

## **AYN RAND AND THE COGNITIVE REVOLUTION IN PSYCHOLOGY**

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Ayn Rand's philosophical epistemology would not have been the same without certain changes that took place in American academic psychology in the mid-twentieth century. In this essay, I will explore how Rand's philosophical thought drew on the developments in the study of perception, attention, memory, concept-formation, thinking, and problem solving that have come to be known as the Cognitive Revolution. The impact of the Cognitive Revolution on Rand is demonstrable despite her limited knowledge of psychology, her marked distrust of the discipline, and her declaration that philosophy in no way depends on psychological theories or findings. Rand's epistemology did not arise in complete isolation from the psychology of her time. Recognizing the relationship between the two may enable Objectivists to have more of an impact on cognitive psychology—and cognitive psychology to have more of an impact on Objectivism.

### **The Cognitive Revolution**

The Cognitive Revolution massively altered the direction of American academic psychology, particularly in the "cognitive" specialties of memory, reasoning, problem solving, categorization, decision making, and language. From 1930 to 1950, the American psychology of perception and learning had been solidly under behavioristic dominion.<sup>1</sup> Behaviorists asserted that psychology could qualify as a science only by restricting itself to the study of observable behavior. Implementations of behaviorism showed some variation. At one extreme lay outright denials of the existence of minds or mental processes (for instance, the doctrines of John B. Watson and Burrhus Frederic Skinner). A more moderate interpretation (like the once highly influential theory of Clark L. Hull) used supposedly non-mentalistic "intervening variables" (miniature stimuli and responses inside the organism) to explain behavior and learning. At the other

extreme, Edward Chace Tolman's conceptions, which appealed to "purposive behavior" and posited "cognitive maps" to explain how rats run mazes, look to us today like cognitive psychology struggling to burst out of a straitjacket. By the early 1950's, however, Hullian theory and other moderate behaviorist conceptions had lost their appeal because of repeated failures to predict the outcomes of laboratory learning experiments, and the radical behaviorism of B. F. Skinner was actually displacing them.

During the heyday of behaviorism, some types of cognitive study were carried forward by the German refugees of the Gestalt school, or occasionally by other figures outside the mainstream. Other research specialties were allowed to continue after making obeisance to behaviorist strictures: sensation and perception research carried on as "discriminative responding to stimuli," and traditional memory studies were relabeled "verbal learning" (relating words on lists, as stimuli, to words that the subject wrote down later, as responses). Under these circumstances, researchers could expend only a fraction of the effort that would normally have gone into cognitive studies.

Several intellectual innovations in the years after World War II enabled psychology to get its mind back. The four main strands of the Cognitive Revolution are usually taken to be information theory, linguistics, computer science, and human factors research. Significantly, as Baars (1986) points out, only human factors research (the study of human beings' interactions with complex machinery, such as airplanes and, later on, computers) originated within academic psychology. Information theory—a mathematical model of information load and channel capacity—was developed by communications engineers working for Bell Laboratories. Linguistics was the formal study of language structure, a discipline that traditionally had tenuous and disputed connections with psychology. Computer science originated as a mathematical specialty and had emerged by the early 1950's as a rapidly developing science and engineering discipline. Some of the most prominent innovators during the Cognitive Revolution—Noam Chomsky (a linguist), Allen Newell (a computer scientist), and Herbert Simon (an economist who studied decision making)—were not trained in psychology at all. The critical need for ideas from outside the discipline indicates just how deep a rut academic psychology had dug for itself.

Although most accounts emphasize the four main strands, it is clear that developments in neurology (mentioned, for instance, by Bruner 1961) made a lesser contribution to the Cognitive Revolution. Still underestimated (in the accounts with which I am familiar) is the degree of

carryover from pre-behavioristic (thus, allegedly prescientific) experimental psychology. Bruner, Goodnow, and Austin (1956) acknowledged that their research on concept-attainment marked a revival of the interest in "higher mental functions" that prevailed in American and European psychology before World War I. The problem-solving research that underwent a major expansion in the Cognitive Revolution often asked subjects to "think aloud" while solving problems (Newell and Simon 1972). Though not so labeled, thinking aloud is really the nonanalytic style of introspection used in the experiments of the Würzburg School *circa* 1905 (as can be seen from the discussion of Würzburg School research in Boring 1950). The notion of *Einstellung* (a stereotyped mode of solving a problem, acquired as a habit after solving several problems that all require the same type of solution, and maladaptive on problems requiring a different approach) is still in use today; it, too, was inherited from the Würzburg School and handed down by Gestaltist researchers.

Of course, we do not know what would have happened had there never been a behaviorist takeover in American psychology. Cognitive research would not have been interrupted, and the rate of progress would have been greater, perhaps much greater. Still it is hard to believe that computer science would not have had an impact on psychology; claims that an abstract computer could carry out any "effective procedure" (Turing 1936), or that computers could exhibit "intelligent behavior" (Turing 1950) were too significant to pass up. It seems likely, too, that information theory would still have brought about a more focused appreciation of limited cognitive capacity. I doubt, however, that we would be viewing these changes as revolutionary had there not been behaviorism to revolt against. In fact, most participants in the Cognitive Revolution, as the interviews in Baars 1986 make clear, had less than revolutionary attitudes about science. While no longer willing to rule out explanatory recourse to mental processes, most shared with the behaviorists a positivistic desire to stick close to empirical data and to keep their theorizing on a short leash.

### Points of Contact with the Cognitive Revolution

#### Limited Capacity

It may not be evident from the foregoing what Ayn Rand shared with the Cognitive Revolution, besides an interest in human knowing and thinking. Rand's *Introduction to Objectivist Epistemology* (1967) is a treatise about the problem of universals. Much of what the book

discusses—abstraction, definitions, hierarchies of concepts, nominalism, Platonic or Aristotelian essentialism—has been the material of philosophic discourse for centuries. Indeed, Rand declared (in Binswanger and Peikoff 1990, 307) that she had sketched her theory in the late 1940's, after being challenged by a Thomist to account for the "manness" in men and the "roseness" in roses. Such an effort to account for concepts without reliance on indwelling essences ("realism") or reduction to arbitrary verbal labels (nominalism) could have been mounted in the 1910's, or the 1860's, if not earlier, though the determination to overcome dichotomies is characteristically Randian (Sciabarra 1995). One prominent emphasis, however, firmly plants Rand's work in the 1960's.

