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DISCUSSIONS ON CHILD DEVELOPMENT

Volume Three

EDITED BY J M TANNER
AND BÄRBEL INHELDER

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DISCUSSIONS ON

Child Development

A Consideration of the Biological, Psychological, and Cultural Approaches to the Understanding of Human Development and Behaviour

EDITORS

J. M. TANNER

M.D., D.SC., D.P.M.

Senior Lecturer in Growth and Development
Institute of Child Health, University of London

BÄRBEL INHELDER

Professor of Child Psychology, University of Geneva

VOLUME THREE

The Proceedings of the Third Meeting of the World Health Organization Study Group on the Psychobiological Development of the Child Geneva 1955

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Abingdon, Oxon, OX14 4RN

MEMBERS OF STUDY GROUP

DR. JOHN BOWLBY Director, Department of Children and Parents Tavistock Clinic, London	*Psychoanalysis*
DR. FRANK FREMONT-SMITH Chairman Josiah Macy, Jr. Foundation New York	*Research Promotion*
PROFESSOR G. R. HARGREAVES Professor of Psychiatry University of Leeds Lately Chief, Mental Health Section World Health Organization	*Psychiatry*
PROFESSOR BÄRBEL INHELDER Professeur de Psychologie de l'Enfant Institut des Sciences de l'Education de l'Université de Genève	*Psychology*
DR. KONRAD Z. LORENZ Director, Max-Planck Institüt für Verhaltensphysiologie Seewiesen, Bavaria	*Ethology*
DR. MARGARET MEAD Associate Director, Dept. of Anthropology American Museum of Natural History New York	*Cultural Anthropology*
DR. KARL-AXEL MELIN Director, Clinic for Convulsive Disorders Stora Sköndal, Stockholm	*Electrophysiology*
PROFESSOR MARCEL MONNIER Professor of Physiology University of Basel	*Electrophysiology*
PROFESSOR J. PIAGET Professeur de Psychologie à la Sorbonne et à l'Université de Genève	*Psychology*

DR. A. RÉMOND — *Electrophysiology*
Chargé de Recherches
Centre national de la Recherche scientifique, Paris

DR. J. M. TANNER — *Human Biology*
Senior Lecturer in Growth and Development
Institute of Child Health
University of London

DR. W. GREY WALTER — *Electrophysiology*
Director of Research
Burden Neurological Institute
Bristol

RENÉ ZAZZO — *Psychology*
Directeur de Laboratoire de Psychobiologie de l'Enfant
Institut des Hautes Études
Paris

GUESTS

DR. D. BUCKLE Regional Officer for Mental Health Regional Office for Europe World Health Organization Copenhagen	*Psychiatry*
PROFESSOR ERIK ERIKSON Austen Riggs Center Stockbridge, Mass. and Dept. of Psychiatry University of Pittsburgh School of Medicine	*Psychoanalysis*
DR. JULIAN S. HUXLEY Formerly Director-General, U.N.E.S.C.O. London	*Biology*
DR. RAYMOND DE SAUSSURE Geneva	*Psychoanalysis*

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PREFACE

Those who have read the first two volumes of this series will already know that the Study Group at its first meeting heard and discussed presentations from representatives of various different scientific disciplines which study some aspect of child development or matters relevant to it, and that at its second meeting the presentations, and the discussions, were focused less specifically on the contributions of different disciplines and more upon aspects of child development in which several disciplines are interested, including such matters as the existence of recognizable stages in development and the phenomenon of learning in immature or developing organisms, and particularly learning under stress.

The third meeting focused upon two related topics: the development of sex differences and the development of individuality or ego identity, and the pattern of psychological development which leads up to it.

The discussion of the first of these topics was based upon two presentations: one by Margaret Mead of comparative data from different societies which might throw light upon the extent to which sex differences were determined by nature or by nurture, and the second by Erik Erikson on sex differences in the play constructions of adolescents.

The opening presentations of the second topic were also made by Erik Erikson. In addition to Mr. Erikson two other distinguished guests joined the Group for this meeting: Dr. Julian Huxley and Dr. Raymond de Saussure.

