

11/10/76.

ACKNOWLEDGEMENTS

The work which is reported in this volume could not have been achieved without the financial support and scientific leadership which came to us from the National Institute of Child Health and Development. The project which bore the title "The Early Ego Development of Children Blind from Birth" (Grant No. HD01-444) was mainly supported by NICHD during the period 1966-71. Leon Yarrow, then serving as a Health Sciences Administrator at NICHD, brought his own extraordinary gifts as a researcher to this administrative position, and within a rigorous peer review system, helped to create a climate in which diversity and even audacity in research design were accommodated.

It was our good fortune to do this work during a period of extraordinary ferment and far-reaching discoveries in the psychological study of infancy. To a very large extent these discoveries were made possible through government sponsored research. As the story began to emerge, early in the sixties, there was a growing consensus among investigators from diverse fields, that the human capacities for love and for learning were rooted in the first two years of life, the embryonic period of

personality development.

Our own study would examine the effects of a visual deficit upon early ego development and, by inference, the specific functions of vision in the organization of early experience.

The comparative data for sighted infants, especially in the area of human attachments, were emerging through a large number of studies during this period. Our own work became dependent upon the work of other investigators and our close communication with them, which brought immeasurable benefits to our own study and a joy in working together, cannot be fairly celebrated in the dry lines of an author's "acknowledgements."

Our indebtedness to these colleagues is very large. It is fair to say that this work could not have been accomplished without the continuous scientific exchange and the climate of mutual support and criticism provided by our colleagues in infant research. And, it must be added, that all of us, in turn, were indebted to the governmental agencies which made our work

and our scientific collaboration possible.

Nearly all of the leaders in infant psychology (and many of them were our consultants) were supported

in their work through one or another of the institutes in the National Institutes of Health. What NIH and its constituent institutes provided for government supported research was a kind of "laboratory without walls," to paraphrase Malraux. A typically modest research grant included a miniscule budget for "consultants" and a miniscule budget for "travel." A thrifty grant recipient learned how to use the continent's resources in his field with, perhaps \$1000. per year, and the aid of an imaginative travel agent. If each of a dozen grant recipients husbanded his funds, and traded services and lab visitations, he had the nation's experts in any of several related fields available for consultation and argument. It was as good as having the nation's best department or institute on the same campus. In our case this anonymous "institute," in a specialized field of infant development, was spread across the continent but represented, as a group, many authorities in this field who would never, by chance or munificence, have all turned up as residents in the same community.

Our meetings were informal and dealt with "work in progress." When we visited each other it was to exchange information and to borrow each other's expertness. We

shared raw data, problems of methodology, puzzles in results, and sometimes, victoriously, a solution to a puzzle. If an important finding emerged from one or another of these investigations we usually knew about it one or two years before publication.

Whether, or in what ways, we ourselves brought fruitful ideas to the work of others I cannot say. But out of these exchanges, in our own lab and those of others, came the shape and the scope of the work as it is presented in this volume.

The cost to the taxpayer for this "laboratory without walls" was very small. An average grant in those days was \$35,000. per year which bought part of the /salaries of a research scientist, two assistant research scientists, a typist, and the previously mentioned budget for consultants and travel. (What might we have done with a larger travel budget!) The returns on this modest investment have been very large. The "basic research" of the sixties in infant development has led to the "applied research" of the seventies, and the new "infant intervention" programs which have begun to proliferate in this decade have made babies the beneficiaries of that research.

Over the years, then, this "laboratory without walls"

brought us the wisdom and expertness of a very large number of colleagues. I recall with special gratitude the counsel of Sibylle Escalona who was, in fact, the first consultant to our study in the early years in New Orleans. It was her encouragement, at a time when the complexities of the study of blind infants were overwhelming to David Freedman and me, which gave us the energies to pursue the work. Later, in the Ann Arbor period of the studies, I turned to her for guidance on a number of occasions and marvelled at her unfailing ability, to get to the core of an issue, to ask exactly the right questions.

