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If this were a clinical history rather than a brief sketch of a career, it would begin with my family, and continue to discuss in detail my relationships with my parents and two younger sisters as they changed in the course of my development. Except for stating that we were a close-knit family with a not unusual mixture of warmth and tensions and deficiencies, I shall confine myself to a few bare facts.

My parents were Pennsylvanians, both graduates of Dickinson College, where my father earned a Master's degree in history before he went to work for a large manufacturing firm in its Cincinnati office. My mother groped for a vocation after her graduation, tried teaching, began nursing training, then was called home because of her mother's illness. Five years after her graduation she married my father, and henceforward had no thought of a career other than homemaking. I was born late in 1913, and my sisters two and seven years later, respectively. In 1918 the family moved to Toronto, my father having been transferred to a Canadian branch of his company.

Later the foreign branches split from the parent company, and in due course he became president of his branch. Realizing that he was committed to a career in Canada, he became a naturalized citizen, as did I as a minor.

From the beginning it was assumed that we girls would go to college, for both of our parents placed high value on a good liberal arts education. My parents' expectations of outstanding academic achievement were reinforced by my eagerness to learn to read when I was three years old. It was my mother who located appropriate materials and got me off to a good start.

With this early beginning, both grade school and high school were easy. I thoroughly enjoyed learning, which posed no problems until high school when I pretended to be indifferent to learning in order to ingratiate myself with peers.

A regular event in our family life was the weekly trip to the library. We returned home with the maximum number of books that our five cards would allow. When I was fifteen and in my final year in high school, one of the books brought home was William McDougall's *Character and the Conduct of Life* (1927), which I read with great excitement. It had not previously occurred to me that one might look within oneself for some explanation of how one felt and behaved, rather than feeling entirely at the mercy of external forces. What a vista that opened up! I decided thereupon to become a psychologist.

This decision was half forgotten when I first enrolled at the University of Toronto in the fall of 1929. Because I was underage I had to enroll in the first year of the "Pass Course," and could not take psychology until my second year, but after an introductory course then my enthusiasm was renewed, and I transferred to the honor psychology course for my final three years. The honor course had a very intensive, comprehensive, structured curriculum, which I was privileged to explore with only four classmates.

I gobbled everything up with great enjoyment and shared in the messianic spirit that permeated the department-a belief that the science of psychology was the touchstone for great improvements in the quality of life. Although now this belief seems naive, we all firmly held it then, even in the midst of the Great Depression-and it has never entirely deserted me. Nothing that I studied seemed irrelevant, and no one who taught me failed to engage my interest in his or her field of expertise.

Before I graduated in 1935, I had already decided that I wanted to continue in graduate work in psychology, and it did not occur to me to apply elsewhere than Toronto. My parents went along with my wishes, although my father had previously thought that it would be nice for me to be a stenographer for a while before marrying. Although my undergraduate record had been excellent, I did not assume that I would be accepted, and was delighted when I was not only accepted but also offered a stipend of $200 for the year as a teaching assistant to the head of the department, Professor Edward A. Bott, in his introductory course for medical students.

If the stipend was low, so were other costs in the thirties. Moreover, I lived at home and my parents refused to let me pay board, which I could have done because from my second year of graduate work on I had quite adequate financial support through scholarships, teaching evening courses, and higher teaching assistant stipends.

I cannot recall how many graduate students there were in the department of psychology. My retrospective impression is that the sexes were evenly balanced, although more of the women than men were headed toward a terminal master's degree. Let me mention a few with whom I overlapped: Mary Northway, who studied for her doctorate at Cambridge with Bartlett, and returned to a position in the university's Institute of Child Study; Donald Snygg who was enthusiastic about phenomenology and subsequently became well-known in American psychology; Louis McQuitty , who went to Illinois after his Ph.D, and subsequently Michigan State, then Miami; Carl Williams, who after faculty appointments at the Universities of Manitoba and Toronto, became president of the University of Western Ontario; Gordon Turner, who had been a classmate in the undergraduate honor course, later went to the University of Western Ontario where he was chairman of psychology for some time; Mary Wright, who also wound up at the University of Western Onario and also served as chairman there; Herbert Pottle, who eventually became Minister of Education for Newfoundland; and Nora Weckler, who is a member of the faculty of California State University at Northridge.

To me it seemed to be a tightly integrated group of graduate students. We had tea together in the late afternoon in the graduate lounge, and planned numerous social events, including an annual picnic with faculty that featured a softball game, and an annual Christmas party with skits and songs prepared for the occasion. I remember our morale as being high.

During the seven years of undergraduate and graduate studies in the department of psychology, three professors emerged as major influences: Sperrin N. F. Chant, William E. Blatz, and Edward A. Bott. Chant spent a year with our fourth-year undergraduate class in an experimental project involving the galvanic skin response. I owe to him chiefly the discovery that research can be fun.

