

**World Health Organisation (WHO) Papers' "by Bowlby and others on psychobiological development, including Piaget, 'Essay on the general problems of the psychobiological development of the child' and comment by Bowlby and others; minutes of the Research Seminar, Feb 1956 to discuss Piaget's paper and Bowlby's reply to it**

**Publication/Creation**

1956

**Persistent URL**

<https://wellcomecollection.org/works/psjw8rhw>

**License and attribution**

You have permission to make copies of this work under a Creative Commons, Attribution, Non-commercial license.

Non-commercial use includes private study, academic research, teaching, and other activities that are not primarily intended for, or directed towards, commercial advantage or private monetary compensation. See the Legal Code for further information.

Image source should be attributed as specified in the full catalogue record. If no source is given the image should be attributed to Wellcome Collection.



Wellcome Collection  
183 Euston Road  
London NW1 2BE UK  
T +44 (0)20 7611 8722  
E [library@wellcomecollection.org](mailto:library@wellcomecollection.org)  
<https://wellcomecollection.org>

WHO 1956

PAPERS

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/16  
22 August 1956

ORIGINAL: ENGLISH

Geneva, 20-22 September 1956

THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Comments and answers on Professor Piaget's essay

by

Grey Walter

First, some candid but incoherent comments on the main text:

Please do not consider Monnier, Rémond, Melin and myself as "EEG people" (page 3). In so far as we are qualified to participate in these discussions we are experimental physiologists, even though we may spend much of our time supervising EEG laboratories. None of us feels that EEG records alone can tell us much about child development - in fact we all do our very best to combine EEG with the recording of other physiological variables and the results of psycho-social estimates. This is not just a status claim; it is an important factor in assessing our contribution; we are eager to see the organism whole and to avoid organ dogma. I think it is as silly to attribute all behaviour to the cerebral cortex (or to the reticular system, which is more fashionable) as it is to the possession or not of a penis. Of course we know that we can never see an organism and its history truly whole, but we must try and try to get a glimpse in 3D if not in the 7D we know we need. Opposite p.148 of our first Proceedings is an old record showing the simultaneous recording of ten variables during a psychological experiment. This is nothing to what we can do now and will do in the future, and by "we" I don't mean my own group but dozens of teams all over the world. The importance of this is not just to exhibit our megalomania - we cannot by definition control the variables when we are studying a complex system, so we must observe as many as we can and make all illuminate all. This is the essence of the Cybernetic approach. There are still exciting shadows and highlights and deep ravines of inspissated gloom but



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/16  
22 August 1956

ORIGINAL: ENGLISH

THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Comments and answers on Professor Piaget's essay

by

Grey Walter

First, some candid but incoherent comments on the main text:

Please do not consider Monnier, Rémond, Melin and myself as "EEG people" (page 3). In so far as we are qualified to participate in these discussions we are experimental physiologists, even though we may spend much of our time supervising EEG laboratories. None of us feels that EEG records alone can tell us much about child development - in fact we all do our very best to combine EEG with the recording of other physiological variables and the results of psycho-social estimates. This is not just a status claim; it is an important factor in assessing our contribution; we are eager to see the organism whole and to avoid organ dogma. I think it is as silly to attribute all behaviour to the cerebral cortex (or to the reticular system, which is more fashionable) as it is to the possession or not of a penis. Of course we know that we can never see an organism and its history truly whole, but we must try and try to get a glimpse in 3D if not in the 7D we know we need. Opposite p.148 of our first Proceedings is an old record showing the simultaneous recording of ten variables during a psychological experiment. This is nothing to what we can do now and will do in the future, and by "we" I don't mean my own group but dozens of teams all over the world. The importance of this is not just to exhibit our megalomania - we cannot by definition control the variables when we are studying a complex system, so we must observe as many as we can and make all illuminate all. This is the essence of the Cybernetic approach. There are still exciting shadows and highlights and deep ravines of inspissated gloom but

we are beginning to see the modelling of the growing brain in its relation to the rest of the body and to behaviour. As a result, the answers I must give to Piaget's questions will be contingent rather than concise. For one thing we are perpetually impressed by the range of variation between children in a given age group - yet Piaget speaks for example of "the child aged 7-8 years". Of course we can accept a statistical norm for some of our physiological variables, but if, as we think likely, development of some variables proceeds by steps, then their time-distribution is NOT normal in the statistical sense and we must specify whether in a particular child a certain step has been made or not. Paediatricians are always talking of "milestones" in a child's development, but this gives an impression of an arbitrary scale at the side of a smooth road - what I am concerned with is an actual change of plane or field or climate, where, as in the dark wood of middle life, the straight road may seem to lose itself in the undergrowth, and we have to take to the trees.

The question of use and design, or nurture and nature, and the notion of equilibration are affected by this concept, for implicit in it is the consideration of the organism and its environment as a closely interacting statistical assembly. Perhaps I mean rather a probabilistic matrix, or a quantum atmosphere; Bertalanffy includes such systems in his General System Theory and Ashby calls certain classes "ultrastable". We are in great semantic difficulty here. I don't fully understand Ashby's equations. I can't write down any better ones and the grandiloquence and ambiguity of the above phrases appals me - forget them. Furthermore, I have to postulate what I have called "speculation" in living systems. My models show what I mean if you can remember them; think of all their components assembled together, receptors, relays, storage systems, motors and all, then imagine wires growing out from one component to the other along the potential gradients, like little coloured worms while you are playing with them, so that at first there are only a few simple reflexes, then the scanner starts up the "speculative" activity, an IRM is triggered, the probability of significance of some sets of events reaches the threshold of implication, the IRM is incorporated into the new association, the original reflexes are adapted to a more complex



application. All this time you could amuse yourself by betting on what would happen next and you might win a little if you knew what was inside the boxes, but the only certain thing is that in the end the batteries would run down; the system would no longer be "open" but very definitely closed, dead.

X 11  
Now, if you had been playing with the model since it was "born" and knew what was inside it, you could give a fairly good account of its state and, as I say, a fair prediction of its next phase. But if you came on it after it had already gone through a few manoeuvres you would be quite unable to describe its internal condition. Obviously you could construct an objective description of its behaviour and if you were allowed to use a few instruments you could also identify some of its internal functions; perhaps relate function to behaviour in a tentative way. But you could not by any means discriminate between built-in and acquired features. This is a corollary of the proposition so carefully set out by Wiener in "Cybernetics" about biological or Bergsonian time as compared with Newtonian time. We exist on an irreversible time-scale; we cannot live backwards and cannot even make legitimate retrospective analyses without inside information. This is nothing to do with any mystical properties of Time but is because what we see of living creatures - and models of them - are statistically determined interactions between structure and experience. What we call logical processes, the rules of arithmetic and of games, are very special cases, artificially isolated and enforced for special purposes. It seems to me one of the limitations of the Freudian way of thinking that it assumes a principle of strict logical causality in mental processes. Another limitation, which also contributes to our semantic difficulties, referred to by Piaget on p.7, is the resort to antithetical argument. Could we not attempt in our next discussion to avoid the Manichean heresy? As Piaget urges, and I'm sure we all agree, we must seek a common language, even if we have to sacrifice some rational sovereignty. Physiologists perhaps have least to lose, since they can offer no general theory of behaviour, and still have surprisingly little dogma. Our jargon of course is tiresome but is easily dispensed with. I know that my use of the term "statistical" to describe the organism-environment complex jars on some people; Piaget uses "probabilistic". Perhaps this is better. There is also the



word stochastic which is smart and has the happy etymology of "aiming at a mark" (unfortunately this term is misused (in my opinion) as a synonym for "random") so that we could think of children growing toward some goal and not merely up.

I suppose Adler is dreadfully outmoded but I have always admired his emphasis on the personal vector. The assumption of orientation need not be coloured with transcendental teleology in thinking about children any more than in watching my models. Artificial goal-seeking mechanisms are novel perhaps, but the essence of Cybernetics is to define and analyse the factors in "purposeful" behaviour which are common to all self-controlled systems, and to suggest tactics and strategy for the study of complex interacting systems - which are beyond the range of classical scientific methods and propositional logic.

What seems to me very important in all our discussions is to recognise that complex behavioural patterns which seem to show intense purposefulness may be the expression of quite simple mechanisms. The elements in the mechanism need not be very numerous either, but of course the number of ways in which they can be combined in permutation is certainly colossal, and as Bowlby mentions, very slight differences in their rate of development or in the details of their connectedness will be grossly amplified by the very operation of the mechanism itself on the environment and vice versa, so that again great differences between young individuals of the same age are to be expected and should be appreciated. Furthermore, these very differences should give us essential clues to the whole problem of animal development, since the permissible range, or "tolerances" as an engineer would call them, of a component or function often reveals far more clearly than its mean or modal value, what part it plays in the whole organism.

When Ross Ashby asked us to help him build one of his homeostatic analogues, he gave us a circuit diagram with the values of the resistances and voltages and so on. Several of these were unusual values, rather hard to obtain and there were rather a lot in each class. Our electronic craftsman looked it over and said, "I'm awfully sorry but I can't get that value of resistance, 63 258 ohms, very easily - would 68 000 ohms do instead?" Ashby replied, "Oh, yes, it doesn't



really matter what the value is - I just measured the resistances I happened to put in and that is the average value - they can be almost anything provided they don't pass too much current." The craftsman looked as though he had found something slimy under a large stone, and we had to sub-contract the job; his reaction was perfectly apt - such a specification is too much like life to please a mechanic who wants his machine to behave exactly according to plan. I should add to point the moral, that Ashby also specified that certain values, of supply voltage for example, should be very carefully fixed and stabilized. This seems to me very important; in complex systems which are capable of self-control and self-development, whether in flesh or metal, some features must be held constant within narrow limits, others can be - in fact must be - left free to vary widely. In other words, in our study of development we might usefully try to decide which aspects of human psychophysiology are homeostatically controlled and which are liable to vary over a wide range. As a simple example, body temperature varies little (as compared with ambient temperature). We know something of the reflexive or "negative feedback" pathways that ensure this in health and use the failures of control as an aid to the diagnosis of faults or diseases in the organism. On the other hand, EEG characters (which can be measured with a comparable degree of accuracy) vary over a wide range and this variation makes the diagnosis of brain disorders in children by EEG enormously difficult. Are these two classes of relationship what Ashby means by "Parameters" and "Variables"? (A parameter being a variable which is not included in a system under consideration.) This is by no means trivial or academic for, to quote Ashby, "a change of parameter-value changes the field, and because a system's stability depends on its field, a change of parameter value will in general change a system's stability in some way".

(These notions seem to me very apposite to the discussion by Piaget and Bowlby on "equilibrium". I feel bound to add that in our studies of human brains we have quite independently been forced to include "stability" as a measurable neurophysiological relationship, among our parameters, and feel justified in identifying certain mechanisms in the brain as serving to ensure failure-to-safety in conditions where stability is threatened by a change in Parameter-value A. I shall be discussing this in more detail in my replies to Piaget's specific questions to me.)



This is saying the same thing as others have suggested about whether a child tends to reach an equilibrium (Piaget), or a steady state (Bertalanffy) or stability (Ashby) or normality, or whether in general organisms are essentially spontaneously active (Bertalanffy) or speculative (GW). I am suggesting that there are indeed reflexive mechanisms (a phrase I prefer to "negative feedback circuits") which are intrinsically homeostatic and self stabilizing, error-cancelling. Further, I agree with Bertalanffy that the signals or events or stimuli which operate these mechanisms are in general (and particularly in humans) of such variety and intensity that it is better to think of the outcome as the steady state of an open system. As Bertalanffy says, this is classical but not trivial and an important corollary is that when one studies the internal economy of an open-system-in-a-steady-state one often finds a surprising amount of activity going on because the channels carrying the error-signals are likely to be pretty busy even when the whole organism seems in "equilibrium". In fact, the more nearly perfect the "dynamic equilibrium" the more internal "activity" there may be. There are plenty of examples of this, of course, in flesh and metal.

A very crude illustration for those who haven't thought in these terms: the humble and hygienic water-closet problem. You want a tank of water to be always full so that it will operate a siphon. You can have a continuous inflow with an overflow pipe - very simple, no moving parts except the water. But very wasteful and the faster you want the tank to fill after it has been emptied, the more wasteful it will be. This is a closed system in a "dynamic equilibrium" of the simplest type, and is adequate if the tank is likely to be emptied on the average almost as soon as it is full. To avoid waste, however, you fit a ballcock which admits water in inverse proportion to the existing water level. Now there is no waste at all, and the rate of filling can be as fast as the supply pressure and capacity permit. But you have a more elaborate reflex mechanism which might go wrong and if you are wise you will leave the overflow pipe too, just in case, to ensure failure-to-safety.



Now note how, from outside the tank, you could distinguish the first system from the second. In the first there would be steady "spontaneous" activity in the input and there would be two alternative outputs, the siphon or the overflow, related in such a way that when the first was operated the second was inhibited and the first would only operate if the second was already working. In the second system on the other hand the input would only appear after the siphon was operated and then for a fixed time, during which the siphon would be inhibited. To take the analogy a step further, if you decide that the whole system should be automatic in the sense of not requiring an initiating stimulus, you could either arrange for an inverted U-tube siphon (which would empty the tank quickly and completely whenever it was full, the frequency of discharge being a linear function of the input rate), or add to the reflex system a clock which would initiate the sequence of siphoning and filling at regular intervals. This last would ensure relative independence of filling rate and supply pressure. It could also be linked with a receptor to provide information about when the discharge would be most effective, thus forming a second-order reflex, with further possibilities of devices for estimating contingency between apparently independent events, conditional probability, and so forth. In these more sophisticated arrangements, inspection would reveal "spontaneous rhythmic activity" which might be significantly related in its phase relation to the pattern of incoming signals, but would show little connexion with the characters of the energy supply. Above all, the more perfect and intricate the dynamic balance, the more characteristic and varied would be the internal spontaneous activity. (Obviously this word "spontaneous" is an awkward one. This is no place to discuss it in detail. Many other terms have been used: "autochthonous", "autogenous", "autogenic" and so forth.)

In the brain we are still debating whether small populations of cells or individual cells do really exhibit gratuitous, autogenous activity. In any case, whether an isolated cell, or a slab of cortex or a whole brain or organism is considered, wherever rhythmic or repetitive activity is observed, there must be a "feedback" of some sort. This is true even of a pendulum; so it is not

surprising that we find such effects in profusion in animals nor that they are often inversely related to outward function or operational activity, for as in the case of our water tank in the household they are usually essentially administrative rather than operational.

This brings us back to the basic question: can we hope to distinguish in ourselves, in children, between on the one hand the administrative homeostatic mechanisms which, from the point of view of mentality and behaviour are concerned with parameters (A) and on the other the operational, speculative processes, the manipulation of essential variables, which gives us such deep satisfaction - happiness as opposed to comfort?

While I have been writing these comments on Piaget, I have had the advantage and distraction of getting other people's answers, since mine have been so long delayed. I was trying to finish some experiments to answer with - we have all yielded too often to the temptation to play with words. Alas, the nearly crucial experiment we wanted to make proved too expensive. What a dinosaur of a tail we wag; the operational expense of the experiments we wanted to do with Bärbel Inhelder and Jim Tanner would have been about \$ 300 - a small fraction of the administrative cost of letting us talk for a week!

However, we have been able to make a few observations that help me to reply more explicitly to Piaget without hedging or prevaricating.

The first question dealt with the possible forms of P.B. development, of which I suggested six. Piaget proposed another to describe the construction of logical relationships, which he feels is an important stage at 7-8 years, and he describes this as Learning by successive equilibrations. For me this presents few problems since I consider such learning as a special case of "Stochastic" learning just as I consider logical reasoning as a special case of statistical reflexion. I agree of course that propositions such as  $A = B$ ,  $B = C$ ,  $\therefore A = C$  are unlikely to be worked out by children under 7-8, but I think of this class of propositions as a member of the (larger) class: "A usually implies B, B usually implies C, so there is a reasonable chance that A will imply C". Remember that in



such a chain of probabilities the uncertainty is multiplied at each stage, so that the chance of an organism making the logical inference is quite small unless the P is near unity on every occasion and at each stage. In the example Piaget gave of his friend counting pebbles, he counted them several times, changing the order and making sure that the cardinal sum was invariant with order. Piaget's point was that the organism here was actually manipulating the environment and I agree that this is of basic importance. But I maintain that in the development of an assembly of statistical associations a stage will sometimes be reached at which a completely invariant component will emerge and this may become a basis for logical reasoning. This is really quite familiar, and is sometimes described as the search for redundancy; for example, in the question of cardinal number and ordinal number, the pebbles or the fingers are found to be "redundant". The relations sometimes described as Natural Laws are in fact examples of enormous redundancy. In general, the degree of redundancy, the completeness of invariance of Natural Laws cannot be determined without experimental verification, that is, physiological action by the organism. There is only a very poor chance of the signals received passively by an organism providing enough information for it to draw a general conclusion. Hence the need for "speculation", the active and assiduous exploration of promising relations; in scientific work the experimental testing of plausible hypotheses, in the vernacular, following hunches. This is included in my class of Learning by Association, since a large proportion of the relevant information received by the brain is from proprioceptors responding to muscular and glandular action, rather than from exteroceptors.

It seems to me that the "learning par équilibations successives" of Piaget would be better translated as "matching" (a word which cannot be translated into French; it is used often in technical French in phrases such as "Pour assurer un minimum de distorsion dynamique il faut que les selfs soient matchés ...": the inductances must be matched. One might make a joke about that, something like "To ensure the least disturbance of life the ego must be equilibrated"). This is a most important concept, but is not outside the mechano-physiological



classification I put forward. Learning by matching obviously implies the existence within the organism of some sort of model to match with experience and I suggest that a child is unlikely to acquire enough information to build up in his brain a stock of "logical" models before the age of 7 or so.

This is just the sort of question I had hoped to be able to answer by experiment. We have evidence - in reply to Piaget's second question to me - that elaboration of sensory signals is rare in children below the age of 7 or so. Is this because elaboration depends upon progressive matching of internal models with external experience and this cannot be done until there has been time for a stock of internal models to be built up, or is it because the neural apparatus is not there at all before this age? This should be a straightforward problem and seems to me a basic one, but it cannot be answered, I fear, without experiments of a fairly intricate nature.

This leads on to my answer to Piaget's third question; why is there so thin a correspondence between EEG and cognitive development? I think this is true only if one is limited to subjective consideration of the passive EEG features. If one makes quantitative studies of the EEG and other physiological variables during activity, particularly during a learning experiment, very exciting relationships emerge which do indeed seem to encourage the search for a firm passage between physio- and psychological domains. Such experiments are, alas, still very involved and expensive, but I don't feel anything simpler is worth doing. Some of you may well feel that our claims are exaggerated and that we make too much technical fuss - you may be right, but the old method of taking a passive EEG and glancing through the record is rather as though Jim Tanner were to glance at one of his child subjects and say, "Yes, that's a well developed kid". May I give another analogy - suppose we were trying to decide how to teach children gymnastics and the question arose, at what age should we expect a particular child to be able to raise itself up to a horizontal bar? Would it be enough to measure the girth of the upper arm at rest? Would we not have to consider practice, incentive, competition, physical proportionality? Our psychophysiological experiments are rather like that; we are

trying to find out how and when people become capable of performing certain mental gymnastics. At the present stage it looks as though there are definite turning points in development, when certain mechanisms reach a threshold of operational efficiency and begin to have external effects. I must stress again the serious difficulties this raises - classical methods of observations and analysis do not allow readily for non-linear or threshold effects. The popular psychological notion of "insight" is one of these. In my model (PDC IV, pp. 32-33) there is a component labelled "Insight" which indicates when the "experience" of the model is adequate to justify external action. Mechanically it is a threshold device which does nothing until the stress on it reaches a certain value when it "fires" and transmits a single "bit" (binary digit) of information to a storage register. This is an abrupt event which apparently - seen from outside - has no precedent - nothing seems to be happening for a long time, then suddenly the whole situation is changed. Actually within the box, plenty is happening but it is not reflected in action. There is nothing mysterious in this of course, but it is important to realise that the link between a great deal of selective activity at the input and a quite elaborate novel action at the output is just a single "bit" of information which tells the storage system that a certain degree of improbability has been surpassed. This relation is very hard to put into words, and beyond my capacity to condense into algebra, which is why I made the model; when met with in a living organism it is terribly confusing and impressive.

That is why the existence of step functions or thresholds in physical development apart from behavioural effects such as "insight" must be investigated very carefully; if a system contains the two together the problem may be very nearly intractable. None of us is really trained to handle this sort of problem - it is hard to preserve one's dignity and poise going upstairs on skis. I say again - mind the step!



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/15  
22 August 1956

ORIGINAL: ENGLISH

REPLY TO PROFESSOR PIAGET'S ESSAY ON THE GENERAL  
PROBLEMS OF THE PSYCHOBIOLOGICAL DEVELOPMENT OF THE CHILD

by

Margaret Mead

In replying to Professor Piaget's challenging paper, I find that I must first distinguish between two approaches which appear in his statement. On the one hand he appears to say that in order to have a unified theory of development, itself dependent upon a common language, which will make it possible to bring our various materials together, we must recognize the three traditional divisions of (a) hereditary factors, (b) the action of the physical environment and (c) the action of the social environment, brought together in terms of a fourth factor, that of development, for which he proposes a formulation in terms of contemporary equilibrium theory. With this general position I am in full accord; I believe the development of such a theory is practicable and that its expression by the use of contemporary mathematical models may be fruitful, although I reserve judgement as to whether the adoption of economic emphasis - calculations of strategy based on gains and losses - is the most rewarding model from among the available models. This aspect of the problem is, however, the domain of General Systems Theory and I shall not address myself to it further.

However, throughout Professor Piaget's paper there appears to be a second and quite contrasting approach, in which the recognition of the importance of individual differences - as opposed to "average" performance of individuals at a given "stage", the recognition of the role of the culture in advancing or retarding any of these assumed sequences, and the recognition of continuity rather than "stages" - are all treated, not as providing additional and needed material for a general theory of the development of the child, but rather as opposing theories or disproportionate emphasis upon one of these three traditional factors.



I refer, for example, to the sentence on page 12: "Doubtless we are unanimous in believing in the existence of stages of development, apart from Tanner who stresses above all the continuity of physical growth, and Margaret Mead, because of the social factors involved." This sentence seems to me to nullify the usefulness of the theory which must be one which can include all the material from each of the three approaches. It seems tantamount to saying that it is possible to establish stages if one confines oneself to the study of the cognitive and affective behaviour of children in twentieth century Euro-American culture, and leaves out of account material on their physical development, and material on children in other cultures. As such an expectation contradicts the whole intention of Professor Piaget's integrating formulation, I merely mention it here, at the beginning, to stress that I am addressing myself to my alternative understanding of his paper, and not to the assumption that study of the factors of physical growth and culture automatically results in disagreement.

In regard to the question of "general stages", our present cross-cultural evidence, admittedly very fragmentary, suggests that it becomes decreasingly possible to relate different aspects of the child's behaviour to its chronological development, or to any other measure of development, with increasing age, but that the duration of development may nevertheless be of some significance in explaining different configurations of learning. I say "duration" to allow for periods of illness or regression, or for extreme differences in the amount of interpersonal interaction in the life of a given child, who may, for example, be said to be equivalent - in this widest developmental way - to a much younger or older child, because of the intervention of factors of acceleration or arrest of interpersonal contacts. For infants and very young children, the gross developmental conditions of learning to walk and talk would also seem to introduce, in all known cross-cultural contexts, a certain degree of generalization into all other types of learning which occur at the same time. However, it must also be recognized that differences in sequence of such learning as walking and talking, characteristics of different constitutions, or expressed in the learning arrangements of different culture - as for example when children are kept swaddled or cradled beyond the period when they could walk, or



hear phrases stated in their name long before they could formulate them themselves, may also be ways in which the behaviour of individuals within a single culture, or the regularities among individuals in different cultures, may vary to such an extent as again to make any idea of general stages appear unuseful.

If a theory of stages is conceived as a progressive series of equilibrium, disequilibria and re-equilibria, in which successive equilibrium levels, even of only momentary duration, may be distinguished, but in which in any given process certain fixed sequences may occur, this formulation can be applied, with our present knowledge, to the investigation of human development within different cultures. As Professor Piaget now formulates the problem, such exploration would have to be done in very great detail, using tests which were formally identical and culturally comparable on a series of identified children, whose physical development had also been studied, over a sufficient period of time so that transitions might be examined and analysed in the case of these identified children. The question of "the average age for the appearance of a stage" (Piaget, p.15), while useful as a corrective for ethnocentric overgeneralization from studies on children of a particular culture, seems to me to be of only very limited significance. Only when the actual succession of stages in the development of any process can be followed in identified individuals, in an identified culture, within an identified social unit (that is a group in which each individual's place in relation to each other individual is known) can we begin to relate the three factors together. I would maintain this as necessary because if, as Professor Piaget suggests, there are no general stages, then retardation or acceleration - (in terms of chronological age or developmental duration - as suggested above) in any one process - which must be attributed either to culture or to constitution - may have the most profound effects on the configuration of learning, and thus on the development of the personality. It may well be that our most acute understanding of the necessary constant sequences in any process may come in those processes where averages are possible - a position which has not, I believe, yet been demonstrated fully - but that for the understanding of total development, it will still be necessary to take account of the effects of different combinations among these constant or fixed sequences, which are themselves



systematically associated with genetic patterns or with cultural patterns. The specific study of identified individuals makes the distinction which Professor Piaget (p.23-3) draws between molar phenomena and molecular phenomena less significant for it makes it possible to address research immediately to the molecular level. When the general system of culture is examined as manifested in the behaviour of identified individuals in their interaction with a new member of the society - a formulation which permits more exact study than a formulation in terms of "generations" - it is then possible to relate this behaviour not only to the whole system of culture which may be expected to be sufficiently redundant to allow for the total genetic range - of contemporary survival possibilities - of homo sapiens, but also to the peculiarities of certain stocks within the society which have become isolated by various breeding barriers - such as those provided by class, cult, sect, occupational lines, etc. In this way it will be possible to investigate the degree of facilitation or inhibition existent, for example, in cultural systems which make a very slight use of mathematics, or in which the perception of time-space relations are very differently organized.

To address myself to the specific questions of Professor Piaget (p.30, III)

I would not say (with Durkheim) that "under all civilizations lies the civilization", but that all civilizations express the conditions of being human (la condition humaine), in that homo sapiens is dependent for his humanity - his survival as a species in a form we have called human - on a system of socially transmitted learned behaviour, and that this learned behaviour shows certain regularities which can be assigned to the requirements of man's biological characteristics - long infancy, properties of the central nervous system, etc. - in combination with the rest of the environment on this planet. Without such an assumption of regularities all comparative work between different cultures would obviously take a very different form, and such an assumption does underlie all contemporary work in cultural anthropology. The way in which children learn natural languages may be regarded as a case in point. As far as is known children learn languages which on other grounds may be classified as easy or difficult, and of many different types of complexity, at the same age in all cultures. This can be



attributed to two factors - the redundancy of natural languages and the fact that all first languages are learned in interaction between speakers and those who have not learned to speak at all, in the same way; the nature of speech, and the particular language spoken are communicated together. With the rationalization of the cultural understanding of languages, the development of such ideas as "a language", "grammar", "word", "alphabet", "verb", "predicate", "utterance", "phoneme", "morpheme", the process of linguistic learning is becoming progressively transformed. So, in response to Professor Piaget's second question, if an adult had to learn a language as a child learns it, without the intervention of any categories of linguistic analysis, there would undoubtedly be found many similarities in the order of acquisition. However, in all known societies, a difference occurs because the idea of the existence of different languages has already been formalized, and while the child learns to speak, the adult, having learned to speak, learns to speak a second language. It is quite conceivable that the systematic and very early teaching of the alphabet and of reading and writing might introduce into the first learning of the child a new factor which would make learning language in complex, fully literate societies no longer comparable with learning languages in non-literate or slightly literate societies, and that some effects of this sort may be making themselves felt in the present difficulties which are being encountered in the attempt to give a type of early education designed for children of the literate to children of the non-literate.

However - still in response to Professor Piaget's second question - it would appear that every cultural system contains within it the provision for the way in which it must be learned by children during their normal development, including sufficient leeway for a range in this normal development - in such respects as type of imagery, capacity to organize, type of memory, etc. - and that one route to a comprehension of another culture, or some complex part of a culture, such as the language, the legal system, the ritual idiom, etc. is to repeat the steps taken by children learning this system. This contrasts with the way in which a linguistically sophisticated adult masters the "grammar" of another language in a matter of hours, or a mathematically sophisticated adult masters a new type of mathematics. It would



seem that once having traversed the steps necessary to become human, in any culture, one may transform that learning, at an adult level, to any other culture, but that cultures differ in the developmental levels which they call into play in certain areas of experience. So western culture has now developed to a high degree ~~the~~ the type of thinking necessary for scientific endeavour, but leaves in a quite uncultivated state various capacities for introspective experience developed in Indian culture. For members of cultures which have not elaborated our type of scientific thinking, immediate transformation of previous learning into understanding of our culture may be possible only for the exceptionally gifted as it may mean an imaginative act of transformation covering a whole series of missing stages, in the form in which they have been culturally elaborated in our own society. For the less gifted, it may be necessary to include in any education in another culture a re-experience of earlier stages of learning, in the different cultural form. As I have understood Professor Piaget's discussions in our meetings this formulation is one which he feels is compatible with his material.

In conclusion, I should like to stress the significance of our failure, as a group, to pin down our various conceptualizations in any single set of individual identified real children. The insights which each of us has brought to these four rewarding years of work have all been based on very careful precise observations made, over time, on individual children, or in Konrad Lorenz's case, other living creatures in known social contexts. However much we may have concentrated on cognitive behaviour, or emotional disturbance, on behaviour as part of a group in a primitive culture, on electrocephalograms or physical measurements of growth, much of the background of the children studied, or of the other aspects of the children's development was always known, and always taken, however inexplicitly, into account. Not until this new integration proposed by Professor Piaget, and modified by the more formal inclusion of differences among children, and among cultures, can be applied to the detailed simultaneous study of a group of children, will we know what we have attained in this new way of looking at the psychobiological development of THE CHILD. And as I wrote the last sentence I realized that I should lament this, but not reproach ourselves for it, for our mandate was, after all, only to think about THE CHILD, to present an integrated set of abstractions which, hopefully, may be used to study CHILDREN in many different parts of the world.



UNITED NATIONS

NATIONS UNIES

WORLD HEALTH  
ORGANIZATION

ORGANISATION MONDIALE  
DE LA SANTÉ

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

WHO/PDC/10  
15 March 1956

ORIGINAL: ENGLISH

STAGES AND MECHANISMS IN PHYSICAL GROWTH

Comments by Dr Tanner on Professor Piaget's essay

Two of the main questions raised by Piaget interest me closely: the existence and stages of development, and the mechanism of transformation from one stage to another. The first of these questions I think largely spurious; that is I consider it chiefly a matter of the different use of words by different members of the group. Here I am in total agreement with the remarks made by Lorenz on this subject at meeting III. An example below will, I hope, make my standpoint clear. It will show, amongst other things, that I assume Piaget's conditions for existence of a stage numbers 1, 2, 3 and possibly 4 (page 15-16) as existing all the time throughout growth and governing continually the physical growth of the organism from moment to moment. I would not myself consider these conditions sufficient warrant for the use of the word "stage".

The second question, that of the mechanism of transformation, I consider the most fundamental question in our whole field and probably the most important. I am only sorry that I can contribute practically nothing to its elucidation, whereas as a physiologist I might be expected to contribute perhaps the most. The fact is that physiological ignorance on this matter is profound, and professional physiological interest and experiment almost non-existent.

Dealing now with the specific questions addressed to me:-

1. The immediate cause of the chief phase of acceleration, that at adolescence, is well known. The spurt in bodily growth is caused by the release of hormones from the gonads, adrenals and pituitary. The cause of the increase in gonad and adrenal secretions is the release of other pituitary hormones, and the release of these pituitary hormones is caused by certain events, the nature of which is obscure, in the C.N.S.



The most we can say about this at present is that at some stage of development some areas in the C.N.S. reach a state of maturity X, and as soon as X is reached, messages pass to the pituitary to release hormones previously stored there, and adolescence ensues. But what causes state X to appear? This question is in my view the fundamental one in all the study of physical growth.

We can sum up all we know about X by saying:

- (i) X does not depend much on the size of the organism; it depends more on the percentage of ultimate adult size achieved i.e. X appears more nearly at say 80 per cent. final size than at a size of so many grammes.
- (ii) If the organism is starved and growth retarded, X is retarded. If the organism is given food after a length of time spent at constant weight, then X appears at about the same percentage of adult weight reached, quite irrespective of how many days have passed. The appearance of X is related to internal maturational time, not to chronological time.
- (iii) The moment when X is reached can be best predicted from observing the previous course of growth. It will then be seen that X occurs chronologically early in organisms all of whose progress-to-maturity has been swift or "advanced". That is, children with a bone age of 11 at chronological age 9 reach X earlier than children with a bone age of 9 at chronological age 9.

This leads one to suppose that the state X proceeds from state W, which in turn proceeds from state V etc. One can think of growth and development as a continuous series of states  $\rightarrow U \rightarrow V \rightarrow W \rightarrow X \rightarrow$ : the organism may be temporarily arrested in any of the states, but the order in which they occur is always the same. One cannot proceed, for example, from U to W except through V, unless some pathological disturbance supervenes.

This formulation lends itself to a symbolization of what I believe to be the relation between those who use the concept of stages (Piaget for cognition) and those who prefer the concept of "continuous" development (Piaget for perception, myself for physical growth). If we symbolize observable change by length of arrow and state by length of dotted line associated with the capital letter, we have:

Stages:  $\rightarrow \dots\dots\dots U \dots\dots\dots \rightarrow \dots\dots\dots V \dots\dots\dots \rightarrow \dots\dots\dots W \dots\dots\dots \rightarrow$   
Continuous:  $\longrightarrow \rightarrow U . \longrightarrow \rightarrow V . \longrightarrow \rightarrow W . \longrightarrow \rightarrow$



In part, I feel sure, the idea of "stages" arises from an inability to measure small increments of function; but in part the idea may truthfully reflect a situation where in one part of the organism no change is occurring (while maturation by continuous growth meanwhile goes on in another part).

As to the mechanism of these  $U \rightarrow V \rightarrow W$ , and the question of equilibria: we know very little about this and I can only repeat what I said at our first meeting - that the process of maturation seems to me like a series of clocks, the signal for the starting of one being the running-down of another. I would now complicate this a little by adding that there are many clocks all going at once, and clock B starts when A reaches 4 o'clock, clock C when A reaches 6 o'clock and so on.

