioning with a flow chart (Lachman, Lachman, and Butterfield 1979, p. 188).



One of the first information-processing diagrams.

SOURCE: From D. E. Broadbent, *Perception and Communication* (Elmsford, N.Y.: Pergamon Press, 1958). Reproduced by permission.

What was this early flow chart like? It featured information coming in through the senses, being placed in a short-term store, and then being selectively filtered before entering into a limited-capacity perceptual system. While a sense organ can take in a lot of information in parallel and retain it momentarily, the job of the selective filter is to block unwanted messages, thereby letting in only those that merit additional analysis. A further property of the selective filter is that it can be so tuned that, at any one time, it allows in only those messages that fulfill certain requirements. The buffer can hold unanalyzed information briefly and thus allows one to report the contents of the second ear, after having spewed out the three digits apprehended by the first ear. According to Broadbent's early model, only information that becomes conscious—that passes through the limit-

ed capacity channel—can enter into long-term memory and thus become part of active knowledge. Information present on an unattended channel, or in an unattended signal, is assumed to decay in a few seconds and to receive no processing beyond the initial "pre-attentive" analysis.

While few, if any, investigators believed that perception or thinking takes place simultaneously or without a series of steps, the option of tracing the stages of information processing had rarely been followed before Broadbent's time. Again, this option became a probability when communication engineering began to impinge on the perceptual and attentional issues that had long interested psychologists. But the model of "flow charting" put forth by Broadbent and his associates and their evidence relevant to specific stages of information processing opened up many productive possibilities. One could now examine the temporal dimensions of diverse psychological processes, and avid experimenters lost little time in pursuing just that course. The lack of attention to the particular *content* being processed, or to the *kinds* of transformation imposed, did not trouble those excited by the Broadbent-Cherry demonstrations.

Jerome Bruner's Strategic Approach

With the collaboration of Jacqueline Goodnow and George Austin, Jerome Bruner published in 1956 a book called *A Study of Thinking*. This volume grew out of the Cognition Project, which Bruner had been directing for several years at Harvard. The subject, well known to psychologists, was classification, categorization, or (as it was commonly called in the trade) concept formation or concept acquisition. And the problem was a classic one: How does a person, confronted with a set of elements, come to group them together reliably into categories—be they all chairs, all atoms, or all large blue triangles?

As psychologists, Bruner and his colleagues followed the tradition of examining abstract forms of categorization, such as those involved in figuring out which of a set of cards, each featuring a different geometric form, belongs to a particular category. The experimenter would target a concept—say, the class of all cards with one red figure, the class of all cards featuring red squares, or, when being particularly diabolical, the class of all cards containing two figures and/or circles. The subject was exposed to one card at a time, asked in each case whether that card belonged to the preordained concept, and then told whether his response was correct. The subject's task, of course, was to figure out the properties of the targeted concept, so as to be able to select all, and only, those cards that exhibited its defining features.

While superficially similar to work carried out in years past, Bruner's approach actually diverged from that undertaken by earlier students of categorization. First of all, rather than treating subjects as deaf and mute animals, Bruner and his colleagues simply told the subjects what to do and relied heavily on their comments as an aid in analyzing the results. Flying in the face of established behaviorist methodology, the subjects were treated as active, constructive problem solvers, rather than as simple reactors to the stimuli presented to them. Their introspections actually mattered.

Reflecting the information-theoretical winds of the day, Bruner and his associates had begun by looking at the bits of information assimilated in subjects' encounters with such simple stimuli. But in another departure from standard operating procedures, they ended up analyzing the informational properties of long sequences of acts called "strategies." It had turned out that the best way to account for individual performances was in terms of these overall patterns of responding over many trials rather than of particular responses to a particular stimulus configuration. This was the most iconoclastic—and the most influential—aspect of the work.

The researchers went on to consider what each strategy accomplished in light of the goals of the organism, such as minimizing risk or conserving storage capacity. Singled out were the strategy of *successive scanning,* where the subject has a single hypothesis (like all red objects) and limits his choices to those instances that directly test this hypothesis; the approach of *conservative focusing,* where one finds a positive instance and then makes a sequence of choices, each of which alters but a single attribute value of the first "focus" card and tests whether the change yields a positive or a negative instance; and the tack of *focus gambling,* where the subject uses a positive instance as a focus but then takes the calculated risk of changing more than one attribute at a time. Focus gambling offers the possibility of attaining a concept far more rapidly than does conservative focusing, but also may necessitate extra trials if one's choices happen to be unrevealing. Conservative focusing is the most foolproof method since it limits the burden on memory and allows steady progress toward a solution. However, in case of a time limit or some other pressure, the respondent may adopt the riskier course of changing several attributes simultaneously.

Perhaps not surprisingly for a work published in the pivotal year of 1956, the Bruner book presented itself as an innovation in psychology. The authors comment:

> the past few years have witnessed a notable increase in interest in and investigation of the cognitive processes. . . . It has resulted from a recognition of the complex processes that mediate between the classical "stimuli" and "responses" out of which stimulus-response learning theories hoped to fashion a psychology that would by-pass anything smacking of the "mental." The impeccable peripheralism of such theories could not last. . . . One might do well to have a closer look at these intervening "cognitive maps." (P. *vii*)

The importance of Bruner's book was signaled by praise from the distinguished physicist J. Robert Oppenheimer: "*A Study of Thinking* has in many ways the flavor of the opening of a new science. . . . The book has a unity of view and a fervor of conviction which makes it point to the future" (quoted in Bruner 1983, p. 121). The possibility that the use of such artificial concepts might invalidate the findings was far from anyone's mind at that time.

