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A. Newell
H. A. Simon

HISTORICAL ADDENDUM

In the text we have limited ourselves to references required by scholarly obligation. This is not the place to undertake a full-scale history of the developments leading up to the work reported in the book--nor are we the ones to undertake it. However, it seems appropriate to describe some of the antecedents of this work--and particularly its less obvious connections with other streams of research--in somewhat greater detail than we allowed ourselves in the text. The following notes, then, are to be taken as a somewhat personal view of history.

The Second World War provides a convenient boundary between pre-history and history. The pre-history, in turn, involves two main strands: one in psychology, and one in formal logic. The post-World-War-II history requires particular attention to the whole collection of ideas and activities sometimes assembled under the label of cybernetics, as well as to the exploitation of these ideas in artificial intelligence and in cognitive psychology.

PRE-HISTORY

Cognitive Psychology Before 1945

On the American side of the Atlantic Ocean, there was a great gap in research on human complex cognitive processes from the time of William James almost down to the Second World War. Although the gap was not complete, it is fair to say that American cognitive psychology during this period was dominated by behaviorism, the nonsense syllable, and the rat. Hull's doctoral thesis (1920) on concept formation was a notable exception, but his desertion of this problem for others more compatible with the Zeitgeist is typical of the period. Not only that, it was widely interpreted as showing the futility of a direct

approach to higher mental processes. In A Study of Thinking (1956), the only American psychologists mentioned more than casually for the pre-war work are Egon Brunswik (to whom the book is dedicated), Hull, and Tolman. The references to Hull are to his thesis, leaving Brunswik and Tolman as the real forerunners of the work, so far as American psychology is concerned.*

Of the leading American psychologists of this period, Tolman was the farthest from the dominant S-R position (excepting only those Gestalt psychologists who migrated from Europe). His relevance to the subsequent development of information processing psychology is clearly suggested by the titles of his principal book, Purposive Behavior in Animals and Men (1932),and of the best-known of his papers with Brunswik, "The Organism and the Causal Texture of the Environment" (1935). He treated man (and rat) as a goal-seeking, hence decision-making, organism, whose behavior was molded by the structure of the environment in which he sought to achieve his aims. Brunswik developed this same point of view by characterizing the structure of the environment in probabilistic terms--terms that would make natural the subsequent application of the concepts of information theory and game theory.

European psychology, less preoccupied than American with methodological rigor and more willing to use data from verbal protocols, had a more active record, during this period, of concern with the cognitive processes exhibited in complex behavior. From a longer list of names, we cannot omit mention, at least, of Bartlett in England, and Selz, Duncker, Wertheimer, Piaget, and de Groot on the continent.

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* One of us (H. A. Simon) whose publication began before World War II, acknowledged James and Tolman as the principal sources of his psychological conceptions (Administrative Behavior, (1947), Chapter 5).

The specific trademark of Bartlett's Remembering (1932), allying his work with the Gestalt viewpoint, was its concern for the way in which information was represented "inside the head," and how it was modified by the processes that stored it there and retrieved it. A similar concern for central representations, and an even greater emphasis on central processes characterizes the Würzburg School in general, and the work of Selz and his followers in particular.

The applications of this point of view to problem solving, in particular by Wertheimer and Duncker, were hardly known in the United States until after World War II.* When post-war work on thinking and problem solving is examined, however, and the connections traced through concepts and citations, the lineages are clear. Our own work and de Groot's (which began before the War) owes large debts to Selz, while the work of Maier, Katona, Guetzkow, and Luchins, among others, develops the lines opened by the Gestaltists.

Apart from Tolman and Brunswik, we must mention one other viewpoint in pre-war American psychology that departs from the position of radical behaviorism. It is the "standard" viewpoint of physiological psychologists, which for our purposes is well expressed in the words of Boring: (The Physical Dimension of Consciousness (1933), preface to Dover edition, pp. vi-vii).

> A fact is a relation, and the simple basic fact in psychology is a correlation of a dependent variable upon an independent one. Ernst Mach made this point and B. F. Skinner took it up

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* Similarly, before the War, Piaget's work was familiar to some American educational psychologists; hardly at all to American experimental psychologists. "Familiar" is perhaps not quite the right term, since there existed no way at that time for bringing that work, even when known, into any kind of relevant conceptual relation to the main stream of American behaviorism.