The seventh of the eight chapters in Rand's monograph is titled: "The cognitive role of concepts." It opens with neither a critique of other philosophical positions, nor with an appeal to introspection, but with an account of an experiment in comparative psychology.

The story of the following experiment was told in a university classroom by a professor of psychology. I cannot vouch for the validity of the specific numerical conclusions drawn from it, since I could not check it first-hand. But I shall cite it here, because it is the most illuminating way to illustrate a certain fundamental aspect of consciousness—of any consciousness, animal or human.

The experiment was conducted to ascertain the extent of the ability of birds to deal with numbers. A hidden observer watched the behavior of a flock of crows gathered in a clearing in the woods. When a man came into the clearing and went on into the woods, the crows hid in the tree tops and would not come out until he returned and left the way he had come. When three men went into the woods and only two returned, the crows would not come out: they waited until the third one had left. But when five men went into the woods and only four returned, the crows came out of hiding. Apparently, their power of discrimination did not extend beyond three units—and their perceptual-mathematical ability consisted of a sequence such as: one-two-three-many. (Rand 1967, 57)

This particular cognitive ability (the ability to perceive small numbers of objects directly, without counting) is known to psychologists as *subitizing*. There is evidence that, by 6 months of age, human infants also distinguish one, two, three, and many, though they may have yet to learn that one, two, and three form a sequence (Starkey and Cooper 1980; Cooper 1984). In the 1950's, subitizing was already known from studies with human adults, and from some studies with other species. More importantly, it came to be seen as one of many instances of strict capacity limits on immediate cognitive processing in human beings. One of the foremost publications of the cognitive revolution was George Miller's 1956 article on the "magic number 7 plus or minus 2." Miller gathered together the results of empirical research on a wide array of topics, such as digit span (the number of digits that a person could correctly reproduce immediately after learning a string of them), absolute judgment (for instance, how many musical tones a person without perfect pitch could distinguish at one time), and, of course, subitizing. He concluded that common limits on capacity for information, varying within a fairly small numerical range, were operating in all of these cases.

There is a direct line of development from Miller's article to the cognitive models of the 1960's that distinguished short-term memory from long-term memory (Atkinson and Shiffrin 1968) and on to the models (in wide use today) that consolidated short-term remembering with attention and at least some aspects of conscious awareness into a limited-capacity working memory (Baddeley 1986). The emphasis on limited capacity is highly characteristic of post-Cognitive Revolution psychology and marks it off clearly from pre-behaviorist conceptions of human thinking.

A powerful inspiration for Miller's work has, in the meantime, dropped away.<sup>2</sup> Though information theory had great heuristic value for psychologists in the 1950's, it proved to be a dead end because the content of human knowledge and the meanings involved in human communication cannot be measured in binary choices, or bits, as communications engineers were able to do with signals transmitted over telephone lines. All the same, Miller (1956), with his multiple assessments of limited capacity, or Wendell Garner (1962), with his view of concepts as means of "information reduction," would have found Rand's conception of unit-economy strikingly familiar:

the range of man's *perceptual* ability may be greater, but not much greater, than that of the crow: we may grasp

and hold five or six units at the most. This fact is the best illustration of the cognitive role of concepts. Since consciousness is a specific faculty, it has a specific nature or identity and, therefore, its range is limited: it cannot perceive everything at once; since awareness, on all its levels, requires an active process, it cannot do everything at once. Whether the units with which one deals are percepts or concepts, the range of what man can hold in the focus of his conscious awareness at any given moment, is limited. The essence, therefore, of man's incomparable cognitive power is the ability to reduce a vast amount of information to a minimal number of units—which is the task performed by his conceptual faculty. And the principle of *unit-economy* is one of that faculty's essential guiding principles. (Rand 1967, 57-58)

And this is no coincidence. Nathaniel Branden netted out to Rand what one of his psychology professors had said about Miller's work; the crow study was presumably one of the illustrations that the professor used. "I remember telling A.R. about Miller's work and she leapt on the implications immediately" (Branden, personal communication, 17 October 1996).

The "crow epistemology," as Rand called it in conversation (in Binswanger and Peikoff 1990, 172-73), is still a mainstay in all presentations of her theory of knowledge (Peikoff 1993, 107-8). The psychological contribution—specifically, the impact that George Miller had on Rand's thinking—has gone unacknowledged.

There is a good deal more that could be said by way of comparison between Rand's account of concept formation and the "concept-attainment" studies that loomed so large in the 1950's and 1960's (Bruner, Goodnow, and Austin 1956; Bruner 1961; Garner 1962; 1974). Rand thought dimensionally, like Garner (1974), avoiding the reduction of dimensions to atomic "features" that so many cognitive psychologists found appealing (Selfridge 1959; Neisser 1967). Her concern with the contextuality of definitions and her insistence on the fundamentality of defining characteristics anticipated "psychological essentialism," which did not enter cognitive psychology until the early 1980's. It was not Rand, whose work was largely unknown to academic psychologists, who brought such concerns into cognitive psychology, but Hilary Putnam (1975).

Rand's conception of first-level concepts as being at an intermediate level of generality anticipated Eleanor Rosch's work on "basic categories," which entered the literature nearly a decade later (Rosch et al. 1976). It will take a full analysis and critique of the *Introduction to Objectivist Epistemology* from a psychological standpoint to trace all of these connections.

