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CREATIVITY : PROCESS AND PERSONALITY

SIX CASE-STUDIES OF EMINENT PSYCHOLOGISTS

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VIII. B. F. Skinner: The science of human behavior

Any catalogue of the most creative and important psychologists alive would have to include B. F. Skinner in a prominent position. Not only is he the leading representative of one of the major forces in psychological theory, but his creative accomplishments are such that they are recognized even by those whose positions are totally opposed to his. Although working within a pre-existing framework, Skinner has changed the nature of this very framework in very significant ways. Pavlov was the ancestor, and Watson the father of Skinnerian Behaviorism, but in the final analysis, the system as it stands is his. Professor of Psychology at Harvard University, Skinner has promulgated his position through five books, ranging from methodological description to Utopian speculation, (his novel, Walden Two, 1948, is the only fiction written, to my knowledge, by a prominent contemporary psychologist), and numerous articles. He has also made significant methodological innovations. Many psychologists may not like his work, but they ignore it at their peril. More than any of the other subjects, except perhaps Maslow, Skinner is a theorist about whom one cannot be neutral.

The interview is, unfortunately, subject to some limitations. First, a heavy schedule limited the available time to one hour, recorded October 15th, 1963. Skinner has published an autobiographical, "Case History in Scientific Method" (1959), and, therefore, did not deal at all with sections III, IV & V of the outline, feeling that the paper covered them. As it happens, it only covers some of the headings. The case-study that follows

will be both limited in scope and detail, and will of necessity rely for large sections wholly on published information.

Early conditioning

"My father was a lawyer, but actually did not go to college, he went directly to law school from high school. My mother did not have a college education. They were, however, both reasonably interested in intellectual matters; there was music in the home, and I think it was a pretty good background. I went to the only college I ever considered, which happened to be recommended by a friend of my father's.

"It was a small town. I graduated in a class of only 12 members. I knew most of the people in the town, one way or another. Actually it was a dying kind of community, which is now practically dead. There was an early Anglo-Saxon, I suppose, population in the town, which was, by the way, Susquehanna, Pennsylvania. There was a large importation of Irish and Italian railroad workers, who settled there and became the important people in the town. I was not strictly from the Protestant, Anglo-Saxon background, but was identified with it throughout my childhood.

"I came under the influence of a very important person, a Miss Graves, a teacher to whom I am going to dedicate a book I am writing now, The Technology of Teaching. Her father was the local atheist, a stone-cutter who tried to keep people from celebrating New Year's Eve, 1900, because he argued that this was only the beginning of the last year of the 19th Century, the 20th Century wouldn't start for a year. He was a Darwinian and a botanist. Miss Graves once showed me a letter he had received from the Prince of Monaco offering to

exchange pressed flowers.

"She taught me Sunday School in a Presbyterian Church for many years. There was a group of boys, we started together and kept together; we went through pretty much all of the Old Testament under her guidance. She was liberal, and I suppose, partly because of that I quickly lost my religion and have no interest in religion at all.

"She taught me drawing throughout the Grade School, then began to teach English, and advanced more or less as I did, and by the time I got into high school she was teaching me English. She undoubtedly had a very important influence on me, as well as on the whole community. She organized a Monday Club, to which my mother belonged. They would spend all winter reading Ibsen's The Doll House, or something like that. Her influence gave me some kind of culture which I would otherwise not have found in that town.

"I did feel a little apart from others. I discovered this slowly. I always supposed that everyone in the community would be as ambitious as I, and it was a great shock to me when other members of my peer-group simply did not go to college and went to work in the local railroad shops. I didn't discover any individuality,any difference from these people till I was hit over the head with it, actually.

"I didn't feel persecuted. There was some feeling in the town, for some strange reason, on the part of the Irish, against me and my family. My father defended an Italian for murdering an Irishman and he got him off. The Irish never forgave him and the Italians never got over loving him for this. I grew up with a warm feeling for Italians, which I still have, and a somewhat doubtful kind of

race prejudice with respect to the Irish. This is ridiculous, but that is the source of it.

"I suppose I was a good deal by myself. I would write various things as a child, for myself. I went through a period in which I believed I had some kind of religious mission, though I lost contact with religion. I went to the same high school my mother and father graduated from; and we were all salutatorians, we always came in second.