When I decided to enter graduate work, he immediately offered to supervise my master's research, and we proceeded to investigate the relationship between attitudes and emotional response, using the GSR as the measure of emotion (with Chant 1937). Whatever skills I now have in supervising the theses and dissertations of graduate students owe much to the supervision he offered.

Blatz's influence also began while I was an undergraduate. I was impressed by his courses in genetic (developmental) and abnormal psychology, in which he talked of his own theory of development--security theory .This was a programmatic theory that owed much to Freud-although Blatz was careful not to acknowledge this because of the very strong anti-psychoanalytic bias in Toronto at that time. It was a theory of personality development, and that was what I had been waiting for! I was honored when, having completed my master's thesis, Blatz proposed that I undertake my dissertation research within the framework of his security theory.

In briefest summary, his position was that in infancy and early childhood the individual needed to develop a secure dependence on parents in order to gain the courage necessary to brave the insecurity implicit in exploring the unfamiliar world and learning to cope with it. A child needs to feel confidence in the secure base provided by parents to learn the skills and to develop the knowledge that will gradually enable him or her to depend confidently on self and eventually to gain a secure emancipation from parents.

However, since it is impossible in this social world to be totally independent of others, the immature dependent security of the relationship with parents should be gradually supplanted by a mature secure dependence on peers from the individual's own age group and eventually on a heterosexual partner, implying a relationship in which each partner finds security in the skills, knowledge, and emotional support contributed by the other. Of course, some persons are characterized by more insecurity than security from all three sources combined, and some rely on defensive maneuvers {which Blatz termed "deputy agents") to hold their insecure feelings at bay.

Blatz's proposition was to develop instruments to assess the balance between security {from all three sources) and insecurity/defensive maneuvers in all major aspects of a person's life. My dissertation research was to be devoted to the construction of two self-report pencil-paper scales to assess young adults regarding their relations with parents and with age peers {1940).

There was at that time no approved quantitative technique for selection and weighing of items on such scales, but with Chant I settled on a roughly satisfactory method. Validation of the scales also presented a problem. This was handled by using the same college student subjects who had written autobiographies for a course of Professor J. Davidson Ketchum's. I found no way of quantifying the autobiographical material, but it "blew my mind" to find out how similar this material was for persons yielding the same pattern of scores on the two scales. I emphasize patterns. I have been searching for-and finding-patterns in my research ever since.

The third person who exerted a profound influence on my career development was Professor Bott, the head of the department. I continued for four years as his teaching assistant, not just in his introductory course, but soon in two experimental/statistical courses that he taught. As a graduate student, I attended his seminar in systematic psychology , which he conducted in a Socratic manner. Like many other Toronto Ph.D. students, I failed to appreciate at the time the significance of Bott's thinking, for his manner was dry .But in later years I came to realize what an important influence he had been in the way I viewed the scientific approach. I believe that science is a "state of mind"-not that he ever said just that. Science is implicit in the way one thinks about problems and approaches data, rather than being irretrievably vested in the hypothetico-deductive method, experiment, or specific quantitative techniques.

Since I later became a clinical psychologist I should mention that I did indeed think of becoming one during my first year as a graduate student. Through the help of one of the faculty , C. Roger Myers, I was appointed as a psychological intern at the Ontario Hospital in Orillia in the summer of 1936. Discouraged by the seeming impossibility of helping effectively the mental patients served by this hospital, I abandoned thoughts of a clinical career in favor of research relevant to personality development.

In the spring of 1939, Professor Bott did what he could to find jobs for all of the new Ph.D.'s, arranging job interviews with potential employers. I did not want to leave Toronto. I was in the full flush of excitement about my Ph.D. research based on Blatz's theory of security. Blatz himself wanted me to stay to become co-director of an expanded program of security research. In blissful ignorance of the academic facts of life, including such matters as vacancies, and tenure-line slots, I told Professor Bott that I wanted a faculty appointment in his department and did not want to go elsewhere.

Nevertheless he arranged an interview for me with Professor George Humphrey, then head of the psychology department at Queens University in Kingston, Ontario, who was searching for a young experimental psychologist to establish a new laboratory. Because I did not want the position I downplayed my skills and abilities and was honest in my admissions of weakness. Perhaps because of this openness, Humphrey decided that I was precisely the person he wanted for the job. Two weeks later he visited again to tell me sorrowfully that the Senate of Queens University refused to appoint a woman. This is the only instance of discrimination in regard to employment that I encountered in my career. As for Humphrey and Queens, everything turned out well, for they hired Donald Hebb. Even then I believed that they made the better bargain. As for me, my naive faith was rewarded by an appointment as lecturer at the University of Toronto, beginning in the fall of 1939.