> about the time this book was being written. He created the concept of 'empty organism' (my phrase, not his), a system of correlation between stimulus and response with nothing (no C. N. S., the 'Conceptual Nervous System'--his phrase, not mine) in between. This book does not go along with Skinner whose 'empty organism' is made of 'empty correlations,' but rather argues that these correlations are early steps in scientific discovery and need to be filled--for the inquiring mind dislikes action at a distance, discontinuities that remain 'unexplained.' Thus my text undertook to assess the amount of neurological filling available in 1932--how much fact there was ready to relieve the psychophysiological vacuum--and it sought to formulate the requirements that more filling would have to meet.

We invite the reader's attention to Boring's last sentence, which contains the important implicit assumption separating the psychophysiological viewpoint (typified also by Lashley, for example) <u>both</u> from behaviorism and from the research we have mentioned as forerunner to our own. The assumption has two parts:

(1) The "empty organism" is to be filled with explanatory mechanisms, an assumption that is accepted by all except the radical behaviorists. But also:

(2) The explanatory mechanisms are to be neurological, an assumption that is not accepted by the psychologists whose views we have associated with ours.

Let us be entirely clear as to what the latter denial means. It is not a denial of in-principle reductionism--instead, it is an assertion that, by virtue of the complexity of the phenomena, the reduction of stimulus-response

regularities to neural processes will have to be carried out in several successive steps, not just a single step. It is a denial modeled on the successful practice of physics and chemistry--and recently, of biochemistry and molecular biology--that accepts, in principle, the reduction of the most complex events to the laws of quantum physics, but carries out the reduction by erecting a number of layers of theory between gross biological phenomena and the sub-microscopic events of the atomic nucleus.

The implicit acceptance of the second part of Boring's assumption by virtually all American psychologists who were not behaviorists (excepting, again, those with views allied to Tolman's) erroneously identified explanation of stumulus-response correlations with neurophysiology. An important post-war example of this confusion was Hebb's insistence, in The Organization of Behavior, that his theory was to be regarded as physiological (Hebb (1949), pp. xvi-xix).

Since the information processing theories discussed in this book represent a specific layer of explanation lying between behavior, on the one side, and neurology on the other, it is to be expected that we would resonate most strongly with those of our predecessors who admitted explanatory constructs of this kind. Whether these forerunners would be pleased to be regarded as "information processing theorists," or whether they would accept our operationalization of their vague (in our eyes) concepts are questions we do not feel called upon to answer. In this note we are acknowledging our intellectual debts, not representing our creditors.

The Influence of Formal Logic

The rise of modern formal logic, a product of the twentieth century--and, more generally, the spectacular development of abstract mathematics--has

greatly enhanced the ability of a science to state its theories with rigor and precision. To build a successful scientific theory we must know what to say, and we must have a language in which we can express what we know. The greatest weakness of the antecedents of information processing psychology was the lack of a language to make its concepts clear and operational. Advances in knowledge that began with the formalization of logic by Peano, Frege and Whitehead and Russell at about the turn of the century have provided that language.

Since the relation of formal logic to information processing theories is often misunderstood, we must expand our discussion a bit. Both logicians and psychologists agree nowadays that formal logic is not to be confused with human thinking. From the logician's viewpoint, they should not be confused because logical inference has objective, formal, standards of validity which, if they must be given concrete existence at all, surely exist in Plato's heaven of ideas and not in human heads. We need not argue whether formalization of logic allows us to squeeze the last drop of intuition out of the act of inference; enough is squeezed out to make inference something quite different from a thought process.

From the psychologist's point of view, thinking must not be confused with logic because human thinking frequently is not rigorous or correct, does not follow the path of step-by-step deduction--in short, is not usually "logical."