As on previous occasions the meeting, which was held in Geneva in 1955, provided for its participants a week of stimulating and vigorous discussion which, since it was conducted as usual in both English and French, could not have been possible without simultaneous interpretation equipment and the remarkable ability of the W.H.O. interpreters to follow, and communicate simultaneously in a different language, discussion which, as the reader will find, was often of an exceedingly technical nature and couched in the terminologies of a variety of different scientific disciplines.

To the participant observer it also seemed that a change in the Study Group itself contributed to the success of the meeting. At its first meeting in 1953 its members had comparatively little knowledge of the interests, the methods, and the theoretical models of those in

other disciplines who studied the problems of child development; by the third meeting their understanding of each other's work was sufficient to enable them to appreciate and to criticize the formulation of those who worked in other fields and to consider its relevance to their own. For this development in mutual understanding the Group owes much to its Chairman—Dr. Frank Fremont-Smith.

The production of the English transcript of the extensive bilingual discussion depended upon the excellent sound recording facilities provided by W.H.O. and above all on Mrs. J. Moser of the Organization's Mental Health Section whose combination of linguistic ability, technical knowledge, and keen interest in the work of the Group has enabled the contributions of its French-speaking members to be rendered in a sensitive and accurate English translation in these volumes.

Dr. Tanner and Professor Inhelder have edited the transcript of this third meeting and have shown again their ability to reduce a week of discussion to the bounds of a single volume while preserving to a surprising degree its content and its savour.

Leeds University G. R. HARGREAVES

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FIRST DISCUSSION

The Childhood Genesis of Sex Differences in Behaviour

CANDAU (Director-General, World Health Organization):
This is the third meeting of this research study group. It is to be devoted to consideration of socio-cultural influences affecting psychological development.

I am very glad to welcome once more to Geneva the original members of the Group and also our three distinguished guests: Mr. Erik Erikson of the United States, Dr. Raymond de Saussure of Geneva, and Dr. Julian Huxley of the United Kingdom.

Members who have been present at previous meetings of this Group already know its purpose, but it is worth emphasizing, for the benefit of our guests, that the Group is not intended to make decisions or recommendations on any subject to anyone. Its aim is to increase mutual understanding between eminent exponents of the many different disciplines that study child development and its disorders, particularly in the fields of psychology and physiology.

It only remains necessary for me to wish the Group, under its Chairman, Dr. Frank Fremont-Smith, a stimulating and enjoyable meeting.

FREMONT-SMITH:
We might remind our guests and ourselves that we operate on an extremely informal basis. As Dr. Candau said, the purpose of our meeting is to increase mutual understanding. I think that we should feel rather free to interrupt one another, and your Chairman will only step in occasionally when more than three speak at once. In this way we aim to create a group discussion rather than a series of speeches to which the group gives polite attention.

Now to begin with we would like our three guests to tell us a little bit about themselves. (The regular members of the group introduced

themselves in a similar fashion at the first meeting, recorded in Vol. I.) How did they happen to arrive at the kind of interest that led them, on the one hand, to be invited to come here and on the other hand, and even more importantly, to accept?

HUXLEY:

I had better begin with the age of about seven when I found myself fascinated by watching birds which were nesting under the eaves of our house just, I think, because they were alive but had a different kind of life from my own. I think it is fair to say that I have always been interested in the fact of difference and the variety of life and of the world as a whole. It was only later that I got interested in the converse problem of what unity there was to be found in it all, and from about 1934 onwards I spent most of my time trying to make a synthesis of the different approaches to biological evolution. How I got into biology was quite accidental. When I was at school I got to a stage where one could take a certain amount of work in a special subject of choice, and I wrote, I remember, to my parents and said 'wouldn't it be a good thing if I took German', because it was then thought I might go into the Civil Service. They wrote back and said 'no, you can go to Germany later and learn German, why don't you take biology?' Well, the biology master at Eton was a genius, and after two months I knew I was going to be a biologist.