Undoubtedly there are feed-back mechanisms wholly within the C.N.S. governing the  $U \rightarrow V \rightarrow W$  mechanism. Whether there are feeds-back of the sort  $C.N.S. \rightarrow \text{endocrines} \rightarrow C.N.S.$  or  $C.N.S. \rightarrow \text{endocrines} \rightarrow \text{peripheral tissues} \rightarrow C.N.S.$  we do not know. (Equally we have no really clear idea of how far environmental stimulation, e.g. by social conditioning or simple physical exercise, can enter the feed-back circuit). We do know of various substances (both estrogens and androgens) which will speed up maturation of particular bits of the organism - for example the ossification of the bones at the wrist, or the appearance of pubic hair - but we do not know of any substances or any treatment (except starvation) which will speed up or slow down the rate of development of the organism as a whole, that is, while maintaining its normally balanced structure.

2. Our knowledge of anatomical and histological changes in the human brain after the age of six months is virtually nil. Probably by the age of nine months, and almost certainly by one year, all the cells and all the fibres of the C.N.S. are present, and all the fibres ultimately to receive myelin have begun to become myelinated. After this time, however, the diameters of the myelin sheaths increase, and probably also the size of some nerve cells increases. About detailed histological changes at the periods  $1\frac{1}{2}$  - 2; 7-8; 11-12, we have no information whatever. I leave the question of EEG evidence for functional changes to Grey Walter to answer: apart from the EEG I do not think there is any useful physiological data on C.N.S. function during this time.

3. See my answer to (1) above.



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/13  
16 August 1956

ORIGINAL: ENGLISH

COMMENTS ON PROFESSOR PIAGET'S ESSAY ON THE GENERAL PROBLEM OF  
THE PSYCHOBIOLOGICAL DEVELOPMENT OF THE CHILD, AND ON THE  
COMMENTS OFFERED BY PROFESSOR BOWLBY AND PROFESSOR BERTALANFFY

by

Konrad Lorenz

I think it advisable to answer Professor Piaget's questions to myself first, and then to proceed to what I have to say on the conceptions of "development" and of "stages" as well as on the urgent necessity of a "common language".

Professor Piaget's first question to me was whether there is not a danger of vitalism surreptitiously introduced by my attitude to the "aprioric" forms of thought and categories. He calls this attitude "dynamic apriorism" - and I think that this term is entirely misleading: I am profoundly thankful that it is so, because any sort of apriorism, however dynamic, would indeed lead to the danger Professor Piaget fears. I am quite convinced that everything that conforms to Kant's definition of the aprioric, e.g. everything that exists in our mind before any experience and which must be there in order to make experience possible, is nothing that exists in the absolute, nothing that is really there a priori. All the forms and functions of our mental processes that really exist independently of experience are correlated to forms and functions of our central nervous system and have developed in phylogeny just as the forms and functions of any of our other bodily organs. All structures and functions have attained their present form in an age-long interaction between the organism and its environment, nothing whatsoever is preformed, unless it be the basic properties of the smallest known physical units. (A short time ago I probably should have said "of the atoms", but nobody knows how much farther down the atomizing will go in the near future of physics.) Nobody in the world is less of a preformationist than is the phylogeneticist. If I may widen the concept of the empiric so far that it includes

not only what the individual derives from personal experience, but everything that the species gains out of its interaction with outward reality, then I should definitely call the attitude assumed by ethologists towards the problem of the "a priori" one of an extreme "phyletic empirism".

The second question, if I understand Professor Piaget rightly, is whether a process of functional "equilibration" is not much more general and primary than the function of innate releasing mechanisms and learned responses. I would, however, at this point, emphasize my agreement with Professor Bertalanffy's objection to the term equilibrium. Before having read his contribution, I commented on Professor Piaget's supposition (page 8) that "certain sectors of the organism can be considered as a closed system" by remarking that this supposition was not admissible even for argument's sake: the closed system and the living system are a perfect contradiction in adjecto. If I may substitute "adaptive interaction" for "equilibration", as I assume I may, the answer is simply and emphatically yes. There definitely are organisms which do not have any instinctive movements or innate releasing mechanisms and also are quite incapable of learning. All organisms are open systems and all of them live only by achieving a regulative equilibration between their inner processes and the requirements of their outer environment. The functions of innate releasing mechanisms and of learning are those only of very highly specialized organs that have been developed by higher animals in the service of that general regulative equilibration and have done so under the selection pressure exerted by its necessity. The same applies to searching behaviour, to all cognitive functions, in short to all structures and functions which develop a survival value. I do not think that the term "compromise" is very descriptive for the co-operation of the innate and the acquired. An organism can be "constructed" in very different ways by all the factors affecting evolution, of which I still think natural selection to be the most effective. A grebe is "so made" that it needs to learn very little in order to survive, having beautifully specialized innate responses and organs, a raven needs a lot of learning and correspondingly is furnished with an inexhaustible source of exploratory behaviour: both "constructions" are equally successful in surviving.



The third question concerns my statement that "logical necessity does not exist per se but corresponds to laws of the nervous system". Professor Piaget fears that the acceptance of existing "laws" may lead back to preformist apriorism. It does not, because the "laws" in question are by no means logical necessities. None of the biological "laws" are. Mendel's "laws" would be entirely different if the structure of chromosomes and the processes of fertilization were not exactly as they are, which might easily have happened if evolution had run a slightly different course, as it might easily have done. Exactly the same applies to all the "laws" prevailing in the function of our brain.

The last question is whether there are any objective criteria for distinguishing mere compromise solutions in cases of conflicting motivation from more stable equilibrations. It is one that is occupying ethologists most seriously. Indeed, the distinction between a mere epiphenomenon and a function which serves "equilibration", in other words one that develops a definite survival value, is, in many cases, of the utmost importance. It can, however, only be answered for each single case separately and only by a thorough experimental investigation.

I now come to the question of common language which, incidentally, is more or less identical with the problem of synthesis. I confess that I heard of general system theory for the first time when I read Professor Bertalanffy's comments, so I know no more about it than what he said in his first three pages. My question to Professor Bertalanffy may therefore be quite beside the point: but is there not a certain danger that, in order to make different systems comparable and describable in the same "language", we strip them of characters which, because they are not comparable, seem to be accidental or non-essential frills from the point of view of theory, but which are highly characteristic and essential to the proper understanding of each of the systems separately?

On the other hand, the study and comparison of extremely different systems may reveal the surprising fact that they contain mechanisms that are directly comparable. Modern physiology of perception in particular and neuro-physiology

in general have discovered processes which are not only comparable, but essentially identical with those known to cybernetics. I entirely agree with what Bertalanffy says about the danger of using fashionable words in a loose way, but this is certainly not the case when Mittelstaedt or Von Holst use cybernetic terms in their studies of optokinetic movements or the function of the muscle spindles. Indeed, the processes investigated in these cases are classically simple examples of positive and negative feedback mechanisms, and it would be a great error and a hindrance to mutual understanding not to use these terms.

Another example: at our last meeting in London I was trying to explain the controlled use of Gestalt perception in the study of animal behaviour. I am afraid it took me a very long time to expound how very many repeated observations of the same process are necessary before our Gestalt perception at last succeeds in disentangling the essential lawfulness from the "background" of inessential, accidental sensory data. Grey Walter was sitting beside me and, looking over his shoulder, I was slightly taken aback to see that he had compressed the whole symphony of what I had been trying to explain into one sentence. He had written: "Redundancy of information makes up for noisiness of channel".

This is an example of a perfect translation of the kind that general system theory should strive for. But we must keep in mind that this kind of mutual understanding is only possible wherever two independent investigations have reached a comparatively high degree of insight into the process investigated. Gestalt perception is a function dependent on a neural organization that is very much akin to a true computer and which consequently lends itself particularly well to a description in the terms of information theory.

In the majority of cases, however, our insight into what really happens in an organism is much too superficial to permit a translation that grasps this kind of fundamentals. We must never forget that the words we use are connected with conceptions of vastly different degrees of clarity. If I speak in the same breath of instinctive movements and of innate releasing mechanisms, I cannot help suggesting, in a most insidious manner, that the conceptions symbolized by these



two words are of approximately equal value. They are not. We can make, to say the very least, a pretty shrewd guess as to the physiological nature of instinctive movements, while we have but the haziest ideas concerning the physiological mechanisms underlying the function of an innate releasing mechanism. Therefore, what ethology calls instinctive movements (I do not accept the English term "fixed pattern" which I deem undescriptive and even misleading, as it implies too much rigidity) can be described tolerably well in the terminology of Von Holst's studies on central co-ordination, while the conception of innate releasing mechanism, which is only functionally determined can not be translated into anything at all until we know much more about it than we do at present.

Nevertheless, these hazily defined conceptions correspond to something real. I have much confidence in the ability of our Gestalt perception to pick natural units out of the immeasurable chaos of sensory data. If an observer like Piaget calls something "affectivity", I rely blindly on the assumption that there is a natural unit corresponding to that term. But I find it very difficult to ascertain what exactly that unit is. All conceptions of this type are what Hassenstein has called "injunctive". Injungere means to enjoin. A number of characters are "enjoined" in order to make a special case fit into the contents of the conception. A number of constituent properties go into the making of the conception, but none of them ever is "constitutive": they constitute the conception only by a process of summation. A special case may lack one, or even several, of these properties, and yet not be excluded from the contents of the injunctive conception. Metabolism and reproduction are indubitably constituent characters of life, yet an undercooled anthrax spore which has no metabolism, or an ox which cannot reproduce, are unquestionably alive. Symbolic speech is a constituent character of Man, yet a patient with total aphasia still is one, etc. etc. All injunctive conceptions merge, without any clear boundary-line, into neighbouring ones which have one or several part-constituent characters in common.

Of course all the words which we coin to describe natural units, of whose existence we are told by our Gestalt perception, necessarily refer to injunctive conceptions exclusively. When we first say "wow, wow", we do not ourselves know whether we mean this dog, any dog, any placentarian, any mammal, any four-legged animal or perhaps everything alive. It is quite difficult to find out what part-constituent properties one enjoins oneself, in order to range a special case under the heading of an injunctive conception. And it is still harder to know exactly what another man is enjoining when he is using the same term. Injunctive conceptions may not only vary as to the size of their contents, but their contents may overlap. The trouble is that real natural units may overlap. Take a zoological example. Every naive person seeing a lamprey for the first time would say it is a fish. It has eyes, gills, a silvery surface, etc. just like any other fish: but it has no jaws. Anybody with an inkling of comparative anatomy would see in an instant that a shark, a frog and a man are more closely related to each other than all of them are to a lamprey. "Fish", including the cyclostomes are a natural unit, and "fish", as a class of gnathostomes, excluding the lampreys, are also a natural unit. Which sort of unit is reported to a given man by his Gestalt conception, and what he consequently subsumes under an injunctive conception, depends on the man.

Consequently, you have to know that man and his whole way of thinking and observing just in order to know what he means when he uses one single word. And the more of an observational genius the man is, in other words, the more unexpected natural units his Gestalt perception makes visible to him, the more difficult we shall find it to get hold of the part-constituent characters that make up his injunctive conceptions. Indeed he will find it so himself! I am sure that Professor Piaget will take it as the compliment which is meant when I say that he is a very difficult man to understand - in the respect just discussed. I do not know what he means, for example, by the word "affectivity". John Bowlby, in his comments, has attempted to translate it into ethologese, defining the conception exactly as I would, but I do not expect Professor Piaget to feel himself very deeply understood.



On the whole I think that we have done marvellously well in learning to understand each other. A good symptom of this is if one finds oneself adopting another person's concepts - not the word, mind, but the concept. Speaking for myself, I have done that extensively. Frank will have found on his recent visit to Seewiesen that the conception of the case-history, which formerly did not play any role at all, now looms very large indeed in our daily work. Conversely, I find some of our study group, particularly John Bowlby, using ethological terms naturally and correctly.

Correct mutual understanding, in other words, exact coincidence of conceptual contents correlated to the words used, is, of course, the primary condition without whose fulfilment there is no hope for a real synthesis of several people's work.

Synthesis of several people's work is nowhere more necessary than in the study of development. This term is, of course, again correlated to an injunctive conception of immense complication. But in the case of words used in common parlance it is, on principle, not necessary to go into a detailed conceptual analysis in order to achieve mutual understanding. We are, I think, all agreed upon what development is and I may start what I have to say about the synthesis of our work by quoting Goethe's old definition: "development is differentiation and subordination of parts". The two hemispheres of a globular, blastula- or volvox-like creature decide to divide the functions of nutrition and defence between themselves, each of them specializing for one of these tasks and consequently becoming as different from the other as ectoderm and entoderm are. By the same act, they become more "subordinated" to the whole system, as they become dependent on each other, each being incapable of fending for itself. This most clear and primitive division of labour that ever took place in a metazoan ought to furnish a good example of what "development" is like and how it ought to be approached in theory. The change of each part has a counterpart in the change of all the others. "Differentiation" always means "becoming different" and the question "different in relation to what?" ought always to be in our minds. In the case of the literal and spatial differentiation of the blastula this question



is easy to answer and it is still answerable in the early stages of embryonic development in which a comparatively small number of tissues have become different from each other so that it is still possible to keep track of the interactions of their functions. Physiologists of development have done amazingly well at these particular tasks. We, of this study group, ought to take the work of experimental embryology as a model, if only to make ourselves realize how immensely difficult our problems are. John Bowlby has already proposed a view of psycho-physiological development which makes use not only of Goldschmidt's principle of harmonized reaction velocities (page 3 in his comments); he has also, without explicitly saying so, introduced another indispensable concept of experimental embryology, that of "regulative" and "mosaic" interaction between the developing parts. Luckily for the analytic biologists, organisms are not "wholes" in the sense that "everything" is in a regulative interaction with everything else: there are some few relatively autonomous structures which influence the rest of the system far more than they are influenced by it in return. These are the archimedic points on which to base investigation. These comparatively invariable and autonomous elements are necessarily more often causes than they are effects in the immensely complicated network of interactions taking place in development. For the same reasons, for which investigation and didactic representation of a whole organism invariably start from its skeleton, we ought to try first to get hold of the most autonomous and independent processes of structural and functional development.

Another reason for doing so is that the harmonization of reaction velocities is most liable to go wrong or to fail in regard to these relatively autonomous processes. I think that Kretschmer is entirely right in attributing a large number of psychological disturbances to the disharmonization of the velocities with which a number of structures and/or functions develop in an individual. In the greylag goose, that invaluable simplified "model", we found that practically all disturbances of sexual functions are due to disharmonization



of developmental velocities in relatively autonomous activities. Oedipus behaviour arises in exactly the way Kretschmer supposes and male homosexual pairs are formed when a certain stage of courtship activities is "skipped" because of environmental conditions which prevail in a state of semi-captivity but which may also, often enough, occur in the wild. Helga Fischer has recently found a highly interesting mechanism by which these homosexual pairs are broken up later on and the partners brought back to "normal".

Even in geese we find it quite unfeasible to describe "stages" in the development of behaviour as a whole. Well defined "stages", however, are found in the development of single, relatively autonomous activities and well defined types of disturbances can be correlated to the temporal lack of coincidence of stages, particularly in individuals with a certain amount of domestic inheritance. But also in pure-blooded wild birds the variation of developmental velocities in different activities is so enormous that it would take a very forced and artificial abstraction of a type termed "normal" to make it possible to speak of "stages" in the development of the whole organism. I confess that I have very strong doubts that the variability of developmental velocities in the child is less than it is in a wild goose and I therefore emphatically agree with John Bowlby's objection to the typification of "stages" in the development in humans. But I have no doubt that very real types of personalities can be explained on the basis of coincidence and non-coincidence of stages in the development of relatively independent structures and/or functions.

The "moral" of all this is perhaps a platitude: each of us ought to be constantly conscious of the fact that he is only investigating the development of a very small part structure and/or function. Each of us ought to be looking constantly for lawful coincidences and non-coincidences of the "stages" he is finding himself in the developmental processes he is investigating with those that another is studying. Each of us ought to be searching constantly for lawful and harmonizing interactions between the processes he is working on himself and the most unexpected and far-fetched developments in other parts of the organism, even if the latter do not interest him in the least. But we ought

not to postulate a priori that such an interaction exists. We know there are highly independent mosaic parts and whether or not they interact, and if they do, to what extent, are problems that must be investigated singly for every single case.

I quite agree with what Professor Piaget says about interactions being "at least as important as their respective actions". But I do not think that we should assume them without knowing whether they exist, and I do not think we should limit the number of interacting factors to three, as Professor Piaget suggests we should. We do not know the number of interacting factors.

These minor amendments notwithstanding, I entirely agree with what Professor Piaget suggests concerning the necessity of synthesis. I agree even that most of us overrate the importance of the structure and/or function he himself is interested in, though I do not think that we are quite as black as he paints us. Margaret Mead certainly does not explain "everything by the social factor", Zazzo does not "base everything on the maturing of the nervous system and the social factor" and neither ethologists nor the EEG people really believe that spontaneous nervous activity is everything and overlook the existence of reflexes and learning. But it is very probably very true that each of us is underrating the importance of what each of the others is studying and if Professor Piaget, for the sake of brevity, overstates the situation, his warning is nevertheless one that I, for one, shall most seriously take to heart.



STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Session  
Geneva, 20-26 September 1956

19 September 1956

SOME REMARKS ON PROFESSOR PIAGET'S ESSAY

by

René ZAZZO

Your essay on the general problems of the psychobiological development of the child has given me once more the opportunity to take up the thread of our discussions. Perhaps never before did you express your point of view so precisely, with such deductive reasoning, in such a masterly manner. For me, never before was the pattern of your thought at once so seductive and so irritating. I am not joining in the discussion in order to continue in my own style the Wallon-Piaget controversy, where all too often neither side listened to the other, nor in order to put forward a personal system which might replace yours, but rather in order that even more weight should be added to your argument, thus entailing my full adherence - or that its fine balance should be upset.

1. The concept of interaction between factors of development

Partitioning into compartments is dangerous, but it is necessary to distinguish between factors. This second proposition must be stated as clearly as the first if we are to avoid falling from atomistic error into a confused globalism.

Moreover the interaction you mention presupposes the existence of factors. Of course, it must be pointed out that these factors do not act as independent variables. It is, for example, an organism which "chooses" and organizes its environment in accordance with its heredity, and in turn this environment acts on the expression of the heredity, and perhaps eventually on its transformation. I agree with you, and the only fault I would find in your formulation would be undue caution. You say "The interactions (of factors)



are at least as important as their respective actions". It seems to me that there are no isolated actions. The action of a factor is accomplished and can be analysed only through interactions even if, in extreme cases, this action of a factor strongly dominates the others.

Although I am stressing this conception of the interaction of factors it does not mean that I read "the factors do not exist" when you write, correctly, that partitioning is dangerous. No personal criticism of your intentions is being made. A misunderstanding is, however, possible. The authority attached to your statements runs the risk of being exploited by all manner of psychologists who delight in the philosophy of "undivided wholes". This exploitation is simplified by the fact that the nature of your work does not absolutely oblige you to take into account factors of a biological or social order. Considerations of differential psychology hardly appear in your work. This is not a matter of chance. Your attempt at analysis proceeds by other means and on another level: to such an extent that when studying behaviour structures you go so far as to propose the existence of a new factor, equilibrium, which obeys its own laws and can occur independently of the classical factors of heredity and environment (p.7).

You do not deny the factors of heredity and environment: but you do not have to deal with them. One might therefore have the impression that for you they are more or less favourable circumstances but that they are not essential causes or principles for the explanation of phenomena.

## 2. Concept of equilibrium

The phenomena of equilibrium and regulation are incontrovertible. But I am never quite sure:

- (a) of understanding exactly the explanatory significance you give to the conception of equilibrium considered as a factor;
- (b) of agreeing with you about the extensiveness which you attribute to the phenomenon of equilibrium in all natural fields - physical, psychological and sociological.

First of all I find it rather difficult to admit under the same category of factors heredity, environment and equilibrium. It seems to me that



equilibrium is always a relationship, a law of organization between elements or between various causal series.

If one classes equilibrium as a fourth factor next to material causes and conditions one runs the risk either of substantialising, of hypostatizing the laws of equilibrium, or else of dematerialising the material factors of development, making them disappear in a pure game of intemporal relations, a mental algebra.

Moreover I was very much struck by the fact that the concrete examples you give of the independence of the equilibrium "factor" are relative to the solution of a problem (guided testing out, feed-back), at a given stage of development and not to an actual genesis: as if what was essential was the equilibrium, a sort of final cause, transcending the conditions of development. Here you stress that which does not change (the eternal and general laws of logic) and you do not consider, as such, that which changes.

In any case it seems to me necessary to distinguish between the process of equilibrium during genesis (an equilibrium established between well-defined material conditions) and the process of equilibrium as the search for a solution.

### 3. My personal method of co-ordinating the factors

You let it be understood (p.3) that every investigator tends to underestimate certain aspects of the real to the advantage of aspects characterizing the field in which he is working. This remark is very true, and this is why I would never make the mistake of reproaching you for neglecting, for example, considerations which are essential to differential psychology.

As regards Wallon and myself, you will remember that we tend to bring everything back to the maturation of the nervous system and to the social factor.

I agree here entirely, but would make two very important matters quite clear:

- (a) I do not consider these factors of maturation and environment to be



additive. They determine each other in a progressive integration. Moreover the conception of the social factor runs the risk of creating misunderstanding if one understands by it only the general framework within which the individual's activity is carried out. The conception of syncretic sociability defined by Wallon at the stage of pure emotivity implies a very archaic level of the "social" and at the same time stresses the original interdependence of the organism and the social. I personally have used the method of twin investigation rather in order to study the dialectics of inter-individual relations than to solve the classical problem of the relative importance of Heredity and Environment (which seems to me a very ambiguous problem in any case). This corresponds with what you call (p.23) - using a debatable term - molecular phenomena of social life. This is debatable in my opinion because the idea of the molecule may lead us to think that social life in the "molar" sense (general form of society) is only the combination of individual interactions.

(b) Psychism cannot be reduced to factors. The human being is not reduced to the conditions of his existence.

A new reality arises from the processes of integration of the different factors.

Through his actions and his conscience (whatever the definition given to conscience) the individual becomes in turn the agent of his development and his behaviour. Although I am reluctant to admit that equilibrium is a factor, I nevertheless consider that, in the dynamics of development, effects become in turn causes. Here we reach the problem of conscience, of interiorization, and of autonomy, which scientific psychology cannot solve by denial.

#### 4. The concept of a common language

The scientific mind requires, of course, that the same things should be said in the same terms and that the same term should be applied exclusively to the same thing. Specializations are accompanied by conceptual organizations and jargons which are often undecipherable from one specialization to another. One can only agree with you when you speak ironically of the incoherence of



present-day psychological descriptions, and when you demand a systematic effort at clarification of concepts (p.5).

However, your argument may sometimes lead one to believe that the idea of a common language and the isomorphism of the different levels of reality are interdependent, for example when you speak of the "translation from one viewpoint into another" (p.5). Whether it is a question of EEG, intellectual operations or social relations, their common laws of strategy, economy and equilibrium would authorize a common language. Of course, the more clearly the identity of the laws emerges from the diversity of the phenomena, the easier it will be to establish a common language. Moreover, scientists have a strong tendency to attempt this reduction of reality, this "identification" as Emile Meyerson has shown so well in his famous thesis Identity and Reality.

However, we cannot postulate what still remains to be demonstrated and consequently we must not base a common language on what is only a heuristic attitude or a working hypothesis.

In short, if "common language" signifies use of common terms to designate the same things, then I am in agreement; if "common language" is the expression of a postulate of isomorphism, then I do not agree.

Certainly our various languages must become inter-coherent in their attempt at expressing the coherence of the various levels of reality. We must look for analogies, parallelisms and common laws and also, within the same plane and from one plane to another, relations between cause and effect where the effect cannot be reduced to its cause.

Practically speaking, the search for a common language is not just limited to the search for common terms in special scientific fields, but includes the statement of clear definitions, that is to say communicable without ambiguity outside the narrow circle of a speciality.

##### 5. The concept of stages of development

I do not think I have understood very well your criteria of the concept of stage. However, I have here two preliminary remarks to present to you:



(a) I am not certain that your definition applies to all that has to be defined, to everything that might be considered as a stage. You have defined the concept of a stage on the lines of what you have observed concerning intelligence. When demanding criteria which are perhaps special to cognitive functions (p.15) and particularly the characteristic of structural equilibrium, one runs the risk of neglecting in other fields the existence of stages which would be expressed only by a constant order of succession and by integration. One runs the risk of ending up for example with the syllogism which would sterilize all scientific initiative: the stage is an equilibrium structure, but personality is not expressed by such a structure, therefore there are no stages in the development of personality.

In the actual state of our knowledge it seems to me much more profitable to agree upon a much less exigent definition, which would be less restrictive, having as criteria only a constant order and integration.

(b) I am not certain that I have correctly understood your attitude concerning the evolution of non-cognitive functions. Sometimes it seems to me that you are sceptical about the existence of stages, other than intellectual stages: in particular, you mention on several occasions that the cognitive factors correspond to the structure of behaviour whereas the affective factors correspond only to their energy component (p.15, p.24).

#### 6. Mechanisms of passage from one stage to another

We must certainly come to an agreement as to what we mean by the word stage. But that is not enough. The controversies, frequently, confused, at which we have been present, at our Study Group meetings and at the last Congress on Psychology for French-speaking participants, clearly showed that we must be fully aware of, and state clearly if necessary, the concepts which take account of the mechanisms and the dialectic of change: concepts of threshold, passage from a quantitative growth to a qualitative transformation, etc.

I should like therefore to come back to some commonplace affirmations which have been neglected during our discussions on the existence of stages:



1. The continuity of a growth (level of calcification, length of limb, increase in angle measuring muscular extensibility, degrees of myelinisation) does not exclude a priori a transformation or a discontinuity on a functional level.
2. At a higher level of complexity the continuity of growth of physiological components or conditions (whether or not they obey the laws of allometry) can determine the appearance of stages on the psychological level. Thus we must look for the coherence of development rather in causal liaisons than in the correspondence of stages, whether or not the development occurs in successive levels.
3. The psycho-physiological evolution of the individual leads him into increasingly complex and extensive environments which can act as external organizers and thus determine stages.
4. It is true that the statement of dominant characters is frequently arbitrary. However, we must distinguish between a subjectivity which brings with it no proof, and an entirely legitimate relativity of points of view. The stages can vary according to the codes one uses for deciphering, but obviously each code must be clearly defined.
5. The hostility of many authors towards a negating conception of stages frequently arises from the desire to preserve whatever might be qualitative in the transformation of the individual, whether it is a matter of psychology or physiology. It is necessary to stress strongly that a qualitative transformation (of the personality, for example) can well be conceived without discontinuity: a gradual transformation, as in light spectra. In this respect the concept of crisis remains to be clearly stated.

WORLD HEALTH  
ORGANIZATION

ORGANISATION MONDIALE  
DE LA SANTÉ

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

WHO/PDC/18  
13 September 1956

Fourth Meeting  
Geneva, 20-26 September 1956

ORIGINAL: FRENCH

DEVELOPMENT OF THE ELECTRICAL ACTIVITY  
OF THE CHILD'S BRAIN

Longitudinal series of EEG examinations  
over a period of five years

by

Marcel Monnier

A. Criticism of method

A study of the electro-cerebral activity in the child by means of repeated EEG examinations in longitudinal series is subject to certain reservations of a methodological order. These reservations are valid not only for our investigations but for electroencephalography in general. The technique is in fact both objective and subjective, it is objective inasmuch as it comprises a physical recording of physiological processes expressed by variations in electrical potential, and subjective inasmuch as the EEG tracings thus obtained are analysed and interpreted empirically by an experienced specialist. Even if one has recourse to certain physical procedures for analysing the tracings, by means of automatic frequency analysers, correlators, or integrators, one can define objectively only certain parameters such as frequencies, voltages and phase relations. These objective methods of analysis are nevertheless unable to show the pattern, that is to say the temporo-spatial configurations of the electrical activity as a whole. In other words, what is essential in the expression of the processes of organization and maturation remains objectively indefinable. Because of its complexity the development of a pattern can be empirically and qualitatively characterized only through the medium of an experienced electroencephalographer. We are, therefore, forced either to accept the principle of this method together with its imperfections or to give up investigations of this type. We have chosen



the first alternative but are nevertheless perfectly well aware of the objections which it might arouse.

Another technical difficulty lies in the fact that it is not always possible, for practical reasons, to repeat the examinations at sufficiently short intervals (for example, three months) and at absolutely equal intervals. Thus we have had to be content with repeating the examinations every six months only and sometimes even with variations of several months. Too great an interval between the examinations, or the omission of a control examination, could grossly falsify the results of the comparison. In fact if, instead of comparing the EEG of a certain age with that of an age six months earlier, we compare it with that of an age 12 months or more earlier, we get the impression that a sudden transformation has been produced in this interval and a level of development has been reached. If, on the other hand, we compare EEGs at shorter intervals (for example, three months) we get on the contrary the impression of a more continuous development. For this reason we cannot conclude that a transformation has been achieved unless, during examinations repeated at sufficiently close intervals (every four months if possible, or every six months as a maximum) the changes perceived are significant, constant and reversible. Only under these conditions can one say that the transformation observed corresponds to a period, or to a stage in the level of development, or a stage in Piaget's sense.

Finally, it is necessary to know that the EEG is influenced by the affective state of the subject at the time of the examination. Nervous tension, emotive reactions and impatience can mask the fundamental activities on which the interpretation is based. This difficulty by itself justifies the repetition of examinations at sufficiently short intervals.

Having studied these methodological reservations let us review once more the main results of our investigations.

B. Results

1. Continuity of development according to a structural plan expressed by the electrographic pattern of the young adult

Quantitative analysis of EEGs in longitudinal series shows that the development of cerebral electrical activity occurs continuously and progressively. It is organized according to a structural plan (Bauplan) which is expressed partially at every recording but only completely at the threshold of adulthood. Thus, the 15-year-old adolescent shows a definitively organized pattern towards which the still imperfectly structured electrical activity of previous periods of development of 6-15 years was tending. The finished electrographic pattern of the young adult therefore enables the structural plan to materialize; it is the mould, it synthesizes the various elements of the processes of development and maturation.

2. Within the continuous development one can distinguish certain epochs at which the organization of the electrographic pattern appears to become more intense, settled and consolidated. The resulting acquisitions mark then certain levels of development. These epochs, which give the impression of a certain discontinuity of development - more apparent than real - merit the name of stages in Piaget's sense.

Thus the child Vincent reaches a first stage of development in his cerebral electrical activity at 6.7 years, then a second at 9.3 years. The acquisitions of this second stage are, however, already manifest, though not constantly, during the eighth year (8.7 years). Once this last stage has been passed examinations carried out during the course of the tenth year do not show any new levels but enable us to foresee the acquisitions which will become concrete only at the beginning of the eleventh year. As regards the stages of development during the pre-pubertal and pubertal period we have some information from the examinations carried out on Vincent's two brothers. François' EEG attains a new level at 12.10 years. Finally in both boys the stage of the adolescent cerebral electrical activity is achieved towards the fifteenth year. These various findings are summarized below.



At each of these stages the electrographic pattern carries the stamp of the structural plan (Dauplan) which is, however, not expressed completely and definitively until the threshold of adulthood. This means that the elements of the general pattern evolve at each stage and retain between them the stable relations which are precisely the characteristic of the structural plan.

3. The characteristics of complete development shown by the electrographic pattern of the adolescent of 15 years are the progressive differentiation, systematization and localization of the cerebral electrical activities.

(a) Progressive differentiation, organization and systematization are shown particularly in respect to the alpha rhythm, whose slower and faster polyrhythmic components diminish in favour of a dominant component and of a more monorhythmic, monomorphous structure whose frequency increases progressively from 8 to 10 c/s in the subjects examined. These dominant rhythmic components increase in abundance, regularity and voltage in the posterior postero-lateral regions with a clear tendency to increasingly narrow localization in the occipital regions of the two sides and the predominance of one side (lateralization). Moreover, the alpha rhythm frequencies tend to become organized into harmonious spindles. This progressive structuration is brought out also by photic stimulation at the moment when the alpha rhythm, refusing to be drawn on by the stimuli to a frequency above 13 c/s, takes back full autonomy. It then appears as a pattern which is symptomatic of the degree of development.

(b) Parallel with the organization of the alpha rhythm the activity of the rolandic temporal regions becomes less and less polyrhythmic and polymorphous; the slower components of the delta band diminish in abundance and in voltage; the theta-rhythm-frequency band becomes narrower. The results are more monomorphous activity with predominance of one of the components of high voltage (6-7 c/s) in the temporal region. Finally the theta components in their turn diminish in voltage and are reduced to a state of traces interfering with the traces of beta rhythm and increasingly limited to rolandic and frontal regions.

These electrographic acquisitions are accompanied by a more stable behaviour in the subject during examination, which is particularly noticeable during activation tests (photic stimulation and hyperventilation). The reactions of nervous tension (hyperactivity and impatience) diminish.

We have summarized these phenomenological data so as to illustrate the complexity of the pattern to be analysed. It is not comprehensible as a whole by automatic methods (frequency analysers, correlators or integrators). Qualified analysis is indispensable where very complex and highly elaborate phenomena are concerned.

4. A comparison of the rate of development of the cerebral electrical activity in two subjects during a similar period (12-15 years) enables us to distinguish slight individual delays in development in one subject as compared with the other.

Thus one of the boys attains certain stages in his development earlier than his control from the same group. The EEG is shown to be better organized and more stable in one subject (Ph.) at 13.5 years than in the other (Fr.) at 13.10 years. It can also occur that an adult pattern may be more harmoniously differentiated and organized in one child than in the other.

Having finished the exposition of our results we now have to compare them with those of the authors who like us have considered the problems of brain development and its functions in the child. As electro-physiologists have carried out their work up to now only through EEG examinations in horizontal series, that is to say among various pupils of the same age, for example of a single school class, and not by repeated examinations in longitudinal series, we shall leave the task of comparing our results with those of these authors to other specialists - Dr K. A. Melin for example. They will need to consider particularly the work of the Bristol school (Grey Walter) and the Hartford one (Henry). We will not study here either in detail the correspondence between frequency structures and cognitive functions. This will be the subject of a comparative electro-physiological and psychological study of the development of the child based on



the results of frequency examinations and on tests of cognitive functions carried out in longitudinal series over the last five years simultaneously in our Laboratory of Applied Neurophysiology and at the Institute of the Science of Education, in collaboration with Professor Inhelder.