"I went on to this college, Hamilton College, which was at the low point in its career, I think. I took a miscellany of courses, under the recommendations of friends and so on. There wasn't a bit of career counselling at the time. I had a lot of English, a lot of French, I took French theatre, modern French literature, Anglo-Saxon, Chaucer, Shakespeare, Restoration Drama, Romantic poets, 19th Century novels, I had Greek and Roman art, I had Political Science, I had Botany, Zoology, cat Anatomy, Embryology, lots of required courses in Public Speaking, required courses in English composition, two years of Greek, twoyears of Spanish. The damndest collection imaginable. It would look like the devil on a program today. But in some curious way I have made professional use of every scrap of that, somehow or other. It is amazing to think of it; and I now recommend to people in a liberal arts program, 'For god's sake, don't specialize! If there is anything you feel like taking, for any reason whatsoever, it will come in handy.'

"I didn't have any outstanding teachers in college, I guess, except a Biology teacher, who, apart from the courses he was teaching, told me about Pavlov and Loeb.

"I wanted to be a writer. When I was a senior in college, I got a letter from Robert Frost, whom I had met the preceeding

summer and to whom I had sent some short stories. And he sent a very laudatory letter, with glamorous phrases like, 'You're worth twice anyone else I seen in prose this year,' and so on. This set me back two years, because it confirmed my resolution to be a writer. And I tried and failed miserably; finally discovering, and I am proud of my honesty in this, that I had nothing to say, and there was no point in knowing how to say it well. And I turned to psychology as the only science in which I thought I would be suited, equipped for. Mainly because it did deal with the same things that literature did. I wasn't shifting my field, I was just shifting my techniques. Although I became very sour on literature, and went through a phase of being very contemptuous of the whole enterprise.

"I had had no psychology as an undergraduate at all. It was recommended to me to go to Harvard by the president of Hamilton College, when I asked about the study of psychology, when I visited the college a year or so after graduating. He reached into a drawer, and took out a musty old mimeographed sheet, which told him that Columbia and Harvard were the outstanding centers for psychology. I suppose this must have been prepared while William James was still alive. In any case, I came to Harvard, didn't find at all what I expected, because I had read Pavlov and Watson and there was nothing at all of that kind going on at Harvard. From here on, I have described my work in A case history in scientific method.

The operant personality

"I am aware that I am highly motivated. I work well. I don't believe I work to dominate anybody. I don't get involved with controversies, to prove someone is wrong. The best way, I find, is to say nothing, and somebody else will prove he is wrong for you,

if he is wrong. I have a certain amount of crusader's zeal, I think, that comes out in Walden II; I am a do-gooder, I have to improve mankind; I have messianic feelings; I have a sense of destiny. I don't enjoy the iconoclastic sides of my work, although that has been relevant every once in a while.

"I was an older child, I had a younger brother, he died when I was 17. We were competitive, there was sibling rivalry, I undoubtedly have something of that in me. I was not competitive in college, I didn't give a damn about honors.

"I've never been under any publication pressure, I've never felt that I had to publish in order to get on in the world. I have never given a damn about politics, in the department or anywhere else. I don't care about honors, I've had my share of them, medals, membership in learned societies, national academy; I've had them, and they're all ashes in my mouth after I get them. I am pleased a bit at the time, but not very much. I don't allow my name to appear for the presidency of the APA, which is an honor, and I'd like to have it, but I don't have the time, or interest, really. I am not in a race with anybody, so far as I regard all of this.

"I am certainly independent, no doubt about this. A friend of mine said I was the most inner directed person he knew, and I dare say that's right. I am anxious not to be influenced by the opinions of my contemporaries, I don't think much of them, and I prefer to settle for my own standards. I have very rigorous standards of intellectual honesty and integrity in my work, and those are imposed by me in a very independent way, I think. I don't think there is any rivalry, or attempt to dominate. If independence means having your own way in that sense, then I am not. But I am very much self-determined.

Of course, I would qualify that to mean I was determined by my early environment very heavily.