Of course, I wouldn't have been invited to join this Group if I had been a biologist interested solely in subhuman phenomena, but I remember that over 40 years ago, when I was teaching in Texas, I had to give a public lecture, and as I had already begun to think of the problem of continuity and discontinuity in evolution I chose as my subject the critical point between subhuman and human evolution. The first book that I published, before the first World War, was about biological individuality, which I am delighted to hear Konrad Lorenz has been re-reading and even finding interesting still. Then I think I am right in saying that the first occasion on which a displacement activity was scientifically recorded was in my long paper on the courtship of the Great Crested Grebe, published 41 years ago. I didn't know what a displacement activity was, but I recorded the facts of this unexplained phenomenon; and I spent a lot of time during the next 25 years, off and on, working on the biological function of epigamic and throat display in birds and their relation to sexual selection. Meanwhile, I was also interested in the various aspects of experimental embryology and growth, especially relative growth of parts; I found myself driven to put forward the

idea that the basis for various aspects of growth and development must be sought in some form of field involving gradients. So I was naturally much interested in the then new ideas of the Gestalt psychologists, who were also thinking in field terms, and were interested in psychological development. Then I found myself dealing with a curious feature of some organisms such as Ascidians and Coelenterates—their peculiar faculty of physical regression or de-differentiation: they not only get smaller but they regress to a simpler stage if you maltreat them in various ways—and this led me to read up all I could about psychological regression. Later I am afraid I strayed rather outside the biological field: in 1928, I think it was, I rushed into print with a book on religion as a natural phenomenon instead of a supernatural one—Religion without Revelation, I called it. And later on I had wished on me the task of relating ethics to evolution by the Vice-Chancellor of Oxford, who invited me to deliver the Romanes lecture 50 years after my grandfather had given his celebrated one on evolution and ethics, and to treat of the same subject, so I had to do some thinking about that problem. Recently, I have had to do some thinking about an equally difficult problem for a mere biologist—the Wenner-Gren Foundation have asked me to write something on anthropology from the angle of an evolutionary biologist.

Then, the fact of being in Geneva reminds me that in 1925 I attended what was, I believe, the first international conference on population, here in Geneva. Ever since then I have been deeply interested in this problem, which I think is one of the world's most serious problems, psychologically as well as politically and socially.

You asked me, Mr. Chairman, why I was willing to come here. I should think that the answer is pretty obvious. This type of meeting seems to me absolutely essential in the present state of the world. We have got to a state where we can't progress if we each of us stay in our own little specialisms. I am sure we have got to a stage where we must somehow organize the synthesis of different branches of knowledge. People say that science has got too specialized for that. I don't believe this is true. Only you have got to find out the technique of getting representatives of different branches together, of getting mutual understanding, and of organizing the synthesis. When I heard about this group here, I thought, well, this certainly is extraordinarily interesting because here we have ethologists and psychologists, medical men and biologists, physiologists and psychiatrists, all interested in one general problem, and I shall be most interested to see how a group of this sort functions, as well as to hear the results of its deliberations.

FREMONT-SMITH:

Now, if I may I will ask Erik Erikson to tell us a little bit about himself.

ERIKSON:

I should state first what I am doing. I am a psychoanalyst who divides his time up about equally between psychoanalytic psychotherapy, the training of young doctors for psychoanalysis, and research. The psychotherapy fuses, of course, with research, for what in most of your fields is experimentation, in our field is the attempt to delineate previously incurable mental states in such a way that they become more curable. I work at a small sanitarium in the Berkshire Hills in New England which is also a research institute. There some of us specialize in work with young 'borderline cases', which means young people on the brink of schizophrenia or psychopathy. Ours is an open hospital which deliberately takes certain chances with the patients in order to understand them not as victims of fatalistic diagnosis, but as individual cases of what I will describe in a later meeting as identity diffusion, i.e., a particular difficulty attending the age of youth, and this particularly where society and family have let young people down in specific ways. In addition to intensive psychotherapy, we experiment by giving the patients a responsible voice in the management of the 'hospital environment', and by developing individual ways of work productivity. This is my wife's domain. I am also a visiting professor in the Medical School in Pittsburgh where I learn about patients with similar symptoms but different social backgrounds.