Under these conditions we shall limit ourselves to comparing our results with the general concepts of Piaget and Inhelder on the problems of development, of its stages and of the mechanism of the passage from one stage to another.

#### Structural plan (Bauplan) and equilibrium factor

Firstly, there exists a close comparison between our conception of the structural plan (Bauplan) as shown through our investigations, and Piaget's equilibrium factor. A structural plan appears in fact to direct the development of cerebral electrical activity. It is expressed partly in the EEG at each age and even better at each stage, but its most complete expression is found in the electrographic pattern of the adolescent of 15 years at the threshold of adulthood. This idea is very similar to the concept of Piaget and Inhelder who, considering the development of cognitive functions, have individualized an equilibrium factor capable of dominating and co-ordinating the other development factors (hereditary factors, influence of physical and social environment) at the same time introducing new elements. The progressive equilibration by successive levels corresponding to the phases of the processes for cognitive solution is a fundamental factor of evolution according to Piaget. This concept comes close to Ashby's cybernetic factor, whose homeostat tends to produce a state of equilibrium through a series of progressive approximations, depending upon a series of feedbacks. Intelligence is characterized according to Piaget and Inhelder by reversible operator mechanisms capable of forming systems which are simultaneously stable and mobile, and forms of equilibrium towards which they are directed. The final equilibrium, in the form of a cognitive system which is simultaneously stable and mobile, resulting from a series of reorganizations of feedbacks towards which the intelligence is directed has its corollary in the finished and stabilized

electrographic pattern of the young adult, a pattern which synthesizes the various electrographic elements and towards which the EEG tends at each stage.

Continuity and discontinuity of development. Stages.

We have distinguished within the continuous development of cerebral electrical activity epochs at which the organization of the electrographic pattern seems to become more intense, settled and consolidated. There results an impression of discontinuity which is more apparent than real. Here too there is a certain correspondence between our conclusions and those of Piaget and Inhelder who have studied the development of the structures of thought. The concordance is found on the one hand in the characteristics of stages and other mechanisms and on the other hand in the chronology, the succession of stages.

According to Piaget and Inhelder the stages are characterized by the formation of total structures including as a particular case all the structures observable during a given stage and becoming integrated as necessary sub-structures of the preceding stage. It is thus that the logical operations of the stage of formal operations (11-12 to 14-15 years) constitute a whole structure which covers on the one hand all the operatory systems of this stage and includes on the other hand, as sub-structures, the whole structures of the preceding stages (stage of concrete operations: 7-8 to 11-12 years). These stages may consist of successive levels of equilibration separated by a phase of momentary transition; their parameters reduce to criteria of equilibration as follows:

(i) Dominant characters linked to a certain property of momentary equilibrium. The dominance may imply a tendency to the integration of the sub-structures in a dominant structure conceived as a whole structure.

(ii) The idea of stage implies the recognition of a chronological order in the form of a constant order of succession; naturally the age of appearance may vary from one individual to another or from one social environment to another.



(iii) One must be able to find in the passage from a lower to a higher stage the equivalent of an integration of previous acquisitions into later structures.

(iv) In any stage one must be able to distinguish an aspect of preparation in connexion with following stages and on the other hand an aspect of achievement as regards preceding stages.

(v) All the preparations leading to a stage and all the achievements characterizing this stage are subordinate to the existence of a whole structure.

(vi) A theory of stages must represent them in the form of a series of equilibrium levels of increasing extent and mobility, whose growing stability depends precisely on the degree of integration and structuration. In fact Piaget admits that the forms of equilibrium comprise three kinds of characteristics or three dimensions:

- (a) a progressive extension of the field of equilibrium expressed in terms of probability of encounter;
- (b) a growing mobility of equilibrium;
- (c) a growing stability and regularity of equilibrium as a correlative of its progression towards mobility and not in contradiction to it.

If we compare these characteristics of the stages according to Piaget and Inhelder with the criteria of our stages we recognize that they correspond in many respects. Like Piaget we speak of dominant characteristics, dominant rhythms, more and more monomorphous, localized and definable as to their frequency, voltage, geometry and phase relations. Like him we recognize a chronology with a constant order of succession of the transformations in the EEG with age. Like him we have also noted in our EEGs signs of preparation which enable the following stage to be anticipated, a stage which represents a completion as regards the preceding one. On the other hand we have also observed that the age at which a stage appears can vary from one individual to another.

As concerns the succession of levels of equilibrium which Piaget conceives as increasing in extension and mobility while maintaining a continually more marked stability, this also corresponds with our own observations. We have in fact mentioned a systematization at the same time as a progressive stabilization, localization and selectivity of rhythms, particularly of the alpha rhythm, with increase in frequency, abundance, voltage, predominance of one side and structuration in parallel spindles in the occipital regions. We have also mentioned the simultaneous diminution in polyrhythmicity and polymorphism with reduction in the slow delta and theta components in the frontal-rolandic-temporal regions where in the young adult the activity is limited to a few repeated beta rhythms and traces of theta rhythm. There seems then to exist a certain correspondence between our criteria of systematization through synchronization of rhythms and Piaget's idea of the extension of the fields of equilibrium. We have in fact described a tendency towards synchronization of rhythmic activities with age, but within definite areas of the brain: synchronization of alpha rhythm in the occipital region, of the theta rhythm in the temporal region and of the beta rhythm in the frontal-rolandic regions, but although there is no direct connexion between the synchronization of these various rhythms in different areas, nevertheless all the areas show the same tendency to increasingly well-defined periodicity. In this sense there would be a possible connexion with Piaget's idea of progressive extension of the field of equilibrium. In the same way there is a convergence between our criteria of stabilization of rhythms in the form of more monomorphic, selective, localized activity and Piaget's idea of increasing stability in fields of equilibrium.

Electrophysiological co-ordination and dilation of perceptive processes as a function of attention

Piaget himself suggested certain possible analogies between certain aspects of EEGs and his personal observations. He wondered firstly whether McCulloch and Grey Walter's hypothesis, according to which the alpha rhythm could possibly be the manifestation of a scanning activity, could be compared with his personal conception of a progressive extension of the field of equilibrium in terms of



probabilities of integration and coupling. Elsewhere he established a connexion between one of Grey Walter's observations, according to which excitation of cortical visual areas can spread in certain conditions to temporal and even frontal areas, and a phenomenon of dilation which he observed himself during certain experiments on the perception of lengths while looking centrally with perceptual attention. According to Piaget there would be a possible correlation between the extension of electrical activity in one region to increasingly extensive areas of the brain, including the association areas, in the child of 11-12 years and the fact that at the same age perceptive couplings are gradually completed by repetitive couplings leading to a combinatory system (formal operations).

If we were to take up a position as to the possibility of a correlation of this type we would be inclined to admit basing our statement on an observation of the behaviour of electrical activity induced by photic or tactile stimulus. In fact Adrian's experiment, which we stressed before during the Macy Foundation Symposium on consciousness (1952), proved that with a progressive return to consciousness on awakening after ether anaesthesia, for example, the excitations induced on the somaesthetic area of the cortex by tactile stimulation of a cat's paw do not remain localized at the primary cortical projection centre but overlap on to the adjacent association areas, whereas in the posterior fields the stopping reaction abolished by anaesthesia reappears. This experiment proves that consciousness is linked with a process of irritation, of excitations produced in the cortex by sensorial stimulus. These experimental observations could be compared from our point of view with the phenomenon of dilation of visual perception under the influence of attention as observed by Piaget.

Chronological concordance of stages of electrophysiological development and development of cognitive functions

Our first investigations carried out jointly at the Laboratory of Applied Neurophysiology and at the Institute of the Sciences of Education of Geneva led us to doubt the existence of a possible correlation between electrophysiological stages and intellectual stages in the child. Mlle Inhelder and I put forward this impression at the first meeting on the psychobiological development of the child in

Geneva (1953). Despite our initial scepticism we have carried on our investigations for five years up to now and we have acquired a less pessimistic impression. In fact, the actual comparison of our electrophysiological and psychological observations in the light of a five-year longitudinal series shows a fairly clear correlation between the stages of development of electrical brain activity and those of the development of cognitive functions. We found that in the same child, Vincent, the levels were at the same epochs (6.7, 9.3 and 10.10 years). The same happened in the boy's brothers, François and Philippe, at 12.10 and 15 years. Moreover, it is undeniable that the comparison between the development of cognitive functions at seven years and that of the EEG at 6.7 years corresponds also to Grey Walter's observations showing that a process of elaboration appearing sketchily at four years acquires a more stable and more general form only at about six to seven years.

The insufficient correspondence which we noted in 1953 between the stages of electrophysiological development and those of the development of cognitive functions came less from the technique of exploration than from the brevity of our longitudinal series which was still incomplete. The failure, moreover, came from a still insufficient theoretical appreciation of the criteria and of the common characteristics and mechanisms shown by the two methods of investigation. It would in fact be astonishing if there were not some general convergence between the dimensions of the organization of rhythms and those of equilibration by successive levels of cognitive functions. The EEG translates the attitudes which occur in the cerebral activity of a subject and lead through a simple waking state to active exploration. One may hope that a perfecting of recording and analysis of the many electrical activities of the brain will enable us to make more precise and complete in future the electrographic picture which we have attempted to compare in its main lines with that of cognitive behaviour.



18 September 1956

Fourth Meeting  
Geneva, 20-26 September 1956

REMARKS ON "SOME CONSIDERATIONS ON PSYCHOBIOLOGICAL DEVELOPMENT"  
BY LUDWIG VON BERTALANFFY AND JOHN BOWLBY

by

Frederick Hacker, M.D., Los Angeles, California

For obvious reasons, actual clinical observation of the first stages of life is especially difficult; and the interpretation of these observations particularly ambiguous. Hence, by necessity, attempts to reconstruct the earliest phases of childhood must have the kind of mythological character that Bowlby recognizes in Freud's instinct-theory. It might be even more correct to state that these reconstructions must inevitably have a theoretical character, which implies that they contain a strongly symbolic element (Cassirer).

Philosophers, in their discussions of symbolism, have investigated mainly the functioning, meaning, and purpose of "symbolic equations" (mathematical symbols, conventional symbols, language, etc. - i.e., psychoanalytically speaking, the use of symbols in the secondary process). In the older psychoanalytic literature, "true symbolism" (Jones) was restricted to mean "symbolic representations" in which the original object is repressed (Freudian symbols, Ferenczi's cryptosymbolism, myths, etc., or the role of symbols in the primary process). This point of view quite naturally implied that "only what is repressed is symbolized and needs to be symbolized", and that "symbolism is an archaic mode of thinking" (Jones). Rank and Sachs write that "symbolism appears as the unconscious precipitate of primitive means of adaptation to reality that have become superfluous and useless, a sort of lumber-room of civilization...". Symbols, then, are seen as fossil-like in quality, and stored in the mind for regressive use.

In contrast to these views, modern psychoanalytic literature insists on a broadening of the concept of symbolization. Melanie Klein feels that fantasy as symbolic activity underlies all human behaviour, neurotic and realistic. Symbolization thus is a general capacity of the human mind; and symbols may be used by primary and secondary processes (Rycroft). This view is completely in



accord with the concepts of Cassirer ("Man is a symbolic animal"); Kant ("The human mind is in need of images"); and Bertalanffy ("The unique characteristic of man is in his symbolism"). Symbolism here appears as a general human potentiality, manifested in widely different fashions and subject to development, to maturation and to pathology. "Human behaviour is not only a strategy but also a language. Every move is at the same time a gesture" (Langer).

Symbolism is thus understood not as a proxy for objects but as a vehicle for their conception, in the various stages of development. Symbolism can be a characteristic of the primary process or become an important part and instrument of the secondary process, which is in many respects dependent on the use of symbols (see relationship of logic to symbols). Symbolism can act in the service of repression or expression. In this sense, then, there is no pre-symbolic phase of development - only a pre-verbal phase (since words are only one form of symbolic expression, underlying and used largely by the secondary process). In fact, symbolism is the essential instrument by which the distinction between I and not-I, reality-testing, and perception are achieved. In other words, symbolism is inherently contained in the ego-structure as constitutive element. In this manner, symbolism also becomes the foremost instrument for the regulation of the primitive intrapsychic process, which Bowlby considers decisive for the pathogenesis of mental illness. Bertalanffy's statement that neuroses arise from a conflict of opposing symbolic worlds implies no culturalistic bias nor does it play down instinctual causation of mental illness; it simply asserts that unconscious and particularly primitive conflicts are undoubtedly expressed symbolically. It follows a fortiori that the devices for the regulation of these intrapsychic conflicts (the ego, ego-nuclei or precursors of the ego, whether seen as a preformed Anlage or as a result of conflicts) will also be of symbolic character. Symbolization, in its repressive and expressive function, is probably the essential basis of the regulative apparatus.

This new view of symbolism (Langer, Bertalanffy, etc.) has indeed a number of non-absolutistic, perspectivistic (Bertalanffy) consequences also for the prevention and treatment of mental illness. It implies no detracting from the essential, in many cases supreme value of the psychoanalytic method in the



investigation and treatment of mental illness, or from its specific, unique contribution. But perspectivism, based on the recognition of the innumerable forms and functions of symbolization, lends renewed emphasis and scientific dignity to the preventive and psychotherapeutic value of "cultural and play activities in the broadest sense" (Bertalanffy). Psychoanalytic treatment offers a selected group of patients the best and sometimes the only possibility of freeing their symbolic capacity from the encroaching inhibitions of archaic conflicts. Psychoanalysis, however, has never made any claim to exclusive salvation. It is to the lasting credit of the new concept of symbolism that it has drawn scientific attention to other naturally "given" and symbolically "made" opportunities for the development of the optimal sublimation potential.

WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/FDC/11  
11 July 1956

ORIGINAL: ENGLISH

SOME CONSIDERATIONS ON PSYCHOBIOLOGICAL DEVELOPMENT

Comments by Professor Bertalanffy on Professor Piaget's Essay

As I am a newcomer to the Group, the present memorandum is intended to give some idea as to the contribution I may be able to make to the discussion at the Fourth Meeting. So I wish to apologize once for all if it is of a somewhat personal nature, briefly indicating some of the ways of my thought and research, and leaving it to the Group to decide which points they are interested in.

As I understand it, the Fourth and final conference is intended more in the way of a "summing-up" than introducing new evidence which was indeed presented admirably by the members and guests, and with a completeness and broad scope as is rarely found in conferences of this type. So I am basing the following remarks on Professor Piaget's excellent Essay.

I. The quest for a common language

Professor Piaget has admirably emphasized that, in order to arrive at some co-ordination and synthesis of various fields, a principal problem is that of a common language which, so to speak, is translatable from one field to the other.

1. I believe I can make a suggestion in this respect. In the last few years, a development has taken place which seems to correspond well to Professor Piaget's quest. It is the development of General System Theory (G.S.T.).

G.S.T. is intended to elaborate such principles that apply to "systems" in general, irrespective of their particular kind, the nature of their component elements, and the relations or "forces" between them. It thus provides a super-structure of theory generalized in comparison with the conventional fields of science. It is capable of giving exact definitions to many notions, such as, for example, wholeness, interaction, progressive differentiation, mechanization,



and centralization, dynamic and homeostatic (feedback) regulations, teleological behaviour, etc. which recur in all biological, behavioural, and social fields, have had some vitalistic or mystical flavour, and were not accounted for in the so-called "mechanistic" approach. In such way, G.S.T. accounts for the isomorphy of theoretical constructs and of the corresponding traits of reality in the diverse fields of science.

G.S.T. has been rather extensively applied in various fields during recent years, and it may be mentioned that a Society for the Advancement of General Systems Theory, which is a group within the American Association for the Advancement of Science, attempts at further development and application of this field. The principal goals of G.S.T. are (a) in trying to integrate individual branches of science in their general principles; and (b) in offering a theoretical structure and models to those fields of science - especially the behavioural sciences - which still lack them.

Ashby's formulations (1952) referred to in previous meetings are closely related to those mentioned above. Even though he does not use the term General System Theory, he starts with the same mathematical model as applied by the present writer. As Ashby and myself have drawn different derivations from the same model, the work of both of us is complementary.

2. I am not going to review in this memorandum what has been done in this line as it may be found in the literature (Bertalanffy et al., see references). Rather, I would like to give some clarification as to the bearing as well as the limitations of G.S.T. and interdisciplinary constructs in general. What will be said applies equally to other attempts at a "common language", such as information theory, game theory, decision theory, cybernetics, and so forth.

Being an experimentalist with strong leanings toward physics myself, I am vividly aware of a danger apparent in much of current literature in the behavioural fields. Nothing is accomplished by loosely applying to unexplained or uncoordinated facts some fashionable term - be it "system", "homeostasis", "feedback", "information", "minimal solution", or whatever the case may be. Attaching some new verbal label must not be mistaken for being new insight or understanding.



What G.S.T. (and related constructs) can do is what Hayek (1955) has aptly discussed as "explanation of the principle". In physics (and to a certain extent other fields such as biophysics, genetics, etc.), there is a hypothetico-deductive system of laws, the appropriate parameters of which can be inserted; so we have explanation and prediction of individual empirical phenomena, be it the positions of the planets at any time in the past or future, the behaviour of atoms, or the result of some crossing in drosophila. Many biological and most behavioural phenomena (except such rather trivial aspects as, e.g., mortality statistics) are too complicated and obscure in their structure to allow for such explanation and prediction. The best we can do - at least at present - is some "explanation in principle". What this means can best be illustrated by a few examples.

There is a highly elaborate mathematical theory of populations, both from the ecological (Volterra, D'Ancona, Gause and others) and genetical viewpoint (Fisher, Sewall Wright, etc.). All biologists agree that this theory provides an important basis for understanding the struggle for existence, biological equilibria, etc., and of evolutionary processes, respectively. It is difficult, however, to prove quantitatively, say, Volterra's laws of population growth even in laboratory experiments, and it is near to impossible to do so in the field as the complexity of natural ecological and genetical systems is prohibitive for giving concrete values to the relevant parameters (coefficients of reproduction, extinction, and competition; selective advantage, coefficients for drift and the like).

Economics and econometrics provide theoretical models which are more or less generally accepted. As a rule, however, professors of economics are not millionaires, showing that they can give "explanations in principle" for the economic process, but are not in a position to predict the fluctuations of the market with respect to a definite date or an individual stock.

Game theory (referred to as a possible model by Piaget, Essay, p.7) is a novel and original mathematical field. However, I understand from competent authorities that hardly any examples except trivial ones can be figured out specifically in the way a physicist or engineer would calculate a phenomenon or a machine, even though the theory may provide explanations "in principle" for psychological and social phenomena.



Similar considerations apply to G.S.T. It is in a position to offer "explanation in principle"; but it cannot be blamed for not giving quantitative solutions for phenomena like embryonic regulation, psychobiological development, etc., where the complexity of the process and the lack of definition of the relevant parameters are prohibitive.

So much about G.S.T. and theoretical models in general. What more can it offer for the problems of psychobiological development?

#### REFERENCES

- Ashby, W. R., (1952) Design for a Brain. Chapman and Hall, London
- Bertalanffy, L. Von, (1949) Zu einer allgemeinen Systemlehre. Biol. gen. (Wien), 19, 114
- Bertalanffy, L. Von, (1950) An Outline of General System Theory. Brit. J. Philos. Sci. 1, 139
- Bertalanffy, L. Von, (with Hempel, C. G., Bass, R. E., and Jonas, H.) (1951) General System Theory: A New Approach to Unity of Science. 1-6. Hum. Biol. 23, 302
- Bertalanffy, L. Von, (1955) General System Theory. Main Currents in Modern Thought 11, 75
- Hayek, F. A., (1955) Degrees of Explanation. Brit. J. Philos. Sci. 6, 209

## II. The question of an equilibrium factor

1. Professor Piaget suggests that, besides the factors customarily envisaged in development, another principle should be considered which he calls "equilibrium factor".

I am, in principle, in full agreement with Professor Piaget, but I believe that this viewpoint can be considerably improved if, instead of the notion of "equilibrium" somewhat different conceptions are taken as the starting point.

Looking first at the organism from the physiological viewpoint, it is a basic characteristic that it is not a system in equilibrium. On the contrary, for a system to be living presupposes that it avoids the state of equilibrium, and if equilibrium, chemical, osmotic, thermodynamic, etc. - is reached, this means death.



This avoidance of equilibrium is possible because the organism is an open system, maintaining itself in continuous exchange, building up and breaking down its components which is the essence of that basic vital characteristic, metabolism. An open system and an organism may reach a time-independent state where it appears to remain macroscopically unchanged; but this is not "equilibrium" but a "steady state".

Although this seems to be trivial and the living organism has been called a "dynamic equilibrium" for a long time, only in recent years has the theory of open systems and steady states - kinetic and thermodynamic - been developed. The laws governing open systems and steady states are characteristically different from those governing the conventional closed systems and equilibria. Again I have to forsake a more detailed explanation and refer to current literature (for a general orientation cf. Bertalanffy, 1950, 1953, 1954; Bray & White, 1955; Jung, 1956). I am taking up only a few aspects which may be important in view of behavioural problems.

It is a basic model in biology and psychobiology that the organism tends to maintain itself "in equilibrium", that is, to react to stimuli in such a way as to return to a state of rest. This is also at the basis of the notion of homeostasis, although this introduces some new ideas as to the mechanisms concerned (feedback, circular processes). Intimately connected with this is the "automaton model" of the organism, that is, the conception that the organism is essentially a reactive system, set into motion only by external influences (stimulus-response scheme).

These models are unrealistic. That the organism is not a restive but a primarily active system is shown by phenomena so diverse as the basic phenomenon of metabolism, the spontaneous movements of lower animals and of foetuses before the establishment of reflexes and in deafferentiation (Lorenz, First Meeting, p. 111 ff.), the EEG of the unstimulated brain, "in vacuo" behaviour, and innumerable others. It is a nice illustration for the bias imposed by model conceptions (or rather metaphysico-political superstitions) when Liddell (Second Meeting, p. 126 ff.) tells us how spontaneous behaviour in the dog was unorthodox, "against the rules", and politically suspicious for the Pavlov school.

In contrast, the open-system model accounts for the characteristics that are essentially "vital". According to this, the organism is an intrinsically active system. Furthermore, the theory of open systems, well-developed physico-chemically



and even in a more general form, accounts for just those properties of the living organism which were considered "vitalistic", that is, violating the laws of physics, such as the equifinality of development (Driesch' "first proof of vitalism"), the apparent contradiction (referred to by Piaget, Essay, p. 8) between the trend towards increasing disorder in the inorganic world according to the second principle of thermodynamics, and the trend toward increasing order in biological development and evolution (another "proof of vitalism" according to DuNoüy and others) etc. These vital characteristics are in contrast with the conventional physics of closed systems, but are perfectly legitimate within and, indeed, necessary consequences of, a generalized physics of open systems. Also the problem of biological time, referred to by Dr Grey Walter (Second Meeting, pp. 18 f., 48 f.) comes under the theory of Irreversible Thermodynamics and biophysics of open systems.

Both Lorenz and myself (Bertalanffy, 1937, p. 10 ff., 133 ff.; 1952, p. 17 ff., 114 ff.) have early stressed the consideration of the organism as an essentially active system - Lorenz in his theory of instinct and behaviour, connected with von Holst's criticism of classical reflexology; I in the context of general biological theory, eventually leading to the modern expansion of kinetics and thermodynamics, as briefly indicated above.

2. Coming to psychobiological matters, we find the development closely parallel. An important basis of Freudian theory is the "principle of stability" he adopted from Fechner. The supreme tendency of the organism, biological and mental, supposedly is to get rid of stimuli, and come to rest in a state of "equilibrium". It is in the same vein when the concept of homeostasis is applied to any sort of behavioural or mental activity - from mountain climbing to science or composing sonatas (cf. Stagner, 1951, and the Criticism of the Concept of Homeostasis by Toch & Hastorf (1955) and Bertalanffy (1951a). Non-homeostatic traits were already mentioned by Bowlby (Comment, p. 13).

The above theoretical notions do not seem to account for those aspects which are variously called play-activities, exploring, creativity and the like, going along with "function pleasure" (Karl Buehler, cf. Mead's remarks, Second Meeting, p. 122 f.), which is so characteristic of human behaviour in general, and mental development of the child in particular. As Lorenz has always emphasized (e.g. 1943), these activities



have their fore-runners in animal behaviour. Just as the physical organism avoids a state of equilibrium, so does the mental organism, an essential aspect of which seems to be not "relieving of tensions" but rather building up new "tensions".

Neo-Freudian theory tries to account for this state of affairs. Thus Alexander (1948) states that "the basic function of the mental apparatus consists in sustaining the homeostatic equilibrium" but adds that besides the principles of "stability" and "inertia" (the latter identical with "progressive mechanization" as mentioned above) another principle of "surplus energy" is required. I have recently heard a lecture by Professor J. Nuttin (Louvain) which, in a criticism of Freud's theory, followed a line closely parallel to the above biological argument, and I suppose that a similar dissertation may be found in Father Nuttin's books. The Montreal experiments, reported at this Conference by Dr Bindra (Second Meeting), dramatically show that the human organism just cannot stand a state of non-stimulation, of complete rest and of "equilibrium". The hallucinations occurring in absolute seclusion are a vivid demonstration of the "autonomous activity" of the psychophysical organism.

So, physiologically, the organism is an intrinsically active system, tending to a steady state and allowing even for "anamorphosis", i.e. spontaneous transition toward higher order. Psychologically, this implies what may be loosely called "creativity"; and in terms of general theory, these are consequences of the organism being not a closed system attaining equilibrium but an open system.

3. Naturally, it remains to be seen in how far the open-system model can be applied to behavioural or psychobiological science. Attempts in this direction have been made by Krech (1950) and Pringle (1951) and in the transactional viewpoint, often quoted nowadays, of Bentley (1950). A "biologistic" reductionism would be no better than "physicalism". Trivially, open systems as treated in physics and biophysics are something quite different from what the psychologist is speaking of. I propose, however, that as a tentative model or analogue, "open system" with "autonomous activity" and "anamorphosis" is a better construct to start with than "closed system" (which actually is at the basis of gestalt psychology, behaviourism, cybernetics, Freudian theory, etc.) (cf. Bertalanffy, 1951b, p. 33 f.), "primary reactivity" (the stimulus-response scheme), and mental organization conceived as an apparatus to maintain "equilibrium".



REFERENCES

- Alexander, F. (1948) Fundamentals of Psychoanalysis. Norton, New York.  
(The theoretical principles reprinted in Dialectica.)
- Bentley, A. F. (1950) Kennetic Inquiry. Science, 112, 775.
- Bertalanffy, L. Von, (1937) Das Gefuege des Lebens. Teubner, Leipzig.
- Bertalanffy, L. Von, (1950) The Theory of Open Systems in Physics and Biology.  
Science, 111, 23. (Partly obsolete.)
- Bertalanffy, L. Von, (1951a) Towards a Physical Theory of Organic Teleology.  
Hum. Biol. 23, 346.
- Bertalanffy, L. Von, (1951b) Theoretical Models in Biology and Psychology.  
J. Personality, 20, 24.
- Bertalanffy, L. Von, (1952) Problems of Life. Wiley, Watts, New York and London.
- Bertalanffy, L. Von, (1953) Biophysik des Fließgleichgewichts. Translated by  
W. Westphal. Vieweg, Braunschweig.
- Bertalanffy, L. Von, (1954) The Biophysics of the Steady State of the Organism.  
Scientia (Milano), 48th year.
- Bray, H. G. & White, K. (1954) Organisms as Physico-Chemical Machines.  
New Biology, 16, 70.
- Jung, F. (1956) Zur Anwendung der Thermodynamik auf biologische und medizinische  
Probleme. Naturwissenschaften, 43, 73.
- Krech, D. (1950) Dynamic Systems as Open Neurological Systems.  
Psychol. Rev. 57, 345.
- Lorenz, K. (1943) Die angeborenen Formen moeglicher Erfahrung.  
Z. Tierpsychologie, 5, 235.
- Pringle, J. W. S. (1951) On the Parallel between Learning and Evolution.  
Behaviour, 3, 174.
- Stagner, R. (1951) Homeostasis as a Unifying Concept in Personality Theory.  
Psychol. Rev. 58, 5.
- Toch, H. H. & Hastorf, A. H. (1955) Homeostasis in Psychology: A Review and Critique.  
Mimeograph, Center for Adv. Study in the Behavioral Sciences, Stanford (Calif.)

### III. Stages or continuous development

In view of the foregoing, the controversy whether development is "continuous" or taking place in "stages" (Piaget, Essay, p. 12 ff.; Comments by Tanner & Bowlby) seems to win a few new aspects.

1. One relevant notion has already been introduced, namely, that of step functions (Grey Walter, Third Meeting, p. VII, 11 f., IX, 4, 6) as discussed by Ashby, meaning that the process is not discontinuous but shows more or less rapid transitions toward higher levels or plateaus (cf. the discussion in Bertalanffy, 1956).

2. Obviously there are no all-embracing steps as Gesell (1956) seems to presuppose when speaking of the "personality of the ten, eleven-year old", etc. as distinct entities. So far as somatic development is concerned, a glance at the figure reproduced by Tanner (First Meeting, Fig. 1) shows that all organs do not follow the same pattern of development.

There are, however, periods where not all but quite a number of characteristics change. Trivially, puberty is one of them (cf. Part V of this Comment). In this sense, it seems legitimate to speak of "phases" or "cycles" of somatic and mental growth.

3. As a somewhat less trivial notion, I would like to introduce that of equifinal steps. As has been indicated, equifinality is a characteristic of open systems if and when they tend toward a steady state. Equifinal phases are such as to be reached from different starting points and in different ways, and maintained over a time till a change in conditions - external or internal - causes a new development in the system and brings it on the way toward another equifinal phase. This is related to Piaget's conception of successive equilibria stages or levels, but brings in a few new viewpoints. I am illustrating this by way of a few examples deliberately taken from very different fields.

(a) The early development of an ovum is very different in the various animal classes or orders, being what is technically known as holoblastic, meroblastic, discoidal, or superficial segmentation. This depends upon factors such as the content and distribution of yolk, varying in different species and even changeable experimentally. However, an essentially similar two-layered stage, the gastrula,



is reached anyway. Similarly, early development in amphioxus, fish, amphibian, reptile, bird, and mammal is very different. Nevertheless, the vertebrate neurula with its characteristic arrangement of primordial organs is strikingly similar in all classes.

(b) Everybody is agreed that species have not arisen by separate acts of creation, as Linnaeus had it, but by natural evolution, presupposing transition from one species to another. However, what we find in nature is separate species, with hardly any intermediates. The nearest explanation is that species are relatively stable systems in genic balance, which therefore show up abundantly in the present fauna and flora and in the palaeontological record, while intermediates are unbalanced, short-lived stages of transition which, for this very reason, usually are "missing links" (for a more detailed discussion, cf. Bertalanffy, 1952, p. 95 ff.). It is in the same vein that Huxley (Third Meeting, p. IX, 14) emphasizes that most species become stabilized at a certain level of organization.

(c) In the history of architecture, we distinguish the Romanesque, Gothic, Baroque styles. There are transitions between Romanesque and Gothic ("Uebergangstil" of the German art historians), between Gothic and Baroque. However, specimens of these transition styles are rare. Suppose Europe were exposed, in a new war - God help us - to even more efficient bombing than took place in the second World War. Then, for statistical reasons, the few specimens of transition style would be extinguished while a number of Romanesque and Gothic churches would still remain. So the art historian of the future would see a jump from Romanesque to Gothic - exactly the same picture as the palaeontologist sees in the animal and plant world. Romanesque, Gothic, and Baroque would appear to be stages of relative "balance" which therefore are maintained for quite a while, till new influences (perhaps Arabic in the first instance, the Renaissance in the second) upset this "balance" and lead to a new development and relatively stable state.

Again, early automobiles were a lot of fancy carriages of every imaginable shape, type of propulsion, etc. But when the development became stabilized, that is, a near-optimal solution of the technical problem was reached, nothing much



happens any more. So present cars of whatever brand are pretty much alike, and the car makers are at pains every year to advertise a "new" model, that is, on which is a little different in trimming from last year's crop. The parallel to evolution of new species is obvious, and has already been drawn by Huxley (Third Meeting, p. IX. 17). I notice that Piaget has already hinted at "cultural equifinality" in his questions to Margaret Mead (Essay, p. 30).

(d) Something similar seems to apply to the psychobiological development of the child. If I am correctly informed, the age of ten to twelve represents a stage of internal balance. Then the beginning action of the sex hormones etc. leads, somatically, to a second acceleration of growth ("adolescent spurt" of Tanner); correlated, electrophysiologically, with the change from theta to alpha rhythm (Grey Walter, First Meeting, Fig. 15); psychologically, to a second "negative" or sulking phase; scholastically, the transition to secondary school (cf. Zazzo, First Meeting). Eventually a new balance is reached in the adult, while the "change of life" would again mark another phase. More detailed are Piaget's (and Freud's) phases of early mental development. Culture-bound variations of these patterns are to be taken into consideration (cf. Part IV of this Comment and Mead in First Meeting, p. 92 f., Grey Walter in Second Meeting, p. 69).

So development seems to take place in a series of equifinal levels, and this seems to take out much of the sharpness of contrast between "stages" and "continuous development". Unnecessary to repeat that these stages are not all-inclusive, and that maturation does not take place simultaneously in all processes, physiological or mental.

#### REFERENCES

- Bertalanffy, L. Von, (1952) Problems of Life. Wiley, Watts, New York and London.
- Bertalanffy, L. Von, (1956) A Biologist looks at Human Nature. Sci. Monthly (Wash.), 82, 33.
- Gesell, A., (1956) Youth: The Years from Ten to Sixteen. Harper, New York.



#### IV. The role of symbolism in human behaviour

Being a physiologist in my full-time job, I feel in a somewhat queer situation when I am going to emphasize non-physiological aspects of human behaviour.

I take it for granted - and this notion is shared by the Conference - that the unique characteristic of man is in his symbolism. I wish to compliment Lorenz on his profound remarks (First Meeting, p. 98) on the possible roots of abstraction in gestalt perception. For my personal formulation of this viewpoint, I refer to some recent papers (Bertalanffy, 1955, 1956). It would perhaps be interesting to compare my exposition of the "Relativity of Categories" (1955) with similar considerations presented in the Study Group (e.g. First Meeting, p. 104 ff., 229; Second Meeting, p. 243 ff.). Here I would like to call attention to a few implications and consequences alluded to in the previous meetings.