Peter Wolff, throughout the early years of the Project, brought encouragement, sound advice and good arguments. He examined object permanence data with us and brought his own expertness in this area to an examination of criteria transposed to a blind child's perceptual field. It was Peter, too, who introduced me to Thérèse Gouin Décarie and gave us another distinguished consultant who applied her Piagetian background to the formidable task for the blind infant in the constitution of an object world. (Since every blind baby in our study solved the problem it is a matter of considerable embarrassment that we have had to summon the world's

experts to help us figure out how he does it.)

It was Thérèse Décarie who told Piaget about our work and our problems and Piaget, who appears cheerfully resigned to such problems and positively enjoys being outwitted by the intelligence of infants, came to visit us in 1967. We had invited him to review a sequence of film clips illustrating stages in the development of object permanence in one blind child. We asked if he would serve as judge and argue criteria with us. When Robbie, on film, made his final demonstration (which we judged to be Stage 6) Piaget tossed his beret into the air and cheered.

It was Peter Wolff, also, who introduced us to Eric Lenneberg. (As I write this I feel that nobody got more mileage out of a poverty travel budget than Peter Wolff. I think he had a bicycle!) Lenneberg arrived on our campus in 1967 and became a volunteer consultant to us for one year.

As a linguist he was immediately intrigued by the problems we were facing in examining our language data. He gave his own time, most generously, most enthusiastically, in teaching our staff and in working with us on design problems. When he moved to Cornell University the following year he maintained close ties to our work and through exchange

visits he followed the development of our work over the years. He was our consultant at many turning points in our work. Eric died in June 1975 and it is painful to record this in a section of this book called "acknowledgements." I have not yet acknowledged his death.

René Spitz, who is justly the father or the godfather of research in human attachments became a friend to me and a consultant to our program at a moment that almost never gives birth to an enduring friendship in science. I was very much a junior colleague; he was a sage close to 70 when I presented our earliest data on smiling in blind infants at a meeting of the American Psychoanalytic Association. The data, with film illustration, did not support Spitz' hypothesis regarding visual stimuli for the smiling response in infants. Blind babies smile in response to the human voice. Spitz, watching the film, took in this new evidence with a cheerful countenance. Like Piaget, he loved to have babies argue with him. He introduced himself to me, wanted to know everything I could tell him about our work, and we began a professional friendship which lasted to the end of his life.

He never left the smiling problem. In 1969 he

brought his colleagues Robert Emde and David Metcalf to Ann Arbor for a two-day marathon weekend in which the Denver team and the Ann Arbor team talked and argued, and the Denver team presented a stunning and exhaustive study of the eliciting stimuli for the smiling response which nicely accommodated both Spitz' early findings and our own findings in the study of blind infants. (See Chapter V.) Spitz, during such team meetings was indefatigable. He was in his 80's, the rest of us were much younger, and he out talked, out argued everyone. I remember that hours after the first day sessions were scheduled to close, he was off on a brilliant discourse while a festive dinner in his honor burned slowly to a crisp in my oven. When he noticed that my attention wavered between his discourse and my dinner he did not hesitate to speak sharply to me.

Later there were team meetings in Denver in which we watched film together for many hours and I presented new material from our language study. Between visits there were letters, exchanges of new papers, more arguments (always graceful) and finally toward the end of his life, no more letters, and I began the mourning for René some months before his death.

Mary Ainsworth consulted with us throughout the last years of the blind studies, keeping us up to date on her most recent findings in her own studies of the course of human attachments in infancy. With each visit our own studies took a leap forward as the data from her work began to narrow the focus of attention on key indicators of attachment. There were visits, exchange of papers, and always the feeling that we might never catch up with the latest word from Mary's lab. When I wrote the final version of the paper on human attachments I sent up a prayer that Mary would not discover anything new for a year.

Over the years many of our most distinguished colleagues came to visit and to join us in watching blind babies on film. The great danger for us in seeing large numbers of blind babies was that we ourselves might become "blind" to the differences, especially the subtle differences in the behavior of blind and sighted infants. We needed Sally Provence's expert eyes to discern a fine difference in patterns of reaching, an ambiguous sign, or posturing in the hands of a baby. We welcomed the questions of Arthur Parmelee and James Prescott as they observed with us the puzzle of transient motor stereotypies in certain blind infants. It was splendid to have James Bosma visit

once or twice a year to ask provocative questions about the mouth and the hand in the early development of blind infants. When Justin Call came to visit he provided a balanced critique of a piece of work in progress that might easily have led to trouble without his counsel. Dan Stern and Ken Robson, both experts in the area of mutual gaze patterns, each with a special interest, brought us to sober reflections on the adaptive problems for blind infants and their parents when visual exchange cannot bind two partners.