Great Britain {and Canada) declared war on' Germany in September 1939, and everyone's career plans were changed. Professor Bott immediately threw himself into plans for maximum contribution by Canadian psychologists to the war effort, and turned over all of his undergraduate classes to me with scarcely two weeks' warning. Within a year Professors Bott and Myers had left to become advisors to the RAF in regard to pilot selection, Line to head personnel selection in the Canadian Army, Chant to do likewise in the RCAF, and Blatz to set up model wartime day nurseries in Great Britain.

Male graduate students and recent Ph.D.s joined the armed services in personnel selection, and several members of the staff of the Institute of Child Study joined Blatz in England. Several recent female Ph.D.s, including Magda Arnold {who subsequently became well-known for her work in the field of emotion), were appointed to help carry on the work of the department. I remained for three years, and then no longer content to be away from where the action was-and belatedly wanting to get away from home joined the Canadian Women's Army Corps in July 1942.

I spent that summer in basic and officer's training and then was tapped by Bill Line-then Colonel William Line, Director of Personnel Selection-to become an Army Examiner, as the personnel selection officers were called. In this capacity I was posted to the Canadian Women's Army Corps Training Center at Kitchener, Ontario. The work had a distinctly clinical flavor, including administering tests, interviewing, history-taking, and counseling, as well as recommending placement. I was impressed with how much could be learned from such an approach and entertained the idea of becoming a clinical psychologist at war's end in order to pursue more effectively my interest in personality development. A few months later, however, I was posted to headquarters in Ottawa, as the CW AC advisor to the director of Personnel Selection. There I encountered the reverse of sex discrimination, despite the fact that CW AC pay was four-fifths of men's pay. After arriving at Headquarters as a second lieutenant, I was promoted to majo within a year.

The directorate of Personnel Selection was under the director general of Medical Services-a psychiatrist, General Brock Chisholm, who subsequently became director of the World Health Organization. He and Line shared the idealistic perspective that personnel selection should be a thoroughgoing clinical service to army personnel with the Army Examiners working closely with company officers on the one hand and with the various professionals of the Medical Corps on the other-physicians, psychiatrists, and social workers. So acute was the manpower shortage, however, that the need for infantrymen frustrated the male Army Examiners in their efforts to place men in the work to which they were best suited. But Line's goal was feasible for the women in the service, and it was my job to work with CW AC Army Examiners all over Canada to see that the goal was approximated. My work was entirely administrative, entailing much traveling back and forth across Canada. In the winter of 1943-44 I was assigned to a four-month tor in England, where I especially enjoyed visiting the Personnel Selection service of the British Army. My opposite number there, Senior Commander Edith Mercer, not only made me welcome at that time, but also later played an important role in my career .

After V-E Day, I was invited to retire from the Army to become the superintendent of Women's Rehabilitation in the Department of Veterans' Affairs. The director general of Rehabilitation Services was my previous mentor, Sperrin Chant, although he was about to leave to become head of psychology at the University of British Columbia. Doubtless he thought that I was set up for a continuing administrative career in government service. The work was significant and demanding, but much like what I had done in the Army. Within a year I felt that I had done all that I could to set up the women's rehabilitation program and was tired of administrative work. When Professor Bott, back at the University of Toronto, invited me to return as an assistant professor, I accepted with pleasure and anticipation. Nonetheless I valued highly my four years in army and government service. I came to value a clinical perspective. I learned a great deal about administration. I learned to value and to work within a multidisciplinary persective.

Upon returning to Toronto, I taught introductory psychology to medical students and experimental psychology as before-a twelve-hour teaching load, made light because of it being my first year. The problem was what to teach, especially to graduate students, the following year. The courses I especially wanted to teach were being dealt with very competently by Magda Arnold, whom I admired. Knowing that it was uncertain whether her war-time appointment was to continue, I wanted to propose nothing to Professor Bott that might encroach on her territory .I asked her how we might share teaching in the personality area. Since she was teaching the theoretical courses, she suggested that I teach personality assessment. I protested that I knew nothing about this specialty, but she replied "You can learn, can't you!" Therefore, at her suggestion, I attended a summer workshop in the Rorschach technique directed by Bruno Klopfer, and contacted WiIliam Henry of the University of Chicago for references to his work on the Theatic Apperception Test. I read aIl that I could lay my hands on relevant to both projective and paper-pencil tests, practiced administering these various appraisal techniques to volunteers. I offered my volunteer services to a Department of Veterans' Affairs hospital, where at least I received neuropsychiatric supervision from the clinical director. And that is how I began as a clinical psychologist.