1. It appears to be an important question in how far psychoanalysis is characteristic of man in Western civilization (or perhaps the Vienna of the 1890's) or of man in general. Such question is implicit in Margaret Mead's presentation (First Meeting): what would the Oedipus complex look like in a civilization where children are raised not by the parents but their grandparents or other children (cf. also p. 22 on thumb-sucking; Third Meeting, p. I, 9 ff., II, 9; Carothers, First Meeting, p. 25 f. Lorenz, ibid., p. 229 f.). It would appear that, in contrast to organic disease, the manifestations of mental disorder are "culture bound" to a high degree. This means that the form neurosis will take strongly depends on the symbolic ambient of a certain time and civilization. Psychiatrists agree, for example, that Charcot's grande hystérie is hardly found any more, and the neuroses encountered nowadays are rather different from what Freud has described (cf. Erikson, in Third Meeting, p. VII, 17).

2. Connected with this is the question of the various forms of neurosis, or, more general, of different mechanisms or levels in psychopathology. There are phenomena such as the "neuroses" produced in animals by conflicting stimulus-situations, as found by the Pavlov school and reported by Dr Liddell (Second Meeting); the deleterious consequences of an early mother-child separation as described by Spitz and Bowlby; the peptic ulcer or heart trouble a business executive develops because he does not get along with his board of directors or has had poor judgment of the stock

{ manifestations - yes  
processes - no.



market. Are we, and in how far are we, justified in putting all these things into the same pot, labelling it "neurosis"?

3. In this context, it should perhaps be emphasized that Freudian and Lorenzian "instincts" are rather different things. Dr Bowlby (First Meeting, p. 201) asks: "Why do some people develop strong impulses to kill? . . . Since they usually don't kill others why don't they?" He refers to the superego, the controlling force, "conscience" or the like as having "an instinctive root".

I think the Freudian picture of man as a born aggressor and fornicator, these mighty "Drives" kept under precarious control by the superego, is a slightly romantic one. \* Homo is not a carnivore; he happens to be a gregarious species with inbuilt social and monogamous instincts, even though beside the social instincts which he shares with so many other species, aggressive instincts are present. Otherwise, institutions such as society and matrimony could not have kept through the millenia, or not developed at all.\*

It seems to me the shoe is rather on the other foot: it is the negative side of the "superego", the symbolic labels invented in society and culture, that are responsible for releasing or eliciting those shadier "instincts" in organized warfare, in national, religious or racial persecution, etc. - phenomena not happening even with the more voracious beasts of prey.

4. If Bowlby (Some Notes, May 1955, p. 4), emphasizes "pathogenesis as sought in intra-psychic conflict of drives", this may be compared to what I have tried to give as a definition of neurosis, namely, that it arises "from the conflict of opposing symbolic worlds" (e.g. Bertalanffy, 1956, p. 40). As Bowlby justly emphasizes, this is somewhat different from the classical or orthodox position of psychoanalysis.

5. In this connexion, I would like to comment on "repetitiveness" as a characteristic of neurosis (cf. the discussion in Third Meeting, p. VI, 5 f.). Kubie (1954) defines neurosis by the preponderance of the unconscious system which pre-determines the automatic and stereotyped character - as, for example, compulsory hand washing - of the neurotic process. Obviously, repetitiveness and determination by the unconscious has, in itself, nothing to do with psychopathology. The basic biological "drives" - seeking for food and sexual activity - of course are compulsory, determined



by the unconscious, and repetitive - otherwise neither the individual nor the species would survive. At a higher level, no cultural activity, artistic, scientific, etc., could be carried on without an amount of repetitiveness.

6. In view of the above, I would like to offer two propositions which admittedly are over-simplified but for this very reason can serve as a challenge for discussion;

- (a) Mental disorder - psychosis, neurosis, and psychosomatic illness - is a distortion of that unique feature of man, namely his symbolism and culture.

Lorenz is able to give us any number of examples where there is conflict between IRM's and in consequence disturbed, non-adaptive or outright deleterious behaviour (cf. First Meeting, p. 234 on displacement activities), and a Pavlovian like Dr Liddell can do the same with respect to conditioned responses. But although, in my experimental work, I have been on intimate terms with hundreds of rats, I still have to see a specimen which is "nuts" and believes he is the Emperor Napoleon or his "ratty" equivalent. Animals do not have anxiety in regard to the future, or fear of death; they do not commit suicide.

- (b) There are to be distinguished two groups of factors active in mental disorder. One is a disturbance in biological mechanisms (instincts, conditioned responses, etc.), the other a disturbance in symbolic structures. It is, of course, taken for granted that there are all kinds of intermediates and graduations phenomenologically, and specifically, that both factors are active in human neurosis.

Expressions of the first mechanism are the so-called animal neuroses as well as the disturbances in early childhood after Spitz and Bowlby; the second appears preponderant in neuroses of the common or garden variety as they line up for the psychoanalyst's couch.

I am in this respect in agreement with Kubie (1953) who, as a "new hypothesis" on the distinction between normality and psychopathology, stresses the importance of the symbolic process in mental disorder as well as its uniqueness to man, and distinguishes "psychopathological processes which arise through the distorting impact of highly charged emotional experiences at an early age" and those "consisting in the distortion of symbolic functions".

*He seems very unwilling to permit much scope to the biologically determined components.*



7. In connexion with the above, and also with the considerations on "equilibrium" according to Part II, is the following. Conditions of monotony of life and job, lack of cultural ambient and interests and the like, appear to be particularly apt to be "neurogenic" (cf. the parallel experience in animal experiments, Liddell, Second Meeting, p. 111 f., 129). This is one of the reasons why mental health has become such a tremendous problem in the United States and in Canada. Speaking in terms of the theoretical viewpoints adopted here, man happens to be an animal moving in a symbolic ambient. He has a tendency to be active and creative in this field - whatever the value and level of this "self-transcendence" (Aldous Huxley) may be. Put him into an environment where his primary or biological needs are granted and he has no struggle for existence biologically, but where he cannot "abreact" his creative drives. Even though he has his "security" or just because he has it, he is very apt to develop neurosis. This is what actually happens and it shows the importance of the symbolic level as well as the inadequacy of considering human behaviour as ruled by maintaining "equilibrium" or "homeostasis".

8. A practical or psychotherapeutic consequence is "culture as a psycho-hygienic factor". If there is no outlet for activity surpassing the maintenance or homeostatic level - what can a person do but develop a neurosis? On the other hand, cultural or play activity in the broadest sense appears to be of a high psychotherapeutic value - ranging from breeding canaries or stamp collecting to being concerned with calculus, art and music. This is an approach somewhat different from both conventional psychoanalysis trying to resolve early conflicts, and from usual occupational therapy. For it implies that the spending of so-called surplus energy (Alexander) goes along with Karl Buehler's function pleasure and so is a reward in itself. The goal would be not to "tranquillize" the patient, that is, to restore his "equilibrium" but rather let go and satisfy the inbuilt activity which, in man, is at the symbolic level.

PARTLY.

#### REFERENCES

- Bertalanffy, L. Von, (1955) An Essay on the Relativity of Categories. Philos. of Sci., 22, 243.
- Bertalanffy, L. Von, (1956) A Biologist looks at Human Nature. Sci. Monthly (Wash.), 82, 33.



- ✓ Kubie, L. S., (1953) The Distortion of the Symbolic Process in Neurosis and Psychosis.  
J. Amer. Psychoanal. Ass. 1, 59.

Kubie, L. S., (1954) The Fundamental Nature of the Distinction between Normality and  
Neurosis. Psychoanal. Quart. 23, 167.

#### V. Some notes on somatic growth

There is, of course, no need to remind the Group that the unique shape of the human growth curve, with its prolonged period of youth and adolescent spurt, is a quantitative expression of the Bolkean retardation of growth (cf. Third Meeting, p.IV, 7 ff.). A few further suggestions may be made.

1. Bolke's concept of "retardation" is sound, his "foetalization" is exaggerated. I would like to refer to the work of Kummer (1953) who, by way of Thompson transformations, has shown that the human skull cannot be considered a product of "foetalization" of the anthropoid skull; rather it is a differentiation in another direction, owing to the increase of the brain and the neurocranium.

2. It would be going far afield to review the theory of animal growth I have developed (surveys: Bertalanffy, 1949, 1951a, b) which, among other consequences, leads to correlations between the so-called "metabolic" and "growth types" in the various animal classes. However, this kind of analysis shows particularly clearly that, while in lower vertebrates growth is continuous, in mammals "growth cycles" appear. The break takes place at the time of sexual maturation where many metabolic characteristics have been demonstrated to change. The culmination of this is the growth curve in man, with its apparent cyclization, prolonged youth period, and adolescent spurt. (Question to Grey Walter: Is anything known as to ontogenetic changes of the EEG in animals?)

A detailed analysis in the line indicated, of how anthropoids (data in Tanner, 1955) fit in as an intermediary state would be very desirable.

Incidentally, the theory takes care of the characteristics of "X" enumerated by Tanner (Comments, p. 2), and gives a quantitative theory wherefrom these and other things can be calculated.

Professor Piaget may be pleased to find in this theory of somatic growth the concept of "equilibrium" or rather the steady state to be basic.

The particular growth curve of the nervous system (Tanner, First Meeting, Fig. 1 and p. 32), resembling a decaying exponential and lacking an adolescent spurt (to this possibly corresponds a similar growth curve of intelligence, cf. Bayley, 1955) may well be explained. It appears to be due to the strong negative allometry of the brain, as could be explained in more detail.

As to the questions of allometry, simultaneous breaks of allometric regression lines of different organs (Tanner, First Meeting, p. 49-50), pituitary action on metabolism and growth, etc., I could offer some of our experimental material relative to these questions. I would not agree with Dr Tanner (Comment, p. 4) that we do not know of factors changing the rate of development of the organism as a whole (cf. the pituitary-dwarf factor and consequent somatotrophin deficiency in mice, the alleles causing cranial or caudal variation in the human vertebral column after E. Fischer, and other "superordinate" genes).

Some consideration as to the equifinality of growth, i.e., reaching normal adult size from different birth sizes or after temporary disturbance of growth, may also be to the point as there is a large material on laboratory mammals comparable to that discussed in First Meeting, p. 38 f., 52.

#### REFERENCES

- Bayley, N., (1955) On the Growth of Intelligence. Amer. Psychologist, 10, 805.
- Bertalanffy, L. Von, (1949) Problems of Organic Growth. Nature, 163, 156.
- Bertalanffy, L. Von, (1951a) Theoretische Biologie. Bd. II. 2nd ed., Francke, Bern. (Not containing more recent material).
- Bertalanffy, L. Von, (1951b) Metabolic Types and Growth Types. Amer. Naturalist, 85, 111.
- Kummer, B., (1953) Untersuchungen ueber die Schaedelform des Menschen und einiger Anthropoiden. Abh. zur exakten Biologie, 3, Borntraeger, Berlin.
- Tanner, J. M., (1955) Growth at Adolescence. Thomas, Springfield (Ill.).



UNITED NATIONS

FILE COPY  
NATIONS UNIES

WORLD HEALTH  
ORGANIZATION

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/9  
6 February 1956

ORIGINAL: ENGLISH

THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Comments by Dr John Bowlby on Professor Piaget's Essay

Introduction

Piaget has raised some fundamental problems which I have found very stimulating and for which I am sure everyone will feel very grateful. In attempting to answer the questions he has put to me I have found it necessary to give some indication of my own position: this I do with some diffidence partly because of the difficulty of the issues raised and partly because I am often unsure how adequately I have comprehended Piaget's ideas. In preparing the following comments I have been much helped by three research colleagues: Peter Hildebrand, whose knowledge of Piaget's writing is far greater than my own, Anthony Ambróse and Robert Hinde.

Though I find myself in close agreement with the early passages of Section 1 of Piaget's paper, I find difficulty over his concept of affectivity and I expect to find much more autonomy in the development of different structures and their related activities\* than Piaget seems to expect. However, before commenting on this I think it would be best for me to deal with the problems of stages of development since this is where ideas derived from psycho-analysis are much concerned.

---

\* To describe the growth of any particular part of the organism, I am referring to the part as a 'structure and its related activity' to make it plain that structure and activity are indissolubly linked. To avoid clumsiness in the text I have often contracted it to 'structure and activity' or even to 'activity'.



Problem of stages of development

In discussing this topic I want to bring out rather fully a distinction which is clearly present in Piaget's discussion though not always very explicit. I do so because I think there is always a danger of confusion arising if we do not clearly distinguish

- (a) phases in the development of the whole organism,
- (b) phases in the development of particular structures and activities of the organism.

Some of the most dramatic examples of phases in the development of the whole organism are apparent in insect life, e.g. larva, crysallis, imago. In mammals one can easily discern the intra-uterine and extra-uterine phases, but when one tries to find substages within the extra-uterine this type of classification quickly breaks down. The second concept - phases in the development of particular structures and activities - seems to me far more valuable than the first.

In the field of physiological structure and activity, development, though in some respects gradual, often proceeds in steps, e.g. when the foetal heart begins to beat or when the child starts to walk. Tanner has emphasized, if I understand him aright, that as a rule in the physiological field different structures and activities develop at different rates and that a major step in development in one area is not often contemporaneous with or even in any obvious way co-ordinated with one in another area. As remarked earlier, a partial exception to this in mammals is birth where several activities change in character simultaneously, though even here in terms of all the activities operating I imagine those which change are only a small minority. The upshot therefore is that in physical growth the picture appears to be one of a multiplicity of developing activities each progressing at its own pace and passing through major phases which are not closely co-ordinated with the phases passed through by other activities. As a result it is not possible when we consider the organism as a whole to discern overall stages of physical growth.

It is my impression that exactly the same picture will be found in the development of psychological structures and activities. Piaget clearly postulates multiple psychological activities; I am not clear how many he expects to find but I have the



impression it may be only a few. My own expectation is that we shall identify very many. Furthermore, whereas Piaget seems to expect to find fairly close parallelism and inter-connexion in their developments, I do not. In so far as development in any one area is influenced by the total field of forces in which it is occurring there must be inter-connexion, but I expect it to be complex in its manifestations with each structure and activity developing at its own pace.

It seems to me that the view that psychological development is the product of the relatively autonomous development of a fairly large number of different structures and activities squares with clinical experience of the ordinary child: the development of each child is extraordinarily uneven in respect of different activities and every child differs from every other in the order in which they develop. This is a view of psychological development which makes use of Goldschmidt's principle, derived from embryology, of harmonized reaction velocities; this principle he advanced to account for physiological development and the differences in its outcome displayed in the mature form reached by each individual organism. Just as, according to Goldschmidt, differential variations in the velocity of development of different structures and activities account for differences in the mature physical form of organisms even as far as the differences between the sexes of one species, so would I expect variations in the velocity of development of different psychological activities to account for differences in the psychic form or personality of individuals, including differences which tend to be characteristic of the two sexes. In other words I am pinning much hope to the systematic application of Goldschmidt's theory to psychological development.

Let us now turn to the various theories of stages of development which have been advanced in regard to children. Wallon's stages seem to be concerned with the organism as a whole and are, therefore, in my view of limited use. The stages described by Inhelder and Piaget are concerned exclusively with cognitive structure and activity, where they appear to be of the greatest value. As indicated earlier, however, I do not share Piaget's hope that they will be found to run closely parallel to the development of other activities. Finally, there is Freud's formulation of the theory of libidinal phases.



### Stages in the organization of the libido

First, it seems plain that in advancing his hypothesis Freud is referring to the development of a particular function, namely the sexual. Any attempt to extend these stages to characterize the whole of the psychological development of the child would seem to me wholly mistaken.

However, with Piaget, I think Freud's libidinal phases differ materially in their nature from the cognitive phases of Inhelder and Piaget. The latter are characterized by true steps, namely the condition of progress from one phase to another is that the prior phase is a necessary precondition for the emergence of the subsequent one. At the most Freud's libidinal phases are, as Piaget points out, only phases with dominant characters. Freud himself remarks: "It would be a mistake to suppose that these three phases succeed one another in a clear-cut fashion: one of them may appear in addition to another, they may overlap one another, they may be present simultaneously".\* Such an assessment seems to me to rob the notion of libidinal stages of most of its usefulness. It happens that in contrast to many psycho-analysts, I have never found the concept of libidinal stages useful and I regard the elaboration of hypotheses which seek to relate particular psychiatric syndromes to particular libidinal phases as mistaken. For all these reasons I recommend we do not spend too much time on them.

It happens, however, that mixed up with Freud's concept of libidinal phases there is what in my opinion is a far more valuable idea; this is his notion of "individual component instincts" which are at first "disconnected and independent of one another" but which later "under the primacy of a single erotogenic zone, forms a firm organization directed towards a sexual aim attached to some extraneous sexual object".\*\* The theme here is that the sexual responses of the mature adult are to be seen as the result of a special synthesis of a number of component behaviour patterns, some of which first make their appearance in infancy and early childhood. Freud's further point is, of course, that sexual disturbances including perversions are to be understood as resulting from a faulty synthesis of these components. These

---

\* Freud, S. (1949) An Outline of Psycho-Analysis, London, p.14

\*\* Freud, S. (1949) Three Essays on the theory of sexuality, London, p. 75



ideas of Freud seem to me to be almost identical in character with ideas now fairly widely accepted by ethologists in accounting for the complex behaviour of other species, e.g. nest building or courtship in birds.

If we select this aspect of Freud's formulations, we see that the phases of sexual development which would correspond to Piaget's phases of cognitive development would be not the manifestations of component patterns, but the various steps in the synthesis of these behaviour patterns into a more complex whole. I am not aware that any systematic work has been done on this though I may well be wrong. If the orientation I am recommending is a useful one, the tasks before the research psychoanalyst are (a) to describe more carefully the component 'part-instincts', (b) to study the stages in their synthesis to form mature sexual behaviour in its varying forms, normal and 'abnormal'.

Here I would like to say a word about the terms fixation and regression. Both are used in at least two different senses.

Fixation can refer either to a pattern of behaviour or to the object towards which the behaviour is directed. In the second usage it seems to me fairly satisfactory, though it tends to be used to refer only to 'abnormal' object choice, whereas a neutral term referring to the selection of any object, normal or 'abnormal' would be better. The term when used to refer to the persistence into adult life of behaviour patterns characteristic of infancy or childhood can be misleading and smacks too much of a static tethering to the past. Instead, as Piaget and I have agreed, it is far more fruitful to think of present behaviour as being due to the ongoing dynamic assimilation and restructuring of the past in terms of the present.

The term regression is often used by psycho-analysts in rather the same sense as the first usage of fixation: but, whereas fixation usually refers to the persistence into mature life of patterns of behaviour characteristic of the immature, regression is often used to describe a recurrence of such behaviour after it has been discontinued. This usage should of course be distinguished sharply from the usage such as that which Lewin adopts in discussing his experiment on Frustration and Regression.\* Here it is used to describe a return to less differentiated behaviour by children capable of more

---

\* Barker, Dembo and Lewin in Child Behaviour and development edited by Barker, Kounin & Wright.



differentiated behaviour. It is imperative we find two terms to describe these two different processes. My own inclination would be to coin a new word for the concept denoted by the psycho-analytic usage; in coining it I would strive to convey the meaning of 're-arousal'. Unfortunately, however, it would hardly be easy to persuade psycho-analysts to forego their traditional use of the term.

I notice (p. 22 English version) that Piaget is still not quite convinced that I share his belief that present behaviour is a result of the on-going dynamic assimilation and restructuring of the past in terms of the present. Actually this is a view which many English psycho-analysts have emphasized for ten years or more as a result of being influenced by Lewin's field theory. The late John Rickman (President of the British Psycho-Analytical Society 1947-50) was an ardent exponent of it\* and most of the analysts associated with the Tavistock Clinic and Institute share this view quite explicitly.

Having lived in this intellectual climate for some years I at first found the delighted incredulity with which Piaget has greeted my agreement with him a little puzzling. However, I have recently come across a passage in one of Freud's later papers which makes it plain that he never reached this view himself but, on the contrary, was a convinced exponent of the opposite standpoint, which I am afraid a majority of analysts may still adopt. In contrasting the work of the psycho-analyst with that of the archaeologist who has to make reconstructions from material much of which has been lost or destroyed, Freud writes "But it is different with the **psychical** object whose early history the analyst is seeking to recover .... All of the essentials are preserved, even things that seem completely forgotten are present somehow and somewhere, and have merely been buried and made inaccessible to the subject. Indeed, it may, as we know, be doubted whether any **psychical** structure can really be the victim of total destruction. It depends only upon analytic technique whether we shall succeed in bringing what is concealed completely to light".\*\*

From this and other passages in the same paper it must be admitted that Freud held the view (a) that analytic treatment was concerned with 'digging up the past' - "we are in search of ... a picture of the patient's forgotten years that shall be alike trustworthy and in all essential respects complete" and (b) that he believed

---

\* Rickman, J. (1951) Methodology and research in psychopathology, Brit. J. med. Psychol. 24, 1-7

\*\* Freud, S. (1950) Construction in Analysis (1937) In: Collected Papers V, London



that accurate reconstructions of the past could be achieved in the course of analytic therapy. As I explained in my contribution to our third meeting and amplified in my notes following it, I and many English analysts do not share this view of therapy. Moreover, my own research programme is based on the view that data obtained in the course of analytic therapy can only be samples of behaviour (including introspections) which, though much influenced by the past, are inevitably influenced also by the present. For this reason what these data tell us of the influences active in the patient's early years is seen as in a glass darkly. Therefore if we wish to know about the influence of early experiences we have no option but to study the individual undergoing them as and when they are occurring.

Incidentally it is useful to note that we can formulate in two ways the process Piaget and I believe occurs. On the one hand we can refer to present behaviour as due to ongoing dynamic assimilation and restructuring of the past in terms of the present: on the other we can say that the present to which the behaviour is a response is assimilated and structured (or interpreted) in terms of the past.

### Affectivity

I find it rather difficult to be clear what Piaget has in mind by this word, which happens not to be one I use. I get the impression that he thinks of something unitary in character in the same way as he thinks of cognition as unitary. My own outlook is probably radically different. I think of affectivity as the accompaniment of an activated behaviour pattern, each behaviour pattern having its own characteristic affectivity. In the following account I realize I am following closely the ideas put forward six years ago by Lorenz.\*

### Behaviour Patterns

In putting forward the following ideas I realize I am giving a rough and ready sketch map involving nothing less than a theory of motivation and affect. My ideas are anything but clear and I am only advancing them now in order to give Piaget and others an impression of the lines along which I am thinking. The extent to which

---

\* Lorenz, K. Z. (1950) The comparative method in studying innate behaviour patterns. In: Symposia of the Society for Experimental Biology, No. IV, Cambridge Univ. Press



I have been influenced by ethological data and theory (much of it culled during discussions with Robert Hinde) will be evident and I shall be interested to hear Lorenz's comments.

Affect laden behaviour I tend to view in terms of structures built of component bricks. The bricks are relatively stereotyped behaviour patterns e.g. bird song or sucking, which, according to the species, may be built-in or learnt or a combination of both. The larger structure, e.g. courtship or nest building, is less stereotyped and a complex synthesis of these components. Although in principle any component is available for any synthesis, in practice each synthesis tends to select a particular group of components. None the less it is probably usual for certain component items to be utilized in more than one synthesis.

A typical example from the bird world is seen in connexion with courtship feeding where, in many species, as part of the courtship the male feeds the female in a way clearly resembling the feeding of the young by parents. There are two patterns to concern us here (i) the food presentation of the male, (ii) the food begging of the female. Food presentation can be seen as a component pattern of the two more complex behaviour sequences (a) parental behaviour and (b) male courtship behaviour; similarly food begging can be seen as a component pattern of (a) chick behaviour and (b) female courtship behaviour.

It is my suspicion that in humans the same principles hold, though, because of our much greater capacities both for conceptualizing and for learning, their manifestation is far more complex. I suspect we shall find them especially clearly when we analyse the component patterns concerned in the three basic social relationships - the infant-parent, the parent-infant, and the sexual. For instance, I think we can identify a number of component behaviour patterns, largely if not wholly built-in, concerned in these three relationships; examples are smiling, crying, cuddling, sucking, the pelvic thrust. Some of these component patterns, e.g. smiling, are usually utilized in all three social relationships and some are confined to two or perhaps only one.

It will be seen that this theory has much in common with Freud's. The main difference appears to be that whereas Freud appears to think that the whole of the earlier behaviour patterns are organized into later sexual behaviour, I am suggesting



that it is only some components of these earlier patterns which are so organized. Though this is not a negligible difference the approach is manifestly the same.

If the view I am advancing is right, one of our first tasks must be to identify these component behaviour patterns and, later, to discover by what means they are selected and synthesized to become parts of greater wholes. As regards the latter we would have to keep in mind the concept of harmonized reaction velocities and the big differences in outcome which would result from even small differential changes in these velocities, whether they were principally the result of the influence of genes or of the environment.

It will have been seen that I do not look on the affective aspects of behaviour as being readily described in terms of gain and loss. I think of behaviour patterns with their corresponding affects as being activated and terminated; from this point of view I am doubtful whether concepts of gain and loss are relevant. I should like to know Piaget's view.

#### Primary of Social Responses

It should be noted that in following an ethological approach in the foregoing exposition I have been making an assumption regarding the primacy of social responses about which I ought now to say a word.

Piaget quotes and seems to agree with Freud in his supposition that the only reason that the baby makes social relations is because he learns to do so: by relating to his mother he discovers that his physiological needs for food and warmth are met. This can be called "the cupboard love theory of infant love", or as Piaget describes it a "remunerative strategy".

Although many psycho-analysts, including Anna Freud, continue to adopt Freud's views on this topic, many others do not. In particular it is called in question by Melanie Klein and many other English analysts, including myself. The issue is basic to all work on social development and many debates between clinicians or between research workers can be traced to contrary views on this issue. Not only are psycho-analysts at variance between themselves here but the same is true of experimentalists: whilst those who follow the ethological tradition tend to assume



that social responses are as primary as are the physiological, those who follow the learning theory tradition tend to assume they are secondary and learnt. Wallon, I gather from Zazzo, assumes them to be primary.

It is my hope that in the work we are doing at the Tavistock on smiling and crying we may gradually accumulate data which will assist in the resolution of this conflict.

Relation of Affective to Cognitive (see Piaget's third question to me)

I find myself puzzled by Piaget's tendency to see the affective aspect of behaviour as dynamic and the cognitive aspect as structural, and I doubt whether this will prove a useful way to look at things. Nor am I inclined to think that "all behaviour is always simultaneously affective and cognitive according to two inseparable but distinct aspects", which is the third of Piaget's questions to me. This is clearly a thorny problem but one which I suspect to be of the utmost importance.

It seems to me that any given pattern of behaviour can, at different times, vary in the amount of emotional and intellectual activity which go with it. For instance at one extreme we know from the work of embryologists (Coghill, Paul Weiss and others) that, at any rate in amphibia, basic patterns of motor co-ordination can develop without major impairment even when the sensory nervous system has been anaesthetized. "The one fact that has been conclusively established by experimental results is that the central nervous system develops a finite repertory of behavioural performances which are pre-functional in origin and ready to be exhibited as soon as a proper effector apparatus becomes available."\* This suggests to me that it is useful to look at the development of behaviour patterns, even affectively toned ones, as being possibly independent of cognitive development in their initial stages.

At the other extreme are cognitive activities, e.g. solving a mathematical problem, which seems to be almost or quite independent of any behaviour pattern.

Furthermore, there are many patterns of behaviour, both learnt and unlearnt, which can vary in their cognitive component from occasion to occasion. An example of the unlearnt is breathing, which usually has negligible emotional or cognitive components but may acquire both if suffocation is imminent. Examples of the learnt are serving at tennis or playing a well-known piece on the piano; both skills have

---

\* Weiss, P. (1955) In: Analysis of Development, edited by Willier, Weiss & Hamburger, Saunders & Co.



been acquired through cognitively directed effort but both may later be performed almost automatically, the established behaviour pattern taking charge.

I realize that in this context I am using the term 'behaviour pattern' in rather a broad way to include co-ordinated movements like walking or breathing which are to a high degree built-in to the CNS during its maturation and are dependent on particular muscle groups, to extremely complex movements which are wholly learnt and which are far less dependent on individual muscles. (A good example of the latter is one's signature which, although usually effected with one's hand, is similar in form when produced by any group of muscles, e.g. those of one's leg and foot when signing one's name in the sand with one's big toe.) It is my belief that this extended use of the term 'behaviour pattern' will prove justified because I suspect that both the unlearnt and the learnt may prove to have basic characteristics in common.

It is clearly one of man's special characteristics that he is able to acquire through learning such an extraordinary diversity of new patterns. Further, as Paul Weiss has shown,\* it is one of man's special characteristics that he can suppress an in-built pattern and utilize instead a learnt one. Whereas a polio patient can learn to use a limb efficiently after tendons have been transplanted and the inborn pattern thereby made inappropriate, rats cannot do so at all and monkeys show only a faint trace of such adaptive adjustments: animals of both species are restricted to the original motor patterns despite the tendon transplantation having led to its activation resulting in incongruous and maladaptive behaviour.

Clearly the development of all the more complex learnt behaviour patterns is dependent on cognitive activity. Furthermore, although I have not grasped fully the implications of Piaget's notion of reversibility as the special characteristic of intelligence, I imagine that reversibility is always a characteristic of the more complex behaviour patterns which man is capable of learning. I shall look forward to Piaget's views on this.

This leads me to what I suspect of being a crucial feature of neurotic behaviour. We know that neurotic behaviour is unconsciously motivated and tends to be maladaptive and repetitive: it is felt as irrational and alien to the personality. Evidently

---

\* Weiss, P. (1950) Experimental analysis of co-ordination by the disarrangement of central-peripheral relations. In: Symposia of the Society for Experimental Biology, No. IV, pp. 92-111, Cambridge Univ. Press



such behaviour is still governed by rather primitive processes. Having, as we know, its roots in infancy and early childhood, some neurotic behaviour may well be due to the activation of almost unmodified in-built behaviour patterns, whilst most of it seems likely to be due to the activation of the kind of behaviour patterns which are partly in-built and partly learnt in the earliest years, and whose cognitive components are therefore still primitive. Piaget emphasizes that during early childhood "we find only uni-directional or irreversible actions" and that reversibility only develops later. All we know of neurotic patterns therefore suggest that their cognitive component lacks that "most specific characteristic of intelligence", namely reversibility.

I am wondering what Piaget will think of this suggestion and whether he accepts the corollary that, at all ages, behaviour is regulated by cognitive processes of different degrees of development - that in some of our actions we operate with a fully-fledged intelligence characterized by reversibility and in some with an extremely primitive intelligence or none at all, and that in respect of any one activity we may shift from one level to another?

In this connexion Paul Weiss in his studies of polio patients has demonstrated that, even after a new pattern of functioning has been learnt, "patients would frequently relapse into the old incongruous pattern", demonstrating that "the latter remained latent but retained its integrity and reappeared periodically whenever the higher replacement went into recess". This line of thought, I think, is fairly consistent with Freud's conception of psycho-analytic therapy. Making what is unconscious conscious is usually interpreted to mean removing barriers between different dynamic systems; such a process may perhaps also involve raising the cognitive component of a behaviour pattern from a primitive to a more sophisticated level.

The upshot of all this appears to be that, in contrast to Piaget, I am inclined to think of some behaviour as being structured in its own right and independently of any cognitive aspect it may later acquire, though I realize that the more complex behaviour patterns are probably structured cognitively from the start. Whether conversely cognition can ever be regarded as dynamic in its own right I find hard to know.



Similarly, I have difficulty with regard to the second part of Piaget's third question to me, namely whether affect is ever the cause of cognition or the reverse. My inclination is to suppose that cognition only develops as a result of behaviour and is therefore secondary to it, but I would like to consider the evidence more carefully before giving any definite opinion. From what I have already said, however, it will be seen that, at least in respect of primitive and more or less built-in behaviour patterns, I do not expect to find any very close parallelism in development between the cognitive and the affective.

Turning to the various hypotheses regarding the degree of synthesis which we may hope to attain, which Piaget lists at the end of his Section 2 on Problems of stages of development (English version, p. 17), it will be seen that I favour hypothesis 1 - that there are no general stages and that "we see an intermingling of processes of development which are evidently interrelated, but to different extents or according to multiple temporal rhythms, there being no reason why these processes should constitute a unique structural whole at each level", and that I do not favour hypothesis 3, that the personality is built up by successive stages of equilibrium and that it will be possible to establish correspondences between stages in the different studies we undertake.

Before discussing the question of transition between stages I want to say a word about equilibrium.

#### Equilibrium

It is plain that the structure and activity of the organism as a whole cannot be understood simply in terms of structure and activities of its parts and that the process of organization of the separate activities into a whole must have laws of its own and that, in so far as the organism persists and develops, there must be an equilibrium of forces. In considering the propositions advanced by Piaget, with which at present I am very unfamiliar, I should be concerned to ensure that they take account of various non-adaptive outcomes which favour the survival of neither the individual nor the species. Examples from my own field would be suicide, a mother murdering her baby, and the affectionless character following prolonged separation. As regard the latter we know it to have tremendous stability but it is certainly inimical to the individual's capacity for participating in family and social life.



I imagine in considering equilibria we have to distinguish rather sharply between the particular outcomes, some of which may be non-adaptive, and the system of feedbacks and governors which, because they more often than not lead to an adaptive outcome, are biologically reasonably efficient.

Transition from one stage of development to the next. (See Piaget's first and second question to me)

This is clearly a crucial issue and there is a good deal of psycho-analytic literature on the subject. For instance there has been much discussion of the factors which lead to a fixation at a particular libidinal phase; the traditional hypotheses are:

- (a) that a particular part-instinct is innately overstrong and
- (b) that environmental factors account for part of it.

Of the latter, both frustration and over-gratification have been incriminated. (I believe the evidence that frustration is relevant is strong; I am much less convinced about the evidence for over-gratification.)

Moreover Melanie Klein, who has done much to call attention to the crucial importance of ambivalence to the love object, has elaborated hypotheses regarding modes of resolving the conflict of ambivalence and transition from one mode to another.\* Although I am in close sympathy with her general approach I do not find the details of her formulation very convincing. In particular I think it more likely that some of the processes she described as occurring in the first few months in relation to the breast occur during the second year in relation to the mother as a whole person. Her views are almost entirely based on a reconstruction of what may happen in the first year of life using for the purpose data obtained from the analysis of patients above the age of two years. In my view little progress will be made in the theoretical debates which have arisen around Melanie Klein's hypotheses until systematic observation, and where permissible experiments, are made on infants in their first year.

---

\* See her article Some theoretical conclusions regarding the emotional life of the infant in Developments in Psycho-analysis by Klein, M., Heimann, P., Isaacs, S. & Riviere, J. London, 1952.