### Sensory Deprivation

A second point of contact with the Cognitive Revolution is Rand's 1966 Ford Hall Forum talk, "Our cultural value-deprivation." This essay opens with an account of the sensory deprivation experiments that were carried out at McGill University in Montréal from 1951 to 1954. These experiments would never have amounted to much had they adhered to behaviorist strictures, which would have required strictly avoiding introspection and using only behavioral dependent variables. The participants' complaints of disorientation, hallucinations, and trouble telling the difference between sleeping and waking were indispensable sources of data.

Rand went beyond the obvious conclusion that "the experiments seem to indicate that man's consciousness requires constant activity, a constant stream of changing sensory stimuli, and that monotony or insufficient stimulation impairs its efficiency" (1966, 4:1). She quoted Jerome Bruner, a cognitive psychologist who was an active participant in several phases of the Cognitive Revolution, to support the analogy she was drawing between sensory deprivation and value-deprivation: "One may suggest that one of the prime sources of anxiety is a state in which one's conception or perception of the environment with which one must deal does not 'fit' or predict that environment in a manner that makes action possible" (Bruner 1961, 206). Once again, it was Nathaniel Branden (personal communication, 29 March 1999) who brought the sensory deprivation work to her attention.

Rand drew a characteristic epistemological conclusion from these findings: "One valuable aspect of the sensory-deprivation experiments is that they call attention to and draw attention to a fact that neither laymen *nor* psychologists are willing fully to accept: the fact that man's consciousness possesses a specific nature with specific *cognitive* needs, that it is *not* infinitely malleable and cannot be twisted, like a piece of putty, to fit any private evasions or any public 'conditioning'" (1966, 4:2).

The essay, unfortunately, does not live up to the promise of its

opening pages. The analogy that Rand wished to draw between sensory deprivation (as it was imposed in these studies) and value deprivation required a virtually total absence of positive moral exemplars or inspiring art in the milieu. What drives the remainder of the essay is a determination to find irrationality, emptiness, and depravity throughout American culture. While the political machinations of Lyndon B. Johnson and the esthetic judgments of New York literary critics were genuine occasions for disgust, Rand overlooked many instances of positive creativity in her surroundings. In just one artistic region—the American music known as jazz—the decade leading up to the publication of Rand's essay saw the appearance of such complex, challenging, and emotionally fulfilling works as: "Love, Gloom, Cash, Love" (Herbie Nichols, 1957); "Haitian Fight Song" (Charles Mingus, 1957), "Ancient Aethopia" (Sun Ra, 1958); "Ramblin'" (Ornette Coleman, 1959), "Better Get It in Your Soul" (Charles Mingus, 1959); "So What" (Miles Davis, 1959); "Giant Steps" (John Coltrane, 1959); "Stormy Weather" (Charles Mingus with Eric Dolphy, 1960); "Jesús Maria" (Jimmy Giuffre, 1961); "Somewhere in Space" (Sun Ra, 1962); "Alabama" (John Coltrane, 1963); "The Black Saint and the Sinner Lady" (Charles Mingus, 1963); "Hat and Beard" (Eric Dolphy, 1964); "Malagueña" (Pete La Roca with Joe Henderson, 1965); "Toothsome Threesome" (Elmo Hope, 1966); "Dancing Shadows" (Sun Ra, 1966); and "Isfahan" (Duke Ellington, 1966). The value deprivation, then, was a good deal less than total.

#### Linguistics and Language Learning

Rand's third point of contact was with the linguistic strand of the Cognitive Revolution. Indeed, she found significant areas of agreement with the leading linguist of the time. In 1972, Rand undertook a response to B. F. Skinner's notorious new book *Beyond Freedom and Dignity*. In this book, Skinner maintained that his efforts in the rat and pigeon laboratory had proven the universal validity of his conception of learning as operant behavior and reinforcement, and generated a "technology of behavior" that could and should be applied to human beings. Only the pre-scientific superstitions preserved in the "literatures of freedom and dignity" blocked the socially beneficial implementation of behavior-control technology. To Rand, Skinner's book represented academic psychology at its absolute worst. She scanned the media for reviews that would be appropriately scathing—scientifically, epistemologically, and morally—and found few. While most reviewers rejected Skinner's overt agenda of politically motivated behavior control, they were all too willing to concede

rationality and science to a radical behaviorist.

But one essay, in the *New York Review of Books*, stood out from the rest: "After a collection of this kind, it is a relief to read. . . . The essay is neither apologetic nor sentimental. It is bright and forceful. It is a demolition job. What it demolishes is Mr. Skinner's scientific pretensions—and, to this extent, it is a defense of science. . . ." (1972, 10:3-4).

The author of the essay was Noam Chomsky (1971), one of the architects of the Cognitive Revolution. Rand applauded Chomsky for taking Skinner's doctrine of operant behavior and reinforcement and turning it on Skinner's decision to write and publish his book. Chomsky concluded that Skinner's own theory could make no sense of Skinner's writing *Beyond Freedom and Dignity*, of anyone else's reading it, indeed of any attempt at persuasion whatsoever. What sort of "reinforcer" is reading Skinner's book? What sort of operant behavior does it increase or decrease the probability of? The behavior that led up to reading the book, no matter what that was? "The reviewer employs one of the best methods of dealing with a false theory: he takes it literally" (Rand 1972, 10:4).

In the end, Rand drew too little encouragement from Chomsky's classic essay. "There are many other notable passages in that review. But its author is Noam Chomsky who, philosophically, is a Cartesian linguist advocating a theory to the effect that man's mental processes are determined by innate ideas—and who, politically, belongs to the New Left" (1972, 10:4).

Rand's response to Skinner makes it painfully clear that while drawing on some of the ideas that moved the Cognitive Revolution, she was not really aware of what it had wrought. From her standpoint, it was not Chomsky's utter demolition of Skinner that mattered; it was the shallow reviews in the mass media and the misguided reactions in the psychoanalytic journals. In her pessimistic conclusion, cognitive psychology had no prospects; no one would displace Skinner and Freud: "There are no defenders of man's *mind*—in the world's greatest scientific-technological civilization. All that is left is a battle between the mystics of spirit and the mystics of muscle—between men guided by their *feelings* and men guided by their *reflexes*" (Rand 1972, 10:4).