If the logicians' and the psychologists' objections to confusing logic with thought are valid--and we agree that they are--what can be the possible contribution of formal logic to information processing psychology? The fundamental contribution was to demonstrate by example that the manipulation of

symbols (at least some manipulation of some symbols) could be described in terms of specific, concrete processes quite as readily as could the manipulation of pine boards in a carpenter shop. The formalization of logic showed that symbols can be copied, compared, rearranged, concatenated, with just as much definiteness of process as boards can be sawed, planed, measured, glued.

The persistence of concern with the mind-body problem can be attributed in part to the apparent radical incongruity and incommensurability of "ideas"--the material of thought--with the tangible biological substances of the nervous system. Formal logic, if it showed nothing else, showed that ideas--at least some ideas--could be represented by symbols, and that these symbols could be altered in meaningful ways by precisely-defined processes. Even a metaphorical use of the similarities between logical manipulation of symbols, on the one hand, and thinking, on the other, had a profound liberating influence upon modes of conceptualizing thought processes, problem solving processes, decision making processes. One of us (Simon, 1944) used this metaphor explicitly as the framework for his research on administrative decision making, and can testify to its crucial importance in the development of his own theorizing:

> Any rational decision may be viewed as a conclusion reached from certain premises. . . The behavior of a rational person can be controlled, therefore, if the value and factual premises upon which he bases his decisions are specified for him (1944, p. 19).

To make this new operationality of symbols and their processing fully viable for psychology, required seeing that "symbol manipulation" could refer to a much wider range of phenomena than simple deductive logic. The beginnings

of this last, and crucial, generalization began to be visible at about the time of the Second World War, though it was not really completed until the modern computer had been fully developed.

Parallel to, and related to, the growth of formal logic was the progress that took place within economics in formal theories of rational decision making--the decision making of "economic man." Because economic man was patently too rational, by human standards, his relevance for psychology was disputed or ignored. Nevertheless, economics was more and more explicitly concerning itself with <u>reasoning</u> about courses of action. The fact that it dealt only with reasoning that was logical, deductive, and correct postponed the recognition by economics and psychology of their common concerns. As matters turned out, it did not, ultimately, prevent that recognition.

The rational man of economic theory who began to emerge before the War was a close relative of the rational man of statistical decision theory--they grew up, in fact, in the same household. Statistics had, in turn, close historical links with philosophy, through concern with the foundations of probability and with the classical problem of induction. These links provided another tie between the emerging decision theory in economics and statistics, on the one hand, and the development of formal logic, on the other.*

One specific development in logic prior to World War II requires special mention. In their striving to handle symbols rigorously and objectively--as

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* Readers familiar with contemporary statistics will recognize that we have used "decision theory" in a more generic sense than is usual--including, say, the contributions of F. A. Fisher and Neyman under this rubric as well as the later ones of Wald and the neo-Bayesans.

objects--logicians became more and more explicit in describing the processing system that was supposed to manipulate the symbols. In 1936, Alan Turing, an English logician, described the processor, now known as a Turing machine, that is regarded as the culmination of this drive toward formalization. At about the same time, Carnap, Church, Curry and Post were introducing formalizations of the syntax of logic that subsequently provided models for the notions of "formal grammar" employed by modern linguistics.

Finally, we should mention a gradually growing belief that mathematics had a role to play in constructing theories of biological, psychological, and social phenomena parallel to the role it had earlier played in the physical sciences. Lotka's Elements of Physical Biology, published in 1924, represented this point of view and foreshadowed some of the central concepts of cybernetics. Nicholas Reshevsky carried this kind of work forward in his theories of mathematical biophysics. Although there had been little contact of this work with psychology prior to the War, a number of psychologists (including Hull) were beginning to show strong interest in its potential.

What we have described is a powerful, growing Zeitgeist, having its origins around the turn of the century, involving a deep faith in mathematics as the language of science, but, more crucially, focussed upon symbols and their manipulation in logical inference (if not in "reasoning") and decision. Symbols became, for the first time, tangible--as tangible as wood or metal. The Turing machine was an all-purpose planer and lathe for symbols. The stage was set, when the Second World War began, for the penetration of this new Zeitgeist into psychology.

THE POST-WAR SETTING

In Chapter 1 we suggested that 1956 could be taken as a critical year for the development of information processing psychology. That date occurs about a decade after the end of the War. Let us sketch how matters stood at the mid-fifties by reference to a few events and publications that occurred in that period.