In respect of first-hand experience the transitions with which psycho-analysts are most familiar are those to be observed during successful therapy. Although occurring much later in the life history than is optimal, such transitions are widely believed by psycho-analysts to resemble, in some measure at least, those which are usually achieved in the early years. A systematic study of the nature of the therapeutic process should cast light on certain important transitions, for instance those concerned with developing more efficient modes of dealing with the conflict of ambivalence.

To return to Piaget's questions. I would like further information about the implications of his "hypothesis that the reactions to a stage n are released by the dissatisfactions, conflicts or disequilibria belonging to the previous stage n - 1". Many transitions in behaviour and psychological function I suppose to be due to the maturation of the C.N.S. and the bringing into use of new groups of cells and new circuits. A case in point would be the transition in the development of motility from crawling to walking. Development of cognition must also be dependent on maturation of the C.N.S., though experience and learning obviously play a larger part than in the development of walking. I conceive of development in the behaviour patterns concerned with social inter-action as being dependent partly on C.N.S. maturation and partly on experience, and in this respect comparable with the development of cognition.

In so far as all growth and development can presumably be traced to biochemical disequilibria I agree with Piaget's formulation. I am doubtful if I agree with it if the "dissatisfaction, conflict or disequilibria" are conceived of as purely psychological.

As regards the stage of latency (Piaget's second question to me) I am inclined to regard it as the manifestation of a phase of maturation which in considerable measure has been built into the human species in the course of evolution. Nevertheless I believe the form it takes to be highly dependent on experience.

As regards Piaget's final question addressed to all of us, there are two areas of transition which I might reasonably attempt to study. One would be the integration into an organized whole of the behaviour patterns which form components of the infant's



relation to his mother. I am thinking here of suckling, smiling, crying, the need for physical contact, etc. Before attempting to do this, however, I think it will be necessary to do a good deal more research on the nature of these different responses. The other field would be in the transition from one mode of handling the conflict of ambivalence to another. Since this was the main theme of the notes of mine which were circulated in May 1955 there is no need for me to say more at the moment.

### Conclusion

I shall look forward to reading the replies of others to the questions posed in Piaget's essay and hope they will be circulated. I shall also look forward to Piaget's comments on these notes of mine and to those of other members of the group, especially Lorenz.



WORLD HEALTH  
ORGANIZATION

RESEARCH STUDY GROUP ON  
PSYCHOBIOLOGICAL DEVELOPMENT  
OF THE CHILD

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/7  
20 May 1955

ENGLISH ONLY

SOME NOTES BY JOHN BOWLBY - MAY 1955

These notes fall into three parts:

1. Reflections on the plan devised for the Fourth Meeting
2. A note on intra-psychic conflict and its resolution,  
with an Appendix
3. My own research plans and ideas.



## 1. REFLECTIONS ON THE PLAN DEvised FOR THE FOURTH MEETING

Reading over the record of our Business Meeting on 23 February 1955 I find myself very much in agreement with Piaget's views. I was frankly alarmed by what I understood of Margaret Mead's proposals which seemed to me impractical and, if adopted, likely to lead to a barren meeting. Members of the Group represent a large number of disciplines which so far are little related to one another. The coordination and synthesis of any two seems to me a formidable task and sufficient for a whole meeting: to do it for all will require the work of a generation (and I suspect a generation junior to most of us attending the Study Group meetings). An ambitious attempt to coordinate all the disciplines in the coming 18 months could only lead either to a superficial formulation or else to a premature synthesis which could greatly hinder further progress. It was recognition of these dangers which led me to describe myself as a 'disintegrator' - or, more accurately, one who is in no hurry for integration.

It was also partly responsible for stimulating me to write the Note on intra-psychic conflict and its resolution which follows. I believe that the principal task of the Group has been and will continue to be the creation of an intellectual and emotional climate which will permit coordination and synthesis to grow. Before any such development is possible, it is necessary for us as participants and, through us, our immediate colleagues and those who read the Proceedings of our meetings, first to respect and then gradually to comprehend the views of others, many of which views appear at first sight either mistaken or irrelevant or silly. It is necessary for us to tolerate all these contradictory and often seemingly mistaken opinions if we are ever to take any steps towards their synthesis; unfortunately it is only human nature to find the toleration, both of conflict and of a state of affairs where there is so far no integration, extremely disagreeable and difficult. I had a feeling in February that there was some intolerance of this state of affairs and that this was prompting to precipitate efforts at synthesis.

Looking at the situation more dispassionately now I am inclined to think we might attempt coordination and synthesis in one or possibly two selected areas.



I am interested to note that Piaget saw two possibilities for himself - a linkage between his own work and psychoanalysis, and one between his own work and neurophysiology. As will be seen later, I can also envisage two possibilities-- a linkage between psychoanalysis and Piaget's work and one between psychoanalysis and ethology. It is probably no accident that each of us at the moment can only consider working on two linkages and that for both of us they are between our own field and two other fields. I shall be extremely interested to see how ambitiously Piaget sets his aims for 1956. For my own part I shall be quite content if he selects one pair of disciplines and confines his synthesizing endeavours to these. In the hopes that psychoanalysis will be one of them I have made the notes which follow.

My own indebtedness to the Study Group has been great. First, it has led me greatly to broaden my horizon and enabled me to spend a good deal of time with a number of stimulating people of whom I should not otherwise have seen much; secondly, it has inspired me to work during the coming years towards the coordination and synthesis of psychoanalysis with fields of work represented by Lorenz and Piaget.

## 2. A NOTE ON INTRA-PSYCHIC CONFLICT AND ITS RESOLUTION

In my contribution to the 1953 Meeting of the Study Group (English version pp. 199 - 204), I argued that "the study of instinct distinguishes psychoanalysis from other branches of psychology", meaning by this the study of the motivational forces which impel us to action, which carry with them strong emotions, and which we cannot always easily control. I then gave two clinical illustrations of patients whose emotional disturbance seemed to spring from their readiness to be motivated by strong but incompatible impulses towards one and the same object - in the adult patient undergoing analysis love for me and hatred for me, in the child patient at a diagnostic interview love and hatred for her mother. In my summary, after referring again to the central problem in human beings of ambivalence to the love object, I remarked that "the disturbed person is unable to tolerate these contradictory instincts" and implied that it is this inability to tolerate conflict which leads to the variety of psychogenic symptoms with



which we are confronted in our patients. I ended by saying that the therapeutic task "is to permit these conflicts to develop within the therapeutic relationship (which they do in some fashion spontaneously) and to help the patient experience the impulses in relation to the analyst, whom he sees as both a satisfying and a frustrating object. When these conflicting impulses are experienced together within a relationship of trust, there tends to be a restructuring of the instinctual life, permitting less conflictual and therefore more satisfying relationships".

The main features of this particular formulation of the central core of psychoanalysis are -

(a) pathogenesis is to be sought in intra-psychic conflict, particularly the conditions which lead to the individual being unable to tolerate such conflict,

(b) therapy is seen as helping the patient experience the conflict of incompatible drives yet once more, but this time in relation to an object (the analyst) who tries to behave in such a way that the patient can tolerate the conflict and so 'discover' new methods of resolving it.

This formulation, I believe, would be endorsed by the majority of psychoanalysts in Great Britain, but there are many analysts both in Britain and elsewhere who would not endorse it. Some analysts place greater emphasis on the conflict between the individual's drives, particularly the child's, and the restricted extent to which the environment meets them. In other words they see the main conflict as lying between organism and environment rather than within the organism itself. (I had this impression in listening to Erikson's exposition, but I may be wrong.) There are many analysts, too, who regard therapy more as a method of dealing with traumatic events of the past than with the active or latent intra-psychic conflicts within the patient in the present which, since they are concerned with his inter-personal relations, will express themselves yet once more in his relation with the analyst. Put



briefly, this contrast is between the analyst who is more concerned with 'digging up the past' and the one who is more concerned with the 'here and now'.

My reason for going over this material again is because this particular version of psychoanalysis is not yet very widely known about, because it underlies all my own work, and because I believe it lends itself more readily than other versions to an integration with other approaches, notably Lorenz's work on instinct and social interaction and Piaget's work on cognitive development.

In this connexion I was struck at our recent meeting with Piaget's reception of some remarks of mine. In considering some of Erikson's clinical material, he had proposed two alternative viewpoints which he characterized as (a) static - the notion that the present is adapted to the past and the earlier conflict is preserved, (b) dynamic - the notion that conflicts are constantly recreated and that the mode they take in the present is determined by earlier experience, almost, he remarked, like the formation of a habit. He made it plain that he preferred the second alternative. In discussion I said that these alternatives were familiar to me, that they characterized different approaches in psychoanalysis, and that I also preferred the second. We were both delighted to have found common ground, but it was evident that he had been unfamiliar with this version of psychoanalysis.

In what follows I shall try to sketch out some of these ideas a little more fully to enable me to indicate both where I think steps at integration with others' work are likely to be fruitful and also the rationale of my own research plans.

#### Ubiquity of conflict

Though the idea that intra-psychic conflict is of central importance for human psychopathology has been familiar to me for many years, it is only more recently that I have come fully to realize that such conflict is the eternal lot of all human beings, whether mentally ill or well, and that it is also the lot of infra-human animals. In coming to realize this I was greatly influenced



by Lorenz's remarks in the discussion of my contribution in 1953 (English version p. 217). "We thought formerly that instincts in animals were mutually exclusive. Julian Huxley, speaking in parables, said that man resembled a ship with many captains on the bridge fighting all the time between themselves, while an animal was a ship which was also governed by a number of captains, but when one captain popped up in command on the bridge, the other one had to go down. I have quoted that again and again because I believed it myself. But it is not true in the least." Paraphrasing one of Piaget's remarks which has been much quoted in the Group, we might say "Il y a toujours un conflit".

Once we realize that conflict is the rule and not the exception, the clinical problem of trying to understand the difference between the mentally healthy and the mentally sick changes. Earlier notions that emotionally disturbed people are alone in being subject to intra-psychic conflict are manifestly untenable and later notions that the disturbed person has conflicts of a kind different from those of others can hardly now be sustained. What psychoanalysts seem to meet with in their day-to-day clinical work are people who have conflicts of a kind similar to those grappled with by other men, but who for some reason deal with these conflicts less successfully. The mentally healthy man becomes angry at times with parents, wife or children but he neither behaves in such a way that he seriously hurts and/or alienates them, nor does he inhibit all expression of irritation, nor does he live in perpetual anxiety and guilt either that he has hurt and/or alienated them or that he will do so imminently. The problem of pathogenesis is to understand why some people grow up with the ability to deal relatively effectively with their conflicts whilst others do not develop this ability. The problem of therapy is to devise the most effective techniques for helping those who have not developed the ability to develop it for the future. My own research is entirely concerned with pathogenesis, with the hope that better understanding will lead to better preventive measures and perhaps also to better therapeutic techniques.

It is evident that there are many methods of resolving conflict, from simple methods such as superposition or reciprocal inhibition characteristic of lower organisms and perhaps the simpler processes in man (see Lorenz's remarks



in 1953, English version p. 217) to the extremely complex weighing of argument and counter-argument characteristic of the more highly developed processes in man. It seems likely that, in the individual human, resolution of intra-psychic conflict is brought about by several different processes varying from the relatively simple to the exceedingly complex.

All the more complex methods of resolving conflicts between the component parts of a unit (e.g. organism or social group) require that there be differentiated out from within the unit an agent governing action (for example, in the social field a government) which stands in some special relation to the various other components of the unit (for example, members of parliament or the members of the electorate). We know from study of politics that relations between the agent governing action and the components of the unit can vary over a great range of patterns - from dictatorship and latent revolution at one extreme to anarchy at another - and that even democratic government can be of many varying patterns and of very varying degrees of efficiency.

The conditions making for effective integration and resolution of conflict are thus far from easy to define. Furthermore, a solution which may be effective in the short run may be a blind alley in the long: I believe this to be true of dictatorship. In so far as one favours democracy in social groups it is because one believes that its better forms are the most efficient modes of integrating and resolving social conflict in the long run and, in comparison to dictatorship, are more flexible and have greater potential for permitting growth and change. In the same way, I believe the resolution of intra-psychic conflict by means of repression is fairly effective in the short run but decreasingly so in the long, whereas a more permissive method, which amongst other things tolerates all components of the conflict, is biologically more efficient because more flexible.

An essential feature of these more democratic modes of resolving conflict is that they require all aspects of the conflict to be brought into relation with one another on some common meeting ground, thereby enabling a course of action to be decided upon which takes account of all. The essence of this form of integration, therefore, is the toleration of conflict.



In seeking to elucidate the problem of why some individuals become emotionally ill and others do not, we are faced therefore with trying to understand what are the factors which influence the differential development of conflict toleration in man. There are presumably many such factors and I only wish to comment on three which interest me and which I believe to be of great importance:

- (a) the strength of the conflicting drives: the stronger the drives, the more violent the resulting conflict and the more difficult, therefore, is it to resolve efficiently;
- (b) the capacity of those with whom we live, in the case of a child especially his parents, to tolerate these drives;
- (c) the relative rates of development in the child of instinctual impulses and of the processes making for their integration and control.

#### Strength of drives

My work on the effects of mother-child separation has led me to believe that, at least in some cases, major difficulties in the individual's capacity to resolve conflict can develop as a result of conflicting drives being evoked at high intensity in the immature organism over a long period. The facts regarding the child's behaviour are now reasonably well established. If a child, say in his second year, who has a close relationship with his mother is separated from her for a few months, his longing for her and hostility towards her are both raised to a high level of intensity and he is in a state of great distress. This persists for a few weeks but, as a rule, does not persist indefinitely: after a few weeks his longing for her ceases to be expressed. However, if after a few months the child returns home, this longing for the mother reappears (often after a time lag of days or even weeks) and remains active at very high intensity thenceforward. In addition the potential for hostile behaviour is found to be raised to a high level. The resulting conflict, in which both components of ambivalence to the mother are running at



high intensity, is extremely difficult for the child to tolerate and seems often to be at the back of symptoms. In certain cases where the separation has been very prolonged, the longing for the mother remains completely unexpressed in any direct form, but it seems none the less to be expressed in modified form in the child's promiscuous demands on other people. If such a child is treated psychoanalytically, once feelings for the analyst begin to be expressed the most intense ambivalence is encountered within the transference relationship.

It seems to me plausible to presume that the great difficulties which these children have in resolving conflict have arisen through their being faced with conflicting drives, both of high intensity, at a time when their capacity for integration and control is limited.

Presumably any other conditions which lead to conflicting drives being evoked at high intensity in the immature organism over a long period can be pathogenic. Such conditions could be due to (a) genetic factors (b) organic change in the brain or (c) other psychological factors, for instance rejection by the mother.

#### Capacity of others to tolerate conflict

Our capacity to tolerate conflict is clearly much influenced by the capacities for this of those around us. In a department with a scientifically intolerant atmosphere a student will become far more intolerant than he would in one with a tolerant atmosphere. In the same way child-guidance practice strongly suggests that a child brought up by parents who can themselves tolerate emotional conflict will find it easier to tolerate emotional conflicts in himself than will a child whose parents are intolerant of conflict. For instance, a child who is having great difficulty over tolerating the intensified conflict of ambivalence resulting from the birth of a new baby is frequently found to have parents who, in their own childhood, were unable to tolerate this very conflict. Thus a parent who, stemming from difficulties in her own childhood, regards jealousy as both wicked and dangerous will make it far more difficult for her child to accept jealousy and, by integrating it with other parts of his personality, learn how to control it.



As clinical illustration (for which I am as enthusiastic as Lorenz) I am adding an Appendix - a record of the play in a nursery group of a little girl of 4-1/2, Mary, who had had a new baby sister born two months earlier. The conflicting motivations towards the baby, those of saving it and killing it, are evident. However, all that is known of Mary suggests that she is an emotionally healthy child who is able to manage her conflicts reasonably successfully; in this case she is no doubt helped by her parents and the nursery school teacher all of whom understand jealousy and tolerate it, thereby helping her control it in an effective way.

This understanding and toleration of emotional conflict are, of course, also characteristic of the psychoanalyst and it is this probably as much as anything which enables him to help his patients. He conceives it as his main task to help his patient first to recognize and then to tolerate many unknown or feared aspects of himself, notably emotionally toned impulses of which he is either unaware or too ashamed or afraid openly to admit. It is particularly in helping the patient recognize the existence of these impulses in his relation with the analyst and helping him tolerate them there that many analysts believe their most effective therapeutic work lies. (With this technique emotionally loaded episodes of the past which have been repressed are seen merely as examples of past occasions when these impulses have been experienced and/or expressed. From the therapeutic point of view the recovery of such memories is regarded as valuable but perhaps less so than for the patient to recognize that these same impulses are also operative in the transference relationship in the here and now.) This technique, of course, is based on the assumption that there are latent powers within the individual enabling him to resolve conflict and to control impulses in a more effective way than he has hitherto done. Experience suggests that this assumption is justified.

#### Relative rates of development of integrative processes

Goldschmidt and others have demonstrated that the form which an organism takes depends in great measure on the differential growth-rates of the parts. This notion has been present in psycho-analytic theory of personality



development though it has never been applied very systematically. I suspect it will prove a very valuable concept in this field.

For instance, there is good reason to think that to insist upon a high degree of control in the second and third years of a child's life makes it more rather than less difficult for him as he grows up to develop a smoothly working pattern of integration and resolution of conflict. The reason for this is presumably that in these years the child's psychic apparatus is as yet insufficiently developed to permit him to utilize any but rather primitive methods of integration and control, and that, once these have become dominant, it is more difficult for him to use the more sophisticated and effective methods which are available to the older child and adult.

The adult human being's capacity to organize his behaviour over a period of time and to resolve conflict by such expedients as postponing the satisfaction of one of its components or displacing the aim of one of them permits of far more subtle solutions than are possible either to infra-human animals or young children. Children in their second and third years have extreme difficulty in organizing their behaviour in time and instead live almost exclusively in the present: events occurring in past or future have little meaning for them. As a result, their capacity for resolving conflict by utilizing a time perspective is negligible or absent. This is probably one of the reasons why disciplinary measures taken in these early years seem often to result in the child developing a system of control which operates on an all-or-none principle - he is either highly controlled or else uncontrolledly impulsive, or else a mixture of the two.



APPENDIX

Clinical Material Illustrating the Conflict of Ambivalence Towards  
a New Baby in a Mentally Healthy Girl of 4-1/2 Years

The following notes were made of the play of four children attending a Nursery Play Group.

Actors

The main actor, Mary, is aged 4 years 7 months and has three younger siblings. The youngest is a new baby born        months prior to the morning when the play described took place. The other participants are -

|         |                        |
|---------|------------------------|
| Annabel | 4 yrs 6 months         |
| Sally   | 4 yrs 0 months         |
| Mervyn  | 3 yrs 11 months        |
| Miss H. | Nursery School Teacher |
| Miss J. | Psychologist-observer  |

Play sequence

Mary is organizing a fireman game, rushing about the room putting out imaginary fires and pretending to use hoses and ladders. (Appropriate sounds made.)

After putting out several fires, Mary says "there's a little baby left in the parent's house, which is on fire. Fireman must be brave you know and save children from burning houses. I'll get the baby out of the house". Having done so, she says "The baby was nearly burnt... it's nose is black" (looks anxious). "I shall have to take her to the hospital." She carries the smaller of the two dolls over to the window and puts it on a cushion on the floor, covering it over with a sheet from the cradle.

Mary, Annabel, Sally and Mervyn are on the floor bending over the doll, all talking together, Sally very anxious about the baby.



Mary: "I'll be the doctor."

Annabel: "I've another bad baby to see the doctor."

Annabel, in a somewhat anguished voice when Mervyn gets up and rushes about and over the doll, cries: "He's treading over the baby's bed."

Mary: "It doesn't matter."

There is a lot of rivalry between Mary and Annabel for the role of doctor. Presently Mary says: "No, she's not feeling well poor thing; so I'll get something to kill the baby that's not feeling well". Mary goes across the room and fetches a piece of wood about 6" long. "I've got the gun... I've got a sword to cut its head off." Hospital play continues. Mary says: "Here's the bottle of medicine" and Annabel leaves to go to the play shop to buy more medicine.

Mary says: "Some of the baby's hair has come out so I must get a pin to stick the hair on again". She also says that it has a bad tooth and that they must put in another of the same colour.

A moment or so later Mary calls out "Who's wicked - I've got a sword for someone who's wicked" and looks around for wrongdoers.

Mary, addressing Miss H. and referring to the baby in hospital remarks "She is nearly better now".

Miss H.: "Then you will be able to take her home."

Mary: "No, her parents will come and get her. She must never go out again, or she'll have to be found again" (said anxiously).

Following this conversation Mary asks Miss J. to come to the hospital to see her daughter. Miss J. does so, taking a toy cat which she 'bought' from Annabel at the play shop set up on the other side of the room. Miss J. asks Mary how her daughter is and she replies "She's not my daughter, she's the daughter of the other parents". Mary goes on to explain that the baby was her daughter, but that she had had to be the fireman who rescued the baby and also the doctor who had cured her.



Mary then sees the 'sword' and asks Miss J. to take the sword back to the 'shop' and to say that she was very worried about it because it should not be near the baby. She goes on to remark: "I don't know why they brought it over here".

#### Comment

Mary's ambivalence to the new baby is shown by the sequence in which, first the baby is harmed by the fire, then it is saved by a fireman and tended by the doctor, next it again suffers damage by being killed, finally it is once again restored by medicine.

Mary's guilt about her hostile impulses is shown first by her threats against someone who is wicked and later by her concern that the 'sword' should be near the baby; in both cases she behaves as though it was someone other than herself who was wicked and a danger to the baby. This disowning of part of the self and attributing it to others (projection) is a very common and unsatisfactory way of dealing with conflict and, if persisted in, would lead Mary to develop a neurotic character. Persistence of this projection as a defence measure would be encouraged by (a) anything which increased the jealousy and thereby the possibility of really damaging the baby which would conflict with her love for it, and (b) disapproval and punishment of jealousy by adults.



#### RESEARCH PLANS

The foregoing is intended to give an outline of the ideas to which I should like to see weight given in any synthesis which the Group may attempt. These ideas also provide my own theoretical background for the research for which I am responsible at the Tavistock, though it is characteristic of the present state of personality theory that no member of the research team shares all these views and some diverge from them considerably: in fact we have to tolerate a good deal of theoretical conflict! At present and in the immediate future this work comprises two main projects:

- (1) Some effects of separating two-year-old children from their parents:  
a comparative study, undertaken by C. Heinicke

This concerns a comparison of the behaviour in a separation situation of two groups of two-year-old children who are matched for age, home background etc. Both groups are observed away from their mothers over a period of some weeks, one group in a day nursery in which they spend only 8 hours a day, and the other group in a residential nursery where they remain for the whole 24 hours. For both groups daily observations start on the first day the child is in the nursery and continues for 3 weeks. Observations are made on a systematic basis in two types of situation (a) in an Everyday Nursery setting and (b) in a special Doll Play session. A main finding to date is that, whilst both groups of children express a longing for their parents, the Residential children cry more in doing so, in other words show the motivation at higher intensity. Similarly, whilst both groups on occasion show hostile behaviour, the Residential children show it both more frequently and more violently - again at higher intensity. Although the number of children so far observed is very small, the differences in behaviour seem fairly clear cut and in many cases are statistically significant at the .05 level. It is hoped to increase the numbers in both groups over the coming year.

Although Heinicke is himself reluctant as yet to draw conclusions from these findings, I cannot help taking some satisfaction in noting that they are in line with views I have several times expressed, though they cannot be said to confirm them. Ten years ago, in discussing the psychopathology of the affectionless character, I commented on "the parts played by libidinal and aggressive impulses, both of which will have inevitably been excessively stimulated by the frustration of separation", and also on



"the swamping of affection by rage" (Bowlby, 1944).<sup>1</sup> Where, as yet, Heinicke's findings fail to confirm this prediction is that he has at present only indirect evidence that the increased hostility he observes is aimed at the absent parents. To say, as I am inclined to do, that his results suggest that both components in the conflict of ambivalence to the mother are stimulated to higher intensity is therefore no more than a reasonable inference.

In sponsoring this work and in considering its implications I find myself very dissatisfied with the theories of social motivation in the human now available. Despite their great value in drawing attention to the primitive nature of the forces which so often impel us, psychoanalytic theories of instinct lack precision. My hopes have therefore turned to the ethological approach which seems to me to have in it many of the features which I value in psychoanalysis, but to be far more precise and to be susceptible of experiment. Apart from the work of Lorenz and Tinbergen I have been interested by Hinde's critique of the 'psycho-hydraulic' theory of instinct (Hinde 1954)<sup>3</sup> and I have also been much impressed by the possibilities of the model of motivation proposed by Deutsch (1953).<sup>2</sup>

<sup>1</sup> BOWLBY, John (1944)  
Forty-four juvenile thieves: their characters and home life  
Int. J. Psycho-anal. 25  
Republished by: Baillière, Tindall & Cox, London, 1946

<sup>2</sup> DEUTSCH, J. A. (1953)  
A new type of behaviour theory  
Brit. J. med. Psychol. (General Section), 44, 304-317

<sup>3</sup> HINDE, R. A. (1954)  
Changes in responsiveness to a constant stimulus  
Brit. J. Animal Behaviour, 2, 41-55



(ii) A study of the smiling response in infants during the first year of life undertaken by A. Ambrose

This second project is an attempt to identify basic components in the interacting motivations which bind child to mother and mother to child, and to study their characteristics. Because of preliminary work which has already been done on the smiling response in babies, we decided to start with this response. We plan to use an ethological frame of reference because we believe this to be the most hopeful. So far only very preliminary work has been done and the study proper will not begin until Ambrose is available in Autumn 1955.

We regard smiling as an innate behaviour pattern in the human and one which matures in the earliest weeks. From Spitz's work one is led to suspect that a configuration containing basic elements of the human face is an innate sign-stimulus. Furthermore casual observation of adults, especially parents, makes it plain that the baby's smile evokes parental behaviour and we again suspect this to be innate, the baby's smile being a social releaser. It would be nice to test the hypothesis that these sign-stimuli are innate in the human being but this would be very difficult - perhaps impossible. It may, however, be possible to build up observations which cumulatively clarify the extent to which these responses are innate or the extent to which they are learnt.

Ambrose has already found that one way of quantifying the smiling response is by measuring the length of time the baby exhibits a smile. This enables him to study various stimuli for their relative effectiveness in evoking the smile. It also enables him to investigate those conditions which make a baby at different times more or less 'smily', namely more or less ready to respond to a given stimulus by smiling. One particular experiment which it is hoped to carry out is to compare the effect on the baby's 'smiliness' of two classes of immediately previous experience (a) social experience (being with other humans versus isolation), (b) food satiation versus hunger. Other responses which clearly have a social function and which are probably innate which it is hoped one day to investigate are crying and the need to be near the mother (or actually



touching her or in her arms). The hope behind all such work is to get a better idea of the basic motivation leading the infant to be attached to its mother. Similar studies of parental responses may help us to understand better their nature and how they interact with the basic responses of the baby.

It will be seen that this second project or group of projects is explicitly aimed at utilizing ethological concepts in the study of social development in man and especially the mother-child relationship which is of such central importance in psychoanalysis. Whether and how far it will be possible for me to extend my interests to research in the other direction, namely to the integrative functions and their development, is unclear. So far I have given much less thought to this side of personality development and I am not hopeful of tackling it seriously in the next few years. If and when I do so I hope to get help regarding the simpler integrative processes from ethology and regarding the more complex and specifically human from Piaget.



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/12  
16 August 1956

ORIGINAL: ENGLISH

A B S T R A C T

SOME EFFECTS OF SEPARATING TWO-YEAR-OLD CHILDREN  
FROM THEIR PARENTS: A COMPARATIVE STUDY

by

C. M. Heinicke

(Published in Human Relations, 9, No. 2 1956)

The aim of this study is to throw further light on the psychological processes set in train when a young child is separated from his mother. For this purpose, two groups of children aged between fifteen and thirty months who came from similar home backgrounds (intact and reasonably satisfactory families) were studied. One group (six children) were in residential nurseries, the other group (seven children) were in a day nursery. The behaviour of the children during their first twenty-one days in the nurseries was recorded and comparisons made between the two groups.

The recording of behaviour in the everyday nursery settings was made every day, often both in the morning and the afternoon, and followed a scheme in which all actions made either by the child or by the adults in the immediate environment were categorized in terms of: the agent of action; the object of action; the general and specific relation of one to the other; the mode of expression; and the intensity of expression. Results for each class of act are expressed as a percentage of the total acts exhibited by the child during the period in question. In addition to recording samples of behaviour in the everyday nursery setting the children were given standardized doll play sessions on four occasions and the behaviour recorded in a way comparable with that in the everyday nursery setting.

When the behaviour recorded throughout the twenty-one days is compared for the two groups the main findings are:



1. Although the two groups of children do not differ in the percentage incidence of their attempts in the everyday nursery setting to regain their parents or other adults, the residential children more frequently accompanied these attempts by crying, i.e. show the response more intensely.
2. The residential children express hostility in a higher percentage of their acts than do the day children, and in particular express their hostility in a more intense manner. This more frequent and more intense expression of hostility occurs both in the everyday nursery setting and during the doll play sessions.
3. A greater proportion of the acts of the residential children are given to sucking the thumb or other object.
4. The residential children show more frequent lapses in sphincter control both in the everyday nursery setting and during doll-play sessions.
5. The residential children more frequently developed a cold while staying in the nursery.

As regards the behaviour of the six residential nursery children on return home:

- (a) Five of them showed a turning away or other actively negative reaction to the mother. All recognized their fathers and seemed glad to see them. (Except in one case where a mother made some visits, the children were visited only by their fathers.)
- (b) Both the parents' reports and the observers' impressions indicated that the frequency and intensity of hostility shown by the residential children when they returned home was much greater than it had been before the separation.



UNITED NATIONS

  
NATIONS UNIES

WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting, 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/8  
11 January 1956

ORIGINAL: FRENCH

ESSAY ON THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

(To serve as a starting point for the discussions of  
the last meeting of the WHO Study Group)

by

Professor J. Piaget

Having devoted our various meetings to the study of special problems, we agreed to devote the last meeting to discussion of the general problems of development, such as the examination of the factors of such development, of its stages and above all of the mechanisms enabling the transition from one stage to another to be explained. Each of us has already contributed a large number of data on these different points, but what is now required is a synthesis, harmonizing as far as possible the different viewpoints presented by each of us.

Of course, such a synthesis is of no value unless it is based on facts and, in order to make such facts available, Margaret Mead, for example, has very kindly lent us a film on a case of development which we have studied with great interest. Unfortunately the facts are only significant when they constitute replies to problems previously propounded in such a manner that nature can furnish sufficient confirmation or rebuttal of the theories advanced to solve them.

Such then are the problems which we shall endeavour to propound as the subject of our attempted synthesis, and we shall try to formulate them in such a way that each member of the Group can add to this preliminary paper so that a more complete version can be prepared.



Consequently the plan of this first draft will be as follows:

I. Development factors, with an attempt to find a common language, enabling the contributions made on this subject by each member of the Group to be co-ordinated.

II. The problem of the stages of development (affectivity and intelligence, physical and mental growth, etc.) or of continuous aspects which cannot be divided into stages.

III. The problem of the passage from one stage to the following or of the mechanism of development.

IV. Conclusion in the form of questions put to members of the Group in order to determine whether they are able to supply any decisive facts in answer to problems I to III.

#### I. DEVELOPMENT FACTORS

Immediately on approaching this first great problem, it can easily be seen, on re-reading the discussions of the Study Group, that we did not keep to the simple and traditional distinction into three main factors of development:

(a) hereditary factors, manifested in physical growth and especially in the maturing of the nervous system; (b) the action of the physical environment (physical nutrition and the experience of objects acquired mentally), and, (c) the action of the social environment. On the contrary, we constantly tried to overcome this dangerous partitioning and, if our respective contributions are carefully examined, it can be seen that we made three kinds of contribution of value in this connexion:

- (1) search for interaction between these three factors;
- (2) Search for a common language making it possible to describe all three and to formulate their interactions more clearly;
- (3) Recognition, either implicit or explicit, of the existence of a fourth factor, (d), additional to factors (a), (b) and (c), and introducing new elements, while at the same time making it possible to co-ordinate them.



1. Search for interaction between factors (a), (b) and (c)

It might have been expected, at first glance, that the position taken with regard to development factors by the members of the Group would be determined univocally by the field where they had made their own discoveries; for example that Lorenz would explain everything by innate mechanisms and the spontaneous activity of the nervous system and that he would be supported by the EEG people, that Margaret Mead would explain everything by the social factor and that Zazzo (as a disciple of Wallon) would base everything on the maturing of the nervous system and the social factor, while slightly under-estimating the activity of the individual at grips with experience in respect to physical objects, etc. etc.

However, the first result of the discussions of the Study Group, or if you prefer, the first concrete element of the "synthesis" which you have asked me to make, is that we are unanimous in considering, (d), that the three factors (a), (b) and (c) never occur independently of each other and, (b) that their interactions are consequently at least as important as their respective actions.

Below are some examples of this:

For the discussions on cerebral activity (EEG, etc.) I shall restrict myself to a quotation from Grey Walter,<sup>1</sup> not included in the records of the Study Group, but which give rise to all the interventions made among us, by that versatile inventor:

"The crude division of all human attributes into "inherited" and "acquired" is excusable but quite unreasonable. Even in the simple models of behaviour we have described, it is often quite impossible to decide whether what the model is doing is the result of its design or of its experience. Such a categorisation is in fact meaningless when use influences design, and design use."

As regards Lorenz, I would recall the moment (which appeared decisive for him and myself) at the end of the discussions during the London meeting, where Konrad accepted and stressed my remark that there is no genotype which is not linked to a phenotype, where he discovered with surprise that I was by no means an empiricist (in the sense of explaining development and learning by experience alone) and

---

<sup>1</sup> Walter, G. (1953) The living brain, London, p. 189



where he briefly described what he termed his "dynamic apriorism". However, Lorenz's dynamic apriorism, i.e. the concept of an internal activity of the organism developing in constant liaison with acquired experience, is not very far from development through constant interaction of internal and external factors which we ourselves describe as a continuous construction of structures by successive equilibrations.<sup>1</sup>

As regards the psycho-analysts who, in the time of Freud, explained everything by instinct, I do not need to remind you of Bowlby's flexible and delicately inflected attitude, based on continued interference between instinctive factors and individual experience as well as inter-individual or social relations.