What Rand almost certainly did not know is that Noam Chomsky had *already* been instrumental in the overthrow of behaviorism.<sup>3</sup> Unlike so many psychologists or computer scientists, who quietly moved away

from behaviorism while refraining from direct clashes with its proponents, Chomsky mounted a frontal attack on Skinner in 1959. He published a devastating review of a book titled *Verbal Behavior*, in which Skinner strove to account for language use and language learning on radical behaviorist grounds. Two of Chomsky's arguments were pivotal at the time and remain classics in the evolution of psychology.

First, Chomsky noted that, from a Skinnerian perspective, children would not learn how to speak grammatically unless their parents constantly trained them or "shaped" them to produce correct sentence structures. Yet, the evidence is overwhelming that parents rarely try to correct the syntax of toddlers and young children.

Second, Chomsky argued that Skinner's theory, which treated every sentence that a person utters as a distinct operant in need of subsequent reinforcement, could never explain how speakers of any language can say grammatically correct sentences that they have never said before (in some cases, that no one has ever said before). The refutation was so overwhelming that Skinner never replied to it; in fact, no Skinnerian published a reply for 11 years, and by then it was too late (Harris 1993). Skinner never published another book about language—in fact, though he continued to write for another quarter century, he never published another academic book.

While Chomsky's politics are quite apparent in his 1971 essay (for instance, he attacks "differential reward" in the form of higher wages for some than others), his already notorious appeal to innate ideas is nearly undetectable:

But what does it mean to say that some sentence of English that I have never heard or produced belongs to my "repertoire," but not any sentence of Chinese (so that the former has a higher "probability")? Skinnerians, at this point in the discussion, appeal to "similarity" or "generalization," but always without characterizing precisely the ways in which a new sentence is "similar" to familiar examples or "generalized" from them. The reason for this failure is simple. So far as is known, the relevant properties can be expressed only by the use of abstract theories (for example, a grammar) describing postulated internal states of the organism, and such theories are excluded, a priori, from Skinner's "science." (Chomsky 1971, 20)

The part of Chomsky's thinking that has proven troublesome for cognitive psychology nearly sneaks by here, in the apparently innocent suggestion that a grammar of a language is the kind of "abstract theory" that would correctly describe what is going on in the mind of a person who speaks that language.<sup>4</sup> The push for innate ideas comes from the difficulty of explaining how a child could learn the rules of a linguist's grammar of the language. Rand never came to grips with this flaw in Chomsky's thinking, or with its epistemological consequences.

While Rand interpreted *Beyond Freedom and Dignity* as proof that the world was headed toward hell in a hand basket, Chomsky knew full well that Skinner was making grandiose assertions and couching them in shocking language precisely because he was losing the war of ideas: "By comparing the results that have been achieved [since Skinner set out to study 'verbal behavior'] with the claims that are still advanced, we gain a good insight into the nature of Skinner's science of behavior. My impression is, in fact, that the claims are becoming more extreme and more strident as the inability to support them and the reasons for this failure become increasingly obvious" (21).

### Points of Divergence

#### Limited Interest in Computationalism

Rand drew on Miller's conception of limited cognitive capacity, and on sensory deprivation research. She found Chomsky's refutation of Skinnerian behaviorism congenial. Meanwhile, other aspects of the Cognitive Revolution barely touched her thinking.

Rand showed little interest in the computer-science strand of the revolution. She never referred to Alan Turing's (1936; 1950) claims that a computer could carry out any "effective procedure." There is no commentary in her writings on computer simulation of human thought processes (Newell, Shaw, and Simon 1958; Newell and Simon 1972), or the ambitious research programs in Artificial Intelligence which were already well under way by the mid-1960's.

All the same, both Rand and Branden likened the subconscious mind to a computer in their treatment of emotions. "The subconscious operates as a store of past knowledge, observations and conclusions (it is obviously impossible for man to keep all of his knowledge in focal awareness); and it operates, *in effect*, as an electronic computer, performing super-rapid integrations of sensory and ideational material. Thus, his past knowledge (provided it has been properly assimilated) can

be instantly available to man, while his conscious mind is left free to deal with the *new*" (Branden 1966, 4). In an essay that ran alongside Branden's, Rand made similar claims: "Man's values control his subconscious emotional mechanism that functions like a computer adding up his desires, his experiences, his fulfillments and frustrations—like a sensitive guardian watching and constantly assessing his relationship to reality. The key question which this computer is programmed to answer, is: What is *possible* to me?" (Rand 1966, 4:3). But Branden explicitly rejected an overall identification of the human mind with some kind of computational device, arguing that computers lack free will and that they do only what they are told to do (to use contemporary terminology, that they cannot do anything that their programmer has not "hand coded" into them).

The concepts of logic, thought, and knowledge are not applicable to machines. A machine does not reason; it performs the actions its builder sets it to perform, and those actions alone. If it is set to register that two plus two equal four, it does so; if it is set to register that two plus two equal five, it does so; it has no power to correct the orders and information given to it. If "self-correctors" are built into it, it performs the prescribed acts of "self-correction," and no others; if the "self-correctors" are set incorrectly, it cannot correct itself; it cannot make any independent, self-generated contribution to its own performance. (Branden 1969, 52)

Rand and Branden's computer metaphors actually went against the grain. They preferred to employ these metaphors in their account of emotions. By contrast, many participants in the Cognitive Revolution (e.g., Newell, Shaw, and Simon 1958) sought to modularize cognition away from emotion, and were far more comfortable giving computational accounts of thinking than of feeling.

Today, Branden explains that the computational appeal was never intended in a strong sense:

It's true that both Ayn and I sometimes referred to the subconscious as if it were a computer. But this is pretty metaphorical, since at that time we knew next to

nothing about computers. Certainly I think any analogies we draw are limited, to say the least. I suspect we will be forced to the conclusion that while the mind may, at certain times and in certain respects, resemble computers (or something else), the likelihood is that in the end we will see that the mind is *sui generis*—there is nothing in nature quite like it. (personal communication, 29 March 1999)

Indeed, while Rand never elaborated a critique of the strong claims made on behalf of Artificial Intelligence, many latter-day Objectivists have been receptive to epistemological critiques of AI, particularly those put forward by the philosopher of mind John Searle (1980; 1992).

