

Harriet Rheingold

- Born 2/13/1908 in New York, NY; Deceased 4/9/2000
- Spouse – Don W. Hayne
- A.B. (1928) Cornell University, M.A. (1930) Columbia University, Ph.D. (1955) University of Chicago

Major Employment:

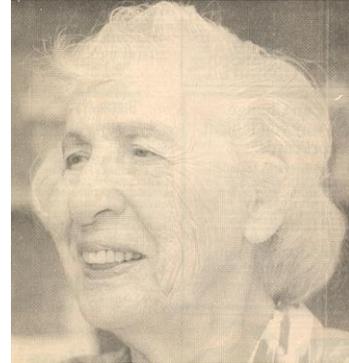
- University of North Carolina – 1964-1978, Research Professor
- University of North Carolina – 1978-2000, Professor Emerita

Major Areas of Work:

- Developmental Psychology
- Development of Social Behavior

SRCD Affiliation:

- Nominations Committee (1970), Chair for the Committee of Ethics (1970, 1978-1980), Long Range Planning Committee (1973-1976), Consulting Editor for Child Development (1957-1962)



SRCD ORAL HISTORY INTERVIEW

Harriet Rheingold

Interviewed by Lloyd Borstelmann
At Chapel Hill, North Carolina
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Borstelmann: Let's talk about how you became involved in child development?

Rheingold: All right. How I became involved in child development is not exactly clear to me, because in retrospect, I don't find various threads that could lead to such an interest, but my clearest picture is when I was working at the Institute for Juvenile Research. We were studying children of all ages with mental and emotional problems. And it turned out that I was more often getting the very small children and the infants. The infants were coming in for measurement before they were being placed in adoption. Now there were two characteristics probably, two experiences that gave me that competence that other members of the psychology team and psychiatric team at the Institute for Juvenile Research did not have; that was first, that by that time I had at least one child if not two, and second that I had worked with Arnold Gesell. Now when I worked with Arnold Gesell for a year at Yale University, I was not particularly interested in the work except very generally as the opportunity to put together various observations that Arnold Gesell made on the development of children at different ages. I was really much more interested in abnormal psychology and wanted very much to do research in a state hospital for psychotic patients, but the first job that came along after I got my Masters was this opening at Gesell's clinic. From watching Arnold Gesell and seeing the care with which he administered the examination to infants, and from years of remembering the careful measurements he made, and how we spent, I and some other researcher assistants, time in codifying the observations he made, that somehow gave me a background that the other psychologists at the Institute did not have. And, therefore, whenever a young child or an infant was to be examined it fell to my lot. I would say that gave me a special knowledge and understanding of the very young child, and coupled with that, my own interest and absorption in the development of my sons.

Then throughout the years that I taught at Rockford College I continued to see children, especially young children and infants, at a branch of the Institute for Juvenile Research that was held in Rockford, Illinois. Then when I returned to the University of Chicago to get my Ph.D., it seemed that an organism I was familiar with, that I could study, was the human infant. Of course, before that my research at the University

of Chicago was devoted to a study of the behavior of chicks. But I chose as my advisor, Helen Koch, and she was more interested in the development of children, and together we worked out my dissertation proposal, in which I was to intervene in the development of children at St. Vincent's in Chicago, in an attempt to improve their intelligence. Of course, if anyone reads that dissertation you will see that the dissertation really concerned the development of social responsiveness to familiar and unfamiliar persons in infants about the age of six months.

Before I go off on a different theme, I'd like to complete the thread I was exploring before this, that is, concerning my dissertation work with infants, and to say that about the time I was completing the study, Dr. David Shakow, who then had a position at the National Institute of Mental Health, in fact, he was the Director of the Laboratory of Psychology, said that if I were interested in continuing that research on infants, there would be a place for me at the Institute in his laboratory. And then from then on my whole career has been devoted to the study of children.