I should have said at the beginning that I do not speak English, but a form of American. This may make it difficult for you to understand what I say, yet it is intrinsically related to our general subject. For my friends (my very best friends) will tell you that I am the best case illustration of an identity diffusion, that is of the clinical picture which I will describe to you when my time comes. Identify diffusion results from the inability to integrate one's childhood identifications and adolescent tasks. Well, I was born a Dane. But my father died around the time of my birth, and my mother and I seem to have travelled a lot. When I was three years old I fell ill in a city in Southern Germany. We stayed there, because my mother married my paediatrician. This, I think, has been a most decisive event as regards my later identity development.

FREMONT-SMITH:

Was it a psychosomatic illness?

ERIKSON:

Well, I certainly needed a father. Later, my stepfather very much wanted me to be a doctor. But I decided to become an artist.

The secret identification with the paediatrician appeared only in the fact that I specialized in baby pictures and portraits of children. To do such portraits, I went to Vienna and joined a friend as a tutor in an American family which was close to Freud. Eventually, I was trained as a child analyst under Anna Freud and August Aichhorn and graduated as a psychoanalyst from the Vienna Institute. In Vienna, I also married an American woman of Canadian origin, and we moved to Cambridge, Massachusetts. One of our neighbours was Frank Fremont-Smith through whose interest I received my first medical school and hospital appointments. From then on I have been living in the medical world as much as anybody could who is not a medical man and my stepfather, the paediatrician, was as right as anybody could be who was wrong! And you may well see that I had to make out of identity diffusion a virtue—and a subject.

Now, as for my preoccupations, the daily work of a psychoanalyst is in many ways cut out for him. But I would mention that the artist in me keeps paying special attention to the 'configurational level', by which I mean something which in psychoanalysis is between the obvious, manifest behaviour and the latent, hidden meaning. I will present examples of children's play, trying to show what a child is saying by the way in which it behaves in space, and how it arranges toys and dolls. In work with very small children, it helps tremendously if one is not restricted to listening but if one can observe, and I think it was here that my paediatric and artistic sides came to some kind of agreement. This was further enhanced when I met Margaret Mead and a few other anthropologists and found that in comparing configurations we spoke, as it were, a similar language and could speak about childhood and culture patterns in a way that eventually made dynamic sense also. Thus I became interested in the relationship of human motivation as discovered by psychoanalysis to people's world images and economic systems: what people are hunting and where and how. Having studied two American Indian tribes, it seemed only right to study American children who were neither Indians nor patients. So I participated for a number of years in a longitudinal study of Californian children to try to understand their particular development in that rapidly growing area which was rather freshly settled compared to the rest of the world.

My special interest is this. In psychoanalysis, which originated in the era of enlightenment and individualism, the original emphasis

was on the dichotomy between the individual and his society, almost in the sense that environment as such was essentially hostile to the individual. This, of course, was an ideological distortion of what Freud, at given stages of his studies, had pointed out. Today, the analysis of the ego makes it very clear that no individual could possibly exist, or grow strong, or, indeed, become an individual without society. But how does he do it, and how does society keep the bargain? I think that the psychoanalysis of the ego permits a new approach to the contract between individual and society. If I may make in conclusion what I hope to be a challenging statement, I think I came here with the expectation of learning from this particular group a new approach to the ethological nature of love. But by this I do not mean only the mutuality between mother and child, and not only the pact of personal love, but also such psychosocial contracts as forms of truth, styles of honesty, kinds of justice. All of these seem as indispensable for the human child's development as the life-preserving features of the physical environment.

FREMONT-SMITH:

If I may I will now call upon Dr. Raymond de Saussure to tell us about himself—it is a great joy to see an old friend from New York who was over from Geneva and is now back here in Geneva with us.

DE SAUSSURE:

Unfortunately, although I am a psychiatrist and a psychoanalyst, I am not able to bring you an account of such a miraculous or spectacular cure as that which Mr. Erikson underwent at the age of three years—his own cure and the cure of his mother were certainly determining factors in his career. So I will stay within more modest limits, and as we have been asked to give a biography which begins with the early years I shall keep to the classical pattern of psychoanalysis—although Dr. Hargreaves asked us not to be too classical —and I will tell you here of my oral stage, my anal stage and my genital stage.