#### Developmentalism

Another distinct emphasis in the *Introduction to Objectivist Epistemology* brings Rand close to the psychology of the time, but simultaneously distances her from the mainstream of the Cognitive Revolution. While American psychologists' interest in cognitive development was much stimulated in the 1950's (Jerome Bruner, in particular, helped to revive interest in the ideas of Jean Piaget), a great many cognitive psychologists accepted the "scientific bet" (Newell and Simon 1972, 7-8) that studies of development and learning were best put off until a fairly complete explanation of adult performance had been elaborated.

By contrast, Rand clearly regarded developmental issues as central to her account of concept-formation. The proposed sequence of development for definitions of the concept *man* is her most prominent excursion into cognitive development in the *Introduction* (Rand 1967, 42-43), but there are several others in the main text, and countless statements about infant and child development in the workshops that Rand conducted from 1969 to 1971 (Binswanger and Peikoff 1990). Rand noted that first-level concepts are typically at an intermediate level of generality; she argued, for instance, that children form the concept of *chair* or *table* before they form the more general concept of *furniture*, but also before they form the more specific concepts of *armchair* or *end table*. The same issue loomed large in studies of child language during the period (Brown 1958), though among developmental psychologists only Bruner's student

Jeremy Anglin (1977) recognized the relevance of Rand's work, and his published allusions to the *Introduction* were fleeting.

### Rejection of Operationism

In her emphasis on development, Rand went beyond some participants in the Cognitive Revolution. In two other regards, she moved beyond virtually all of them. Even today, psychologists are burdened by mistaken notions of what it means to be scientific: most cling to grossly inadequate presentations of psychological measurement, and few dare to question determinism.

American psychologists committed a tremendous blunder in the 1930's, when they seized upon a physicist's proposal (Bridgman 1927) to expel troublesome, changeable theory from the definitions of scientific concepts. Bridgman sought to characterize scientific concepts entirely in terms of the operations used to measure them. "Operational definitions" seemed to preclude messy theoretical disputes and speculative references to the unobservable by allowing psychometricians to define intelligence as scores on an IQ test, allowing clinicians to define anxiety as a galvanic skin resistance reading, and so on. Of course, strict operationism also precluded comparisons between different ways of measuring the same thing. Bridgman was so horrified by what the psychologists had done with operationism that he eventually repudiated it, and members of the Vienna Circle (whose positivistic fervor could hardly be questioned) showed that, under operationism, room temperature measured with a mercury thermometer and very low temperatures measured with a thermocouple would have to be entirely distinct dimensions (Bickhard 1992; Green 1992; Koch 1992).

The Cognitive Revolution did not induce most psychologists to face the challenge of approximately measuring the unobservable thoughts and feelings of other people through fallible indicators. Instead, the leading methodologists of the period sought to loosen operationism by introducing "converging operations," which allowed more than one way of measuring intelligence or anxiety, but without openly attacking the prohibition against evaluating standards of measurement by reference to theoretical conceptions of what was being measured (D. T. Campbell and Fiske 1959; Garner 1974). In the 1990's, most psychology students are still taught the doctrine of operational definitions, usually in completely uncritical terms.<sup>5</sup>

Rand had no sympathy for operationism. She put it out of its misery in a memorable, albeit somewhat ponderous, passage:

When discussing man's consciousness, particularly his emotions, some persons use the word "measurement" as a pejorative term—as if an attempt to apply it to the phenomena of man's consciousness were a gross, insulting, "materialistic" impropriety. The question "Can you measure love?" is an example and a symptom of that attitude.

As in many other issues, the two allegedly opposite camps are merely two variants growing out of the same basic premises. The old-fashioned mystics proclaim that you cannot measure love in pounds, inches, or dollars. They are aided and abetted by the neo-mystics who—punch-drunk with undigested concepts of measurement, proclaiming measurement to be the sole tool of science—proceed to measure knee-jerks, statistical questionnaires, and the learning time of rats, as indices to the human psyche.

Both camps fail to observe that *measurement requires an appropriate standard*, and that in the physical sciences—which one camp passionately hates, and the other passionately envies—one does not measure length in pounds, or weight in inches. (1967, 38)

A clearer appreciation of the difficulties of psychological measurement, and the best available ways to overcome them, would put an end to the false dichotomy between operationism, which declares that anxiety is a score on a questionnaire, and the hermeneutic or "humanistic" skepticism which notes that a score on a questionnaire may fail to track how anxious the respondent actually feels and concludes from this that anxiety can never be measured at all.

### Advocacy of Free Will

Rand was farther ahead of the pack in her treatment of free will. Even after the initial success of the Cognitive Revolution, positivistic taboos lingered in psychology. Some realms of research were still shunned, for fear that entering into them would threaten the scientific

credentials of cognitive psychology. Thus, visual imagery was not treated as fully legitimate until the early 1970's, and studies of conscious awareness were not fully restored to respectability till the mid-1980's (Baars 1986).

As we reach the end of the 20<sup>th</sup> century, free will remains a taboo subject. Psychologists suppose that science must be deterministic, and a writer who expresses a belief in free will (Ó Nualláin 1995, is a rare example) is widely assumed to be anti-science. Even the rather facile compatibilism favored by so many contemporary philosophers—the claim that an entirely deterministic system exhibits free will, er, well, “the varieties of free will worth wanting” (Dennett 1984)—is rarely mentioned in psychology books. Albert Bandura (1997), a philosophically minded ex-behaviorist, actually devotes three pages to free will in his recent volume on self-efficacy. More significant than his advocacy of compatibilism is the impatient tone of his discussion, which seems to be admonishing the reader to move along smartly to the empirical data so as not to be caught dallying in such speculative indulgence.