The Program of Cognitive Psychology

The lines of research inaugurated by Miller, Broadbent and Cherry, and Bruner, energized the psychology of the late 1950s and the 1960s. In the face of the artificially tough strictures that had been imposed by behaviorism on issues of cognition, these young psychologists were willing to introduce notions that had long been ruled "out of court." Talk of built-in limitations to the amount of information that could be taken in, attempts to trace the steps involved in processing such information, and positing of overall strategies employed to solve a problem—all of these signaled a greater willingness to discuss issues of mind directly, without attempting to explain them away in terms of long series of publicly verifiable stimuli and responses.

Such a change cannot be attributed to a single factor, but it is clear that the advent of the computer, as well as the information-theoretical language by which it was commonly characterized, helped to legitimize such approaches. No longer were psychologists restricted in their explanatory accounts to events that could either be imposed on a subject or observed in one's behavior; psychologists were now willing to consider the representation of information within the mind. To be sure, this willingness to deal with mental representation took different forms in the writings of different psychologists. Miller looked at the structural properties and limitations built into the representational system; Broadbent and Cherry examined the transformations on the information as it came in from the senses and was stored in memory; Bruner attributed to subjects a variety of approaches or strategies which governed performance on a task. While the issues being probed—memory for isolated units, processing of words or tones, sorting of concepts—were scarcely new to psychology, the prospect of applying concepts from information theory, of building on the model of a computer, of countenancing various forms of mental representation, and of allowing subjects to use their full reflective powers was bracing and freeing.

Psychology is a discipline central to any study of cognition. Yet it is also a difficult discipline to pursue and one where genuine progress has been hard to achieve. Nearly every conceivable element is relevant to a subject's performance, and few issues having to do with human nature and behavior can be excluded from the laboratory *a priori.* Thus, choosing a problem, and screening out all competing ones, becomes an especially vexing task.

Psychology also poses special problems for the historian of cognitive science—a problem in no way minimized when the historian is also a psychologist. It is an enormous field—there are many more psychologists than there are representatives of the other fields—and there are consequently more programs of research to survey. While it is oversimplification to organize any field around one or two themes, it is especially difficult to select key issues in psychology. Should one, thus, pay attention to the particular content of information (auditory or visual, musical or linguistic) or instead treat all contents as if they were interchangeable? Does one approach research in order to illuminate those processes that are true of all individuals, or look instead at pertinent individual differences—child versus adult, male versus female, naïve versus trained in experimental tasks? Does one examine behavior in its natural context or try to strip away all everyday accouterments and resort to artificial laboratory conditions? Does one assume that the individual approaches tasks by building up larger elements of meaning from small, isolated units? Or does it make more sense to assume that one comes to tasks with general strategies or scripts, which one simply imposes upon a task, irrespective of its particular dimensions, details, and demands?

I have elected to organize this chapter in terms of a distinction that touches upon some of the aforementioned ones but is perhaps better phrased in somewhat different terms: that is, the distinction between *molecular* or small-scale units of analysis and *molar* or large-scale units of analysis. For reasons of scientific strategy or simple personal preference, it seems possible to classify most psychological research programs along this dimension. Some programs, such as those of traditional psychophysics and contemporary information processing, show a penchant for small-scale units (bits, individual percepts, single associations examined in brief periods of time) on the assumption that a thorough understanding of these elementary units and processes is the surest path toward the ultimate explanation of complex units and entities. A contrasting faith is found among proponents of the molar approach—those who look at large-scale problems tackled over a long period of time and invoke analytic concepts

like schemas, frames, or strategies. According to these researchers, these large-scale properties are most salient in human cognition and thus serve as a logical point of departure. Why gamble that an elementaristic approach will eventually yield larger units, when one has the option of *beginning* instead with these larger units, which seem closer to the data and the experiences of everyday life?

The contrast between molecular and molar approaches resembles, but is by no means identical to, the distinction between the *top-down* and the *bottom-up* approaches. In a top-down approach, which has rationalist overtones, the subject is assumed to bring to the task his own schemes, strategies, or frames which strongly color his performance. In a bottom-up approach, more allied to the empiricist camp, the actual details of a focal task or situation are assumed to exert primary influence on a subject's performance. In what follows, I shall often identify *molar* with "top-down" and *molecular* with "bottom-up"—not because each is logically bound to the other but because they often, and perhaps typically, occur together.

Like all dichotomies, this one is easily exaggerated, with subsequent distortion of the field. Nearly all psychologists have some sympathy for each tack, and many move from a molecular to a molar approach (and back again). For example, George Miller favored a molecular hat when pondering the number 7 but readily shifted to a molar one when discussing plans and goals in the 1960 volume. Indeed, when the computer is used as a model, it is equally justifiable to focus on the most molecular level (individual bits, symbols, "on-off" circuits) or the most high-level programming concepts (goals, means, and routines). Also, one can embrace a molecular (or a molar) approach for different reasons: some psychologists begin with a molecular approach in the hope of being able to adapt their methods to molar entities; while others believe that ultimately all behavior can be reduced to, and explained by, molecular entities. Thus, in embracing this dichotomy, I seek to convey an ongoing tension or struggle for the soul of psychology—not to label two bins into which one can readily and reliably sort all experiments, concepts, and psychologists.

Two other trends must be mentioned as well in any thumbnail sketch of psychology's first one hundred years. The first trend is the increasing splintering of the field. The American Psychological Association alone has over fifty thousand members (including several thousand active researchers) who spread themselves over forty divisions and several hundred special interest groups, many of which are completely ignorant of what is going on elsewhere in their association and their discipline. In this climate, efforts to find unifying concepts are vital but by no means easy to sustain.

The second is the trend toward methodological perfection. With the