"I don't know what self actualization means, pardon me Professor Maslow. I like to be creative, at times I have painted, sculpted, I play the piano, recently I picked up an electric organ, and I also own a clavichord, I play them a bit. I like security, and have carefully arranged my life so that I feel secure, and don't find myself lying awake at night, or bickering with my colleagues, and so on. I think I am warmly affectionate, particularly to people I love. I express myself very easily to my two daughters. I think they would say I am a very warm kind of parent. I feel very close to them, I have been a successful father, although I haven't done half of the things I knew I should do. I have tried to apply my own thinking to my role as a father. For example, I found out recently that I was annoying my daughter in my letters to her, by praising her a bit too much. So I made a specific resolution. For a while my letters will contain no word of commendation. If this was bothering her, I'll just stop it. If this means I am not really being myself to my daughter, well, all right, I say the hell with that. If I find that I am bothering someone in a given way, I change. It's important to me not to bother them.

"What are my peak experiences? Well, I assume you rule out orgasms and things like that, and great relief from anxieties and so on. I don't know that I've had any, I've always been bothered by the fact that the things I have done, which I now feel were valuable, I didn't realize at the time were valuable. It would take a long time to realize that I had discovered something, for example. There is no moment in history when you suddenly realize

that you have done it. With one possible exception, I think. When I first worked out the lever-box system, I was then very much under Pavlov's influence. But I had something different, and didn't realize at the time. But I was reinforcing a rat for pressing a lever to get food, and watching the rate change as the rat ingested the food. And one day I disconnected the food dispenser and recorded an extinction curve; it had cycles, and it was very smooth. Then I do remember this feeling, that this is terribly important, and I was very careful how I crossed the street for two or three days, until I had gotten that written up in some kind of permanent form. With that exception, I don't think I've ever felt that anything I was doing at that moment was really very great.

"My life goals are certainly fulfilled. I still have a lot of work to do, but if I were to die tomorrow, I would feel that I had lived, and very well. I have got work planned for the next five to ten years, and I am trying to keep the decks clear so that I can devote myself fully to it. It will be important to me in rounding out what seems to me to be a reasonable pattern of one's intellectual life. I don't have day-dreams that don't get fulfilled, that I know of. I don't think, actually that I could make three wishes.

"I became convinced, largely I think because of Bertrand Russell's interest in a behavioristic epistemology that behaviorism and positivism were the way out, and I have continued to develop that. Curiously, I think if I could have specified, if I could have seen in 1928, where I am now, it would have been precisely what I wanted to do, at that time. I have been very lucky in

being able to follow up and develop along lines that I wanted to follow, I have never shifted my course at any point. Now this may be rigidity, but on the other hand I think it has been determined by success. I have done what I hoped to do, and feel perfectly happy about the current situation.

"I didn't learn much from other people. I have been a very close friend of Fred Keller, professor of Psychology at Columbia, since graduate school days. We have influenced each other considerably. He was a die-hard, stubborn behaviorist at the time that I needed to have someone to stand up for that, and he was very important in my development, I am sure. I worked with Crozier, in general physiology, rather than with the psychologists at Harvard, and I learned a great deal about methods from him.

"I have never collaborated with anybody in any important way. I have published a few papers with students, but it has never been an extensive collaboration. I don't know which books have influenced me particularly, or what peers. At Minnesota, I found no one who particularly bolstered my activities or interests there, certainly no one when I was at Indiana briefly as chairman, and I think no one here.

"I have lived relatively independent of the intellectual environment in which I have been, I would say. I hoe my own row of beans, and am satisfied with doing that, and am not in any hurry to move on. I have never had any doubt at all of the general correctness of the point of view I have taken, or of the value of what I have done. I have been able to survive long periods of neglect on the part of my colleagues without too much trouble. Now it is the opposite, I suffer from too little neglect. I am trying to

generate some more so that I will have time to do the things I really want to do.

"I have noted somewhere that practically all of my non-experimental papers have been prepared for colloquia, conferences, etc. Evidently I need some kind of external spur to get me going. Most of the non-experimental papers in Cumulative Record (1959), were done for special occasions. I have, however, planned and carried through on books without any pressure of that sort. It took me, off and on, 20 years to write Verbal Behavior (1957). I wrote three other books during that period, I kept at it and finally got it the way I wanted it. I am now working on a book on the technology of teaching, which will be my swan song in that field. I've been at it pretty intensively for 3 or 4 years now. I'd hoped to finish it in one, I still have one to go. I am then planning a book on the design of cultures.

"My students have always been very helpful. Two or three times I've had seminars which really developed new ideas, went along very effectively. Sometimes it doesn't work that way. My big course, for which I wrote Science and Human Behavior, has always been a good testing ground.