Next autumn, 1947, I offered a graduate course in personality appraisal, and it captured the interest of my students as weIl as engrossing me. The foIlowing summer I attended another K\opfer workshop, this time at the advanced level. I prepared a mimeographed booklet for the use of my students-to fiIl in the gaps that I observed in The Rorschach Technique by Klopfer and KeI\y (1942), which was distributed by the university bookstore.

Magda Arnold departed for an appointment at Loyola University in Chicago, and I fell heir to the courses of hers that I had coveted-emotion and motivation and theories of personality .And throughout the years 1946- 50 I was co-director, with Bill Blatz, of a research team focused on developing scales to assess security in various aspects of life-a clear sequel to my dissertation research.

I became engaged to marry one of that team-a veteran student, Leonard Ainsworth, who was just completing his master's degree. The prospect of his continuing for a Ph.D. in the same department in which I had a faculty appointment seemed uncomfortable, so when he was accepted by University College, London, as a doctoral student it was there that we went after our marriage in the summer of 1950. Len had DV A educational benefits and expected to be able to pick up the same kind of teaching and research assistantships there had been available to him in Toronto, although that proved not to be the case. My efforts to line up a position for myself in advance proved to be unavailing. But I thought that I might write a book. Since my mimeographed manual on the Rorschach technique had unexpectedly sold hundreds of copies, a book-length version of it seemed worth considering. I wrote to Bruno Klopfer seeking his approval, since it was his version of the technique that I proposed to write about. He replied with an invitaion to be a co-author in a book he was planning. So we set off for London in September 1950, with high hopes but inadequate financial resources. As it turned out, I did collaborate with Bruno and Walter Klopfer and Robert Holt in Developments in the Rorschach Technique, volume 1, conducting all our exchanges by correspondence. The book was not finally published until 1954, so it did not help at all with our immediate financial needs, but royalties have been steady ever since.

Upon arrival in London, I immediately cast about for a job. I also looked up relatives and friends, including Edith Mercer from my army days. One day she drew my attention to an advertisement in the London Times Educational Supplement for a job that seemed precisely suited to my qualifications. It was for a research position at the Tavistock Clinic in an investigation, directed by Dr. John Bowlby, into the effect on personality development of separation from the mother in early childhood. I applied, was interviewed, was enthusiastic about the project, and was hired. So Edith Mercer and a newspaper advertisement reset the whole direction of my research career .

Psychologists do not ordinarily expect crucial research to stem from a psychoanalytic setting, so it may seem paradoxical that it was at the Tavistock Clinic that I finally realized what kind of research strategy would best serve me in exploring the problems of personality development in which I had been interested from the beginning. First, the clinical perspective tends to emphasize patterns of personality or behavior (syndromes) as they relate to patterns of antecedents, rather than searching for a one-to-one cause effect relationship between a single antecedent and a single outcome variable. Second, James Robertson, one of my new colleagues, had been observing at first hand the responses of young children to separation from and reunion with their families in the course of visits to the home before and after the separation and to the separation environment to observe the child's responses. Although he himself was very modest about his data-transcriptions of his observational notes-I was deeply impressed wth their value. I was entranced with the prospect of a future study of my own in which I would employ simple, direct, naturalistic observation, and use simple descriptive statistics to deal with its findings.

Third, both the problems in which John Bowlby was interested and his nondoctrinaire approach to theory were very congenial to me. To be sure, Blatz had-been .theory-oriented, but my experience with Bowlby was my first with a theory in the making. John became increasingly interested in the implications of evolutionary theory and the ethological approach in accounting for the findings of separation research - findings that could not be accounted for adequately by either psychoanalytic theory or psychological learning theory .Although I was intrigued with Lorenz's imprinting studies, 1 myself was so brainwashed by psychological theories of the day that I felt uneasy. To me at that time it seemed self-evident that a baby becomes attached to his mother because she fulfills his basic needs or drives. Indeed, after I left London, John and I had an exchange of correspondence in which 1 urged him to reconsider his new theoretical position. He may have reconsidered, but fortunately he was not deterred by my reaction.

My husband, who was completing his Ph.D. in the autumn of 1953, had been talking about how much he would like to go to Africa. Again it was our friend Edith Mercer who drew his attention to an advertisement in the London Times for a research psychologist in the East African Institute of Social Research in Kampala, Uganda. I was not enthusiastic about this prospect, fearing that it would be even more difficult to break into the academic stream in Canada or the United States after such a venture than before it. Nevertheless, Len's application and interview resulted in an appointment. We had scarcely arrived home in Canada when the news of the appointment reached us, and on New Year's Day, 1954, we sailed from Halifax, bound for London, and then Mombasa and Kampala.