The Cybernetic Revolution

The developments we have been tracing came to public notice at the end of World War II under the general label of cybernetics, a term that Norman Wiener devised to embrace and characterize them (Wiener, 1948). Among the elements that Wiener grouped under this label were (1) information theory, (2) the theory of feedback systems (servomechanism theory, control theory), and (3) the electronic computer. In other countries, particularly behind the Iron Curtain, where it has gained considerable currency, the term "cybernetics" is used even more broadly, to encompass, in addition to the topics just listed: (4) game theory, (5) mathematical economics and statistical decision theory, and (6) management science and operations research.

Wiener has presented, in the first chapter of Cybernetics, his version of the history of these developments, to which the interested reader may refer; and others have also given their accounts. Without repeating details, we offer several comments:

1. The broad use of the term cybernetics in Europe provides further confirming evidence for the unity of the Zeitgeist we have described, and for the roles we have attributed not only to logic but also to mathematical economics and decision theory in that Zeitgeist.

2. From the beginning, cybernetics had a strong concern with physiology, and with biological feedback mechanisms; thus linking back strongly to the homeostasis concepts of Claude Bernard, Lotka, Cannon, and Henderson. The considerable numbers of physiologists already attracted toward those concepts rapidly became acquainted with the cybernetic developments in other fields.

3. The Second World War did not produce the cybernetic developments; it more likely delayed them slightly. In particular, the ties with formal logic were especially evident in the earliest cybernetic publications, preceding and during the early years of the War: Shannon's Master's thesis (1938), employing Boolean algebra for the analysis of switching circuits; the closely parallel Pitts-McCulloch paper (1943), providing a Boolean analysis of nerve networks; and the Rosenblueth-Wiener-Bigelow paper (1943) on "Behavior, Purpose, and Teleology."

4. Most of the figures prominent in these developments had early in their careers been deeply immersed in modern symbolic logic. Wiener had been a student of Russell's in Cambridge; much of von Neumann's work in the 20's and 30's was in logic; Shannon's and Pitts' and McCulloch's use of logic has already been mentioned.

5. Despite these intimate connections with logic, the computer as symbolic machine played a muted role in the early days of cybernetics. The main underpinnings were feedback (especially in continuous systems) and the information theory of Wiener and Shannon. Indeed as late as the early fifties booklength expositions of cybernetics hardly mentioned the digital computer, and when they did it was only as "the biggest mechanism" (Ashby, 1956; Guilbaud, 1959). Analog and digital computers were treated on a conceptual parity, each

taken to illustrate a form of encoding. And a perusal of Warren McCulloch's Summary of Points of Agreement at the end of the Macy Conference in Cybernetics (1955) makes a place for logic and for Turing machines, but none for symbolic mechanisms. These had to await the further development of programming.

It is well known, or at least widely believed, that work that too far anticipates its appropriate Zeitgeist is ignored, while work that is appropriate to the contemporary Zeitgeist is recognized promptly. von Neumann's contributions to game theory in the 20's were known to few persons before 1940, Lotka was read by a few biologists, a few logicians were aware of the rapid strides of logic and the esoteric discoveries of Godel, Turing, Church, and Post. All this changed in the early post-war years.*

Biology and the various sciences of Man did not long stay aloof from cybernetics. The feedback notions soon were being used, particularly by the physiologically inclined, and most enthusiastically in Great Britain (Ashby, 1952; Walter, 1953). The brain-computer metaphor suggested itself almost immediately--

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* One of us (H. A. Simon) can document from his own experience the dramatic change that took place during this period in the climate of ideas. When he first became interested (1935) in what he then called decision making and now calls information processing, he sensed vague affinities between his concerns and the lectures of Carnap on logic, Rashevsky on mathematical biophysics, and Schultz on mathematical economics and statistics. Through these teachers, he learned of Lotka, of recent developments in statistical decision theory, of Gödel--but not immediately of Church or Turing. He found a few other teachers and fellow-students who shared his vague sense of the Zeitgeist. His dissertation (published in 1947, with some revisions to take account of the publication of von Neumann and Morgenstern's book on game theory) reflected the intellectual climate about 1940-42. The invisible college operated with some efficiency, then as now, for news of the new contributions, published in widely scattered journals and books, spread with great rapidity. His attention was called to the work of Shannon, Pitts and McCulloch, the servomechanism theorists, von Neumann and Morgenstern, and Wiener either before publication or within a few months thereafter. Similarly, the decision-making approach of his dissertation rapidly became known to cyberneticians working in economics and operations research.