Now I want to back up and start with another theme and explore it. That concerns my career at Cornell University. I went there when I was sixteen years old. I had various scholarships and, in addition, I worked several hours a week. My interest then was in philosophy and it remained in that discipline for four years. I wanted to be a philosopher without really knowing what a philosopher was or would do. Of course with an interest in philosophy you take a course in psychology, it seems to me that would be natural. The first course I took in psychology was listening to Titchener lecture. That was in my junior year. Unfortunately, Titchener died and I have only vague memories of him. The laboratory work in psychology, which was a subsequent course, I found rather unrewarding. My memory of it is that we sat in the dark room and tried to explain, to describe in ordinary words such sensations as you might have in fingering materials of different textures; how is satin different from velvet. But there was another course in psychology that I found interesting, and that was the history of psychology, and a man named H.P. Weld talked about the different schools. Well, it seemed as though I ought to have something that I could do after I graduated, so I began to take courses in education. Fortunately in education there was Robert M. Ogden, who taught a course in psychology in education, and Frank Freeman, with whom I took an independent course, as I was able to do because of my good grades. And he gave me a copy of Terman's book on *The Measurement of Intelligence*. For the first time I saw then how you could put the study of behavior together with an interest in the development of human beings, and I found that book very exciting.

After graduation I obtained a position in a small high school not too many miles away from Cornell, actually near Elmira. We were only three teachers. I was one of three, and we had only thirty-five students. Yet, this was a New York State public high school. And my job was to teach Latin 1, Latin 2, Latin 3, biology (now see that began to get me close to behavior), art, and boys gym; that would seem to be an unlikely combination. I was struck then by the differences among my pupils and didn't know how to account for it. There was one student who had a problem with reading; there was another student who kept spelling words backwards. I decided I needed to know more about that. I attended a class nights at Elmira College given by a psychiatrist, and he said I should study further and go to Columbia, which I did, but somehow became interested in abnormal psychology rather than child psychology. I got my Masters in one year working with Harry Hollingworth, had to do a Masters thesis, and it was from there that I went to Gesell's Clinic, so then I pick up the thread that I was just talking about before.

After my year with Arnold Gesell, I was able to get an internship at Worcester State Hospital. For the record, I was paid thirty-five dollars a month, but had room and board, and laundry. And laundry was important, because being around the patients, we had to wear uniforms, but wearing uniforms was very convenient because you never had to think what you were going to put on the next day. The rule at Worcester State Hospital was such that the occupational therapists, psychologists, and all the young people who were studying mental disorders had to live on the wards with the patients. And I lived on the ward with the women schizophrenics, and I always have said that from that experience, I have now first-hand knowledge of schizophrenia and can diagnose it absolutely, accurately, at once. I'm not saying that I'm correct, but at least that is what I think.

Now I did not want to be a tester. Somehow it seemed to me that that was less important and more menial than doing research, which is rather amusing if you will consider that at the Institute for Juvenile Research

(or IJR), I did nothing but test and talk also to parents for eight years. I was assigned work on free association in disturbed and not disturbed mental patients, mostly men, and also in seeing whether music had a calming affect upon disturbed female patients who, to calm them in the first place were wrapped in cold sheets for many hours to avoid the use of drugs, which, of course, at that time were not as well developed as they are today. The greatest experience, or I should say one of great experiences, at Worcester State Hospital was getting to know David Shakow. He was not too many years older than myself, and himself had spent many years trying to finish his Ph.D. degree. But he was a person who had enormous interest in the young people who worked with him, and throughout my life he had a very inspiring effect, in that whatever I told him I was interested in or wanted to do, he thought was the most exciting thing that anybody could think of, and his eye always twinkled when he talked to me. This is my definition of a civilized human person.