Rand, of course, was always uncompromising in her treatment of free will. She considered human rationality and morality impossible without it. It appears, in fact, that she did not realize how remarkable Chomsky's position was. Unlike his colleagues in academic psychology, Chomsky (1971) openly defended incompatibilist free will:

Suppose that in fact the human brain operates by physical principles (perhaps now unknown) that provide for free choice, appropriate to situations but only marginally affected by environmental contingencies. The task of scientific analysis is not—as Skinner believes—to demonstrate that the conditions to which he restricts his attention fully determine human behavior, but rather to discover whether in fact they do (or whether they are at all significant), a very different matter. If they do not, as seems plausible, the “task of a scientific analysis” will be to clarify the issues and discover an intelligible explanatory theory that will deal with the actual facts. (19)

All the more reason, then, for Rand not to be so discouraged by the responses to Skinner's polemic against freedom and dignity.

### **Philosophy versus Psychology**

I have mapped out the points of contact between Rand's philosophy and the Cognitive Revolution in psychology. It is clear that Rand made significant use of ideas from the Cognitive Revolution (most importantly, the idea of limited cognitive capacity). Yet, she remained largely unaffected by the computational side of the new psychology, and she did not comprehend the full impact that the revolution was having, especially its overthrow of behaviorism.

In some respects, as we have seen, Rand went farther than cognitive psychologists were willing to go in the 1950's and 1960's. She was more developmental in her orientation than many participants in the Cognitive Revolution. She had no legacy of operationism to shake off, no taboo against questioning psychological determinism. But as any careful reader of her essays from this period has probably noticed, Rand evinced a certain distrust of the empirical findings of psychology. While psychologists often express caution when interpreting their findings (and those who do not would frequently be well advised to), Rand's repetition of such caveats in her rendition of the sensory deprivation studies gives them more than customary status: “The scientists pursuing these inquiries state emphatically that no theoretical conclusions can yet be drawn from these and other, similar experiments, because they involve too many variables, as well as undefined differences in the psychological character of the subjects, which led to significant differences in their reactions” (Rand 1966, 4:1).

She also seeks to distance herself from the crow subitizing study: “I cannot vouch for the validity of the specific numerical conclusions drawn from it” (1967, 57). It is particularly odd that she would attach a disclaimer to a study that indicates tight limitations on the perception of numerosity—it is much easier to identify the limits of subitizing experimentally than it is to measure the capacity of working memory. A great many experiments, some of them in areas of psychological inquiry much better established than the study of sensory deprivation, were cited in Miller's 1956 review—but Rand never mentioned Miller, or sought to trace his sources, which could be found in any library reasonably well stocked with psychology journals. One gets the impression, in fact, that Rand actually inverted the positivists' methodological priorities.

Even today, some cognitive psychologists distrust introspective evidence (e.g., Reisberg 1997). But this positivistic holdover has become

completely senseless now that their colleagues freely use "thinking aloud" and psychologists of every stripe rely on self-report questionnaires. Contrariwise, Rand (1967) seems to be saying that behavioral evidence is actively inferior to introspection: "Whether this particular experiment is accurate or not, the truth of the principle it illustrates can be ascertained *introspectively* . . ." (57). Something more is at work here than Rand's view that a scientific theory must pass a broad variety of tough empirical tests before its claims can be accepted as fact, or the concerns she might have harbored about operating without a comprehensive philosophy of science (Binswanger and Peikoff 1990, 301-4). Whatever else it requires, an adequate philosophy of science for psychology must provide a balanced appreciation of what introspection is good for, and of what behavioral measurements are good for (besides, if *crow* psychology cannot be done with behavioral measurements, it cannot be done at all!). Distrust of behavioral measurements and behavioral data indicates distrust of psychology as a discipline.

There is further evidence of such distrust. According to Branden, who introduced Rand to what she knew of academic psychology, she held some common misapprehensions about the subject. Any academic psychologist who has ever attended a cocktail party knows how many people regard psychology as the exclusive preserve of "shrinks" and clinicians, confined to the irrational and to psychopathology. Rand's conception of the discipline was not that different:

Her attitude, in effect, was that rational minds do not require psychology. Philosophy is enough. Psychology is essentially for pathology—that is, for the irrational in people. I argued with her about this, and she would always concede that I was right: "Yes, of course, Nathan, we all have a psychology, and the operations of the mind do need to be studied, but . . ." And a week or two later, she would say, "Oh, how I hate your profession, Nathan. How I hate the irrational. How I hate having to deal with it or to struggle to understand it." (Branden 1999, 97-98)

Rand insisted that philosophy in no way depends on the theories or findings of psychology. This tenet is usually not taken to indicate a distrust of psychology *per se*, although a pejorative comment can be found in her epistemology workshops (Binswanger and Peikoff 1990, 216):

when another participant brings up mathematicians who are interested only in mathematical concepts, not their referents, she reacts, "That would be psychology, or psychopathology, and I can't go into that." Normally her position is understood as an old-fashioned separation between philosophy and the "special sciences."

Philosophy by its nature has to be based only on that which is available to the knowledge of any man with a normal mental equipment. Philosophy is not dependent on the discoveries of science; the reverse is true.

So whenever you are in doubt about what is or is not a philosophical subject, ask yourself whether you need a specialized knowledge, beyond the knowledge available to you as a normal adult, unaided by any special knowledge or special instruments. And if the answer is possible to you on that basis alone, you are dealing with a philosophical question. If to answer it you would need training in physics, or psychology, or special equipment, etc., then you are dealing with a derivative or scientific field of knowledge, not philosophy. (289)

Such a demarcation procedure runs up against the fact that what is considered specialized training has changed over human history; knowledge that was once highly specialized has become available to any educated person. Rand, of course, presumes that the ability to read is not a specialized skill. In our culture it is not so regarded. But in Ancient Egypt, where learning to read and write usually required a long apprenticeship in the guild of scribes, reading surely qualified as a specialized skill. In chemistry as taught in better American high schools in the 1990's, students learn to explain the behavior of acids and bases in terms of subatomic particles such as protons and electrons. Such explanations would not have been part of high school chemistry in 1930. And in 1830 they *could not* have been part of the most advanced training in chemistry, for no one yet knew that there were protons, neutrons or electrons. The sort of partition that Rand sought to establish between philosophy and the sciences, natural or social, would, at the very least, have to be movable over time.