followed almost as immediately by warnings against taking the analogy seriously. The analogy, of course, that suggested itself initially, and against which these warnings were directed, was the analogy between the neurological organization of the brain and the wiring of the computer (von Neumann, 1956). Turing was one of the first to see that there was another level, the more abstract level of information processing, at which the analogy could be pursued more profitably. It is not at all surprising, in view of his conception in 1936 of the Turing machine, that he should have had this insight from the beginning.

Feedback concepts had considerable, but relatively unspecific, impact on psychology. But the influence of information theory was clear and precisely defined (Miller and Frick, 1949). Hick undertook to explicate and test a relation between response time and amount of information contained in the response, while others attempted to measure the information capacity of the human sensory and motor "channels."

Limits on the applicability of information theory to psychology gradually became clear. The high water mark had perhaps already been reached by 1955 (Quastler, 1955). Certainly by the time of Garner's monograph on Uncertainty and Structure (1962) information theory had receded to the status of a useful but specialized working tool for the analysis of variability.

The Situation Within Psychology

The Würzburg and Gestalt Schools. The European work on thought processes was just beginning to be made available to Americans through translation and migration. The translations of Duncker's On Problem Solving and Wertheimer's

Productive Thinking appeared in 1945; Humphrey, in Thinking (1951), provided the first extensive English-language discussion of Selz's research and theories; Katona's Organizing and Memorizing and a number of Maier's papers on problem solving had already appeared in 1940, but had been almost lost in the distractions of the War.

Concept Formation. The formalisms of information theory, statistical decision theory, and game theory had stimulated new interest in the classical research area of concept formation, and had suggested new research methods and theoretical concepts. Hovland's " 'Communication Analysis' of Concept Learning" was published in 1952; while George Miller's "Magic Number Seven" paper and the Bruner, Austin and Goodnow Study of Thinking both appeared in the critical year 1956.

Human Factors. The War had produced a vast increase of research in human skills and performance--an area often referred to as "human factors" research. Moreover, much of this work was concerned with the human members of complex man-machine systems: pilots, gunners, radar personnel. As a consequence, the researchers not only were led to an emphasis on psychological processes, but were in a position to observe the analogies between human information processing and the behaviors of servomechanisms and computers. Broadbent's "A Mechanical Model for Human Attention and Immediate Memory" (1954) is illustrative both of the main research emphases (e.g., attention and short-term memory), and of the kinds of theoretical concepts that began to emerge.

There was considerable overlap, in conceptions, personnel and communication between the research in human factors and in concept formation. These areas provided a bridge, also, between the main stream of psychology and the newly emerging field of computer science, and a major route through which concepts drawn from the latter field began to be legitimized in psychology.

Linguistics

A long-standing concern with formalization and methodology clarity within linguistics received its own new impetus with Zelig Harris, Methods in Structural Linguistics (1951). The work of Chomsky, which was to redirect much of linguistics from a concern with structure to a concern with processing (that is, with generative systems) was just beginning (Chomsky, 1955), although it did not have much impact until a few years later (Chomsky, 1957). It is not possible for us, as outsiders, to assess the extent to which these developments in linguistics reflected the larger picture we have sketched. Some connections with logic are clear (e.g., Chomsky, 1954). But the efforts in the mechanical translation of languages (MT), which might be thought to demonstrate the close interconnection between linguistics and the computer were made outside the main stream of linguistics (see Locke & Booth, 1955, for the early history).