Although I had it in mind to undertake a short-term longitudinal and naturalistic study of mother-infant interaction at the first opportunity-and now the opportunity was in Uganda-I was unsuccessful in obtaining funding from such a distance and at such short notice. I was happy that Dr. Audrey Richards, director of the institute, scraped together enough salary for me and for an interpreter to make such a study feasible. The study of Ganda mother-infant dyads turned out to be every bit as rewarding as I had hoped a short-term longitudinal, naturalistic study could be. 1 welcomed Dr. Richards' directive that there be an anthropological component to the study, for this ensured that I would view current mother-infant interaction and maternal care practices in their cultural context, and I valued the opportunities presented by the institute again to interact with a multidisciplinary team.

It is a pity that one cannot require field work in another society of every aspiring investigator of child development. Despite all the language and other difficulties, I am convinced that it is easier to be objective when viewing another society , and then, as I discovered later, it is easier to take a fresh, unbiased view when later undertaking research in one's own society. Despite many cultural differences, it was a profoundly moving experience for me to perceive the basic common core of parental concern for their children's welfare. Furthermore, I had not spent many week in observation before I was convinced that the previous "self-evident view" of the basis of an infant's attachment to its parents squared not at all with what my eyes saw, and that Bowlby's new ethological approach did indeed provide a much more useful framework. I am sorry that I did not immediately inform him of my *volte face*.

For complex reasons, the analysis and publication of the Ganda data was substantially delayed (1963, 1967), but perhaps a few reflections are pertinent here. The hypothetico-deductive method that has guided so much psychological research is inapplicable to the kind of exploratory , naturalistic study that I undertook in Uganda. To be sure, one needs to have some notion of what one is looking for, and hence some selectivity of observations-and indeed I did have some such notions. But I left myself open to observe and descriptively record as much as possible beyond these initial notions, rather than boxing myself in with check lists conceived a priori. I learned so much new that was not covered by my initial notions (hypotheses) that ever since I have tried to avoid deciding in advance what the relevant variables must be and how I am going to analyze my data. I let the raw observational data suggest to me what the relevant variables are. In exploratory studies post hoc variables may well be the most valuable.Whereas I acknowledge that later replicatory studies of a more rigid kind are needed, for hypothesis-discovering studies, too rigid an adherence to the hypothetico-deductive approach is clearly counterproductive.

At the end of our two-year tour in Uganda, it was not easy to find jobs in Canada or the United States from our Kampala base. Acting on the assumption that it would be more difficult for Len, with his relatively new Ph.D., to get placed than for me to do so, we put the emphasis on the position for him. With the initial aid of the APA Employment Bulletin, he found a position as a forensic psychologist in Baltimore. It was not until late 1955 that our visa arrangements were completed and we were settled. I then began my explorations for a job by visiting the chairman of the department of psychology at Johns Hopkins University, Wendell Garner. Extrapolating from Professor Bott's intimate knowledge of opportunities in Ontario, I expected Garner to be knowledgeable about opportunities in, the Baltimore area. He did offer me an evening course (which I snapped up), and made several suggestions about possibilities for full-time jobs in the area. I began to follow up his suggestions, but within two weeks Dean Wilson haffer of Johns Hopkins called me in to offer me a position. It emerged that he and Garner had been hoping to find someone to offer some clinical-type instruction in an otherwise highly experimental department, and to provide supervised clinical experience to a few students who wished it. There was no ready-made slot for such a person, but they patched up a position for me, supported in part by the department, in part by the evening college, and in part by Sheppard and Enoch Pratt Hospital, where I was to work two days a week providing psychological service with the aid of one graduate student assistant. I jumped at the opportunity to join the Hopkins faculty , even though I was disappointed to be appointed as a mere lecturer. Paradoxically, it was this academic appointment that gave me my first extended opportunity to gain clinical experience. The work at SheppardPratt was essentially diagnostic evaluation, and I had no difficulty with this.

I have never been able to understand why American clinical psychologists have so chafed at the diagnostic role, feeling that this limited them to being mere psychometricians, subordinate to all medical personnel. On the contrary, I found that diagnostic skills gave me very substantial status and respect. In addition to a quite heavy hospital load, I gradually set up a part time private practice, on referral from psychiatrists, psychoanalysts, social agencies, and schools in the Baltimore area, being mostly concerned with children. In the beginning, my research experience at the Tavistock Clinic was especially useful. At that time there was almost no literature pertaining to diagnostic evaluation of children. It was necessary to extrapolate principles from adult evaluation to work with children, and of course research experience with disturbed children was very helpful.

Our marriage came to an end in the summer of 1960. Although I do not wish to write about this personal disaster, I can say that I do not believe career conflicts to have been a factor. A depressive reaction to divorce led me to seek professional help, which culminated in an eight-year psychoanalytic experience. Sometimes I believe that this was the most important positive influence on my career, despite the fact that I had already been very fortunate in both mentors and turns of fortune. Certainly analysis helped me to become very much more at peace with myself and very much more productive.

