

**Donald S. Blough**  
NIMH 1954-1958

This is an interview with Dr. Donald S. Blough, former member of the Perception and Learning Section of the Laboratory of Psychology currently Laboratory of Brain and Cognition of the NIMH Intramural Research Program.  
December 10th and 18th, 2001 *via* e-mail.  
Interviewer: Dr. Ingrid Farreras of the NIH History Office.

Farreras: Why don't we start by your introducing yourself and telling me a little about your family background and upbringing?

Blough: I was born in Madison, Wisconsin on September 13<sup>th</sup>, 1929, where my father was Professor of Economics. My mother was and continued to be a homemaker. I was the youngest of three children, all boys. My family moved to Cincinnati when I was about three, stayed there until I was about eight, my father still a professor. When my father entered the Government (as Director of Tax Research in the U.S. Treasury) we moved to Arlington, Virginia. We left for Chicago for my last year of high school.

Farreras: How was your high school and college education; were there any courses or mentors who influenced your decision to become a scientist?

Blough: I wanted to be a scientist for as long as I can remember. I did simple experiments in Junior High, chemistry and electrical experiments at home during my senior high school year. I went to college expecting to be a chemist (I had two uncles who were chemists) but disliked the quantitative analysis lab. My favorite course was Introductory Physics, taken in my second year. I thought I didn't have the background for physics, and though I loved analytical geometry I rather disliked integral calculus. Swarthmore had a brilliant Psychology faculty (partly

displaced German Gestalt psychologists, including Wolfgang Köhler). I wasn't that impressed by Introductory Psychology, but after a difficult mental struggle I decided on a Psychology major. I did not have any particular mentors in college. I then went on to graduate school as a matter of course. At the time, graduate school also kept men from being drafted; I came as close as having the pre-induction physical exam. Harvard was supposed to be good, so I went there. In retrospect, and having seen other programs, I think the graduate instruction at Harvard was spotty. True to stereotype at the time, it had some *prima donnas*, Skinner for example. (Although many people have since thought I worked in Skinner's lab, I didn't. I talked with him once or twice for 5 or 10 minutes; much more with his graduate students.) I did my dissertation with Floyd Ratliff, combining Skinnerian methods with a my longstanding interest in perception. Floyd was as close to a mentor as I had, but he was mainly interested in visual physiology, and I was largely on my own. (Floyd left Harvard for the Rockefeller Institute shortly after I left Harvard.) I finished my graduate work in three years (Sept. 1951 - July 1954) and then went to the Laboratory of Psychology [at NIMH/NIH].

Farreras: When and how did you arrive at NIH?

Blough: I arrived in August of 1954. I was a Senior Assistant Scientist (post-docs were much rarer then than they are now). For the first three years, I was an officer in the Public Health Service and then I was briefly in the Civil Service. There were no post-doc positions or graduate students in the Lab at that time (1954-

1958), so far as I know, which is one of the reasons I ultimately left. One of my letters comments that there were “a few” graduate students somewhere at NIMH. I went to NIMH because my fiancée spotted an announcement of openings at the new NIMH Laboratory of Psychology on the Psychology bulletin board at Tufts University, where she was doing graduate work. At the time most job openings were announced by sending fliers to all the relevant places; there was also a lot of word-of-mouth transmission. When I was chair here at Brown I had an occasion to go through some old department files. We would now be appalled at the way hiring was done. No documented “national search” was required. I remember a letter from Walter Hunter, who was chair here until the late 50s, in which he asked a friend at another institution to recommend “a good man” for a faculty appointment. I’m sure Hunter could have hired such a “good man” on the spot if he wanted to, with the President’s approval, of course. My fiancée also noticed that the advertised job would be in the Public Health Service, which would fulfill the draft requirement. (The draft was still going on, although the Korean War was over.) I took a day-long test in psychology, which must have been similar to the psychology GRE today, and I evidently did well. I believe that B. F. Skinner also gave me a good recommendation. I think that David Shakow, the head of the lab, was the person responsible for my being hired. In a few later conversations with him I had the impression of a smart man with broad interests who wanted the Lab to reflect a wide range of psychology.

Farreras: Who did you work for?