Digital Computers

As we noted, the development of digital computers, was a prominent event of the early post-war era. Their general theoretical character as universal machines was clearly understood; but they were viewed as primarily mathematical machines, that is, as devices that worked with numbers, rather than symbols. Programming was still in a very rudimentary state. Although the first book on programming was published as early as 1951 (Wilkes, Wheeler and Gill) the development of programming languages other than assembly languages only started towards the end of the period in question; for example, FORTRAN was begun in 1954 (Backus, et al., 1957). Thus, the powerful experience of

working with a system that seemed to interpret symbols much as the user did, was not available in the early fifties.

There was already some work using digital computers to simulate cognitive processes. Indeed, the use of mechanisms (robots) to illustrate psychological theories and enforce operationality has a long history, predating the computer. In an article on "Mind and Mechanism," Boring (1946) presents the view with his customary pungency and provides access to the earlier work. And with the cybernetic developments there was an additional pulse of mechanical robot building (Walter, 1953; Ashby, 1952). But all of these efforts were rather separate from simulation on the computer, which tended not toward activating mechanical beasts, but toward programming game playing and other symbolic activities. The first checker program was coded in 1952 (Strachy), and other game playing schemes are described in Bowden (1953). And Turing (1950), in a justly famous discussion, had put the problem of simulation in a highly sophisticated form.

THE CRITICAL YEARS: 1954-1958

We now turn from consideration of the general Zeitgeist in the early fifties to the events directly responsible for the theory in this book. The narrative necessarily becomes narrower in scope and somewhat more personal.

In late 1954 one of us (A. Newell) initiated in earnest an effort to program a digital computer to learn to play good chess. The general context was the Systems Research Laboratory at RAND (later to metamorphize into the System Development Corporation), engaged in simulating an environment for an Air Defense radar center, in order to understand its effectiveness (Chapman, et al.,

1959). Thus the environment was a conceptual conglomerate of all the pieces we have been listing. But the proximate cause was the work of Selfridge and Dineen (reported in 1955), which seemed to make clear how computers were to become truly non-numerical processors.

In the first published description of this research effort, Newell (1955) said:

> "This is the means to understanding more about the kinds of computers, mechanisms, and programs that are necessary to handle ultracomplicated problems."

The research undertaking was thus identified as "artificial intelligence" (though that name was not used); but the task to be examined was one of great psychological interest which had already been studied extensively by de Groot. The initial approach also established the precedents, followed in all of the subsequent work of our project, that artificial intelligence was to borrow ideas from psychology and psychology from artificial intelligence. Thus, Newell's programmatic description of his chess learning problem proposed to use aspiration values and notions "of satisfactory solution" in evaluating chess moves, and discussed the necessity for "rules of thumb" (later called "heuristics") to chop down the enormous problem space to manageable size. Always following this course, the research pursued prior to 1957, although it had a heavy artificial intelligence emphasis, was carried on with one eye to its potential for psychology.

By the beginning of 1955, J. C. Shaw and H. A. Simon had associated themselves with Newell. Cliff Shaw had been concerned with the programming systems on JOHNNIAC, the computer built at RAND in the early fifties. Simon's

early concerns have already been noted. His focus on limited rationality, satisficing and selective search at the juncture under discussion is summarized in Simon (1955, 1956). Considerable attention was being given to the programming language requirements for the project, and the relative advantages were being weighed of continuing with chess as the initial problem solving task as against first designing a theorem-proving scheme in geometry or logic. For several reasons, the latter tasks appeared to be somewhat simpler for an initial effort, and attention gradually shifted to them.

The work on programming language design moved in parallel with the substantive investigations through the Autumn. By late October, proving theorems in the propositional calculus (within the system of Whitehead and Russell) had been selected as the first target, and on December 15, 1955, the first successful hand simulation was carried out of the proof of a theorem in Principia Mathematica (Theorem *2.15), using essentially the methods and flow diagram that were incorporated in the Logic Theorist.

The formalization of this scheme, described in IPL-I (Information Processing Language I), was published in September, 1956, as has already been remarked in the text. It was announced, in less detail, in the unpublished RAND Report P-850, "Current Developments in Complex Information Processing (May 1, 1956), a paper read by Newell in a series of Lectures on Computers and Automation, in Washington, D. C., May 2, 1956.