"My thinking is a much less structured activity than the hypothetical-deductive design type of thing. My methods were not designed in advance, I don't believe in experimental design a la R. A. Fisher. The modern structure of the analysis of operant behavior has developed piecemeal, nothing was seen in advance. There was no hypothesis testing.

"I don't pay too much attention to the feedback, it doesn't matter much to me, and I don't think I change my views or plans in

light of that feedback at all. Further work grows out of the past, and I think of trying to be as honest as possible in avoiding ways of smudging over inadequacies and so on.

"I think in general my writing shows deep organization. I organize a manuscript through a very elaborate decimal system, which I wish I could have in the final form; but publishers hate decimals in front of paragraphs, it scares off readers, I guess. But all of my books have been carefully designed, organized in that way. I write with elaborate outlines in book form, card indices, and then when I am writing a couple of paragraphs, I write on a large sheet, 22x34, and I can see the whole set of paragraphs, or even the whole chapter in outline form on that sheet. I work in the Baconian fashion, I am assembling ideas, integrating them, putting them together till they flow. I will often write a chapter fifteen times before it says what I really want it to say. Actually, it is a process of discovering what you have to say, and is very different from thinking you know in advance and then writing it.

"The only exception to that rule is the writing of my novel, Walden Two, which is the closest thing to automatic writing I've ever done. I wrote the book often with great emotional involvement, almost in a frenzy sometimes. That book is undoubtedly a catharsis, a clarification of two sides of my personality, in which Frazier emerges victorious. I became a Frazierian after writing the book.

"I always avoid labelling if possible. I am always suspicious if I continue to have to use one label too long. It does distort, it keeps you from seeing what you're talking about. I have made up a few words in my time, such as operant behavior, but only when absolutely necessary. I don't like to invent words, some people

seem to love it, but I don't. But sometimes a point is reached where you must do something about it. For example, the names of the schedules which we have analyzed, these had to be invented, there was nothing to describe them. But, in general, I am very suspicious of formulations which I use too long, I think they restrict your thinking. I don't label if I can help it."

A case-history in operant behavior

A large part of Skinner's paper, "A case-history in scientific method" (1959, pp.76-100) deals with the development of his behaviorist belief system from the time he first accepted this as the "true" way till the point at which he published The Behavior of Organisms (1938), his first important work. This is, in fact, a report of the processes that resulted in some of his most creative innovations, conceptual and methodological. I will not give the whole report here, but will rather select what I think are the essential points for our concern in the same way that I have done in the other studies. All quotes in this discussion will be from the paper, to which the reader is referred for the complete exposition.

We have already seen that before coming to Harvard, Skinner had read Loeb and Pavlov, and had been further influenced by reading Russell's discussion of the epistemology of Watson's behaviorism. This led him to Watson and to behaviorism. "Many years later when I told Lord Russell that his articles were responsible for my interest in behavior, he could only exclaim,'Good heavens! I had always supposed that those articles had demolished behaviorism!' But at any rate he had taken Watson seriously, and so did I."

We also know that he worked at Harvard under Crozier in physiology. "It had been said of Loeb, and might have been said of Crozier, that he 'resented the nervous system.' Whether this was true or not, the fact was that both these men talked about animal behavior without mentioning the nervous system and with surprising success. So far as I was concerned, they cancelled out the physiological theorizing of Pavlov and Sherrington and thus clarified what remained of the work of these men as the beginnings of an independent science of behavior. My doctoral thesis was in part an operational analysis of Sherrington's synapse, in which behavioral laws were substituted for supposed states of the nervous system....So far as I can see, I began simply by looking for lawful processes in the behavior of the intact organism. Pavlov had shown the way; but I could not then, as I cannot now, move from salivary reflexes to the important business of the organism in everyday life. Sherrington and Magnus had found order in surgical segments of the organism. Could not something of the same sort be found, to use Loeb's term, in the organism as a whole? I had the clue from Pavlov: control your conditions and you will see order."

The first experiment he tried had to do with the behavior of a rat emerging into a new environment. "The major result of this experiment was that some of my rats had babies. I began to watch young rats. I saw them right themselves and crawl about very much like the decerebrate or thalamic cats and rabbits of Magnus. So I set about studying the postural reflexes of young rats. Here was a first principle not formally recognized by scientific methodologists: When you run into something interesting, drop everything else and study it. I tore up (the experiment) and started over."