In my oral stage I was very greedy and I think I was able to sublimate a part of this greediness in the desire to read and study later on. The anal stage was characterized for me by a liking for collecting and studying butterflies, and throughout my adolescence (which is a rather late anal stage) I developed this liking for making collections. The genital stage was very much retarded too, this time through Calvinism and its very strong influences to which I was subjected, so that I had to go off to Vienna to be analysed by Freud and to get

rid of some of these overpowering ancestral influences. I was very grateful to him, but I did not immediately realize the full value of this method and returned later to Berlin to undergo a second analysis with Dr. Alexander and to follow courses at the institute.

Actually, I owe my interest in psychoanalysis to Théodore Flournoy who was Professor of Psychology here in Geneva. Immediately after my secondary schooling I became deeply interested in his lectures in which, as early as 1914, he used to expound his psychoanalytic theories. My interest turned very quickly first towards the similarities or the concordances which could exist between the ideas of Freud and Jean Piaget. Here there were two points of view about psychology, two genetic points of view, which seemed to complement each other but which unfortunately had previously been put in opposition to each other. During meetings of French-speaking psychoanalysts I had the opportunity of establishing the first connexions, which later on were well worked out by my friend Charles Odier. Another of my interests was to find the possible relationships between certain social structures and certain forms of neuroses (de Saussure, 1929, 1946).

In my book on the Greek Miracle (de Saussure, 1939) I tried to show why scientific civilization was born in Greece rather than elsewhere. My great interest for all the problems of psychology intermingled with those of biology and politics dates from this time. When I went to the United States in 1940 I had the very great privilege of following many inter-disciplinary discussions where psychological, biological, social and political points of view were intermixed.

For this reason I was extremely happy to receive your invitation which corresponds to one of my major interests. I am looking forward immensely to hearing you and I shall try to contribute in my modest degree to your discussions.

FREMONT-SMITH:

Now we have had our introductions, and begun to group, if one can use that word in the intransitive sense. May I turn to you, Margaret, to open the discussion and do it in your own way—you have quite a period of time, two-and-a-half days. The principle, as you know, is to ask the opener to have each day about 20 minutes of material to present and then if they are really successful in the two-and-a-half days they can't get it all presented because of the discussion which is stirred up.

MEAD:

My task now is to describe what light anthropology can shed on

the occurrence of what can be regarded as universal sex differences, however differently these may be patterned and institutionalized in different societies. Conversely, I want to provide a way of removing from our present formulation the culturally limited ethnocentric and provincial material on sex differences. This means that I will use for the most part extreme anthropological cases. If I talk about something that is reasonably widespread and universal it would be impossible for me to give you any detail—for example of the fact that mothers breast-feed babies in all parts of the world. So I shall select the most interesting cases, with your understanding that these are extreme and that we assume, in between, the existence of a very large number of less peculiar, less extreme behaviour patterns.

I think that we should really start with the child's realization of the fact that it is a human being, which is, I think, more important than its realization of which sex it is. For instance, in Bali, people are unwilling to treat a child under three months as a human being; the new-born baby has no name, is not allowed to enter a temple and is called a rat or a caterpillar. Then there are societies that identify children with vegetables or with animals for a very long time and undoubtedly change the individual's sense of who they are and what it means to be a human being.

Coming to sex differences, I think that we will find it useful to consider first those differences between the sexes which have occurred in every society to date, but which may nevertheless be modifiable. Until recently, for instance, human infants have been breast-fed, the feeding bottle being a comparatively recent invention. As long as human infants had to be breast-fed by human mothers there was a universal condition of the early differentiation of sex roles, as children of both sexes were fed by a creature of one sex everywhere. This ceased to be universal as soon as the feeding-bottle was invented. What looked like a very long and very important element in the differentiation of the two sexes could be wiped out by a single invention. Equally, of course, it could be restored. Thus all of the points that are connected with maternal care of children, or care by another female such as a nurse, a wet-nurse, a foster mother, etc., although they have been almost universal to date, may nevertheless be a biological condition which society is able to abrogate, or change. The whole question of social inventions which may alter present differentiation of the sexes is exceedingly important.

On the other hand, a great proportion of our present psychological theory about differences between the sexes is based upon the presence of clothing. Psychoanalytic theories that are based on the importance of the revelation of the anatomy of adults, or of your little brother,