Programming Languages

The history of the programming languages employed in this research has been reported elsewhere (Newell, Tonge, Feigenbaum, Green and Mealy, pp. xx-xxiv; 1964

see also Knuth (1968), vol. 1, pp. 456-463). The first versions of LT actually run on a computer were programmed in IPL-II, and run on the JOHNNIAC computer at the RAND Corporation. The first complete machine proof of a theorem of Principia Mathematica (Theorem *2.01) was produced on August 9, 1956.

The suitability of the information processing languages for writing programs expressing psychological theories was not entirely accidental. They were designed, first and foremost, to handle complexity and unpredictable complexity. In arriving at the designs, every effort was made to take advantage of features of human memory thought to contribute to its flexibility--and, specifically, to exploit its "associative" organization. This cue from psychology was no doubt one important hint that pointed toward the list processing ideas.

The Dartmouth Summer Seminar

A research project in artificial intelligence had been organized at Dartmouth for June, 1956, by John McCarthy, Marvin Minsky, Nat Rochester and Claude Shannon. Attendants at the seminar included a large fraction of all persons who were working actively in artificial intelligence at that time: McCarthy, Minsky, Rochester, Oliver Selfridge, Shannon, Ray Solomonoff, Trenchard Moore, Herbert Gelernter, Newell and Simon. Many ideas for programs that would perform problem solving, pattern recognizing, or game playing tasks were in the air, but the two concrete new schemes brought to the conference were the Logic Theorist and a program for proving theorems in the propositional calculus making use of the method of natural deduction, devised by Trenchard Moore (1957). Moore had constructed a flow diagram, and hand simulated his scheme. The description of the Logic Theorist, later presented in September in Cambridge,

was distribured to the group, and early debugging outputs were exhibited.

Marvin Minsky's well-known essay, Steps Toward Artificial Intelligence, although not published until several years later, was first drafted and circulated as a technical report late in 1956. It reflects very well the general body of knowledge in artificial intelligence that was pooled at the Dartmouth conference, and subsequently was itself an important document in organizing and codifying that knowledge.

Prior to the seminar, several persons and groups (including Minsky and Newell and Simon) had given some attention to proving theorems in geometry. This was an important topic of conversation at Dartmouth, giving impetus to the successful work of Gelernter and Rochester on this problem, which followed shortly thereafter. At the conference, Minsky prepared two sets of mimeographed notes on the geometry problem.

Human Problem Solving

The research up through the successful completion of LT, in 1956, involved much more borrowing of ideas from psychology and human behavior to advance artificial intelligence than reverse borrowing from artificial intelligence to advance psychology. Psychology was very much on the research group's mind, however. The first file memorandum sketching a possible approach to EPAM (Elementary Perceiving and Memorizing Program) is dated February 18, 1956. The notion of applying information processing ideas to human behavior in classical verbal learning tasks apparently was suggested while searching the standard psychological literature for clues to improved organizations of associative memories. E. J. Gibson's classical paper on stimulus and response

generalization (Gibson, 1940) was perhaps the most important single source of the ideas that E. Feigenbaum subsequently developed into the EPAM program.

With respect to problem solving, however, serious attempts to interpret LT as a psychological theory got under way in the autumn of 1956. We had learned, meanwhile, that O. K. Moore and S. B. Anderson had used problems in logic theorem proving (disguised as "decoding" exercises) in their research on problem solving at Yale University (Moore and Anderson, 1954a and b). Their formal system was close enough to that of Whitehead and Russell to suggest using their task as a basis for comparing human behavior with the behavior of LT. With that end, Peter Houts, a graduate student at Carnegie Institute of Technology, began to make tape recordings of thinking-aloud sessions with subjects performing the Moore-Anderson logic task.

The first thinking-aloud tapes were transcribed in the spring of 1957, and their analysis was begun with a view to defining a computer program that would parallel, hence explain, the human behavior exhibited in them. It soon became clear that the LT organization did not fit the detail of human behavior at all well, and that the latter showed evidences of what we would now call the means-ends method.