With the young rats Skinner eventually became interested in recording quantitatively various movements. Then this became the primary interest and he returned to adult rats. He began developing instruments for measuring and recording their movements. "Now for a second unformalized principle of scientific practice: Some ways of doing research are easier than others. I got tired of carrying the rat back to the other end of the runway." He now developed a continuous runway along which the rat moved, and with which his movements could be measured. He noticed that the rat would delay at one point on the runway. "When I timed these delays with a stop watch, however, and plotted them, they seemed to show orderly changes. This was, of course, the kind of thing I was looking for." In other words, a lawful behavior process. "I forgot all about the movements of the substratum and began to run rats for the sake of the delay measurements alone. But there was now no reason why the runway had to be eight feet long, and as the second principle came into play again, I saw no reason why the rat could not deliver its own reinforcement." Here we have what will be a major methodological innovation. He built a new apparatus, in which the rat, by moving along the runway, caused a food-pellet to fall into a cup. Each tilt of the runway was recorded on a moving kymograph.

"A third unformalized principle of scientific practice: Some people are lucky." The food magazine he had used was an old piece of something else, a central spindle." One day it occurred to me that if I wound a string around the spindle and allowed it to unwind as the magazine was emptied I would get a different kind of record. Instead of a mere report of the up-and-down movement of the runway, as a series of pips in a polygraph, I would get a

curve. And I knew that science made great use of curves, although, so far as I could discover, very little of pips on a polygraph. The difference between the old type of record and the new may not seem great, but as it turned out the curve revealed things in the rate of responding, and in changes in that rate, which would otherwise have been missed. By allowing that string to unwind rather than to wind, I had got my curve in an awkward Cartesian quadrant, but that was easily remedied. Psychologists have adopted cumulative curves only very slowly, but I think it is fair to say that they have become an indispensible tool for certain purposes of analysis." From this point Skinner abandoned the runway, thus making a complete break with the problem that he had been dealing with at first, or rather, secondly. The methodological innovation of the cumulative record led to the next important instrumental invention, and together these culminated in the first important theoretical innovation.

He now has the rat reaching for food, and in doing so causing a pen to record one step in a cumulative record. "The first major change in rats observed in this way was due to ingestion. Curves showing how the rate of eating declined with the eating time comprised the other part of my thesis. But a refinement was needed. The behavior of the rat in pushing open the door was not a normal part of the ingestive behavior of Rattus rattus. The act was obviously learned, but its status as part of the final performance was not clear. It seemed wise to add an additional conditioned response connected with ingestion in quite an arbitrary way. I chose the first device which came to mind--a horizontal bar or lever placed where it could be conveniently depressed by the rat to close a switch which operated a magnetic magazine....Now, as

soon as you begin to complicate an apparatus, you necessarily invoke a fourth principle of scientific practice: Apparatuses sometimes break down. I had only to wait for the food magazine to jam to get an extinction curve. At first I treated this as a defect and hastened to remedy the difficulty. But eventually, of course, I deliberately disconnected the magazine. I can easily recall the excitement of the first complete extinction curve. I had made contact with Pavlov at last! It was an orderly change due to nothing more than a special contingency of re-inforcement. It was pure behavior!" It is this we have seen above as his best recollection of a peak experience. " I am not saying that I would not have gotten around to extinction curves without a breakdown in the apparatus; Pavlov had given too strong a lead in that direction. But it is still no exaggeration to say that some of the most interesting and surprising results have turned up first because of similar accidents." At this point he began to study the conditioning and reinforcement curves in great detail and with many rats.

"At this point I made my first use of the deductive method." He was running tests with eight rats, each consuming about 100 pellets a day, and it was very difficult to keep up the supply. It was this purely external problem that led to the next major leap.

"Since I do not wish to deprecate the hypothetico-deductive method, I am glad to testify here to its usefulness. It led me to apply our second principle of unformalized scientific practice and to ask myself why <u>every</u> press of the lever had to be reinforced... I decided to reinforce only once every minute and to allow all other responses to go unreinforced. There were two results: (a) my supply of pellets lasted almost indefinitely, and (b) each rat

stabilized at a fairly constant rate of responding.