I also met my first husband, who was a psychiatrist in training, and his next position after we were married at the end of my year at Worcester, we moved to New Hampshire State Hospital, where he served as a psychiatrist and I set up the first laboratory of psychology. The next two years do not need any reporting because I became pregnant and my son Paul was born at Massachusetts General Hospital. By that time we were in Boston. I had attended lectures at the Boston Psychopathic Hospital and had done some minor research with F. Lyman Wells, a famous psychiatrist.

I now pick up this story when I'm in Chicago where my husband is in private practice and Paul is growing up, and I see an opportunity to take a state Civil Service examination, to work at the Institute for Juvenile Research. It seemed to me with my Masters and experience, that that would be a position of interest to me. It was, and I stayed there for eight years. We were under the influence of psychoanalysis, nobody was kicking and screaming about it, it was accepted. It was our explanation for everything. All mothers were rejecting, and that was the cause of all the behavior problems of the children. One of the outstanding experiences at IJR was the position that psychologists held. If we were subservient to psychiatrists, I don't think we knew it, felt it, or experienced it, and we had as much authority to explain and to treat as any psychiatrist on our staff. I think it was understood that they held preeminence because of their medical degree, but in actuality there was very little difference.

One of the psychiatrists was Adelaide McFadyen Johnson, who was an alumna of Rockford College ninety miles away from Chicago, a woman's college, and she advised the president, Mary Ashby Cheek that I should be hired. It will interest anybody who reads this, and we're talking now about 1945, this is the end of the war, that Rockford College had not had a position for a psychologist. Psychology was taught by the philosophy professor and by an occasional course taught by Bruno Bettelheim who came out from Chicago. I accepted the position for several reasons: one, my husband at that time was in service, he was stationed in Panama, and though actually we were coming to the end of the war in the beginning of 1945, when I accepted the position, we did not know how much longer the war would go on. His office had been kept in downtown Chicago, but the building was sold. We had to give up the car because there wasn't gasoline, and I was working at IJR in another part of the city from my children and it was becoming more and more unpleasant. The chance of going to Rockford held for me the opportunity to live and work in what seemed to me a small town. Rockford is not a small town, but after Chicago, it seemed like that. I could be closer to the children. I could teach, which I knew I would enjoy, and we would do it until the war ended. Well, it happened the war ended very quickly. It happened that my husband liked Rockford and set up a practice there. For any young person who reads this, just contemplate, that up to this time I had only taught occasionally courses to medical students, and once I had taught a summer-school course at the University of Illinois down state, at Champaign-Urbana, and that course had been on the emotional problems of children. But here I am in Rockford, the only professor of psychology, wishing to set up a major in psychology, studying catalogs realizing that I had to teach at least eight different courses so that the students could qualify as majors in psychology. So you can imagine that I did a great deal of reading and studying.

Now I'm back to Rockford College where I taught until 1953. In the last few years in my teaching there, it became clear to me that the field of psychology was moving towards demanding the Ph.D. You have to recall that I was working all this time with only a Masters, and a Masters that I obtained in one year, and received in 1930, and here we are in the beginning 50's. There were two reasons why I wanted a PhD: one because I should have it – maybe there are three reasons. The second one was because of the rules of the

American Association of University Professors, AAUP, a person could not be advanced from assistant to associate professor without a Ph.D., and small as Rockford College was, and perhaps unknown as it still is, it had very high standards and followed the dicta of AAUP exactly. So if I were going to move ahead and be something more than assistant professor I needed a PhD. I said there was a third reason, but at the moment I can recall only those two practical ones. Of course, one always wants to know more. And I had been impressed all the years I was teaching at Rockford College, that psychology had moved way beyond what I had been taught in school.