As regards the cultural and social aspect (in the molar sense and not only inter-individual), we may recall, for example, how Margaret Mead, who proposed repeating in New Guinea certain of our intelligence tests (conservation, spacial relations etc.) admitted the legitimacy of the theory according to which the stages of reactions to these tests could be the same as regards the order of succession, but with big differences as regards average ages or even of the non-attainment of higher levels. However, if this were so, it would mean that social factors are constantly interacting with other factors (physical experience etc.) even in such a sphere as the organization of concepts which is sometimes interpreted sociologically in a rather exclusive and rigid manner (Durkheim, etc.).

However, all this is self-evident. What is more exciting is to trace how the members of the Study Group, who are unanimous in considering the interactions between facts (a), (b) and (c) to be as important as the factors themselves, endeavoured to co-ordinate their viewpoints and to describe these interactions.

---

<sup>1</sup> See Inhelder and Piaget, De la logique de l'enfant à la logique de l'adolescent. Presses universitaires de France, 1955.



2. Search for a common language

In order to describe development "synthetically" and, above all, to make some progress, even very modest, in the explanation of these general mechanisms, it is essential to have a common language. Indeed, without a common language we shall never succeed in analysing the actual interactions between the factors and will always return, despite ourselves, to a description by juxtaposition (or accumulation) of influences and not by interactions.

Let us imagine, for example, that some poor child has been studied by each of us for a month or a year and that we then meet to co-ordinate our results. We would know its brain rhythms, rates of physical growth, family conflicts, relations with its social environment, its reactions to problems of intelligence, its reactions to the 25 perceptive laboratory tests which my co-workers have already studied in children, the extent of its vocabulary, its drawings etc. etc. However, and this is the tragedy of present studies on development, we would be incapable without a common language of achieving anything other than an enormous dossier consisting of a series of small mosaic-like chapters, and a concluding essay on the "personality" of the child (with photographs) which would link together with varying degrees of imagination a few facts taken from each of the preceding chapters. We would naturally make films and sound recordings to show how "alive" all this is, but we would nevertheless continue, in the absence of a common language, each to tell his own separate, little story in another language, without making a real synthesis.

Of course, we have often worked like this during the meetings of the Study Group, but we also did something else and your unfortunate colleague given the task of making this synthesis had the great pleasure to find, on re-reading our reports, that very often we also made an effort to translate from one viewpoint into another and that at certain particularly decisive moments we even glimpsed what might be our common language, or the new language of the future ....

I shall start with an example. During the last meeting, Erikson gave us a remarkable table of the elementary affective stages, going beyond a narrow Freudian framework, and endeavouring to characterize general forms of behaviour by bipolar



links such as "giving-getting", "autonomy-shame and doubt", "initiative-guilt", etc. However, it is clear that such a table, although it may be immediately usable by all those among us who have specialized in the affective development of the child, or of its personality in general, represents only a collection of problems without any solution at present, to those who have limited their field of study to questions of intelligence or thought. Each of the criteria employed by Erikson could, naturally, also be applied to the field of learning and the structuration of knowledge. But instead of remaining well-defined, as in the field inspired by Erikson, they run the risk of becoming more and more vague the more general they become. Consequently, what we require is not a mere extension, with the risk of increasing inexactitude, but a translation into a common language.

While Erikson was speaking, however, Grey Walter was looking for such a translation, of which he gave shortly afterwards a series of examples. Speaking from the viewpoint of "statistical neurophysiology" he endeavoured to re-interpret Erikson's stages in the context of information theory and, even if we do not accept this parallelism in detail, we cannot help recognizing the fact that he made use of a much more general language, enabling more precise comparisons to be drawn between the various aspects of behaviour and in particular between its affective and cognitive aspects.

For example, the stage of "giving-getting" with, as poles, "trust" and "mistrust", would correspond to an initial insufficiency of information, such that the elementary exchanges "giving-getting" are accompanied from the viewpoint of the "baby-computer" by a degree of approximation large enough to make the system less precise and consequently more "trusting". Similarly, a certain type of learning would correspond to stage II and a certain manner of choosing between unconscious "hypotheses" or possible directions of action would correspond to stage III, etc.

However, it should be repeated that the details of such parallels thought out on the spot by Grey Walter are of little importance. Their great significance is to show that one of us who works continually with mechano-physiological models



in mind was able to give, in terms of probability of information, an immediate translation rendering the stages of affective development still clearer for those of us concerned with intelligence or with learning.

In fact, this probabilistic language is clearly the common language that we are looking for, provided that the information and communication schemata are supplemented by those which were very soon linked with them, by introducing the concept of "strategy" and the different models of the theory of games. It then becomes clear that, in this broadened form, the probabilistic language may be suitable for all of us. In the first place, its generality makes it possible to establish fairly direct correspondence between the mechano-physiological models and the various forms of behaviour observed in the psychology of the cognitive functions. In the second place, it is not restricted to describing the information as such, under its cognitive aspect, but, by introducing the concepts of gains and losses it provides a means of analysing the "economics" of forms of behaviour and it is doubtless this "economics" of forms of behaviour which constitutes the most natural transition between their affective aspect (which can always be translated in terms of enrichment and impoverishment) and their cognitive aspect. In the third place, it enables isomorphisms to be found between models of intra-individual communications and inter-individual or social models, which makes it possible to overcome the over-simplified and coarse antithesis of the individual and the social, which is as much a drawback for the theory of development as that of the innate and the acquired (you all know how many discussions have been rendered sterile by antitheses of this nature).

### 3. Recognition of a fourth factor (d) of development

- *Equilibrium*

As soon as we adopt this broader viewpoint, as imposed on us by the search for a common language, we perceive that there exists a fourth factor, more general than the three classic factors of innateness, physical experience and social environment, and obeying its own special laws of probability and the minimum: this is the factor of equilibrium which is found associated with each of the three preceding ones,



but which governs particularly their interactions and which, moreover, reveals itself frequently in an independent manner.

To give an idea of what such independence may signify, let us take an entirely theoretical example but which has the advantage of posing the problem in one of its most general biological forms. Let us suppose that in the course of development certain sectors of the organism can be considered as a closed system and are found to obey the second law of thermo-dynamics. In this case the constant increase in entropy, tending towards that state of equilibrium which is maximum entropy, would constitute neither an innate mechanism nor an acquisition in terms of environment, but the result of a purely probabilistic mechanism. Let us suppose, on the contrary (like Lord Kelvin, Helmholtz, Ch. Eug. Guye, etc.) that physical development is outside the second law: in this case the state of equilibrium towards which growth tends would be characterized by a system of regulations controlling chance<sup>1</sup> and whose overall form would constitute a factor which it is much more useful to know for the development theory than the details of various hereditary, acquired or social factors.

To return to concrete problems which are apparently completely different in each of our many fields of investigation, it is very striking to observe how the equilibrium problem constantly returns, either explicitly or implicitly in each field which we are studying.

To begin with social factors, even if we accept the astonishing plasticity which Margaret Mead attributes to mental characteristics under the influence of various communities, nevertheless society is not the source of the nervous system, and consequently the many more or less stable reactions which we observe in the different communities constitute a more or less complex group of forms of equilibrium between the psycho-physiological aptitudes of the individual and the actions of the environment. Thus it is not by chance that in the book entitled

---

<sup>1</sup> This would not bring us back to Maxwell's demon because, as Szilard (1929) showed, Maxwell forgot to include entropy and cost on the same balance-sheet.



"Family, Socialization and interaction Processes", Talcott Parsons and R. F. Bales particularly stress states of equilibrium and of disequilibrium, as well as the optimum balance of entries and exits and the double equilibrium peculiar to the internal system of the personality and the system of social exchanges (see in particular a formalized diagram of these equilibrium systems in Appendix B, by Morris Zelditch, jr.).

In the field of affective development it would be particularly interesting to translate into the language of equilibrium, social and dynamic psycho-analysis as understood by Bowlby or as explained to us by Erikson. It is clear, for example, that the Oedipus stage represents a certain form of affective equilibrium, characterized by a maximization of the "gains" expected from the mother and by a minimization of the "losses" expected from the father. In this connexion it would be of interest to examine whether the equilibrium point corresponds merely to a Bayes strategy, the criterion of which would be a simple maximum of "gain minus loss" or whether it corresponds to a minimax strategy, with a search for the minimum of the maximum loss which the subject supposes a hostile environment is trying to inflict on him. It is evident that such a problem cannot be treated in general and that it depends for each child on the overall environmental conditions. Besides these problems of synchronous equilibrium raised by each of the essential phases of affective development, there remains of course the essential problem of the equilibrium between the previous affective schemata of the subject and the exigencies of the present position.

From the mechano-physiological viewpoint, the part played by the concept of equilibrium, above all of progressive equilibrium, is particularly important by reason of the perspectives it opens up not only as regards the process of solution of problems and what Ashby calls the "finalized mechanisms", but also, it seems to me, as regards the general lines of development of the cognitive functions. An apparatus such as Ashby's homeostat, which solves problems by a succession of approximations based on a series of feedbacks, shows in the most decisive manner the part played by the concepts of disequilibrium and of progressive equilibration. As long as there is disequilibrium, i.e. while the problem still remains unsolved,



a new negative feedback is set off, whereas the attainment of the correct solution is marked by the production of a state of equilibrium. Furthermore, successive approximations to the solution correspond to a progressive equilibration, in accordance with a series of steps which can be interpreted as corresponding to phases in the process of solution, taking the viewpoint of "actual genesis" ("genèse actuelle"), or even of stages, taking the genetic viewpoint proper.

It is in fact very suggestive to compare this equilibration mechanism of Ashby's homeostat with the processes of solution of a conservation problem in the child, since this last class of facts is of such a nature as to show the fundamental part played by the concept of equilibrium, not only in the mechanism of solution of problems but also in the development of cognitive functions in general.

From the first of these two viewpoints, if we study a child aged 7 or 8 years who begins by denying that there is conservation of matter when a ball of clay is moulded into a sausage and then discovers during the actual experiment the need for <sup>a heavy</sup> such conservation, we can distinguish the following phases in the solution of a problem:

1. During an initial phase, the child perceives perfectly well the lengthening and gradual thinning of the sausage, but he chooses the simplest strategy and reasons only on one of these two properties: he will say, for example, that the sausage in state A contains more modelling clay than the ball because it is "longer" and that the sausage in state B contains still more because it is "still longer", etc.

2. During the second phase, the error is corrected by its very exaggeration (negative feedback): when the sausage has become too long (state C or D) its thinning, which up till then was forced into the background, reappears in the foreground by a kind of backward step or regulation and the child says: "Now there is less clay because it is too thin".

3. During a third phase, there is a sudden change in strategy and an arrival at the equilibrium point: instead of reasoning as heretofore on the properties of the states ("longer" or "thinner") the child begins to reason about the transformation itself: the ball is drawn out, consequently it is lengthened and made thinner at



hm-reversibility  
of naming

WHO/PDC/8  
page 11

the same time, thus one of the two changes compensates for the other, consequently there is conservation.

*peut-être  
not so  
enough*

This small example reveals the development of the cognitive functions as a whole, since equilibration plays the part there of a fourth fundamental factor of evolution. Indeed, the clearest result of our researches on the intelligence of the child is that intelligence in course of formation is oriented in the sense of a progressive reversibility. Thus the act of intelligence consists in grouping or co-ordinating operations: however, operations are actions which are interiorized and have become reversible, like addition which is derived from the action of bringing together and which can be reversed in the form of subtraction. On considering the evolution of operational systems as additive structures, one finds a series of three stages, corresponding in outline to the phases of the "genèse actuelle" or of the solution of problems.

1. During an initial stage which marks the commencement of early childhood, we find only uni-directional or irreversible actions tending towards a material aim or success. This explains the absence of notions of conservation.
2. During a second stage, this initial irreversibility, which characterizes intelligence or thought in course of formation as well as the most general cognitive functions (from perception to habits, associations and memory) is tempered by a more and more complex system of regulations, which constitute a state of semi-reversibility.
3. It is against this background that, in a third stage, develop the operational structures proper, characterized by their strict reversibility. The most direct result of these operational mechanisms is then the formation of concepts of conservation: the invariants or conservations (of geometric or physical properties of objects, or of whole or discontinuous quantities, etc.) always, in fact, appear as the product of a particular form of operational reversibility.

However, this reversibility which may usually be considered as the most specific characteristic of intelligence, is nothing other than an expression of an equilibrium law. Whatever the contributions made by the maturing of nervous



co-ordinations, of physical experience and of social relations to the formation of reversible operational mechanisms, their principal property is to constitute systems which are both mobile and stable, characterized by a minimum of virtual transformations and by their exact compensation (their algebraic sum being zero). These are then essentially forms of equilibrium which make it possible to conceive of the development of intelligence as being directed towards such forms of equilibrium, consisting moreover of general structures (groups, lattices, etc.) which express the sum total of possible transformations and above all the possibility of compensating for them.

To summarize, it is thus found that if, in order to describe the classical factors of development, one adopts a common language consisting of the modern forms of probabilistic language (theory of information, games theory, etc.), then one is forced to recognize the existence of a fourth factor of development, or equilibration factor. Moreover, it is seen that this fourth factor is common to all our respective fields of investigation, since it is found in the social field, the affective field, in the mechano-physiological realm and in that of the cognitive functions.

## II. THE PROBLEM OF THE STAGES OF DEVELOPMENT

Doubtless we are unanimous in believing in the existence of stages of development, apart from Tanner who stresses above all the continuity of physical growth, and Margaret Mead, because of the social factors involved. Strangely enough, however, although we are specialists on development, we have still not faced up jointly to the main problem, that of finding out whether we understand the concept of stage in the same way and whether we can hope some day to establish some relationship between our respective stages! Nevertheless, this two-fold problem was the starting point of our first meeting; thus it would seem essential to revert to it in the final synthesis.

Consequently, the first problem confronting us is that of the actual concept of stages, regarding which our respective positions diverge rather considerably.



Certain schools, for example limit the characterization of the stages to a consideration of "dominant characteristics". Thus, Freud speaks of an oral stage at an age where the child already makes use of his anus and of an anal stage at an age where he still makes use of his mouth. Similarly, Wallon, represented in our Group by Zazzo, speaks of an emotional stage at an age where the infant is already exercising all kinds of sensorimotor functions and of a subsequent sensorimotor stage from which emotions are not absent. Others among us are much more exacting. For example, Inhelder and I, when considering the development of structures and thought, speak of stages only in connexion with the formation of total structures, including as special cases all structures observable during a given stage which integrate with the structures of the preceding stage as necessary sub-structures. In this way the logical operations of the "stage of formal operations" (11-12 and 14-15 years) constitute a total structure whose two complementary aspects are the formation of a "lattice" (combinatory aspect) and the constitution of a "group" of four transformations (double reversibility). However, this general structure covers, on the one hand, all the operational schemata of this stage (combinatory operations, proportions, double systems of reference etc.) and implies as sub-structures, on the other hand, the general structures of the preceding stages (in particular the characteristic "groupements" of the "stage of concrete operations" from 7-8 to 11-12 years: classifications, seriations and correspondences). If we wish to aim at a synthesis in the fundamental problem of stages, we must first agree on the criteria of the stages. We have just indicated two possible criteria, (dominant characteristic or total structure) but there may be many others. However, even if we limit ourselves to these two possible criteria the problems which they raise are immediately visible.

1. If we restrict ourselves to dominant characteristics, then by what objective signs can we recognize a characteristic as really dominant? Can we hope to furnish statistical criteria of frequency, or must we be content with a clinical impression, which runs the risk of being subjective? Does dominance imply a tendency towards integration of the other characteristics under the dominant characteristic (which would bring us close to the concept of "total structures") or shall dominance be defined only in terms of relative importance from the viewpoint of frequency?



2. If the requirement of general structures is imposed, then what would be, on the other hand, the field of application of such structures and, in the case of several distinct fields which one wishes to inter-connect, what language should be used to describe these structures? Inhelder and I have restricted ourselves to the study of intelligence and of thought and in this field - perhaps a privileged one as regards stages - the concept of total structure takes on a precise sense which can be defined in terms of general algebra and symbolic logic. But these concepts already no longer apply to perception and we have not found stages of development of perception, at least in a form as simple and clear as our stages of development of intellectual operations. Can we, then, hope to apply the criteria of total structure to the stages of social development, affective development, or psychomotor development, and, if the answer is "yes", in what language should they be expressed, since affectivity, for example, characterizes the energy component ("énergétique") of behaviour rather than its structure?

It can be seen that this initial problem in the delimitation of stages brings us back to questions very close to that of the part played by equilibrium, which we discussed previously. Indeed, to the extent that objectively certain stages exist (and this is indisputable in certain fields), they cannot be considered as a product of subjective cuts arbitrarily made by the research worker in a rigorously continuous development. If stages exist objectively, they can therefore only consist of successive steps or levels, separated by a phase of transition or crisis and each characterized by a momentary stability. Either the stages do not exist (except in the imagination of the authors) or they consist of levels of equilibrium, and consequently of an equilibration phase. The criteria employed would then reduce, more or less, to criteria of equilibrium: the "total structures" are "equilibrium forms" and the "dominant characteristics" are linked to a certain property of equilibrium, existing at least momentarily. Generally speaking it would thus be again the language of equilibrium which would be the most suitable for reaching co-ordination between our different viewpoints on this problem.

Consequently, it is essential for us to begin by establishing a series of criteria of what we now call stages<sup>1</sup> by proceeding systematically from what could be termed a minimum programme to a maximum one:

<sup>1</sup> See on this subject B. Inhelder, Proceedings of First Meeting, (1953), page 83



1. The minimum programme for establishment of stages is certainly the recognition of a certain chronology, in the sense of a constant order of succession. The average age for the appearance of a stage may vary greatly from one physical or social environment to another: for example, if the young inhabitants of New Guinea, studied by Margaret, manage to grasp, like those of Geneva, certain structures of Euclidian geometry, this may occur much later or much earlier. This is of little importance, but one could not speak of stage in this connexion, unless in all environments the Euclidian structures were established after and not before the topological structures considered as primitive.

2. A further step is taken in establishing a programme of stages when one succeeds in finding the equivalent of an integration in the transition from a lower stage to a higher one. As regards intellectual operations it is clear, for example, that the initial sensorimotor structures are integrated into the structures of concrete operations and the latter into formal structures. But can one say as much of the classic Freudian stages, and is it possible to agree that the elements of oral and anal stages are integrated at the level of the Oedipus stage? The great merit of Erikson's stages is precisely that he attempted, by situating the Freudian mechanisms within more general types of conduct (walking, exploring, etc.), to postulate continual integration of previous acquisitions at subsequent levels, and it seems to us that Bowlby, while laying greater stress on the essential reality of conflicts, both internal and external, is nevertheless in search of an ideal not very far from such integration.

3. The integration of the elements of a stage n into the achievements of stage n + 1 gives rise to the supposition that if the stage n + 1 is really new with respect to stage n, then in any stage n it should be possible to distinguish an aspect of achievement with respect to the stages going before and also an aspect of preparation with respect to the stages coming after, it being naturally possible for both to be promoted or hampered by favourable or unfavourable external situations (hence the possibility of crises as natural transitions between one stage and the next).



4. A further advance in the maximum programme of a theory of stages may then be made, insofar as it is justifiable to subordinate all the preparations leading to a stage and all the achievements which characterize this stage, to the existence of a general (or total) structure in the sense defined above.

5. However, as the concept of structure is perhaps peculiar to certain aspects of development (cognitive functions) and as the corresponding affective aspect is ascribable more to an energy principle ("énergétique") than a structure (the term of affective structure being understood in general in a metaphorical sense, except when it comprises in a single whole the affective energetic aspect and the cognitive structural aspect of behaviour), the most general and the most elaborate programme for a theory of stages doubtless consists in representing the said stages in the form of a series of equilibrium levels, the fields of which would be always more and more extensive and the mobility always greater, but whose increasing stability would depend precisely on the degree of integration and of structuration which has just been discussed.

The first problem in our synthesis of stages would consequently consist in deciding whether we accept such a programme and if not, why not, and in the event of our accepting it, which of the five aims of this programme we believe it possible to reach at present in the different disciplines which we represent. It would already be a considerable advance in the study of development if we could agree on the actual concept of stages, whereas, in actual fact, almost all authors interpret this quite differently.<sup>1</sup>

Next however comes the second question, which is much more serious: to what extent can we establish co-ordinations, not only between our concepts of stages, but also between our stages themselves? This leads to a still more fundamental problem: do general stages exist, i.e. including at the same time, for a given level, the totality of the organic, mental and social aspects of development? As regards this general correspondence between the stages, I would like to submit to the Study Group the following hypotheses, which seem to be the most cautious

---

<sup>1</sup> Examination of this problem by the third meeting of the Association de Psychologie scientifique de langue française, at Geneva, in 1955, already revealed all its inherent difficulties.



expression of the degree of synthesis which we may hope to attain and which should be discussed in our final meeting.

1. There are no general stages. Just as, in connexion with physical growth, Tanner showed us that there was an absence of direct relationship between the skeletal age, the dental age, the endocrine age, etc., similarly, in the various neurological, mental and social fields, we see an intermingling of processes of development which are evidently interrelated, but to different extents or according to multiple temporal rhythms, there being no reason why these processes should constitute a unique structural whole at each level.

2. The unity of the "personality" is a functional unity, i.e. a unity which is in search of itself and builds itself step by step, but for which it has never been possible to give an adequate and verifiable structural expression; consequently we cannot take such a concept, which conceals an indefinite number of interfering biological, mental and social factors, as a starting point for postulating the existence of general stages.

3. On the other hand, to the extent that the unity of the body and the personality is built up by successive equilibrium levels and through an indefinite series of disequilibria and re-equilibria, it is possible, by following by partial correspondence, or simply in pairs, the various genetic series which each of us is studying in our general interest, to establish a group of special convergences all the more instructive in that they will be better limited and further advanced in the detailed analysis.

4. It is not only in the correspondence of the stages, or perhaps not even in such correspondence, that one can hope to find the convergences sought for, but perhaps above all in the mechanism of the transition from one stage to the following, i.e. in certain characteristic processes of the actual mechanism of development.

*Is any st or study  
far enough ahead to  
keep pace with P.D.  
his cognitive stages*



### III. THE PROBLEM OF THE TRANSITION FROM ONE STAGE TO THE FOLLOWING OR OF THE MECHANISM OF DEVELOPMENT

In this part we shall proceed by means of the discussion of a few examples:

#### (1) The construction of the sensorimotor object and "objectal" relations

As we showed long ago,<sup>1</sup> the infant begins by not believing in the permanence of objects when they leave his field of perception: he does not look for a toy which he was going to grasp when it is covered with a cloth; when, at feeding time, I hide his feeding bottle behind my arm a few centimetres from his hand, he screams (even at seven months) instead of trying to grasp it<sup>2</sup> (whereas he immediately grasps it if an end of the object still remains visible) etc. Towards the end of the first year, on the other hand, he looks for objects which have disappeared and finds them in accordance with their successive displacements.

In the field of psycho-analysis (with which our researches have had no direct connexion) Freud described, moreover, how the infant, at first interested only in his bodily functions (oral, anal stages, etc.), ends by objectivizing his affectivity on persons, and Spitz has devoted a recent series of studies to these "objectal" affective relations.

In such cases it can be seen straight away how we are faced with the question of the correspondence of stages. At almost the same ages and in both fields one witnesses a parallel transition from an initial state of centration on the subject's own activities (reality is reduced to a dependence on perceptual pictures as related to the momentary actions of the subject) to a final state of decentration where the subject, who has become conscious of his subjectivity, (affective ego or consciousness of the cognitive self) "places himself" with respect to a world of external objects and persons. The problems are, then, to know whether one will come across the same intermediate stages at the same ages, or whether the affective stages precede the cognitive stages, or vice versa, etc. On this will

---

<sup>1</sup> J. Piaget. La construction du réel chez l'enfant, (The Child's Construction of Reality). Chapter I.

<sup>2</sup> Or instead of trying to see it behind my arm, by bending slightly forward.



be based hypotheses on the interaction, or rather the indissociable complementarity, of the affective and the cognitive, or on the primitive and causal character of one of these two factors with respect to the other (for our part, we believe in the indissociable complementarity of the cognitive structuration and the affective energy principle ("énergétique")), but this opinion is of little consequence).

A still more interesting problem is that of the mechanism of the transition from one stage to another (even if the two series of stages do not exactly correspond) for this is a fine example of those general problems of strategy and equilibrium which were discussed in Part I.

(A) From the cognitive viewpoint the problem is to explain by what strategy a baby, who begins by reducing everything to itself, and by not comprehending the existence of changes outside its own actions, eventually succeeds in objectivizing these changes and attributing them to causal sources independent of itself. However, it is clear that this discovery is not innate since it results from a long construction. Neither is it due to the teaching of experience alone, since experience itself, which cannot even contradict a radical solipsism, is also inadequate to correct this kind of egocentric perspective linked to the absence of consciousness of the self which characterizes the initial actions of the baby. In other terms, this discovery is due to a decentration or inversion of the sense of cognitive perspective, i.e. to a new structuration or to a general equilibration of the spatial, temporal and causal relationships involved, and not to an acquisition based solely on experience. However, this equilibration is produced fairly clearly by a combined mechanism of least action and sequential probability:

(a) In the first place, the strategies of the infant may be classified in accordance with an order which is simultaneously chronological and of increasing complexity: the infant localizes the object only in its perceptual field without paying attention to its movements nor to its localization at the moment it disappears perceptually; the infant begins to look for it after its disappearance, but in relation solely to previously successful actions (consisting in finding it in a given place) and not in relation to the successive displacements (although visible) of the object itself; the infant looks for it in relation to visible and successive displacements, co-ordinating the latter according to a group structure, etc. (The next step consists of an extension to certain invisible displacements.)



(b) It is next found that these strategies are more and more costly to construct but give more and more remunerative results: to be concerned only with perceptual localizations is very simple, consequently costs little but is profitless, since the object which had disappeared becomes non-existent and is in any case no longer to be located; to localize in relation to previous successful actions is already more costly, since this calls for more complex co-ordinations, but the result is a little more fruitful, although not always; to localize in relation to a "group of displacements" is much more costly, since this consists in adding to the direct perceptual experience new relationships of a temporal nature, spatial references etc., but the result is much more remunerative, since it makes it possible to foresee the successive positions of moving objects according to a system of localizations and reversible displacements.

X | (c) Consequently, equilibrium is attained when reality is understood in relation to a system of minimum changes (a system of displacements instead of the initial system of continual creations and annihilations) but supplying the maximum information (on utilizable objective relationships).

(d) From the viewpoint of strategies, equilibrium is consequently attained with the strategy furnishing the maximum of "gains minus losses", which was the most costly to construct but which has become the simplest to apply (= minimum losses) and which gives the maximum results.

(e) From the viewpoint of probabilities, one may consider each new strategy as being the most probable, once the results of the preceding one have been obtained (and once the inadequacy and indeterminateness of the information to which it led has been observed). The final equilibrium is consequently not the most probable product a priori (at the outset), but is the end result of a series of re-organizations each of which is the most probable one after observation of the failure of the preceding ones (a series of feed-backs finally culminating in stable equilibrium). *Also: one strategy may be the most probable, others will in practice often be adopted.*

X | (B) From the affective viewpoint we believe, despite the surprising and paradoxical nature of this opinion, that the procedure followed is not very different! In brief, it is not entirely unreasonable to believe that the fact of



maintaining "objectal" relations with the persons about him is, for the infant, a much more costly strategy, although much more remunerative in affective values of all kinds, than to be content with merely giving play to his sucking reflexes or even his sphincters. What is required is once more to analyse, in terms of minimum and of sequential probability, the successive strategies leading from one of these extremes to the other and here we hope that Bowlby will assist us.

We should note, however, that an explanation in terms of profits and losses of the phases of affective development does not signify that the subject (the infant!) has himself made a calculation of his interests in each situation lived through. This calculation is made solely in the sense that the fact of experiencing positive or negative values (an affect consists essentially in attaching value to a given action) amounts precisely to enriching or impoverishing oneself, from the viewpoint of functional exchanges with surrounding persons. P. Janet has already reduced the elementary feelings (joy and sadness, effort and fatigue etc.) to the rules of an internal economy of action. This point can be extended to form a theory of values considered as the external economy of the action, and to deduce a theory of affective equilibration therefrom. From this point of view, the achievement of objectal relationships as enrichment, but also as a more costly strategy, probably marks the arrival at a certain level of equilibrium, which is, moreover, at the same time a point of departure for numerous and profound disequilibria.

2. The problem of affective schemata (in particular of the super-ego) and of representative and operational schemata — Bowlby

A series of convergences between the work of Bowlby and that of the Institute of Geneva seemed possible during our last meeting and I should like to indicate in a few words how the second example could be developed.

Despite the absence of all direct relationship between our work and psychoanalysis, there exists nevertheless a certain similarity in the methods of posing our problems which might be adopted even by anti-psychoanalysts. This common point of departure consists in accepting that all feelings and knowledge have a history and in considering consequently that any external influence whatsoever



intervening at a given moment of development can never represent an entirely new commencement, but is always assimilated to all that has gone before, and similarly may modify the subsequent course of the history by giving it an impulse in a partly new direction. The problem is then to understand how this assimilation takes place and, above all, the organization of the old and new factors up to the point of the existing adaptation.

To this problem, Freud replied that, in the affective field, we retain in our subconscious all our experiences of the past, particularly infantile or former conflicts and that our subsequent affective life always consists, to some extent, in the identification of new situations with previous ones, by a kind of fixation on initial imagos and complexes.

The history of the individual development of knowledge, on the other hand, gives a rather different picture, for in general it is not the memories as such of things we know which are retained (ready-made representations) but rather schemata of actions or operations derived one from the other but with a constant adaptation to the present and with a structuration which is continuous (or in steps), orientated in the direction of equilibration.

However, during a very instructive discussion on the search for common mechanisms of development, Bowlby seemed to admit, if I am not mistaken, that the history of affective schemata and of conflicts was not so far distant from such continuous structuration, in the sense that there is dynamic assimilation rather than strict identification, and in the sense that such assimilation proceeds by analogies and transfers, in the course of a perpetual reconstruction based on the past, but without any exclusive fixation, adapting it to the present in various conflictual or equilibrated ways.

The problem of choosing between these two interpretations arises notably in regard to the super-ego, which may be conceived either as a simple fixation on past imagos and imperatives or as a group of schemata of affective reactions presenting the same factor of continuity in the presence of each new situation but with a progressive flexibility of accommodation in regard to the data of the present.



However, it is clear that such a problem is at the root of the question of convergences or of divergences between the affective and cognitive forms of equilibration. Of course, one must be very careful in using a term which is too convenient and so often merely verbal or literary, such as "equilibrium", and the strict rule should be followed of using it only in situations where one is in possession of objective criteria such as the indices of minimum (including the minimax) or of fairly convincing probability schemata. But despite these reservations, it seems impossible to succeed in expressing affective conflict situations in this way, as well as, in particular, the various modes of solution of conflicts, without finally adopting a somewhat precise terminology employing the language of equilibrium. Even in a conflictual situation where one is losing all round, as in the case of an individual whose super-ego prevents all adaptation and whose liberation from his super-ego would represent, moreover, a definite privation, one might still ask whether such an individual would not finish by choosing, out of all these possible losses, the solution consisting in minimising the maximum loss inflicted on him by his history and environment; however, this would constitute point of equilibrium according to the minimax strategy!

Consequently we feel that a discussion on these basic problems would lead to appreciable progress in our projects of synthesis in the interpretation of development, according as to whether there is a possible convergence or a necessary divergence between the processes of the history of affects and those of the history of intellectual operations and representations.

3. The forms of social interaction and the development of the child — Mead

Let us agree to distinguish in social life between molar phenomena (general form of society and transmission of community culture from one generation to the next) and molecular phenomena (interaction between individuals on various levels). It is evident that there are all kinds of transitions between the molar and the molecular and that the general molar forms influence to a high degree the molecular interactions. However, this very fact makes it all the more interesting to consider whether among the modes of molecular interactions there exist certain tendencies



towards equilibrium and, if so, what their relationship to mental development may be.

\* In European societies such as that in which we live, it is, for example, very interesting to follow up step by step the spontaneous forms of collaboration between children in well-defined situations, such as a constructional game, which can remain individual with various imitations, or become collective to different degrees. Mlle Inhelder, who has recently made such studies, with the collaboration of G. Waelting, will be able to give us more details; for the time being I will simply point out the fact that there is a remarkable convergence between the stages of this social collaboration and that of the formation of intellectual operations, to such a point that one has the impression that there are here two complementary and inseparable aspects of the same process of equilibration.

To cite only one example of such complementaries, the social relation of reciprocity, which gradually imposes itself as a form of equilibrium between individuals considering themselves as equals, assuredly corresponds to the logical and operational transformation of reciprocity which dominates the logic of symmetrical relationships ( $a = b$  therefore  $b = a$ ) and certain equations in the logic of propositions ( $p \supset q = \bar{q} \supset \bar{p}$  and  $\bar{p} \supset \bar{q} = q \supset p$ ). However, genetically speaking the progressive organization of inter-individual reciprocities and that of operational reciprocities in the field of thought certainly constitute two correlative phenomena, without mentioning the moral reciprocity which is of importance in the organization of normative values and which is, in the opinion of all authors, simultaneously social and relative to "practical reason".

This having been made clear, a clearly circumscribed problem of possible synthesis between the researches of cultural anthropology and those of child psychology would consist in determining up to what point a molecular tendency to reciprocity is found, considered as a most probable form of equilibrium between equal individuals, ensuring the maximum of performance compatible with the minimum of change, and this whatever the molar form of the situations in question. Margaret Mead has described social situations in which a baby sucks anything except



his thumb, others in which he does not smile because he passes his existence on his mother's back, without seeing her face, etc., but are there societies without any reciprocity? Sociologists have described primitive forms of exchange, finding them in the gift, but in gifts or presents which sooner or later give rise to reciprocal reactions, and in this respect we have been given an analysis of all forms of the "pot latch".<sup>1</sup>

These are institutional reciprocities which have become molar. What are the transitions between these molar reciprocities and the many possible forms of molecular reciprocity?

4. Perceptual activities, intellectual operations and the EEG

*Walker*

The role of your rapporteur in drawing up this draft for discussions directed towards the synthesis of our results is certainly not to appear to have an extensive competence in all our fields of study, but on the contrary to provoke the reactions of the competent specialists by simply imagining problems of general interest capable of linking together the fields which he knows and those with which he is not familiar. In other words, part III of this report, devoted to problems of mechanism such as that of development, brings us quite naturally to part IV, devoted merely to a listing of the problems to be discussed, as the author leaves further and further behind his own field and approaches what for him appear merely to be "promised lands".