I felt a great urge to immerse myself in the psychoanalytic literature, especially Freud. I emerged with a profound respect for psychoanalytic therapy, and with a firsthand understanding of the psychoanalytic process-unconscious processes, repression, transference, resistance, and the like-experience that has made me a better psychologist, even though there remains much in classical psychoanalytic theory that I believ~ to be obsolete, especially instinct theory and metapsychology. All of this both enriched my teaching of courses focusing on personality and various approaches to assessment and my understanding of research data.

The Sheppard-Pratt responsibility left very little opportunity for research. All that I could do was to work on the data analysis and publication of research from previous years and settings. I proposed to Garner, my chairman and good friend, that I withdraw from the hospital commitment, shift my teaching to developmental psychology, and begin the naturalistic, longitudinal research into mother-infant interaction that I had been longing to do ever since leaving Uganda.

He readily agreed, and indeed both then and since could not have been more encouragingly supportive of what I wanted to accomplish. So in 1961 the shift was implemented.

Let me interrupt this narrative to mention the degree to which I experienced discrimination. It must be clear that I had experienced none in regard to appointments since 1939, and that for a position that I did not want. It was otherwise in regard to salary at Johns Hopkins. My low initial salary was understandable because the appointment was not to a "tenure-track" slot; I had to wait only two years before being appointed associate professor, but it took a very long time to overcome the initial salary handicap. Three chairmen in a row recommended me for annual increments designed to bring my salary to the level appropriate to my age, experience, and contribution; year after year these were cut back, and it was clear that the difficulty was sex-linked. It was not until Hopkins faced the pressures of affirmative action that the situation was rectified, and then only after I wrote a strong letter to the Dean.

It rankled also that at noon the Johns Hopkins Club relegated women to a separate dining room for lunch, so that female faculty could not meet members of other departments in the normal way. The House Committee felt that it would be offensive to the sensitivities of the gentler sex to encounter male faculty in informal garb at lunchtime, not seeming to recognize that they encountered their male colleagues in the same garb in their departmental interactions. It was not until late in 1968 that this ridiculous restriction was lifted.

Soon reverse discrimination set in! Suddenly the few women faculty members were in great demand. Every university committee had to include a woman. We were very overworked, and this situation still continues in many universities. At Hopkins I was eventually elected by the faculty to the Academic Council-the body responsible for advising the administration on matters of academic policy, appointment, promotion, and tenure. I could detect no signs of discrimination against women in these matters in this council, nor could I do so later when I moved to the University of Virginia, neither in the department of psychology nor during the year when I was a member of the dean's Promotion and Tenure Committee.

I find it difficult to write about my major project-the short-term longitudinal research into the development of infant-mother attachment that I launched at Johns Hopkins in 1962. There is too much to be said, and much of it has already been said in piecemeal publications. This research has turned out to be everything that I had hoped it would be, and it has drawn together all the threads of my professional career. I opted for direct observation in the natural environment of the home supported by a specifically designed laboratory situation-the strange situation. The combination of the two highlighted the importance of observing in various contexts if we are to understand infant behavior and development-a lesson that was also implicit in the cross-cultural comparisons I made with the findings of the Ganda study. Indeed there are many other areas of science in which the mutual feedback between observations in the natural environment and observations in the laboratory yield more understanding than observationsin either context alone.

At the Tavistock Clinic I heard the dictum "no research without therapy." In no position to give therapy and not wishing to deliberately intervene, I adapted this dictum in both Ganda and Baltimore studies to a principle of not attempting to take data away from participants without giving something appropriate in return, and I took some pains to find what would be most appropriate in each instance.

Visits were made to the home at three-week intervals from three to fifty-four weeks after the baby's birth, each visit lasting about four hours, which resulted in about seventy-two hours of observation for each infant. These long, frequent visits had several clear advantages. The mother could be more easily induced to behave as usual and to follow her normal routine. We attempted to span all aspects of a baby's day, although we could not cover nighttime hours. Frequent visits made up for the inevitable variability of behavior from day to day, so that measures could be used that combined the findings from four visits together, thus making for more stable measures without unduly sacrificing the picture of developmental changes. Seventy-two hours provided a broad data base. Finally, we got to know our families very well, which helped enormously in the identification of possible variables that might be involved in individual differences, and that could then be put to a systematic test.

The evolutionary-ethological orientation provided by even the earliest formulations of Bowlby's theory of attachment (e.g. , Bowlby 1958) proved indeed to be helpful. It, as well as my experience in Uganda, suggested behaviors and possible "activating and terminating" situations that we wanted to be especially alert to when they occurred. On the other hand, I tried to keep our observations as open and comprehensive as possible to maximize the chances of finding new hypotheses about how behaviors become organized together and linked to situations. Thus, although benefiting from theory-based expectations, we felt free to undertake post hoc analyses of data.