Blough: I had no immediate supervisor. Virgil R. Carlson was acting head of the Section on Perception and Learning, where I was placed, but he was about my age and had little interest in or influence on my research. At one time we did both study the effects of LSD on visual thresholds, he in humans and I in pigeons. I don't remember his other perceptual research. The other member of the section was John Calhoun, whose research on many rat behaviors (sleep, eating, exploration, etc.) was, I thought, overly complex and ill-conceived.

Farreras: Did you interact or collaborate with any of the other scientists from the other Sections in the lab or with other labs?

Blough: I was acquainted with a few of the scientists in other sections (see below). I wish that I had interacted more with Mort Mishkin, but I was in the west end of the gigantic Clinical Center and he was away in some other building.

Farreras: At the time of its establishment, I believe the lab consisted of six sections: Aging, Child Development, Animal Behavior, Perception and Learning, Personality and its Deviations, and the Chief's Section. Do you remember who was working in those sections when you arrived? Did Shakow hire members for each Section or did each Section Chief hire its own investigators?

Blough: Shakow did the hiring in my case; he had a broad vision of the lab. Despite the nature of my research, he tried hard to persuade me to remain in the Lab when I decided to leave in 1958. The following may be of interest; it is quoted from a letter I wrote in November 1956, to a young Ph.D. who was interested in applying for a job in the lab: "I will tell you what I can, but I am afraid I cannot offer you much encouragement. Professional appointments in the Laboratory of

Psychology have in the past ranged from newly minted Ph.D.s to senior, established psychologists. The responsibilities and salaries have, of course, been dependent on experience and are generally in line with Civil Service policy. A recent Ph.D., for example, might get a Public Health Service appointment as Assistant Scientist at \$4817 per year, with promotion in a year to Senior Assistant Scientist at \$5718 (with in-service tax benefits). A civil service appointment would probably start at GS11, with a salary of \$6390. Once appointed to such a position, the appointee ordinarily engages in full-time research. Members of this Laboratory enjoy a large degree of freedom in their research. Unfortunately, the Laboratory roster has been pretty well filled out for the present. As a matter of fact, we have become quite crowded. Although I understand that one or two positions remain to be filled, I believe these are to be in lines of research other than the one in which you are interested. However, there would certainly be no harm in writing to Dr. David Shakow, Chief, Laboratory of Psychology, summarizing your interests and background.” The Perception and Learning Section was in the west end of the Clinical Center. Aging was somewhere nearby, I think. James Birren was an impressive person, working in the Aging section, and I also recall Jack Botwinick. The Section on Child Development was also there. I remember the head, Nancy Bayley, and my wife and I knew Harriet Rheingold fairly well, too. Harriet thought she could use a Skinner “baby box”, which had some notoriety at the time, in her research, so she asked us to try one out for us with our second son Steve, who had just been born. The “Skinner baby box” we tried out for Rheingold was basically just a heated and cooled enclosed

crib, with a mesh plastic floor on which the baby could, theoretically, go naked, although one can imagine the messy consequences of that. The model we used was very awkward to use; we concluded that an ordinary crib would be far better than any such box. I don't think Harriet ever used a baby box.

Farreras: What about issues of funding? Did the funding go to already established researchers or did it bring new people into the field? Did funding patterns direct the development of research and/or lead to scientific progress?

Blough: I could order, or have built, equipment that I needed, though I often had to wait a long time to get the equipment. There were funds for travel to meetings. Since my research seemed far a field from "mental health" this suggests that funding patterns were not steering research at that level. I don't know the larger picture. The following is from a letter I wrote in 1956: "I have been very satisfied here with the support I have had to do independent research. There are difficulties, of course, most of which are common to everyone here. The most pressing of these is scarcity of space, due possibly to Congressional forethought in providing more money for profession positions than space in which to put staff."

Farreras: Were there any specific mechanisms for stimulating and encouraging research?

Blough: The job of the people in the lab was to do research. I am not sure this needed to be encouraged. It seems that the directions of research were determined by who was hired, and they were left alone to do what they wanted after that. I wasn't there long enough, perhaps, to see programs change.

Farreras: What research did you conduct while you were at the lab and with whom?