Progress toward a new program was accelerated in discussion of the problem and of the transcriptions of the tape recordings with the members of a research seminar on organizational behavior that met on the Carnegie campus in the summer of 1957. During the summer and autumn, the analysis of the protocols gradually converged toward the first versions of the program that was christened the General Problem Solver. The general flow diagram of GPS was produced before

the end of October, 1957, and the planning method was sketched a few days later (Newell, Shaw and Simon, 1962).

The first published attempt to draw implications from the theorem proving programs for the psychology of problem solving was the paper, "Elements of a Theory of Human Problem Solving," in the Psychological Review for July, 1958. Written more than a year earlier, this paper was based on the experience with LT, hence emphasized the broad resemblances between the heuristic problem solving program and characteristics of human problem solving without attempting detailed comparisons of behavior. The "Elements" paper was organized around the thesis that: "an explanation of an observed behavior of the organism is provided by a program of primitive information processes that generates this behavior." The methodological assumptions of the information processing approach to psychological theory were explored, and the relation of information processing theories of thinking to neurological, associationist, and Gestalt explanations discussed at some length. The "Elements" paper was thus the first explicit and deliberate exposition of the position now known as "information processing psychology"--although, in fact, that precise phrase was not used in the paper.

GPS (including the planning method) was first publicly described (though its name was not used), in "The Processes of Creative Thinking," a paper read on May 14, 1958, at a University of Colorado symposium and again during June at the RAND Summer Seminar discussed below, but published only in 1962. This paper contained the first fragment of informal comparison of computer trace (hand simulated) with human thinking-aloud protocol.

Chess

Work on a chess program, which had been nearly dormant since 1955, was resumed toward the end of 1957. Meanwhile, discovery of de Groot's work, Het Dencken van den Schaker (1946) led to contact and collaboration with the author and his colleagues in Amsterdam that has continued down to the present. A history of computer chess programs, and a description of the NSS program, which became operational in 1958, can be found in "Chess-playing Programs and the Problem of Complexity" (1958, particularly pp. 322-331), and in Chapter 11 above, which also reviews the histories of psychological research on chess as a problem solving task and of chess-playing computer programs.

The RAND Summer Seminar

By the Spring of 1958, extensive experiments had been carried out with LT, GPS had been conceived and hand simulated, the NSS chess program had joined its several predecessors as an operative program, the EPAM program was in process of construction, and the gathering and analysis of human protocol data were well under way. Research was also progressing at a lively pace at M.I.T. (Minsky and McCarthy), but with emphasis upon artificial intelligence rather than psychology. Apart from the RAND-Carnegie group and the M.I.T. group, however, there were no other large-scale programs of research on problem solving employing computer simulation as a main technique. Moreover, the communication was still weak because these efforts and the other main lines of development in information processing psychology, already mentioned. Among the most important of these were: (1) the burgeoning field of psycholinguistics, whose leading representative within psychology was George A. Miller of Harvard, (2) work in areas like concept formation, deriving from information-theoretic

there were no limits to human rationality administrative theory would be barren. It would consist of the single precept: "Always select that alternative, among those available, which will lead to the most complete achievement of your goals," (Simon, 1947, p. 240). If one substitutes "psychology" for "administrative theory" one has an adequate statement of the problem addressed in Chapter 3. Yet this theme does not figure prominently in the papers on computer simulation, either ours or others. Nor, to the best of our recollection, was it raised by the critics of simulation. In our own formulations, the theme returns again in full force in The Sciences of the Artificial (Simon, 1969).

A final element of the theory on which we may comment is the notion of symbolic system, as it is set forth in Chapter 2. From formal logic, information processing inherited an emphasis upon syntax--upon games with arbitrarily defined rules, played with symbol tokens. The meanings of symbols--what they denote--play a muted role in this scheme. When we reintroduce the relation of the information processing system with its environment, with the real world, the reference of symbols again becomes a central concern. Even without external reference, complex processes operating on complex memory structures require addressing schemes so that one symbol can denote another.

Semantics, the denotative aspect of symbol systems, has re-emerged as a central concern in information processing, and one reflection of this re-emergence is the emphasis on the relation of denoting in our discussion of such systems.

Milton somewhere spoke of "the never-ending flight of future days." We have here reached the point where past joins future--a proper point for an historical addendum to end.