Of the various universities that were possible, the University of Chicago seemed to me to have the best reputation, and by that time David Shakow was a professor there, and he encouraged me to apply. My children were grown to a point where it was possible for me to start graduate education. For the record, one needed to enter the University of Chicago as a graduate student in psychology. You had to have proficiency in two foreign languages, and you had to pass an examination in analytic geometry and calculus. I had never had more than geometry and trigonometry in high school, but there were enough books that one could obtain so you could learn it by yourself, plus a correspondence course in analytic geometry. All the calculus I know, apparently enough to pass the examination, I got out of reading books. As for the foreign language, which today is almost unheard of, by the time I got to Chicago they had reduced it to one foreign language. Well, before I was to enter as a graduate student there I had studied French, which I'd had one year of in high school back in Brooklyn.

Borstelmann: Brooklyn?

Rheingold: Yes, Brooklyn, Bay Ridge High. Sometime I'll talk about Bay Ridge High, but not now. I hadn't prepared German, because I knew German better than I knew French, but the first examination that came along was in French and I took that. And we had to pass it at the distinguished level. Just imagine those were the requirements for being selected for graduate study in psychology at the University of Chicago. It was a great course. It was a very anxiety-provoking situation because I was in my forties and the students were in their twenties, but, of course, I had been in psychology for many years, and I'd been teaching psychology. We had a pro-seminar course of instruction so that we had a different topic in psychology every week, and we had comprehensive examinations that went on for five days. We wrote from eight to five, five days of the week, before we were thought capable perhaps of going on for a Ph.D. One of the courses we had to take was Statistics, and the only Statistics I'd had back at Columbia was descriptive statistics and I knew nothing about inferential statistics. So when I came to the end of my first semester we took our first examination in Statistics a week before finals, which by the way is a very poor way to teach, and I got a twenty-eight on the examination. So I wouldn't take the final, because I'd never had a failing mark on my record. But by the second semester I began to understand what was meant by probability, and I went on to get an A, because I found Statistics one of the most exciting things that I had ever tried to master. I also had a course with Eckert Hess in ethology, and there did a research project with chicks, and found that very exciting, because I was able to treat the chicks as just one after another objects instead of having to work with complex human beings, and was able to assign numbers to the chicks, and to test them by random numbers; something I had never done before, and somehow that seemed to me the height of science. When I finished the comprehensives at the end of the first year, my advisor, I think that was Donald Fiske, who is still at Chicago, said I didn't have to take any more courses, I could go on to my Ph.D. dissertation research. I'm more thrilled now thinking about it than I was at the time. I appreciate it more now than I did then.

It might interest whoever reads this for me to put another theme in here before I go into behaviorism, that is, when I went to Chicago to obtain a PhD, I also had another incentive. Here I had been under the influence of psychoanalysis for many, many, many years and was quite a Freudian, never challenged any of the psychoanalytic doctrines, which is all the more surprising, because one with general intelligence of some order ought to question some of it. But after I had settled on my dissertation proposal, and we'll talk about that in a moment, when I was working under Helen Koch, I decided that I also had time to go into psychoanalysis with a man named French, first name eludes me at the moment, but I saw him I guess several days a week for months. At some point Doctor French said I was through, and I find it interesting now to report that the day I walked out of his office I left psychoanalysis behind me. Now that might be Doctor French's fault, my fault, psychoanalysis short-comings, but also at this time I had been indoctrinated