Blough: I had little research interaction with others, which is one of the reasons I left the

lab. The kind of research I conducted is well represented by the papers in my vitae for the years 1955-1959. As one can see, they all concern pigeons, visual discriminations and sometimes drugs, and all are single-authored. I worked on the visual processes in pigeons, and eventually published one of my favorite papers, on the spectral sensitivity of pigeons. Don't ask me what this had to do with the mission of NIMH, but I guess it was within Shakow's vision for the lab. The use of drugs is the one influence I would attribute to my presence at NIMH. My interesting findings with LSD, for example, came from the explorations with that substance that were going on in the lab at that time. This was a long time before LSD became widely known and abused. One or more of my drug papers were sufficient to get me invited to give a paper at the New York Academy of Sciences and another at a conference in Italy, funded, I believe, by drug companies. The closest I came to formal collaboration was a proposed project to be conducted by me and Dr. Conan Kornetsky at St. Elizabeth's hospital on *The effects of various drugs on operant conditioning in the psychotic patient*. (July 1956). This never got off the ground, for what reason I don't remember.

Farreras: Please share any recollections and anecdotes of any events, research, or individuals during your time at NIH.

Blough: Perhaps the following says something about the state of administration of the Psychology Lab in 1954. When I arrived in August of that year, carrying my letter of appointment, it turned out that nobody knew I was coming. The fastest way I could receive any paycheck at all was to be signed on as a clerk-typist, so I took the typing test (I did pretty well) and was paid as typist for a month or two,

although I was actually busy setting up my lab. Another thing about the lab during the time that I was there was that, compared with a university, it took a long time to get things that were ordered. Most universities also have a user-friendly shop where one can go and build equipment. NIMH had only a professionally-run machine and electronics shop where, if one went to the trouble of drafting plans and waiting in line, often for weeks or months, something could be built. In the beginning I ran my lab with equipment and pigeons that Harvard let me bring in. I had one very nice piece of equipment built in the shop, did some work at home, and bought other items that I needed. The one built in the shop was a box containing an optical system and motor-driven optical wedge, used for *Spectral Sensitivity in the Pigeon* and one or two other papers. I had to leave it at NIMH, together with anything else purchased there. Government regulations at the time made it essentially impossible to remove any equipment - theoretically, items were placed on a list to be made available to many research labs, and then (how many months/years later?) put up for sale or gift on a surplus-property list. I am sure that item, plus other relatively specialized stuff that I and some others like me could have used, never found a proper home. Another anecdote concerns my removal of equipment that I owned, but couldn't easily prove I owned (mostly brought from Harvard). I took it to Brown by loading it into picnic ice chests (which also served as experimental chambers). These I placed on the curb outside one of the exits during the 5 PM rush, when all the security guards were directing traffic. I drove up in my car, loaded the ice chests, and drove off. (This equipment was standard pigeon operant stuff, like relays and mechanical timers,

long gone.) My pigeons were left behind; do you suppose they got to the surplus property list?

Farreras: You left NIMH in 1958 to become an Assistant Professor at Brown University. Could you expand on the reasons why you left?

Blough: I left largely because a University setting was attractive both for me and for my wife, who was ultimately able to join the psychology faculty and is now a Research Professor at Brown. Equally important, I wanted to teach as well as do research. Brown had an excellent reputation in experimental research and in teaching, and a very congenial faculty. I continued in much the same general line of research, initially started at Harvard. The main influence of NIMH was my drug-related research, but I did much less of that after I left. Two papers on research that I did at NIMH especially influenced my later research and that of others: *Spectral Sensitivity in the Pigeon* (1957) and *Delayed Matching in the Pigeon* (1959). At the time, another noticeable downside of NIMH was the bureaucratic nature of work for the government. This was more of an annoyance than anything else. I've mentioned the long time it took to order equipment, because of red tape. I also mentioned the impossibility of taking anything with me when I left. There were the usual stories: e.g. in one of the offices near our section there was a secretary who was totally impossible to deal with - rude, suspicious, resentful, etc. Anywhere else she would have been fired, but she had some seniority in Civil Service, and the rumor was that she kept getting recommended to other offices in order to get rid of her. But for one who just wanted to do research, NIMH was a very attractive place, within the relatively

minor limitations I've mentioned. I was fully supported, able to do research full-time, and essentially free to follow my own interests. That was really quite remarkable. I don't think all those factors would be found in many places.

*End of transcript*