More to the point, it is hard to believe that epistemology can be

successfully walled off from the relevant subdisciplines within psychology. Psychology only began to differentiate from philosophy around 1860 (Boring 1950), and a philosophical account of the way human beings ought to think might be expected to pay some attention to the way human beings actually do think (cognitive psychology) or the way in which their thinking actually develops (developmental psychology).

Rand not only borrowed her conception of limited cognitive capacity from cognitive psychologists, she made frequent assertions about the manner and the sequence in which children form and use concepts. In *Introduction to Objectivist Epistemology*, the most prominent (but by no means the only) discussion of child development is the developmental sequence of definitions of the concept *man* (in Binswanger and Peikoff 1990, 43-44). Are we to conclude that the data collected by child psychologists have no bearing on whether children form and define their concepts of *human being* in the manner and order that Rand specified? That is what we would have to conclude, if we adhered to Rand's stated views on philosophy and psychology. In the epistemology workshops that were later published as an appendix to the *Introduction*, Rand makes frequent and confident assertions about the way that children (and sometimes infants) develop cognitively.<sup>6</sup> These assertions cannot have been based on Rand's introspection! And if they are supported by observations of children, what makes Rand's observations of children admissible evidence for a philosopher, whereas the observations or experiments conducted by a trained child psychologist are to be rejected as inadmissible? For instance, are philosophers supposed to ignore the data of developmental psychology when they evaluate the assertion that children do not begin to count until well after they learn to use words for other purposes, or when they set out to analyze the cognitive prerequisites for the correct employment of counting (200)?

Jean Piaget (1950) also considered the means by which children acquire new knowledge to be crucial to an account of concept formation (or, for that matter, of any other problem area in epistemology). But for that very reason he rejected all attempts to assert that philosophy has priority over psychology, and all attempts to wall off philosophical inquiry from the theories and findings of psychology (Campbell 1997). Rand's understanding of the critical role of cognitive capacity limitations and her interest in cognitive development should have led her to regard epistemology as a cognitive science (as Ó Nualláin 1995 does), or even as a developmental science (*à la* Piaget, or Feldman 1993). A truly systematic and integrative conception of knowledge of the sort that Rand

aimed at (Sciabarra 1995) would overcome another dichotomy, the dichotomy between epistemology and psychology. Instead, her attempts to isolate philosophy from the sciences<sup>7</sup> have obstructed the assimilation of cognitive psychology by most Objectivists. Even David Kelley, who once taught in a cognitive science program, and has made his own extensive use of ideas from cognitive psychology, still accepts the formulations of Quine (1969), a philosopher who never gave up his allegiance to behaviorism, as circumscribing the potential of "naturalized epistemology," and continues to insist on one-way traffic between philosophy and psychology (Kelley 1986; 1998). I am convinced that Rand's antipsychological metatheory (a set of declarations more than a little at odds with her actual practice) has significantly inhibited the further growth of the Objectivist epistemology, within the areas sketched by Rand as well as beyond them.

### Beyond the Cognitive Revolution

*Introduction to Objectivist Epistemology* would have been a different document without the Cognitive Revolution. What it drew from the Cognitive Revolution strengthens it—and dates it. Historical trends are more easily recognized in hindsight; those who were in the midst of the turmoil did not always register the direction of the changes, their extent or their depth. We know now that the root ideas of the Cognitive Revolution were developed, and available in published form, by 1960. True, the effects took another decade or so to percolate through the discipline of psychology. But under the tenure system, and the other guild privileges and insulations that buffer against new ideas in academia (Bartley 1990), a change of this magnitude that works its way through academic departments, textbooks, learned societies, and journals in 10 to 15 years is actually moving with remorseless rapidity.<sup>8</sup>

In 1972, Rand was under the impression that behaviorism and Freudianism still ruled the roost. She was wrong then. And we are no longer 10 to 15 years past the Cognitive Revolution; we are 40 years past it. Behaviorism is dead. The few protected remnants in the universities have no intellectual vitality, and the occasional recitation of behaviorist slogans by the practitioners of behavior modification has little to do with the actual success or utility of their methods. Though psychoanalysis also stood in danger of erosion by cognitive research and theorizing, it did not bar the doors of the academy against cognitive psychologists the way behaviorism did. Its decline was consequently more gradual. Beginning

in the late 1970's, however, a vast range of publications cast deepening shadows on psychoanalysis.<sup>9</sup> At the end of the 20<sup>th</sup> century, it has no scientific credibility remaining. Psychoanalysis has trouble finding any defenders in departments of psychology; it has taken refuge in the humanities, as an esoteric rite of symbolic interpretation, not the psychological science or even neuroscience that Freud wanted it to be.

If Objectivists fail to recognize the distinctly psychological roots of Rand's epistemology, if they maintain the diremption between philosophy and psychology that Rand's pronouncements require,<sup>10</sup> they will be unable to respond to significant contemporary developments in psychology and the cognitive sciences. Such conceptions as the new connectionism (McClelland and Rumelhart 1986; Rumelhart and McClelland 1986), perception and action robotics (Brooks 1991), interactivism (Bickhard and Terveen 1995; Campbell and Bickhard 1986), and Dynamic Systems Theory (Port and van Gelder 1994)<sup>11</sup> open up new epistemological opportunities and pose new challenges.<sup>12</sup> I do not believe that Objectivists will take any of them on successfully unless they realize that Rand herself drew on the cognitive psychology of the 1950's and 1960's, and unless they expect today's cognitive psychology to have an impact on them, as well as the other way around.