by two disciplines or concepts: Behaviorism (Skinner's theories) and Statistics. And the two things went together in my mind as to how you would conduct experiments. Helen Koch was the only woman --that doesn't matter, I write off her sex -- she was the only person among the professors at the University of Chicago who was interested in the development of children. Why was I going to study children? Well, I was experienced with children. I knew a lot about children. I had my own. I had followed their development with great interest and I was an experienced worker with children, little children. And I had the idea that the younger the organism the greater the effect that I could produce in the shortest possible time. I think my idea was to work in this orphanage, this institution, St. Vincent's downtown, just north of downtown Chicago on North LaSalle Street. It was filled with infants and children of all ages from birth on up to, I don't know, maybe four or five, and I was going to improve their intelligence because we were hearing about Spitz and how institutionalization depressed intelligence had led to feeble-mindedness or mental retardation. I set out going to St. Vincent's every day. Particularly I worked in the afternoon. I fastened on two or three infants and I would play with each of them for a couple of hours, and I'd do that the next day and the next day, and before I'd begin I had tested them. You have to remember that I knew how to test infants. I used Gesell's scale, so that was easy. And after weeks it made no difference. The children tested just the same. There was a perceptive and kind nun, Sister Anthony, with whom I continue to correspond every Christmas. She got interested in what I was doing and she said, "How about I give you a whole room of infants," and that was it. Of course, I had to go back to my committee, tell them what I was going to do. They picked at every little thing. You couldn't have imagined nastier people. At one time they put me into such a depression that for three days and three nights I read mystery stories and forgot all about the world that was facing me. Of course today, and as I will tell my students, and as I hope to write in my book, never again in your life will you have the advantage of so many intelligent minds trying to help you realize your dream. I don't think I want to go into my dissertation, but I want to say that I had two rooms of infants, one was mine, and I took care of those children from seven-thirty in the morning till three in the afternoon, five days a week for two months, and the other room of four infants were cared for by the nurses assigned to the room and the volunteers. There were many, many women who volunteered and came in and worked in this orphanage. And after I was all through with my measurements, never did increase their intelligence, and I had shifted then, the main interest was, "Will they be more or less friendly to a stranger if they become attached to one person?" And as you know, the infants who I cared for became, of course, more responsive to me on the tests I had devised than the children in the other room. Both sets of infants were tested blind, and the infants that I worked with were more friendly to a stranger than the other infants who'd been cared for by all the different strangers all the time. And after the study was completed, then I repeated it with another eight infants, and that was my dissertation.

I'm now talking about joining the Department of Psychology at the National Institute of Mental Health at the invitation of David Shakow. We were divided into groups, and my group was the one on early development, the Chief of which was Nancy Bayley. The other persons were Earl Schaefer, Dick Bell and myself. The great thing about NIH at that time, we are now talking about 1955, 1956 and I stayed there until 1964, seemed to be the possibility of doing anything that you thought was important. It wasn't that the money was flowing freely, you had to make a proposal and a sound plan that was evaluated, presumably very carefully, but there was the general attitude that not only were great discoveries being made down every corridor and around every corner, but that you too could do that, all you had to do was dream up some great plan. It was while I was at NIH that I was able to take a trip to Puerto Rico to study the monkeys with Stuart Altman, and see what I was interested then and continue to be interested in, what we call maternal-infant behavior, and particularly the effect of the infant upon the care, which he receives. Notice I go to the masculine pronoun out of habit. I was also at that time able to take three months, go to Bar Harbor under the aegis or directorship of John Paul Scott to study maternal behavior in the dog. As for my research, I continued to study types and kinds of maternal care and child behavior, did some work with Dan Berlyn. We never were able to publish it because we couldn't bring it to fruition, but he influenced some of my ideas with his interest in novelty and familiarity. As I look back over my research, I'm struck these days by my continuing interest in novelty and familiarity. And to me, one of the great things in the world is novelty, and this, of course, was of great interest to Dan Berlyn, and also one might recall that more recently, at least within my recent memory, I wrote a chapter for the Annual Review of Psychology on Learning as the Acquisition of Familiarity. And that goes back to my interest in the institutional infants' interest in a stranger versus a familiar person, a continuing interest that I carry with me to this day.

I think the great thing about the National Institutes of Health was the enthusiasm of the persons who worked there. The sense one got of there being no door closed to you, and of support and interest in anything that interested you. If it were not for my present husband's moving to North Carolina State University in Raleigh because of his interest in working with graduate students, prior to that he had been working with the U.S. Fish and Wildlife Service, I would still be at NIH. Don accepted the position, at first tentatively, but later seriously. For a year and a half he worked in Raleigh during the week, came home weekends, and I hoped he would hate the position because I was so fond of the National Institutes of Health. But he found it quite rewarding. Halbert Robinson, the late Hal Robinson, then at the University of North Carolina, was very much interested in setting up a major in developmental psychology, and when he learned about the quandary I was in, and the possibility that I might be coming down to this general area, he arranged for me to have a position at the University of North Carolina at Chapel Hill. So Don and I were both in the same system but in different universities, and separated just geographically, although we set up our residence in Chapel Hill.