This is the spirit in which I would like to conclude part III, by describing with some degree of naivety what I would expect from a theory of the change in the EEG with age if I wanted it to link up with the problems of equilibrium which seem to me most general and the most "synthetic" as regards the mechanism of development.

On considering in their most general form the equilibrium of forms of behaviour (it being understood that the affective factors corresponding to the energy principle of such forms of behaviour and the cognitive factors corresponding to their structure are always complementary and indissociable) I would say that such forms of equilibrium comprise three types of characteristic or three dimensions:

---

<sup>1</sup> Marcel, Mann, Davy, etc.



1. A progressive extension of the field of equilibrium, i.e. of the objects as a whole to which the forms of behaviour apply. This extension of the field may be expressed in terms of the "probability of encounter" and we have endeavoured to give a very simple mathematical model of this probabilistic mechanism in the case of perceptual centrations and of the explanation of the elementary perceptual illusions, but the process of "probabilities of encounters" may be generalized on all levels, on the understanding that "encounters" on the higher levels may themselves be a function of a continually more complex schematism dependent on the mechanisms which follow.

2. An increasing mobility of the equilibrium, since the equilibrium of behaviours is an equilibrium between actions and movement; actions may be described in their most general form as a system of "couplings" between the elements "encountered" and, here again, one can assign a probabilistic form to the couplings in order to take account of their complete or incomplete character with respect to a given field of extension. This form is very simple when the couplings are independent (and one can, for example, interpret in this way the well known Weber's law), but more complex in the case of sequential probabilities, as in successive strategies applied to the solution of one and the same problem (such as the problems of conservation or of construction of the permanent object, examples of which we have given above).

3. An increasing stability of the equilibrium (which does not contradict its mobile character) which will tend to the minimum characteristic and to exact compensation (reversibility) of the virtual transformations involved in the system, these transformations being on a higher level than that of the couplings, and consisting in co-ordinating the said couplings in various ways.

However, if we examine by means of such schema, but in full consciousness of our ignorance, present EEG data relative to the development of the child, we cannot but be struck by certain analogies, which may be superficial or profound. First of all, it is evident that in passing from the slow waves, which are the earliest ones, to the rapid waves, which do not become general until 10 or 11 years of age (at the commencement of the level which we characterise by "formal operations")



we see an advance in mobility; it is however correlative and not contradictory to an advance towards stability and regularity. On the other hand, above all if we agree with Grey Walter that the  $\alpha$  rhythm is the manifestation of an exploratory or scanning activity, corresponding to the principle of what we call "encounters" and which gives rise to "couplings", it is very striking to follow the progressive extension of the field of this activity, from the visual occipital regions in the direction of the temporal and even frontal regions. If one first considers the level of visual perception, we have been able to establish on the basis of the probabilistic schema of encounters and couplings, on over-estimations (and consequently the correlated under-estimations), that the lengths perceived are a function of the centration of vision, particularly under the action of perceptual attention. It is suggestive to compare these facts with the results observed by Grey Walter on the manner in which a perceptual excitation extends further and further into the brain and very quickly goes beyond the frontiers of the visual cortex to reach the motor regions and beyond; this irradiation may thus be related to dilatations due to centration. It is very interesting, on the other hand, to note that if, in behaviour, the perceptive couplings are gradually supplemented by representative couplings, bringing about between 11-12 years of age a combinatory system proper (commencement of formal operations), we see a correlative extension of the  $\alpha$  rhythms to regions of the brain which are increasingly more extensive and closer to the paths of association themselves.

In short, even if these attempts to establish two corresponding series of stages for intellectual operations and for the EEG's have so far failed<sup>1</sup> (which may be due, moreover, to the inadequacy of the means of detection used for the latter) nevertheless one may hope to establish more general convergences between the dimensions of the organization of the rhythms and those of the equilibration by successive steps of cognitive links, ranging from perceptual couplings to the combination characterizing formal operations. It would moreover be surprising if it were otherwise, since although the EEG's do not of course represent the expression of operations proper, they seem to translate, on the other hand, attitudes arising

---

<sup>1</sup> My attempt (first meeting) together with Monnier and Inhelder to find such a correspondence will be recalled.



in the cerebral activities of the subject, attitudes proceeding first from mere watchfulness to active exploration, but which can become differentiated in the field of such exploration into a multiplicity of forms as varied as those seem on observing behaviour. Indeed, if it is legitimate to bring together all types of exploration into a single general schema of encounters, couplings and transformations (or coupling raised to the second power), despite the considerable distance between the simple perceptual regulations of higher operations, then one can count on refinements in the analyses of the multiple varieties of alpha waves leading sooner or later to a picture corresponding in broad outline to that of the different types of behaviour.

#### IV. QUESTIONS TO MY COLLEAGUES IN THE STUDY GROUP

The preceding pages (I, II, III) represent an attempt to propound, in a common language, a certain number of common problems, discussion of which could serve as a synthesis for our work on development. Of course, each of us remains free to declare that the problems are ill-chosen and that their discussion will lead to nothing, but showing why they are ill-chosen or will lead to nothing would itself bring about a synthesis.

However as, like a good paterfamilias watching over the liberty and independence of his children, our Chairman appears to fear directed discussions, it may be useful in concluding this report to put a few questions personally to each member of the group in the hope of a systematic reply. I shall not put such questions to Inhelder nor to Zazzo, who are too close to the way of thinking embodied in this report, and I shall consider Melin, Monnier and Remond as sharing in the questions put to Grey Walter (while hoping that Melin will give us details on the stages of the EEG in the child after the systematic study he has made of this subject in view of our final meeting).

##### I. Questions to Lorenz

1. Static apriorism is only an over-simple preformism, which Lorenz rightly rejects and replaces by a "dynamic apriorism". However, is there not a risk that



the latter will bring us back indirectly to vitalism, i.e. to the convenient theory that "life" can always arrange everything? Is not the only way of escaping from vitalism, if one is not an empiricist, to have recourse to probabilistic equilibration processes?

2. Lorenz has shown us (I.216) various compromises between the IRM and learning and above all he has shown us (II.246) that a characteristic may be innate in the case of one species and based on an individual apprenticeship in the case of another. Does not this show that the appearance of this characteristic is made necessary as part of a certain functional process of equilibration which is more general than the innate or acquired? This appears to me to be the case, for example, in behaviour entailing search for an object which has disappeared, behaviour which is acquired in the baby (see above III.1,A) but is innate in many animals, (digging instinct, etc.)

3. "Logical necessity does not exist per se but corresponds to laws of the nervous system" (II.246). I agree if these are laws of equilibration (such as the law of "all or nothing" in which has been seen the starting point of binary arithmetic (one or zero), which is isomorphous with Boolean algebra, consequently with logic) otherwise we fall back into preformist apriorism.

X 4. Are there objective criteria for distinction between compromise solutions and more stable equilibrations in cases of conflicting tendencies (I.217)?

## II. Bowlby

1. Are there at present any psychoanalytical attempts to explain the transition from one stage to the following? Is Bowlby himself interested in the solution of this problem? Would he agree with the hypothesis that the reactions pertaining to a stage n are set off by dissatisfactions, conflicts or disequilibria pertaining to the preceding stage n-1, which hypothesis would favour the interpretation of such transitions on the basis of an equilibration process?

2. What in particular is his attitude to the "latency" stage? Can one interpret it in terms of equilibrium or is it only the manifestation of a maturation phase?

anxiety



3. Does Bowlby agree that all behaviour is always simultaneously affective and cognitive, in accordance with two inseparable but distinct aspects, one of which constitutes the energy component ("énergétique") of this behaviour (affective aspect) and the other the structure of the same behaviour (= cognitive aspect)? Does he agree in concluding therefrom that an affect is never the cause of a cognition, nor the reverse, since both are built up together in an indissociable manner (for example, the cognitive "permanent object" and the affective "objectal relation")?<sup>1</sup>

Odier, who dealt at some length with this problem, accepts in all cases the priority and the causal action on the cognitive of the affective, which seems to me to complicate matters without yielding any advantage. On the other hand, no one supports the view that the cognitive is the cause of the affective.

### III. Margaret Mead

1. Everything varies from one society to another, in particular systems of numeration and the circumstances under which one learns to count. But why is it generally accepted that  $1 + 1 = 2$  or  $2 + 2 = 4$ ? This is not innate. It is not learned from experience, since two objects are not equivalent to "two" unless they are counted (= activity of the subject). Is it social as thought by Durkheim? But he was then obliged to suggest that "under all civilizations lies the civilization", consequently a certain common functioning which seems to me characteristic of the laws of equilibrium (but applies equally well to the operations of the individual and to operations between individuals). Does Margaret Mead accept the possibility of arriving, thanks to the mechanisms of equilibration, at such common elements despite the diversity of the cultural points of departure?

2. When an individual is transplanted from one civilization to another or subjected to a new training, can any convergence be perceived between the order of acquisitions during this kind of "genèse actuelle" or late genesis, and the genetic order?

<sup>1</sup> One could say, on the other hand, that relationship with persons precedes and modifies relationship with things, or vice versa. However, this is a quite different problem since relationship with persons is always at the same time cognitive and affective, like relationship with things; the only difference is that it can be more interesting and give rise to earlier cognitions.



order observed in the development of the child as studied among us? Example: the acquisition of the operations of measurement?

#### IV. Grey Walter

1. Grey distinguished between (II.7) six possible forms of psychobiological development: (1) evolution (mutation, etc.), (2) tropisms, etc., (3) instincts (IRM's, etc.), (4) learning by repetition, (5) learning by association, (6) social communication. I feel that the construction of logical relationships by the child aged 7-8 years (for example, the previously unrecognized relationship:  $A = B$ ,  $B = C$ , therefore  $A = C$ , which presupposes the retention of A, B and C during the process of reasoning, etc. etc.) does not enter into any of these six forms, and that we must accept a further category (7) learning (individual or social, in the sense of inter-individual) by successive equilibrations. The examples of cognitive equilibration mentioned in this report all come within this category (7) and cannot be explained by categories (1)-(6). I should be glad to have the agreement or objections of Grey Walter on this point, since I was not able to make myself understood in London in this connexion, (II, 39-42) when I stressed the fact that, in this kind of learning, acquisition is not drawn from objects but from co-ordinations between the actions of the subject. Consequently, it was my fault that Grey Walter did not reply precisely to my question (II, 43). However, if one is to hope for convergences between the development of the EEG and the evolution of intellectual operations, it is fundamental to know whether such learning by equilibration (of which the finest mechano-physiological model is Ashby's homeostat) reduces to the six forms of Grey Walter or not.

2. The most important critical age for our stages is, on the average, 7 years. However, Grey Walter has shown (II, 57) that "elaboration" appeared only towards 6-7 years. Could not this "elaboration", of which there is no trace before 3-4 years, which increases from 4 to 6-7 years (taking on a more general and more stable form from 6-7 years of age) be related to the type of structuration to which we have just drawn attention?



3. Generally speaking, is the inadequate correspondence observed so far between the EEG and the cognitive structures thought by Grey Walter to be due to gaps in recording, to the nature of the EEG, or might it not also be due in part to the inadequate theoretical development of possible common mechanisms (I have in mind the development still called for in connexion with a theory of equilibration)?

#### V. Tanner

1. In the absence of stages of growth, Tanner recognizes the existence of "phases of acceleration". Could these be characterized by the causal mechanism of this acceleration, for example, by a more or less regular interaction between the nervous and endocrine mechanisms, or should one restrict oneself to observing this acceleration as such?

This question is indirectly related to the one which seems to me the most important of the general problems: the mechanism of the transition from one stage to another, the mechanism of continuous transformations.

2. What outstanding transformations in the nervous system might it be possible to relate to the levels of 1.1/2 - 2 years (beginning of language and of symbolic function in general), of 7-8 years (beginning of complete operations) and 11-12 years (beginning of formal operations, linked with the functioning of the frontal lobes)?

3. Organic embryogeny (which is continued in physical growth) has often been thought of as directed towards the form of equilibrium which constitutes the adult state of the corresponding species. Could one at the present time say something positive regarding the criteria of this equilibrium (minimum and compensated virtual transformations), its mechanism (regulations) and above all, the mechanism of the successive equilibration phases (other than the final state), which might be compared with analogous problems in other sectors of development?

#### VI. General questions (for all)

Discussion of this attempted synthesis cannot be fruitful unless each of us, in his own field, supplies a few well-defined and well-analysed facts relative to



the passage from one stage to the following (preparation or reaction in the opposite sense, etc.) or to some remarkable continuous transformation occurring in the course of development. It is by comparing such facts that we shall be able to decide whether it is reasonable or whether it is definitely premature to attempt to characterize certain general mechanisms of development. I am almost certain that the equilibration mechanisms explain the development of the logico-mathematical operational structures, because these structures are specifically nothing more than the equilibrium forms peculiar to the intellectual operations, but only a general discussion would show whether we have here a possibility for a general or for a particular explanation.



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/20  
29 October 1956

ORIGINAL: FRENCH

REPLY TO COMMENTS BY MEMBERS OF THE STUDY GROUP  
CONCERNING THE PART PLAYED BY EQUILIBRATION  
IN THE PSYCHOBIOLOGICAL DEVELOPMENT OF THE CHILD

by

Professor J. Piaget

This is perhaps not the place to thank my colleagues and to express my great appreciation of the very varied, as well as really interesting and constructive replies, which they drew up for our last meeting, following my attempt at a synthesis. The discussions which took place at that meeting, in addition to clearly indicating my gratitude, showed above all how much attention I have paid to their comments. However, in view of the fact that these discussions have ended in a measure of agreement much greater than seemed possible at the outset, particularly by clearing up certain semantic misunderstandings, I have been asked to draw up, after the meeting, a brief reply to the documents prepared beforehand, so that it can be included together with the said documents in the first part of the proposed volume.<sup>1</sup>

The aim of the reply is simply to show that if it is granted (contrary to the impression which my report may have given) that the organism is an open and essentially active system then development cannot be explained without having recourse to equilibration processes. In fact, although mental, like physical, life (and even more so) is a perpetual process of construction (and sometimes even of invention), it is by no means incoherent because of this, and what is required is to understand how the mechanism bringing about this continual construction may constitute at the same time a regulating mechanism ensuring coherence, which is really an equilibrium problem. In the field of the cognitive functions in particular, the problem is to understand how the opening of the system in the form of apprenticeship, discovery and creation may not only

---

<sup>1</sup> It is, of course, understood that I alone am responsible for this reply which must remain without an answer.



be reconciled with but take place at the same time as control and checking, in such a way that the new remains in harmony with the acquired, which is once more an equilibrium problem. However, although everyone stresses the activity and renewal aspect of development, the equilibration aspect is only too often forgotten. Above all, it is often not sufficiently realized that these two aspects are inseparable and that the very same agencies which effect the new constructions are also those which simultaneously ensure their regulation. An example of this is afforded by logical operations, under their double aspect of agencies of indefinite construction and coherent reversibility; but although this example is almost unique, as we shall stress, from the viewpoint of degree of perfection in equilibrated adaptation, it constitutes no more than the final term of a long series of regulations of all kinds, which come into play with the most elementary apprenticeships and perceptions, and whose semi-equilibrated mechanisms of retroaction and anticipation prepare this logical reversibility which will characterize the operations. Furthermore, this example illustrates well what is doubtless true in a general way, namely that analysis of regulation, in other words of equilibration, throws a certain light on the mechanism of construction itself (in the case of operations, in fact, every new construction, and consequently every invention, is reversible from the outset and therefore can be equilibrated).

1. The result of stabilization and, in particular, compensation processes, can be designated by the term equilibrium<sup>1</sup> or by that suggested by Bertalanffy, "stable state in an open system". Whatever the vocabulary employed, however, it must be stressed at the outset that such processes always exist in a living being, which amounts to saying that, for it, equilibrium does not represent an occasional or extrinsic characteristic but an intrinsic one, taking into account a certain number of specific functions. Thus, for a pebble, the fact of being in stable, unstable, or metastable equilibrium in no way affects its other properties: thus, its equilibrium is an occasional or added characteristic and the proof thereof is that in order to define a state of stable equilibrium the physicist calls in a system of "virtual work"

---

<sup>1</sup> It should be noted that in French the word "équilibre" has a broader sense than the words "equilibrium" or "balance" in English.



which exists only in his mind and not at all in the pebble itself. On the contrary, a higher vertebrate which cannot stand on its paws would be a pathological case and a homeostatic disorder constitutes a disease. From the mental viewpoint, an adult whose thinking remains unstable as regards definitions, inferences or decisions is considered to be abnormal. In each of these cases, equilibrium under one form or another thus constitutes an intrinsic and not an extrinsic characteristic of the fields considered (naturally this does not signify that we have here a specific property of life, but only that wherever there is life there is also equilibration).

2. In the second place, it must be stressed that the equilibration process which thus constitutes an intrinsic characteristic corresponds, in the case of fields concerning living beings, to specific needs, tendencies or functions and not merely to an automatic balance independent of the activities of the subject. Thus, in the case of the higher cognitive functions, there exists a tendency to equilibrium manifesting a need of coherence. In the case of the elementary cognitive functions (perception) the same holds true, although the forms of equilibrium attained are more fleeting and less stable. In other terms, the force of the tendency is not entirely determined by its results and this is why it is better to speak of equilibration as a process corresponding to a tendency rather than of equilibrium only.

3. To these needs, tendencies or functions correspond special mechanisms or equilibration agencies whose activity is complementary to that of all behaviours aiming at the exploration or modification of the environment during the exchanges between the latter and the organism. Thus, all sensorimotor activity is accompanied by regulation of posture and tonus, etc. In the case of the cognitive functions, one may conceive of the elementary logical operations as constructing new forms or new assemblies within the environment (classifications, seriations, correspondences, etc.); but these activities are necessarily accompanied (necessarily, because this is a condition of their success) by a stabilization of their forms and elements (conservation, etc.). From this viewpoint, it may be said that the inverse and reciprocal operations taking place in this stabilization constitute the equilibrium agencies, it being understood, however, that these mechanisms or agencies are indissociably linked with those effecting the new constructions.



4. In the sense in which we understand this term, equilibrium is therefore essentially bound up with the activities of the organism, not only because equilibration presupposes activities, but also because the stable states or equilibrium forms reached at the end of equilibration processes always represent the play of compensation between activities proper. Stable equilibrium may be defined locally by assuming that if a small perturbation  $\Delta E_p$  is introduced in a state  $E$  by the observer or by nature, the subject reacts by a spontaneous movement of the same order,  $\Delta E_s$ , which returns the system to the state  $E$ , or to a state close to this. It is then said that the reaction  $\Delta E_s$  constitutes an activity.<sup>1</sup>

5. If we prefer the term equilibrium (mobile or dynamic) to that of stable state, it is because the concept of equilibrium implies that of compensation and because the activities of the subject (see 4) are always compensatory at the same time as constructive. This concept is of general importance, since it doubtless concerns the fundamental mechanisms of acquisition or apprenticeship. If these mechanisms are assumed to be based on a simple process of association, the problem then remains of understanding why certain associations are unstable (for example, conditioning considered as merely association remains temporary or unstable) whereas others are stable. The problem can be solved only to the extent that a stabilization factor is introduced, in the form of the satisfaction of a need (which is thus a compensation in the sense that filling a gap is a compensation): in other words, in the event of a stable association between  $x$  and  $y$ ,  $y$  is not only associated (externally) with  $x$ , but assimilated to  $x$  in the sense that  $y$  is merged into the  $x$  schema and fills a momentary gap, (need) relating to this schema.

These considerations confirm what has been said (under 3) regarding the complementary and indissociable nature of equilibration and of acquisition; the concept of assimilation explains more than does that of association precisely in so far as it includes a stabilization factor.

6. The compensatory activities just discussed (5), and which therefore constitute the specific agencies of equilibration (cf. 3), play a considerable part at all levels of behaviour, in the form of retroactive processes necessary for the anticipations involved in construction. In this respect, it may be considered that

---

<sup>1</sup> For more detailed definitions, see the publication on logic and equilibrium which is being prepared by the Centre international d'Epistémologie génétique, Geneva.



the agencies of equilibration correspond in general to all regulatory systems in their dual retroactive and anticipatory aspect. However, these concepts recur continually in all theories explaining behaviour, from the "feed-back" common in the Anglo-Saxon countries to the reafferences and models of action of Soviet psychology. Even in a theory of apprenticeship as associationist as Hatt's model, the retroactions play an essential part.

7. However, even if all this is commonplace, it is not often understood (and the silence of my colleagues in the study group on this point shows that I continue to be not very comprehensible in this connexion) that the higher cognitive operations constitute, in fact, structures similar to those of the regulations, with their characteristics of combined retroaction and anticipation; however, there are two differences, namely, that they attain complete equilibrium and that, thanks to the complete reversibility which characterizes this equilibrium, the operational structures take an algebraic form simpler than the mathematical expression of "feed-back".<sup>1</sup>

8. Thus, reversibility for an operation leading from state A to state B consists in the presence of an inverse operation leading back from state B to state A. Reversibility (in the form of inversion or reciprocity) is thus a special case of retroaction: that in which the retroaction brings about a complete return to state A and not only to a state A', close to A. It may therefore be said that, in the case of operations, operation BA is the same as operation AB, but reversed, (an identity which is indicated by the consideration that when a subject understands an operation he also understands, by this very fact, the possibility of its inverse), whereas, in the case of a regulation, no matter of what kind, the two actions which lead from A to B and from B to A or A' respectively, are different. Apart from this distinctive characteristic, however, operational reversibility is nothing more than retroaction. It may therefore be said that operations represent a direct prolongation of regulations and it may even be considered that, from the viewpoint of equilibrium, the three great structures which dominate mental life and arise in hierarchic order during development are the basic rhythms, the regulations and the operations. These logical structures consequently do not represent an isolated

---

<sup>1</sup> These operational structures take simple forms such as groups, groupings, lattices, etc., while a "feed-back" must be expressed as a complicated integral.



sector of mental life (or a characteristic formed from outside by language, etc.) but the final stage of an edifice all of whose parts are interdependent.

9. However, the value of an equilibration theory is precisely in explaining this completion of the active-cognitive structures (if they can be so termed). Indeed, it is this progressive equilibrium of the compensations ( $\Delta E_s$  in relation to  $\Delta E_p$ ) which takes into account operational reversibility, and not the reverse. If it were necessary to explain equilibrium by reversibility, it would be impossible to understand from whence the latter could arise, whereas one can understand (in outline) how coarse compensations become finer and how, with the aid of symbolic function and representation, these compensations may finally bring about, in thought, exact reversibility. To employ a comparison, which is more than a mere image, it might be said that when a physicist describes the equilibrium of a body, he calls into play systems of "virtual work" which exist in his mind and not in the said body, while in bringing about the equilibrium of his interiorized actions (which are his operations), a living and thinking subject establishes an interplay of compensations between the different components of virtual work, which then play an effective role in his actual thought<sup>1</sup>: this system of virtual work constitutes, in effect, a system of all possible operations for a given structure and it is precisely these possible operations which represent logic.

10. From such a viewpoint, logical structures are the only completely equilibrated structures in the organism (apart from a few similar structures which approach without attaining the same precision, i.e. perceptive constants and certain sensorimotor schemata relative to space and objects). As such, the operational structures constitute a very special case, whose properties cannot be generalized for the whole of mental life, even under its cognitive aspect, but as this special case also represents, at the same time, the final point of a very general process of equilibration and as this process concerns the regulations as a whole (and, beyond them, more basic rhythms), the study of logical structures is very important in order to determine the real significance of equilibrium mechanisms.

---

<sup>1</sup> We might thus define virtual work without calling on concepts of force, etc., but considering merely  $\Delta E$ 's which are imaginable (in the true sense of the word) without being actually carried out.



11. It should be noted further that, although equilibration thus constitutes a developmental factor to be added to the three classic factors of heredity, environment (external or internal), and social education, it is a factor which cannot be dissociated from them. To be more precise, equilibrium is a form (and equilibration a structuration), but this form has a content and this content can only be hereditary or acquired by physical or social apprenticeship. However, as none of these three factors acts alone, it would be useless to try to isolate the equilibration factor; it intervenes in every hereditary or acquired process, and intervenes in their interactions. It is in this sense that it is the most general of the four, but this in no way signifies that it is superimposed on the other three by an additive process.

12. In particular, the equilibrium factor is dominant in exchanges between the organism and the environment (exchanges which thus comprise an interaction between hereditary or genotypic factors and the acquisition factors which give rise to "accommodates" or phenotypes). These exchanges correspond to what is generally termed "adaptation" (and K. Lorenz suggested that I replace the term "equilibrium" by "adaptive interaction"). However, all adaptation, both mental and physical, includes two poles: one corresponding to the assimilation of energy or matter from the environment by the structure of the organism (or mental assimilation of data perceived in the environment to the schemata of action followed by the subject); the other corresponding to the accommodation<sup>1</sup> of structures or schemata of the organism or subject to environmental situations or data. Adaptation is then nothing more than an equilibrium between this assimilation and organic or mental accommodation and this is why the most elementary exchanges between subject and object are already determined by the equilibrium factor.

#### Conclusion

This last remark (12) enables us to conclude by putting the equilibrium factor in its true perspective, which is a biological and not a logical one, although the special equilibrium of logical structures is one of the finest achievements of vital morphogenesis.

---

<sup>1</sup> We use this term in the sense of "accommodates" or phenotypes, i.e. variations undergone by the organism in relation to the environment.



We shall therefore conclude by saying that life, like thought, (or thought, like life, ...) is essentially active because it constructs forms. From this viewpoint, thought forms are a prolongation of vital morphogenesis through the intermediary of nervous co-ordinations, sensorimotor schemata of action, etc., without forgetting social structures, since the operation of reason is always dependent on co-operation. However these forms or structures, whether biological or mental, must constantly comply with the double requirement of assimilation of objects or external data to them and, in return, of accommodation to these objects or data. Without assimilation, the organism or subject would be like soft wax, as in the reproach levelled against empiricism, ceaselessly modified by chance encounters or changes in the environment. Without accommodation, the organism or the subject would be withdrawn within themselves and beyond the reach of any external action. This equilibrium between assimilation and accommodation can only be limited and relatively unstable on the organic level, since the effects of one are attained at the expense of the other: equilibrium is only a compromise at the level of organic morphogenesis or variation of the species. With nervous organization and mental life, on the contrary, a two-fold power of retroaction and anticipation, of reconstitution of the past and the foreseeing of the future, considerably enlarges the field of this equilibrium and replaces fleeting compromises by actual syntheses. Schemata of action already constitute such syntheses, with their power of general assimilation and multiple accommodation. Nevertheless equilibrium is only attained, from the operational and cognitive viewpoint, with logico-mathematical structures capable of assimilating the whole universe to thought, without being ever broken or even shaken by the innumerable accommodations called for by experience. We have studied the background of this cognitive equilibration in the modest sector represented by child development; but, even within this limited field, it is remarkably instructive and becomes much more so once properly situated in its general perspective.



WORLD HEALTH  
ORGANIZATION

STUDY GROUP ON PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Fourth Meeting  
Geneva, 20-26 September 1956

ORGANISATION MONDIALE  
DE LA SANTÉ

WHO/PDC/14  
20 August 1956

ORIGINAL: ENGLISH

SOME CONSIDERATIONS ON PSYCHOBIOLOGICAL DEVELOPMENT

Comments on Professor Bertalanffy's note on the role of  
symbolism in human behaviour

by

John Bowlby

Since reading Bertalanffy's Problems of Life (during which, amongst other things, I first became familiar with Goldschmidt's principle of harmonized reaction velocities) I have been very interested in his biological ideas and look forward to meeting him in Geneva. I only wish I understood his General Systems Theory more adequately so that I could discuss it properly.

In his comments on Professor Piaget's essay he devotes four pages (pp. 12-15 of English Edition) to the role of symbolism in human behaviour and advances some views about mental disorder. It seems from reading this that Bertalanffy differs fairly widely from me on a number of points and to facilitate further discussion I am making some notes for prior circulation.

In paragraph 4 on page 13 I gather Bertalanffy is explicitly contrasting my view that pathogenesis is to be sought in the intra-psychic conflict between incompatible impulses directed towards the same object and his view that it arises "from the conflict of opposing symbolic worlds". Although I would like to know more of Bertalanffy's views, I feel fairly sure that these two views are radically different.

Rephrasing my own position, I would say that a characteristic of much mental illness - character disorder, neurosis, psychosomatic illness and at least some psychosis - is the inability satisfactorily to regulate intra-psychic conflict, that the intra-psychic conflict in question is often that between rather primitive love and hate directed towards one object, and that the disability is a result of



psychic events in the early years which lead to the processes by which conflict is regulated becoming stabilized in an unsatisfactory form. At the time when the unsatisfactory pattern of regulation is laid down, the impulses concerned, I believe, are very primitive and not greatly different from those which Lorenz describes. Although Freud in his later work developed what is frankly a mythology of instinct, in his earlier work, as I attempted to show in my remarks at our First Meeting (pp. 199-204) and also in the comments on Piaget's essay which I circulated in February last (pp. 4-5), there seems to be a close resemblance between Freudian and Lorenzian views of instinct. Thus I do not wholly share Bertalanffy's view that "Freudian and Lorenzian instincts are rather different things". But in advancing this view I realize that in attempting to evolve a better theoretical form of psycho-analytic theory I tend to follow the principle of paedomorphosis, preferring as a starting point some of Freud's earlier and less elaborated formulations.

In any case, it seems to me that Bertalanffy is rather over-eager to play down the part which primitive "instincts" of love and hate play in neurosis and psychosis. He writes "I think the Freudian picture of man as a born aggressor and fornicator, these mighty 'drives' kept under precarious control by the super-ego, is a slightly romantic one". He then proceeds to support his view with a number of points with which I agree but which seem to me irrelevant to the argument: "Homo is not a carnivore; he happens to be a gregarious species with inbuilt social and monogamous instincts, even though beside the social instincts which he shares with so many other species, aggressive instincts are present. Otherwise, institutions such as society and matrimony could not have kept through the millennia, or not developed at all". Intra-species fighting is not restricted to carnivores - even cows can kill each other in confined conditions - and it is plain that our "inbuilt social and monogamous instincts" frequently come into conflict with and sometimes lose to those impulses towards aggression and fornication the importance of which Bertalanffy thinks psycho-analysts so greatly exaggerate. The existence of conflicts in Homo can hardly be gainsaid and clinical experience seems to show that what characterizes so many of our patients is both the intensity of the emotionally toned drives comprising their conflicts and their inability to regulate conflicts satisfactorily. In them the conflicts tend to give



rise to excessive degrees of anxiety and guilt and the drives (hostile, sexual, flight, attachment to a love object) tend to be either completely inhibited, or are given full rein, or else vacillate between the two extremes. Often they are inhibited from direct expression but nevertheless manifest themselves in devious ways. I wonder whether Bertalanffy questions these observations or whether he accepts them but attaches a different significance to them?

In his views on mental disorder I think Bertalanffy has been over-impressed by some of their external manifestations. I agree, of course, with his statements (page 12) that "in contrast to organic disease, the manifestations of mental disorder are 'culture-bound' to a high degree" and that "the form neurosis will take strongly depends on the symbolic ambient of a certain time and civilization" (my italics). These statements, however, say nothing about the nature of the underlying processes. It is, of course, possible that the primary disturbance lies in the symbolic process itself, but it is equally possible that it lies at a deeper level of the organism and that the disturbance of the symbolic process is only secondary. A sub-oceanic volcano may manifest itself thousands of miles distant and in the form of a tidal wave.

I imagine that psycho-analysts differ as to which of these two views they take. Some, I imagine, would attach much importance to disturbances originating in the symbolic process itself. However, I see no reason to suppose that Freud would have done so. Indeed, his preoccupation with conflicts between instincts which he regarded as biologically rooted, a preoccupation which runs right through both his clinical and theoretical papers, leads me to think he would not have. I shall be interested to hear Erikson's views on all this.

In supporting his position, Bertalanffy seems to lean very heavily on some rather controversial ideas advanced by Kubie.<sup>1</sup> Kubie argues in favour of making "a clear-cut distinction between psychopathological processes the essence of which consists in the distortion of symbolic functions, and on the other hand those psychopathological processes which arise through the distorting impact of highly charged emotional

---

<sup>1</sup> KUBIE, L. S. (1953) The distortion of the symbolic process in neurosis and psychosis, J. Amer. Psychoanal. Ass. 1, 59-86



experiences occurring at an early age, before symbolic processes are established". Although both Kubie and Bertalanffy admit explicitly the existence of disturbances in the presymbolic biological mechanisms and refer to the work of Spitz and myself, by inference they both seem to regard such disturbances as playing only a trivial part quantitatively in human mental disorder. Kubie, after recognizing that these primitive disturbances may "occur as a response to primitive emotional stresses, when these occur in preverbal stages in human life, while the capacity for symbolic function still remains similarly limited", that "such presymbolic changes leave residual emotional disturbances which in turn influence the symbolic aspects of all later responses to injury", and that "as Spitz and others have shown, they may influence the later acquisition of symbolic functions", none the less disregards these primitive disturbances in the remainder of his paper. Taking his stand on the assumption that "it is the disturbance in the symbolic function itself which characterises adult human psychopathology in a pathognomonic fashion" he discusses the manifestations of neurosis and psychosis as though it were self-evident that the primary disturbance lies in the symbolic process itself. Bertalanffy, whilst recognizing that "there are all kinds of intermediates and gradations phenomenologically" between the two kinds of disturbance (presymbolic and symbolic) which he and Kubie postulate and "specifically, that both factors are active in human neurosis" proceeds to tell us that "the second (i.e. disturbance in symbolic structures) appear preponderant in neuroses of the common or garden variety as they line up for the psycho-analyst's couch".

Naturally, I have a vested interest in advancing the view that a majority of cases of mental disorder, whatever their manifestations may be, are the result of a primary disturbance in biological mechanisms similar in kind to, though different in origin from those caused by the various forms of maternal deprivation, and that as a rule the disturbances in symbolic functions are secondary. However, I do hold this view and I believe it to be as tenable as the alternative. I am not very impressed by Kubie's argument that the origin of neurotic and psychotic illness lies in the symbolic process itself. His case seems to be expressed thus: "As analysts we take it as axiomatic that in every psychogenic psychopathological process the symptoms and the symptomatic behaviour of the patient constitute an



unconscious symbolic language for the expression of repressed intrapsychic conflicts. The conflict, however, is not the illness. Psychopathological illness begins as the conflict engenders a repressive-dissociative process which obscures the links between symbolic constructs and the percepts and conceptualizations which represent the body and its needs and conflicts i.e. the "I" pole of reference. This is the primary point of rupture in any psychopathological process." My criticisms are two-fold. In the first place I do not regard it as axiomatic that symptoms and symptomatic behaviour always constitute the symbolic expression of repressed intrapsychic conflicts. It is true that many do and this has led to the assumption that all do. I suspect this to be mistaken and that, for example, many tics and other somatic symptoms are due to displacement activity (using the term in its ethological sense). Secondly, I am not convinced that the repressive-dissociative process to which Kubie refers affects only "the links between symbolic constructs and these percepts and conceptualizations which represent the body and its needs and conflicts". I can imagine repressive-dissociative processes affecting links at an infra-symbolic level. In this connexion the inadequacy for recovery of so-called intellectual insight and the need for "an emotional living through" may be cited. This "emotional living through" process I suspect to be related to a restructuring of processes active at an infra-symbolic level.