From the beginning emphasis was on understanding the variables involved in individual differences as well as on learning about the normative course of development-as might be expected of a clinician. Finally, although I had for a long time deliberately put my Blatzian orientation and research aside in the interests of making a fresh start with a new approach, I was eventually delighted to realize that there was a striking congruence between the old and the new, and especially in the phenomenon of an infant using his attachment figures as a secure base from which to explore the world. Furthermore, the pattern approach that I had found so useful in my Ph.D. dissertation emerged as the obvious way in which our new data could be ordered to describe qualitative differences in infant-mother attachment.

Soon after I shifted my academic field from clinical to developmental psychology increased numbers of graduate students began to seek me for a supervisor. The ongoing longitudinal research project provided a convenient focus for an aspect of their research training. I have been very fortunate in the associates and students who collaborated with me, and indeed the success of the project owes very much to their time, efforts, and ideas. Of those who remain primarily in academic teaching and research, I want to thank: Mary Main, who is now an associate professor at the University of California, Berkeley; Everett Waters, who is now at the State University of New York at Stony Brook; Mark Greenberg, now at the University of Washington; Inge Bretherton, at Colorado State University at Fort Collins; Rob Woodson, at the University of Texas at Austin; Sally Wall, who teaches at Towson State University in Maryland; and Michael Lamb, professor at the University of Utah, who was with me at Hopkins for only one year. It s perhaps not surprising that even more of my ex-students and ex-associates have moved from research into clinical applications, although they are still known chiefly for their research publications: Silvia Bell, Donelda Stayton, Robert Marvin, George Allyn, Alicia Lieberman, Russel Tracy, and others. Mary Blehar, with whom I have co-authored several publications, has been at NIMH for some years. Barbara Wittig, whose sensitive observations were crucial to my Baltimore project, was a clinical psychologist before working in the project, but has moved into other fields of endeavor. Including also the undergraduates in my project who contributed through laborious coding and the undergraduates in my courses who have gone on to careers in psychology , psychiatry , or pediatrics, I although childless find myself to have a large academic family-dear to me and very gratifying.

As a developmental psychologist at Johns Hopkins, and a relative newcomer to the American scene of development research, I suffered the disadvantages of isolation, as the only one of my kind in the department. Nevertheless, "support systems" soon became available. The friendship and encouragement of my colleagues Wendell Garner and James Deese were significantly helpful; although in the experimental tradition they valued the kind of research I was doing. The Society for Research in Child Development w~s important to me, both because of its meetings and because of the between-meeting contacts with friends I made through the society .My long distance interaction with John Bowlby had never lapsed, but from 1960 on it picked up impetus when we realized that our thinking had developed along extraordinarily similar lines. Ever since, we have functioned as partners in attachment research and theory .The renewal of contact with John led to my inclusion in the Tavistock Mother-Infant Interaction Study Group which estblished a basis of communication with leading developmental scientists of various nationalities and disciplines. For a long time this combination of resources functioned very well for me.

But Garner left Hopkins for Yale, and later Deese left for the University of Virginia. With their departure I felt that I had lost effective intra-departmental encouragement for my approach, and began to feel restive. In due course I accepted a position at the University of Virginia, beginning in the fall of 1975. The Virginia department included a number of other developmental psychologists; communication with them has been a significant feature in my enjoyment of this new and congenial milieu.

Finally, I would like to consider the relation of my research contribution to the women's movement. By some it has been viewed as a stroke against women's liberation, since it has highlighted the importance of sensitive responsiveness to infant behavioral cues on the part of the mother figure and the desirability of continuity of the infant's relationship with that figure, unbroken by separations that are unduly long or frequent. It has been assumed that I believe in full-time mothering during the child's earliest years, and indeed this does seem to be the most usual way of ensuring adequate responsiveness and continuity .I acknowledge that satisfactory supplementary mothering arrangements can and have been made by a not inconsiderable few. Had I myself had the children for whom I vainly longed, I like to believe that I could have arrived at some satisfactory combination of mothering and a career, but I do not believe that there is any universal, easy, ready-made solution to the problem.

I have sometimes been accused of being out of touch with current changes in life-styles, but I believe that the problem is that infants are perhaps a million or so years out of touch with them. Their inbuilt evolutionary adaptations tend not to match new life-styles, much as we would like to believe infants to be infinitely adaptable. In a sense the traditional role of women is also tied to evolutionary considerations. The child-bearing and -rearing role is so essential to the survival of the species-and has for so long absorbed women's energies-that it is small wonder that women have been constrained to that role over many millennia.