Now I'm talking about the University of North Carolina at Chapel Hill, of which I can speak favorably. Halbert Robinson was interested in raising the I.Q. and improving the outlook of under-privileged poor children, children in this area at risk for later development, and he set up the Frank Porter Graham Child Development Center, which continues to the present time. After a few years Halbert and his wife Nancy left for the University of Washington, but it was Halbert's optimism and executive abilities that were responsible also for the present building that psychology occupies on this campus at Chapel Hill, namely Davie Hall, which was partially supported by the National Science Foundation. Under various chairmen, developmental psychology has always been looked upon with favor. I was able to get a Career Award, not a Career Development Award, but I think it's called a Scientist Career Award, a program that was terminated shortly after I received mine (which would have been about 1964 or 1965), because universities were complaining that professors who held those positions were more attached to the Institute which gave them the award than to the university. As a consequence, I was never a line item in the budget at UNC.

A couple of things that might be of general interest: first is, that my interest, my definition of developmental psychology encompasses the development of behavior in all organisms. I think you cannot mention an organism too lowly for me to be interested in its development. It seems to me there are principles of development that will hold across all species. I've never been able to demonstrate that, to explore it, but it remains a conviction that I have. My interests in research gradually were shaped by what we might call a very ordinary problem of the times. That is, I found it more and more unpleasant to attempt to modify children by the usual methods we had of conditioning. When you would explain to parents what you were trying to do, gradually it became more and more interesting to me to have the parent be part of the study I was conducting, then I was not into any ethical problems whatsoever, the parent was there in the room with me and the child. The parent could see what I was doing and I could explain what I was doing to the parent. And as I say, sometimes the parent became a partner in the research, and that seemed to suit the times and my needs to observe the principles of ethical behavior, which became more and more important to me, until the present time when I'm working on the Institutional Review Board at UNC, and also now at Carol Woods, and Chairman with my husband of the Research Committee, because groups in town and away in the medical schools and other disciplines at UNC and Duke are interested in studying the process of aging.

A great thing, characteristic of my years at UNC, has been the students with whom I've had the pleasure of working. And as I have often said, my big mistake was not keeping them more dependent on me. I wanted them to be independent, as I myself wanted to be, but if I had kept them more dependent on me maybe they still would be here and we all could be part of this great enterprise. Nevertheless, they continue to carry on what I consider their own work, but in much of it I can see -- I think I can see the influence, which I have had upon them. I would say that I've had a very fortunate career. I do not think that there has been opposition between my status as a wife and a mother, and my career. I've tried to say when I write about it, that these are activities that are combined, they're not in opposition; they each help the other. I don't think that I have ever experienced prejudice because of my sex. There was a time when as a woman I would be part of committees, study sections, conferences and so on, and I don't think anybody paid attention to the fact that I was a woman, certainly it seemed natural to me. In recent years, as my career ended, I was at times annoyed when I realized that I was chosen to be on this or that committee, or attend this or that

conference or study section because I was a woman, and I think that was demeaning. I'm talking about the quota system, which went into effect. I think that I would say, as I said to repeat myself, that my experiences have been fortunate and that I am optimistic and I see psychology changing under my eyes and I cannot think that that is anything to deplore. I think it will change because you have to remember that when I began we were talking about the Gestalt movement, and I've been through dozens of movements, and in the end we're studying the behavior – I don't want to say of human beings -- the behavior of organisms. That's the end of my story.