Our gratitude to all of these colleagues is immeasurable. We hope that this report of our work will fairly acknowledge their support, their counsel and their generosity.

Then, how can one acknowledge fairly the work of an extraordinary staff in bringing this study and this book to completion? I am glad that we chose from the start to place central responsibility for data collection and data analysis in the hands of senior, most able investigators. Each of these staff members brought exceptional gifts in observation and interpretation to this study. It was plucky of us, now that I consider it, to insist in our proposal to NICHD that this work, data collection in unknown territory,

be trusted only to the most skilled investigators. I remember that this idea did not sit well with at least one of our original site visitors. When the site visitors left, one of them left behind a page of note paper with doodles and a single scribbled line: "Too many chiefs. No Indians." In short, as we understood it, our budget would be considerably relieved by the substitution of low paid student assistants for the "high-paid" senior investigators. Fortunately, this view did not prevail in the site committee and the "chiefs" were approved as requested. If good and useful things have emerged from this work I must praise "the chiefs," a number of whom are co-authors of chapters in this volume. Edna Adelson, Evelyn Atreya, Ralph Gibson, Marion Ross, Barry Siegel, Marguerite Smith and Lyle Warner were senior investigators who brought exceptional gifts to the primary investigation. To David A. Freedman, my friend and colleague during the New Orleans phase of this work, my special gratitude for a collaboration which brought rewards to both of us and the discovery of the joys of team research.

Yet we might have all drowned in the vast accumulation of data generated by the senior investigators if we had not had a splendid system for data retrieval, for which Edna Adelson took central responsibility along with

an exceptional group of student assistants who worked in teams with senior investigators. We are grateful to Susan Tracy Adelson, Ted Grossbart, Gloria Hool, Karen Hufnagel, Judith Larrick and Mary Ann Malefakis for indispensable contributions to our work.

The prodigious task of preparing this manuscript was undertaken by Beverly Knickerbocker, Adele Wilson and Anita VanderHaagen of our staff, who somehow have managed to combine meticulous attention to editorial detail with good humor, infinite patience and devotion to the work.

We are all grateful to Linda Herwerth for her fastidious reading of galleys and the preparation of the index for this book. Lily Ladin, administrative assistant for our program, performed her customary miracles in organizing work schedules to accommodate this heavy task and in supervising many details which required her editorial judgment.

With all this, it must be said that this volume would never have reached fruition without the collaboration of my husband, Louis. By 1975, four years after the completion of the original research, there was not yet a book. (One of the melancholy facts of research direction is that before one program has reached the

"write-up" stage, a new study is under way, and the new commitments have been made.) In 1975 we had a group of papers which constituted the core of a volume, but other facets of the work were not yet in written form. The papers, themselves, fairly constituted a report to our sponsors and represented our commitment to NICHD but did not, as a group of papers, cover all major aspects of the original investigation. Nor did they flow into one another to tell the story of the research as it unfolded. New chapters needed to be written. Old and new material needed to be integrated. It seemed to be an impossible task and I was ready to forsake it.

It was at this point that my oldest collaborator, Louis Fraiberg, re-read the old papers, read drafts of new papers, and told me that there was a book, that he thought the work was important, and offered to work along with me to produce a coherent manuscript. In the summer of 1975 we turned our dining room table into an editorial desk and began the collaboration. Louis, who exercises a stern blue pencil, gave shape and continuity to the writing. I finished the writing of new chapters and transitional material and turned them over to Louis for criticism. It was, as always, a most amiable collaboration and, for me, the work became alive again.

The book was largely completed by the end of the summer. As it appears now, the book is the result of this collaboration. One change in organization was suggested by Midge Decter of Basic Books (another form of Chapter I) which we all agreed benefited the structure. In other ways too we are indebted to Midge Decter for her personal devotion to the task of bringing this book to publication and for suggestions which were unfailingly in the service of clarity and exposition.