Now, however, it is clear that the human species has been too successful in that the world is overpopulated; at least some women have been relieved of their age-old responsibilities for at least some period of their lives. There seems little reason to doubt that the intelligence and dedication that women have devoted to their traditional role can now, when not required by that role, be channeled elsewhere without undue hindrance.

REFERENCES

Ainsworth, M. D. S. See Representative Publications.

Bowlby, J. 1958. The nature ofa child's tie to his mother. International fournal of Psychoanalysis, 39:350-73.

Klopfer, B. and D. M. Kelly. 1948. The Rorschach Technique. Yonkers-on-Hudson: World Book.

McDougall, W. 1927. Character and the conduct of life. London: Methuen.

Representative Publications by Mary D. S. Ainsworth

1937. [Salter] With S. N. F. Chant. The measurement of attitude toward war and the galvanic skin response. Toumal of Educational Psychology, 28:281-89.

1940. [Salter] An Evaluation of Adjustment Based upon the Concept of Security. University of Toronto Studies, Child Development Series, no.9. Toronto: University of Toronto Press.

1953. With J. Bowlby. Research Strategy in the Study of Mother-Child Separation. Paris: Courrier de la Centre International de I'Enfance.

1954. With B. Klopfer, W. F. Klopfer, and R. R. Holt. Developments in the Rorschach Technique, vol. 1. Yonkers-on-Hudson: World Book.

1962. The effects of maternal deprivation: A review of findings and controversy in the context of research strategy. In Deprivation of Matenal Care: A Reassessment of Its Effects. Public Health Papers, no. 14. Geneva: World Health Organization.

1963. Development of infant-mother interaction among the Ganda. In B. M. Foss, ed., Determinants of Infant Behavior II. New York: Wiley.

1967. Infancy in Uganda: Infant Care and the Growth of Love. Baltimore: Johns Hopkins University Press.

1969a. Object relations, dependency, and attachment: A theoretical review of the infant-mother relationship. Child Development, 40:9691025.

1969b. With S. M. Bell. Some contemporary patterns of mother-infant interaction in the feeding situation. In A. Ambrose, ed., Stimulation in Early Infancy. London: Academic Press.

1971. With D. J. Stayton and R. Hogan. Infant obedience and maternal behavior: The origins of socialization reconsidered. Child Development, 42:1057-69.

1972a. Attachment and dependency: A comparison. In J. L. Gewirtz, ed., Attachment and Dependency. Washington, D.C.: V.H. Winston.

1972b. With S. M. Bell. Infant crying and maternal responsiveness. Child Development, 43:1171-90.

1973a. With D. J. Stay ton and M. B. Main. The development of separation behavior in the first year of life: Protest, following, and greeting. Developmental Psychology, 9:213-25.

1973b. With D. J. Stayton. Individual differences in infant responses to brief, everyday separations as related to other infant and maternal behaviors. Developmental Psychology, 9:226-35.

1973c. The development of infant-mother attachment. In B. M. Caldwell and H. N. Ricciuti, eds. , Review of Child Development Research, vol. 3. Chicago: University of Chicago Press.

1974a. With S. M. Bell and D. J. Stayton. Infant mother attachment and social development: "Socialisation:' as a product of reciprocal responsiveness to signals. In M. P. M. Richards, ed., The Integration of a Child into a Social World. London: Cambridge University Press.

1974b. With S. M. Bell. Mother-infant interaction and the development , of competence. In K. J. Connolly and J. Bruner, eds., The Growth of Competence. London: Academic Press.

1974c. With I. Bretherton. Responses of one-year-olds to a stranger in a strange situation. In M. Lewis and L. A. Rosenblum, eds. , The Origin of Fear. New York: Wiley.

1976. With R. "L. Tracy and M. E. Lamb. Infant approach behavior as related to attachment. Child Development, 47:571-548.

1977a. With M. C. Blehar and A. F. Lieberman. Early face-to-face interaction and its relation to later infant-mother attachment. Child Development, 48:182-94.

1977b. Infant development and mother-infant interaction among Ganda and American families. In P. H. Leiderman, S. R. Tulkin, and A. Rosenfeld, eds. , Culture and Infancy: Variations in the Human Experience. New York: Academic Press.

1977c. Social development in the first year of life: Maternal influences on infant-mother attachment. In J. M. Tanner, ed. , Developments in Psychiatric Research: Essays Based on the Sir Geoffrey Vickers Lectures of the Mental Health Foundation. London: Hodder & Stoughton.

1978. With M. C. Blehar, E. Waters, and S. Wall. Patterns of Attachment: A Psychological Study of the Strange Situation. Hillsdale, N.J.: Lawrence Erlbaum Associates.