Although a detailed critique is the topic for another essay, it is clear that the Cognitive Revolution solved some problems for psychology, while leaving other fundamental concerns unaddressed. But because of that revolution, the alternative pathways for the psychology of knowledge are far richer today than they were in the 1950's. And whatever their faults, most are a good deal less threatening to rationality, freedom, or dignity than behaviorism and psychoanalysis were in their heyday.

#### NOTES

1. See Boring 1950 for the rise of behaviorism, and Baars 1986 for a detailed history of its overthrow.

2. Compare Miller 1956 with Miller and Johnson-Laird 1976, or Garner 1962 with Garner 1974.

3. Branden, personal communication, 29 March 1999, recalls no discussions of Chomsky with her. However, Rand's journals include her notes from a conference on "Methods in Philosophy and the Sciences," which took place on 21 May 1961, at the New School for Social Research in New York City. One of the lectures was "Some Observations on Linguistic Structure" by Noam Chomsky (Rand 1997, 683-84). Rand (with typical delicacy)

characterized Chomsky as "an expert social-metaphysical-elite witch doctor," and wondered, "is the manner of presentation always in mid-stream, assuming previous knowledge?" She questioned his use of "simple" tree diagrams for sentence structure, finding them "Pure Rube Goldberg," and inquiring, "How many trees would I need to build in order to understand *Atlas Shrugged*—and in how many volumes?" (The editor of Rand's *Journals* wrongly equates Chomsky's tree structures with the diagrams used in modern symbolic logic, which are a somewhat different kind of mathematical notation.) Rand concluded with the question: "Is Chomsky trying to systematize all conceptual relationships in language?" Chomsky was never interested in literary analysis, and, in later years, his answer to Rand's epistemological question would be a resounding no. At the time, however, he was ambivalent about whether semantics (word and sentence meaning) and pragmatics (language use) were to be included in his linguistic theory, and consequently willing to entertain much broader epistemological ambitions than he does today, when only knowledge of sentence structure officially remains within his purview. The tremendous schism among Chomsky's followers over "generative semantics," which raged from the late 1960's till the mid-1970's (Harris 1993), centered precisely on whether all conceptual relationships should be systematized in linguistics. One further point of contact: around 1969, Sandra Pinkerton (then a fan of Rand's work and a graduate student in linguistics at the University of Texas) wrote to Ayn Rand, recommending that Rand read Chomsky's 1966 book, *Cartesian Linguistics*. There is no evidence that Rand followed this advice, but she did call Chomsky a "Cartesian linguist" in her 1972 article. In all, the evidence indicates that Rand had a little acquaintance with Chomsky's ideas, but had not studied his linguistic system. Given the date of the talk she attended, and her complaint about jumping right into technical details and presuming too much specialized knowledge from the audience, I doubt that she heard Chomsky expound his variant of the doctrine of innate ideas or his refutation of behaviorism.

4. For example, Campbell and Bickhard 1986; Campbell 1998; Dartnall 1997.

5. Green (1992) reports that virtually every textbook in experimental psychology still endorses operationism; I can confirm his verdict from my own experience as an experimental psychology instructor.

6. See Binswanger and Peikoff 1990, 147, 151-52, 162, 167-74, 178-81, 200, 206-10, 212, 217-18, 231-32. This tradition is carried on by Peikoff (1993, 14) in his account of the way children learn about causality.

7. A particularly egregious example of Rand's desire to distance philosophy from the sciences is her professed agnosticism about biological evolution. "Darwin's theory, Ayn Rand held, pertains to a special science, not to philosophy. Philosophy as such, therefore, takes no position in regard to it" (Peikoff 1993, 476 n.19). (While I have no reason to question Peikoff's report of Rand's views in the footnote, I hope that Peikoff is solely responsible for the ignorant dismissal of Darwinian evolution on p. 405 in his main text. He brushes evolution off as "the intellectuals' fad" of the late 19<sup>th</sup> century, and claims that Herbert Spencer borrowed from Charles Darwin instead of the other way around.) The precise implications of evolution for epistemology are highly controversial, as is manifest in the major disagreements between sociobiology, Piagetianism, memetics, interactivism, etiological

functionalism, and diverse other flavors of evolutionary epistemology. But if human cognitive powers are the product of an evolutionary process—as they must be, if any variant of evolutionary theory is true—how could this fail to have a bearing on their capacities and limitations, or on their proper employment?

8. Eight or nine years was all it took to sweep away pre-Chomskyan conceptions in linguistics, according to Harris 1993, but linguistics is a far more compact and centralized discipline than psychology.

9. Some of the peaks are Sulloway 1979; Grünbaum 1984; and Kitcher 1992.

10. The denial of Nathaniel Branden's contributions to the philosophy that is still *de rigueur* in some Objectivist circles also obstructs the assimilation of contemporary psychology.

11. Barry Vacker (1999), in a fascinating examination of Rand's visual esthetics, has argued that she anticipated fractal geometry, chaos theory, and other conceptions of dynamic systems that have only come to the forefront in the 1980's and 1990's. He is able to demonstrate Rand's fascination with near-chaotic dynamic relations by pointing to many passages from *The Fountainhead* (particularly her descriptions of Howard Roark's buildings). But it is incorrect to extend this analysis into epistemology, as Vacker seeks to do. In her theory of concepts, Rand (1967) sought to undermine dualism (Sciabarra 1995)—in this case, the dualism of universals as intrinsic (indwelling in objects, out in the environment) or subjective (in the mind, without any basis in reality). But no commitment to dynamics or non-linearity follows from this, and it is hard to find any signs of such a commitment in Rand's epistemological writings. Rand sought to overturn various forms of dualism, and she was a systems thinker, but she was not a proto-connectionist or dynamic systems thinker. Rand (1967) adhered to a traditional symbolic orientation: every concept was incomplete without a word, and she understood words to be symbols. It would be interesting to know how she would have reacted to dynamic ideas, but they were simply not part of her milieu, and nothing in her epistemological writings seems to require them.

12. See Livingston 1997, for one Objectivist's response to some of these.

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