"Now, a steady state is something I was familiar with from physical chemistry, and I therefore embarked on a study of periodic reinforcement." It was while doing this that Skinner decided to try reinforcing, not according to time intervals, but number of responses. This was originally meant to produce varying states of deprivation, but it turned out that the rat adapted to this also. "This is 'fixed-ration'rather than 'fixed-interval' reinforcement and, as I soon found out, it produces a very different type of performance. This is an example of a fifth unformalized principle of scientific practice, but one which has been named. Walter Cannon described it with a word invented by Horace Walpole: Serendipity--the art of finding one thing while looking for something else."

"This account of my scientific behavior up to the point at which I published my results in a book called The Behavior of Organisms is as exact in letter and spirit as I can make it. The notes, data, and publications which I have examined do not show that I ever behaved in the manner of Man Thinking as described by John Stuart Mill or John Dewey in reconstructions of scientific behavior by other philosophers of science. I never faced a problem that was more than the eternal problem of finding order. I never attacked a problem by formulating a Hypothesis. I never deduced Theorems or submitted them to Experimental Check. So far as I can see, I had no preconceived Model of behavior--certainly not a physiological one or a mentalistic one...Of course, I was working on a basic assumption that there was an order in behavior if only I could discover it--but such an assumption is not to be confused with the hypotheses of deductive theory. It is also true that I

exercised a certain Selection of Facts but not because of relevance to theory but because one fact was more orderly than another. If I engaged in Experimental Design at all, it was simply to complete or extend some evidence of order already observed."

We have here the most complete disavowal of the hypothetical deductive system as a mode of research that I can find in any of the subjects, though they have each made similar statements.

But this does not show that Skinner did not proceed in a manner consonant with our conceptualization. His research was guided by a specific, paradigmatic question: What are the objective laws of human, or animal, behavior? This question was posed as a clear renunciation, although not original to Skinner, of the theories that postulated an important role for the nervous system in determining behavior. Skinner went farther than ever before in finding methods to demonstrate the existence of, and formulating the nature of "laws of behavior."

Skinner's answers to the question eventually, if not during the solution process, took the form of a theory which purports to be applicable to all animal behavior. The basic concept here is the operant, the unit of behavior that becomes a discrete, known performance activity as a result of correct re-inforcement. Closely connected is the possibility of extinguishing behavior, reversing operants. And the major methodological techniques developed were, on the experimental level, the cumulative record and the lever box--for training and observing operant behavior; on a more general level, the reinforcement schedules--for developing various operants. These are significant answers to a basic question. And they are so conceived as to imply a theory of human nature. Further implications

of the concept would possibly be, as in other concepts above, the explanation of educational and cultural processes in these terms, and the use of these techniques to influence the processes.

In the field of education Skinner applied his paradigm theoretically and developed learning theories. These theories implied the value of utilizing the Skinnerian techniques in education And here we have one of Skinner's socially creative products, the teaching machine. He is dealing here with change-research, as were McClelland and Bruner.

On the more basic level of human nature in a social context, Skinner's has restricted himself to the speculative. His next planned book will attempt to be more systematic in dealing with the design of cultures. One of the things that follows from the basic paradigm, is that you can, with these techniques, change society, mold it. And Skinner's system is so complete that it even takes in a set of derivative social values. And the result is Walden Two, a fictional utopia in which people are trained to be happy, conditioned by tried and true Skinnerian reinforcement techniques, to fit the concept of the ideal man that is held by Frazier, the leader of his people who represents Skinner's social interest. Walden Two is the perfect society of a system whose values and basic belief derive from the Skinnerian paradigm. This, too, however hard to swallow for people outside the paradigm, is a creative extension of the implications of the system. I would like to end what is, I hope, a clear exposition of the creative nature of Skinner's research with a quotation from Walden Two, spoken by Frazier. In this passage we see something of the character of Skinner's view of research, and we get an interesting

insight into the nature of his ideal society and its rulers that reveals both the authoritarian and the benevolent social interest.

> I have only one important characteristic, Burris; I'm stubborn. I've had only one idea in my life--a true idee fixee...to put it as bluntly as possible, the idea of having my own way. "Control" expresses it, I think. The control of human behavior, Burris. In my experimental days it was a frenzied, selfish desire to dominate. I remember the rage I used to feel when a prediction went awry. I could have shouted at the subjects of my experiments, "Behave, damn you, as you ought!" Eventually I realized that the subjects were always right. They always behaved as they ought. It was I who was wrong. I had made a bad prediction. And what a strange discovery for a would-be tyrant, that the only effective technique of control is unselfish. (1948)