In my view Kubie puts his finger on the problem when in the earlier part of his paper when he is referring to the work of Spitz and others he writes: "such presymbolic changes leave residual emotional disturbances which in turn influence the symbolic aspects of all later responses to injury" (and, I would add, responses to all later intra-psychic conflict situations) and proceeds to emphasize the need "to work out in detail the inter-relations of these two fundamental aspects of psychopathology". It seems to me of great importance that as a first step we should learn more about the nature of the biological mechanisms which comprise the motivational and emotional aspects of human personality and, at a presymbolic level, provide the means of regulating conflict. Whatever these mechanisms may be, it is plain that we not only share them in some measure with the animal world and that the human infant is almost entirely dependent on them but that they continue to underlie



the behaviour of even the most highly educated and symbol-using adult. Furthermore, it is plain that, as both Kubie and Bertalanffy agree, they can be the seat of serious disturbance. My guess is that the study of the infant and young child's earliest social responses from an ethological viewpoint will prove rewarding in this inquiry. And having got so far it will then be necessary to understand how it is that, as the child develops, these primitive mechanisms come to be influenced increasingly by symbolic processes and how the two processes are linked together. Often it looks as though in the mentally ill the linkage has either never been made or has become broken, temporarily or permanently, and the two processes are working independently of each other. These are some of the problems to which I would like to see research workers address themselves.

The difference in viewpoint between Kubie and Bertalanffy on the one hand and myself and those who agree with me on the other are far from being of only academic interest. The success of both preventive and therapeutic measures turns on our adopting the right hypothesis. In his paragraph 8 on page 15 Bertalanffy has described what he believes to be the therapeutic conclusions of his point of view, (conclusions which I suspect Kubie would not share). For reasons which will be obvious, I tend to stick to the view that what our patients need is help in discovering new ways of regulating intra-psychic conflict and of freeing themselves from modes of regulation which were adopted in their earliest years at a time when symbolic function was either not in being or was operating at a very rudimentary level. (I prefer this formulation to Bertalanffy's reference to "conventional psycho-analysis trying to resolve early conflicts"). But whilst I believe this goal to be the right one I am far from convinced that we have yet found the best way of helping our patients achieve it. In any case I think it more useful to concentrate on understanding how infants and young children develop a satisfactory capacity for regulating conflict and on helping their parents provide the necessary conditions for them to achieve it.



STUDY GROUP ON THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

4th Meeting, Geneva  
20-26 September 1956

24 September 1956

SIX APPLICATIONS OF PIAGET'S DEVELOPMENTAL THEORY  
TO PSYCHODYNAMIC THEORY AND PRACTICE

by

E.J. Anthony, M.D.

(Delivered at the meeting of the Swiss Psychological  
Society at Zurich in July, 1956)

In recent years there has been an increasing interest in ego psychology and in early ego development, both from the cognitive and emotional points of view. Three historical stages can be discerned. At first, there appears to have been a concern with simple sensorimotor function, the ego being regarded as an intermediary between the Id and the environment, acting as the eyes and ears of the underlying insensitive "unconscious". In the next phase, attention became focused on the role of the ego as a controlling force with regard to perception and motility, and as the agent of reality organizing the defence mechanisms against inner and outer dangers. It was the "storm centre" of conflicts involving Id, superego and the environment. The ego was defined largely in terms of its functions, the key function being the synthetic activity. In the latest stage, the ego concept has been enriched still further by its equation with such global terms as "personality" and "character". Culturally-oriented analysts have laid emphasis on the influence of training techniques within different institutions on the basic structure of the ego. Others have defined in detail its "frames of reference", its system of "involvements" and its apperceptive appreciation of the external world. All this has given it a new importance in therapy. The days of an "Id analysis" are passed. Today, we are equally interested in the complex behaviour of the ego during treatment, in its past history, and in its post-therapeutic future.

\*\*\*\*\*

Concurrent with this expansion of the ego concept, there has been, both in Britain and in the USA a renaissance of the work of Jean Piaget, a large part of it being published and republished on a huge scale. So far the interest in his theoretical system has been greater among non-analysts, chiefly because Piaget has been basically disinterested in therapeutic aims and has tended to confine himself to what Hartmann has referred to as the "conflict-free sphere" of the ego, and to "conflict-free development", meaning by this the maturational and learning processes. I will not argue against the existence of this autonomous ego, since



we are often confronted clinically with severe character deformations that allow certain functions to develop relatively undisturbed or even enhanced. We can therefore accept, with less misgiving, this conflict-free psychological system of enormous dimensions and admire both its patient construction over a period of thirty years, and its viewpoint that is at once developmental, dynamic (although not psychodynamic), empirical, experimental and epistemological. In my opinion, these are very great virtues indeed, and I would consider it the only system, apart from the analytic one, worthy of any serious consideration. It is up to us, as clinicians, to find a proper use for it in our work.

It would be a great mistake, I think, to attempt to fuse it electically with our own ideas, as Charles Odier tried unsuccessfully to do. Such hybrid products are dissatisfying to both sides. We should, instead, do what Anna Freud has counselled us to do in all such cases - we must look at this magnificent data through analytic and therapeutic eyes and consider seriously how it can help us to clarify some of our own hazier concepts. In the section that follows I shall try and illustrate how I think this might be done. It must be borne in mind, however, that these applications of a biodynamic theory to psychodynamic findings are prone to all the pitfalls of transductive reasoning so characteristic of the Piagetian child of the 2nd stage. Such untuitive childish argument may nevertheless carry us a long way in our attempt at clarification. Later, we may perhaps achieve a more "operational" level of scientific application.

\*\*\*\*\*

Application 1    The significance of the development of the object concept to object relations

Piaget believes, on empirical and experimental grounds, in what has been called the "chaos" theory of neonatal development. This view postulates the genesis of a substantial and permanent object in time and space over the first twelve to eighteen months of life. In the first six or seven months, the infant is only aware of fleeting impermanent images, although there are "hints" at future permanence, such as would correspond with Glover's nuclear theory and Spitz's concept of "pre-objects". On first principles, it would seem necessary that an object would need to exist, in both space and time, before a relationship could be established with it.

In psycho-analysis, there are two opposing views on the question of object development. The first, that of orthodox theory, well represented by Anna Freud, postulates a primary, objectless, narcissistic state, during which the infant's cathexes are directed towards the satisfaction of his needs. It is more concerned with the act than with the object. True object relations do not begin until the sixth or seventh month.

The other theory, that of Melanie Klein, rejects the concept of a primary narcissistic objectless state and postulates a relation to objects or part-objects from the very beginning of life. This Kleinian object, however, is a primitive,



almost archaic construction, sometimes internal and sometimes external, and at the mercy of rapidly fluctuating introjective and projective mechanisms. It is doubtful whether we should really concede it the title of object; it corresponds more to the Jungian archetype, and it would be more correct, perhaps, to call it an archetypal object, since its existence is not wholly dependent on the experience of actual objects.

Of these two interpretations, Piaget's experimental findings would favour the former and concur with the postulate of an objectless stage in development. Piaget would differentiate between searching behaviour at the breast and the later searching behaviour for the lost or displaced object. The former would resemble a conditioned response activated by the kinaesthetic stimulation of the feeding posture. He would see no need to postulate the existence of the breast as a permanent substantial object. He would, however, qualify the concept of narcissism by adding "without narcissus", thereby indicating his own theoretical belief in the adualistic unself-consciousness of the baby.

Application 2    The significance of the object concept to the development of separation anxiety ensuing from loss of the object

Bridges, in her careful observations on emotional development does not credit the very young infant with the capacity for anxiety. Her infant, starting with an undifferentiated primitive response, gradually accumulates an increasing number of emotional responses that also show greater refinement and complexity. In contrast, Otto Rank ascribes anxiety to the baby at birth, and Ribble to the neonate reacting hypertonically or atonically, in answer to any threats to its physiological homeostasis. Spitz has described so called "anaclitic" responses to separation and absence of maternal care after the first six months. Other authors have observed separation anxiety at a much earlier period. One must be careful here to differentiate between the complex emotional response resulting from the absence of a particular object and the distress occasioned to the helpless infant through the absence of certain regular functional and contact satisfactions.

Piaget's findings would logically favour the occurrence of genuine separation reactions at a later date than the first six months, even allowing for the fact that the normal infant's response to its human environment always antedates its response to the physical environment. On first principles, again, an object would need to exist in time and space before its loss could be appreciated.

In our experimental work with psychotic children at the Maudsley Hospital, London, we have observed that where the object concept is lost (on testing) through developmental regression, anxiety from separation is correspondingly reduced. (see below)



### Application 3    The regression of the object concept in the psychotic child

The total concept of the object involves four stages of development, according to Piagetian theory. In the first stage, a substantial, permanent object is gradually established between the sixth and the eighteenth month. In the second stage, the practical object obtains a name, a use and a symbolic representation that allows for substitution under certain conditions. In the next stage, the object is endowed with dynamic properties, such as animism, realism, magic etc., as a result of a distorting egocentric perspective. That is, the child, because of its characteristically subjective orientation at this time, misinterprets the external world of objects and invests it with his own projected dynamisms. In the final stage, the object is conserved with respect to matter, weight and volume.

At the Maudsley Hospital, we have investigated seventy schizophrenic children who have undergone, in varying degrees, regression, withdrawal and rigidification, so that their social and developmental quotients, in other words, their "functional ages", extended to earliest infancy although chronological ages were all over four years. These children reproduced in reverse the stages of object development described by Piaget, the very regressed cases achieving an objectless narcissistic state that could be demonstrated by means of systematic tests.

It could therefore be inferred from this work and from the work of Piaget, that normal development leads to the genesis of the object concept, and that psychotic regression leads to the loss of the object concept.

### Application 4    The coherence of intellectual and emotional development

It is generally admitted that Piaget's developmental theory is weak on the affective side. His one great contribution, however, has been his emphasis on the coherence and mutual relationship between intellectual and emotional development. The clinical inference that can be drawn from this view is that where one of these interdependent elements is disturbed, the other will be correspondingly disturbed.

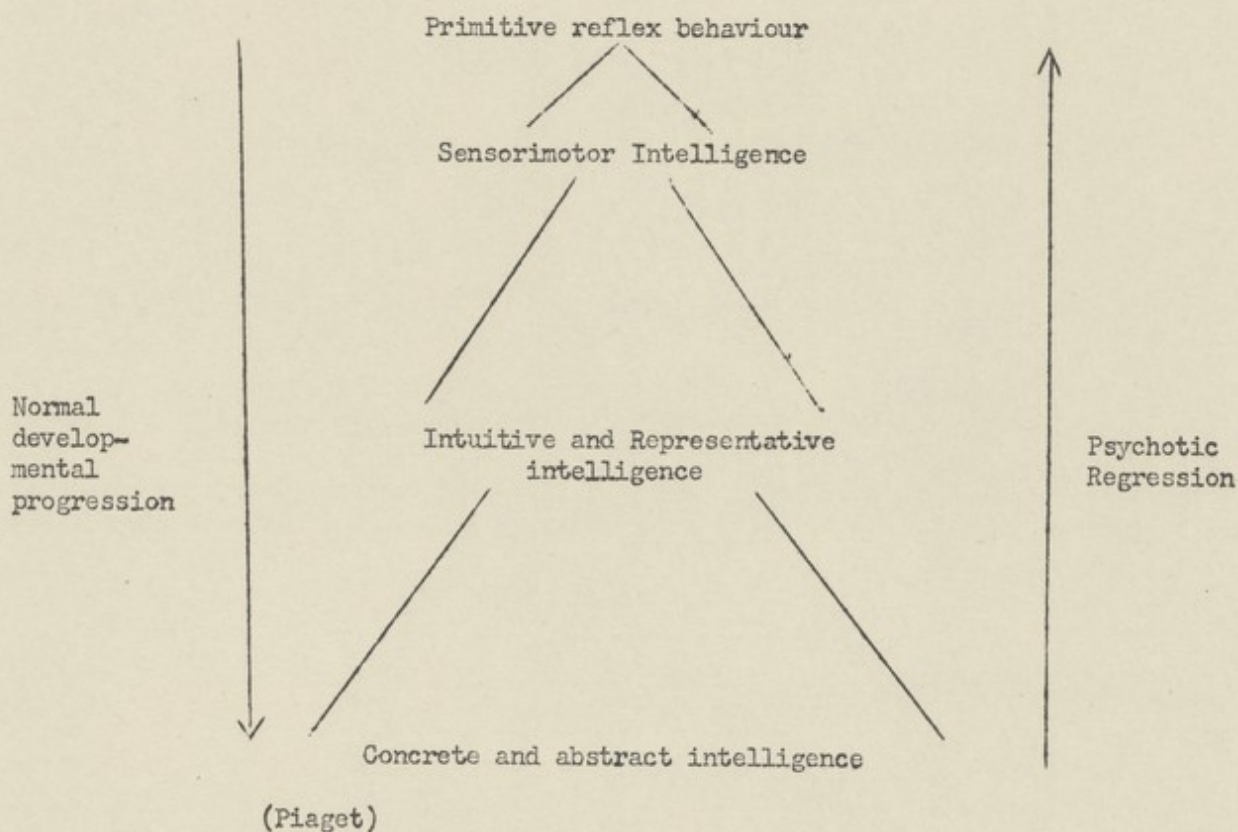
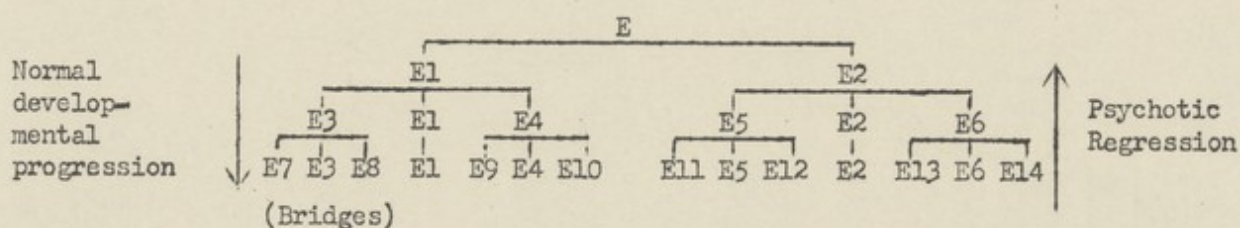
Psycho-analytical theory has been weak on the cognitive side, basing an inadequate theory on the functioning of the primary and secondary processes. Because of this, learning disabilities on the whole have not been very satisfactorily explored apart from some interesting applications of symbolic theory. Here we can learn a lot from Piaget.

For him, intellectual disturbances are as "deep" as emotional ones, and form resistive "unconscious" complexes that affect our intellectual lives in many irrational ways. Our "basic assumptions" also go back to infancy and require a thorough "genetic analysis", as prescribed by Piaget, for their eradication.

His synthetic theory of intelligence and affect also serves to explain the progressive diminution in emotional and intellectual range as a result of the psychotic process, as observed in the group investigated by us.



In the following illustration of Bridges' scheme of emotional development, it will be seen how similar her approach is to Piaget's, who also postulates on the cognitive side, complete continuity between primitive undifferentiated reflex behaviour and abstract operational intelligence:



In some psychotic children, with extreme retrogression, we observe no mental activity beyond the sensori-motor level of intelligence or repetitive circular reactions in the form of mannerisms, associated with a severe impoverishment of the emotional life so that the child seems capable only of undifferentiated primitive excitement or simple distress and delight.



Psychoanalytical theory, on the other hand, credits the child from the very beginning with complicated libidinal and aggressive feelings related to a well-developed memory apparatus, whose infantile storage later undergoes regression. On these counts, Piaget criticizes Freud for not being a true genetic psychologist but depending on "ready-made" functions and structures.

#### Application 5 Moral development and the Superego

Psychoanalysts have been especially prone to disagree with Piaget on the matter of moral development. There seems to be, however, some semantic confusion on this point. Piaget postulates two stages in moral development. In the first stage, the child's moral evaluations are said to be external, "heteronomous" and derived from parental views. In the second stage, starting about the age of 7 or 8, his views are internal, "autonomous" and derived from the codes of behaviour current in his peer groups.

The origins of the superego in psychoanalytical theory have been thrust further and further back. In the original classical interpretation, the superego developed with the passing of the oedipus complex, when the parental prohibitions were internalized. Before that, there was only a vague sort of "sphincter morality". Later work, however, has revealed antecedents to this highly developed conscience of the oedipus, and in Kleinian theory the superego is considered to function from the beginning. The discrepancy between the two views can probably be explained thus: Piaget's autonomous specialized conscience is much more akin to ego functioning as described today than to superego activity. It is rational, reality-based, and controlling through acceptable standards. His "heteronomous" conscience is, like superego conscience, irrational and parent-derived. This conscience dominates the child's life in the first five years, and then gives place to the ego function which can go on developing along with further intellectual and emotional development combined with peer group contacts.

Here I feel Piaget is describing, in his usual brilliant intuitive manner, the taking over by the ego of the control of behaviour from the primitive irrational conscience. His description of the former is "internal" and of the latter as "external" would be more properly and less confusedly described in terms of introjective and projective mechanisms. The little child projects his harsh standards outwards on to the adult world.

We have occasionally in psychotic children seen transitional forms intermediate between superego and ego activity, and the explanation possibly lies in this clear description of Piaget's. Here, once again, he seems to have contributed fundamentally to our understanding of ego development. (It is also interesting that the legal definition of moral responsibility dates its onset to the eighth year.)



Application 6    The significance of "normal" symptoms

Most developmental psychologists are now aware that a longitudinal study of any normal child brings to light many apparent transient set-backs and reversals of development that are characterized by the appearance of "symptoms". Gesell has formulated this finding under the concept of "spiral" development. Thus we are confronted with the appearance of "negativism" at 2 and 3, of phobias at 4 and 5, of obsessions and rituals between 8 and 9.

A good developmental theory should be capable of explaining these "normal" symptoms. One of the most famous stages in Piaget's developmental scheme is the representational one, when the child is passing through a period of partial adaptation to the external world. Because of his egocentricity and subjectivism, he makes his universe alive and teeming with magical forces. To a sensitive child, this aspect of things can take on a peculiarly frightening quality, and Piaget gives examples of this in his book dealing with animism. It is however a passing phase, and if we wait the "normal" child will "grow out" of it.

This developmental viewpoint clarifies the occurrence of such symptoms, explains their disappearance in time, and serves as a salutary warning to the omnipotent therapists among us, who treat our patients with such great and unexpected success from time to time.



A G A P E     September, 1956

F irst we were many, each wi th morning pride  
I mpatient of the others, seeking fame.  
N ow, in our evening, gladly we confide  
A ll in each other to define our aim.

L earning in practise what we sought to teach:  
P assion can grow with knowledge as with lust.  
D reams that are shared bring faith within our reach,  
C ancelling secret fears in common trust.

G overned by love for those who take our place  
E ven our parting is a little birth -  
N eed we regret that in our last embrace  
E cstasy ends in labour, not in mirth?

V ainly we seek the Question of Time;  
A nswer we cannot - but at least we rhyme!



19th April 1956

THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Comments by Lorenz on Bowlby's comments on Piaget's Essay  
March 1956

Extract from accompanying note

AA/JE

*John*. Here are a few comments  
for what they're worth. They  
refer to the sections in your  
which I have marked with

Dr. Bowlby

①

The  
clo  
the  
or  
one  
the  
pec  
to  
do  
wh  
mut  
or  
"pe  
ton  
env

Refined

KUBIE, L.S.

Two fundamental

of the development

of human & non

②

sex  
to  
eme

Psychoanalytic

③

the  
and  
ex  
beh  
in  
I  
ter  
way  
the

23

④

sub  
as  
say  
pat  
as  
ver  
wh

"affect laden behaviour".

⑤

at  
it  
me  
in  
a lot.

active content  
a drawing  
they need not  
fog involved



appeared to enjoy it. Her mother feels that she is not learning much, but there is other evidence to suggest that she is learning quite a lot. She recites in an easy spontaneous way verses which she has learned and shows more ability to express freely her phantasies in her games and story telling, presumably because these phantasies have become less sadistic. She has on one occasion copied accurately a row of letters of the alphabet written out and given to her by her father, which suggests that symbol formation and the free use of symbols has sufficiently developed for her to take in these symbols, to sort them out inside, and to put them out again in the form of printing. This of course is essential to the process of learning and use of knowledge. Previously this was too much inhibited by anxiety ridden phantasies about the objects and part-objects that the symbols represented in her earlier development. In fact her ability to learn in this way from her father, seems to represent an ability to form a sustained emotional relationship with him of a positive kind. Likewise in the transference she is freer and less controlled towards me. Nevertheless a good deal more treatment is considered necessary before she will be stable enough to proceed with her learning activities unhampered.

S.F. Lindsay.



Comments &c



Reprint

KUBIE, L.S. 1954

The fundamental nature  
of the distinction betw.

Normality & Neurosis

Psychoanalytic Quarterly

23. 167



*John. Here are a few comments  
for what they're worth. The numbers  
refer to the sections in your paper  
which I have marked with similar numbers.*

3rd February, 1956.

Dr. Bowlby

*Fanny*

- ① As to the nature of this inter-connection might not a sort of Field Theory viewpoint be relevant? One might regard the development and clocking in of each function as taking place in an "environment" of all the other functions, each at its own particular phase of development or maturity according to its own velocity. The extent to which any one function can develop, and degree of dominance it has in relation to the others at any one time, might be determined partly by innate factors peculiar to itself, but partly by the extent to which it is permitted to operate, as determined by the relative degrees of development and dominance of the other functions. In other words the inter-connection, whatever else it is, is likely to be one not of co-ordination but of mutual facilitation and/or limitation, each function having a more or less permissive effect on the others. At the same time this total "permissively interacting" system is influenced in its development towards synthesis by facilitative and restrictive forces in the external environment.
- ② Do you mean by "true steps" something like "phases in the proper sense of that word, namely, that the condition of progress from one phase to another is that the prior phase is a necessary precondition for the emergence of the subsequent one"?
- ③ Is this issue just an either/or one? I agree with your formulation that "present behaviour is due to the on-going dynamic assimilation and restructuring of the past in terms of the present", but would not exclude the opposite process, namely, that "the present to which the behaviour is a response is assimilated and structured, or interpreted, in terms of the past". Perception experiments seem to make this clear. I agree that there can be no static tethering, but prefer to think in terms of a two-way dynamic process in which the present influences the way the past operates, at the same time as the past influences the way the present operates.
- ④ This term 'affectivity' is a derivative from the old division of the subject-matter of Psychology into cognitive, affective and conative aspects, and I think we would probably be better off without it. To say that "affectivity is an accompaniment of an activated behaviour pattern," does not really clarify what you regard this concept of "affect" as referring to, and although your next section on Behaviour Patterns is very illuminating about behaviour patterns, it still does not clarify what you mean by 'affect' in the statement "affect laden behaviour".
- ⑤ Is it not the case that any given pattern of behaviour can, at different times, vary in the amount of emotional and intellectual content it has? Breathing usually has no emotional content for us, but for a drowning man it probably has a lot. Driving along a main road in the country needn't involve much intellectual content, but driving in town in a thick fog involves a lot.



⑥ I agree. Do not cognitive functions develop on the basis of already given behaviour structures, in the course of the modification or elaboration of these?

Once complex behaviour structures have developed as a result of the operation of cognitive functions, may they not be motivated by two classes of drive?

a) Physiological + social.

b) Cognitive. The occasional sheer satisfaction of solving an intellectual <sup>ordinarily something difficult</sup> problem, merely for its own sake. In this sense could we not say that the cognitive functions can be dynamic? Perhaps all developed functions can be dynamic in the sense that they require exercising if deprived of it?



Feb 2

Dear Phil, Please excuse a train-written reply, but it's the only way I can be certain of getting it to you by Sat. I found the MS extremely inspiring, giving me a wonderful synthetic view of what you are aiming at, & making me glad to be able to play a small part in it. Much of it has not convinced me to comment on, & what I am, I found excellent. But a few points:

General. This is a personal matter, but to me as a biologist I found your use of the term function confusing. Behaviors have functions, but I would never use 'function' as a label for <sup>questions</sup> behaviors - & I suspect you would soon by <sup>development</sup> ~~functions~~ if you did.

I like to regard ~~development~~ as a series of branching & interlocking biochemical changes. The degree of correlation between stages of different activities depends on how much they share ~~the~~ chains. An aspect in the mechanism of development will upset more or less as aspect of development according to how early it occurs & how much the chain concerned interlinks with <sup>other</sup> ~~upstream~~ <sup>upstream</sup> effects in part. ~~for~~

p 4-5. When you say 'widely accepted' by ethologists, I think it is, but I hope there was no hidden reference to Kottlandt's recent paper, which stank.

None!



p 8. I feel strongly that you shouldn't use ethologist  
as necessarily equivalent to a believer in  
innateness. Some ~~father~~ are (Lorenz is an  
extremist), & much of behavior certainly is inborn  
in his sense — but many of us are more liberal  
than he!

The important point is that species differences  
in behaviors are innate — the two species  
brought up in different environments show different  
behaviors.

✓ But let's use ethology for an approach,  
not a doctrine!

p 10 re Paul Weiss. ? Insert a caveat that he  
used mainly Amphibia. There is no doubt that  
work with different groups produces dissimilar  
✓ ~~behavioral~~ results — though how fundamental the  
differences are remains to be seen.

p 10 para 2. Can cognitive structure be independent  
of motor patterns? This is a thorny question,  
& I think you should tread warily — Certainly  
I would have thought that the evidence suggested  
that cognitive structure didn't develop without  
behaviors (eg the work on perception). How then  
can they be subrequeently linked together? Or do  
I misunderstand?

p 12. Would it be better to make a ~~the~~



make emphatic distinction between equilibrium,  
which may be functional, & the processes which  
give rise to them, which, presumably, biologically  
speaking we not?

Forgive the haste with which this is  
written, but I couldn't see a certain niche  
in the next few days when I would have more  
time.

Many thanks for letting me see it  
— may I keep it for further pondering?

Yours ever Robert.



Heide's criticism.

My picture is too preoccupied w motor-  
patterns.

is poor but no method  
so tho' all action appropriate

[building cycle of 3 weeks.]

once got into neurotic pattern, v. diff.  
to get out of again.

to Grandmother Ed.



19th April 1956

THE GENERAL PROBLEMS OF THE PSYCHOBIOLOGICAL  
DEVELOPMENT OF THE CHILD

Comments by Lorenz on Bowlby's comments on Piaget's Essay  
March 1956

Extract from accompanying note

I am very much in agreement with your answers and comments to Piaget's questions. I have written my own comments as I went along reading.

Note: pages references to Bowlby are to the W.H.O. roneo version

Page 2 - line 6 from below: "each progressing at its own rate etc".

And the rates of these developing activities are tremendously different individually. Even in wild geese, the sequence, in which new, developing activities appear in the adolescent bird, is about the most variable item in behaviour. I am very ready to believe Kretschmer that this is the same in humans. We have had a wonderful case of a gander who matured sexually abnormally early (having domestic blood in him) but was abnormally late untying the infantile ties tying him to his mother, and lo, a beautiful Oedipus situation was the result, NEVER before seen in any bird! (Three cheers for Frank for making us do these goose case histories so thoroughly). The great breadth of this developmental variability of independent activities makes it difficult to speak of "typical" phases in the development of the individual as a whole, for every phase, there is a great number of "types" according to which activity gets there first, or disappears first, and so on.

Page 4 - concerning simultaneous co-existence of "libidinal stages"

Ilse Prechtel found in her Doctor's thesis (which ought to have been published years ago) on development of passerine babies, that there are quite a number of cases, in which the first appearance of one reaction neatly switches off the one preceding it. (Gaping to concussion of the nest ceasing at the very moment when acoustical response to the feeding call was first released, etc). Overlapping of responses was rare, in some Passerines there is an overlap between the early stage of gaping vertically upward, oriented to gravity, and the later stage of gaping at the parent with visual orientation. In thrushes there is a short stage of gaping in the resultant direction between both orientations. Heinz Prechtel brought about a "regression" by sealing the nestling's eyes whereupon it gaped vertically again.

bottom: Faulty synthesis is, of course, frequent. Particularly, where higher functions (conditioning!) is needed to integrate innate elements. Eibl is just busy studying this phenomenon and trying to produce it experimentally. But also, very often, similar effects are produced by the variation of first appearance of activities that must be synethetized to perform a common function. If, in a goose, the triumph ceremony appears early and with particular intensity and long before copulatory activities, the result may be a bird with an unbreakable triumph-bond to another male (in which process a lot of conditioning takes part!), consequently totally unable to marry (Odysseus, \* whom you may remember). If on the other hand, copulatory behaviour appears long before triumph activity you get a completely promiscuous bird (most barnyard females but also slightly runtling wild ones - Schiefschwanz) who, of course, may become perfectly normal later, after the maturation of triumph has caught up with copulatory activity.

\*

Footnote: Odysseus is one of Konrad Lorenz' geese.



<sup>top</sup>  
Page 6/- A term for "re-arousal" is very necessary indeed.

Page 6 - lower: concerning the persistence of unchanged "fixations".

As so often Freud may be right in very special cases: Certain Erbkoordinationen, like sucking, "Such-Automatismus" and other MOTOR patterns certainly persist unchanged, though never again used, coming out in insulin shock etc. But, I should say, these things are unimportant, like the appendix. The more interesting remains of early stages are certainly those that are dynamically woven into the whole context of later structures, because they go on influencing that whole being as an integrated part or member of it. In this way, as a part of a dynamic unit, the primitive structure certainly goes on existing. It is quite correct to say that it never is "the victim of total destruction". But this does NOT mean that the original particulate element is, even on principle, still distinguishable as such, even through the cutest analytical techniques.

Page 7 - I agree entirely about "affectivity". Piaget conceives something "unitary" under this term, just as you say. If I understand him rightly, he means the general plus-or-minus of appetitive or aversive response to whatever is responded to. I know I am always too prone to read my own meanings into the things other people say, but if I read his question to you aright, he simply means that an appetitive or aversive source of motivation (his "energetic" component) must be present in order to make any learning, or any cognitive process possible. Without it, the situation in question is simply uninteresting.

Page 8 to upper half of Page 9 - integration of part-patterns:

Agree entirely. I think we could give you, in a near future, still better parallels to the human integration of such part patterns, by which I mean integrations which are brought about by learning and "cognitive" functions. (Eibl's rats, Geese).

Page 9 - Primary of social responses:

This damfool nonsense of "remunerative strategy" can only be believed by somebody who has no idea about the complete independence of conditioning through feeding and social attachment. A gosling is NEVER fed at all, yet its first response to the big moving object is greeting (actually the first appearance of low-intensity triumph ceremony, as we have found out) in other words a response directly analogous to smiling. Knowing what we do about the social attachments of animals and man it seems absurdly silly to me that out of a pseudo-scientific materialistic bashfulness we do not dare to say that animals and men love each other. You can define this "love" very well in the parlance of strictly objectivistic ethology: One animal behaves to the beloved conspecific exactly as it would to its well-known home. The "animal with the home valence" is the wonderful term Monika Holzapfel has coined. If I take away Max from Odysseus, Odysseus will behave exactly as if he were put in a strange surrounding. He will cease from feeding, develop an intense flying mood, roam about searching and loose all aggressive behaviour. When he finds his friend again, he will explode into a triumph ceremony and attack the next best goose. Odysseus has a permanent sexual affair and four illegitimate children, no sexual relations to Max whatever and he certainly has never been fed by him. Do you know how totally irrelevant feeding is to gaining the affection of a dog?



I, myself, am struggling with the problem of formulating objectively the facts underlying my conviction that "social attachment" is an autonomous innate response and/or activity, and a very complicated one at that, into which conditioning "enters" as a factor not only determining, but actually building up the object. The smile in humans and the triumph ceremony in geese (and, incidentally, tail-wagging in dogs) are motor patterns functioning as an integrating part of this complex.

Page 10 - Relation of affective to cognitive:

I think I can make some sense of what Piaget means when he regards cognition as structural, in the sense of being non-dynamic. Exploratory behaviour, the basic force underlying cognition, is un-affective, in a certain way, because it is NOT motivated by any other known appetites and aversion. The raven exploring an unknown object does not want to eat, but wants to know whether the thing can be eaten IN THEORY. That is what Bally means with his conception of the "Entspanntes Feld", and that is why exploratory behaviour and research contain such a strong affinity to play. Also I think that the pure cognitive function of "understanding" or "grasping" a given situation, in other words, building up a model of it in our CNS is another thing than the application of this knowledge in the finding of a solution to a problem.

Page 12 - middle - cognitive processes of different degrees of intelligence

All these levels of intelligence, from lowest to highest occur in the one function of driving a car.

Page 12 - bottom - cognitive/dynamic in its own right.

I should say that cognition, if dynamic at all IS "dynamic in its own right", because curiosity and exploratory behaviour are something very articular. Not at all a basic element contained in all animal behaviour, but a definitely new "invention" made by the evolution of the very highest animals.

Pages 13 - 16 - No comments.



m/2/1  
Please return

MINUTES OF A MEETING OF THE RESEARCH SEMINAR

CURRENT STUDIES NO. 2.

Wednesday, 29th February, 1956.

Psychobiological Development of the Child.

Present: Dr. J. Bowlby,  
Mr. T. Ambrose  
Mr. C. Heinicke  
Mr. P. Hildebrand  
Mrs. J. Fopperon  
Mr. J. Robertson  
Dr. J. Sutherland  
Miss I. Westheimer

The topic of the meeting was a discussion of Piaget's memorandum to members of the W.H.O. Study Group and Dr. Bowlby's reply of 6th February, 1956.

Common Language

Dr. Bowlby pointed out that Piaget's interests were those of a developmental epistemologist and that the main intention of his paper was to ask members of the Study Group whether they considered it possible to describe the development of the human organization as a whole. Piaget, himself, in his work on cognition, had not dealt with questions of motivation, and his questions addressed to Dr. Bowlby (and Lorenz) were aimed to elicit information on this side of development and to see whether there could not be a common language which described all the aspects of development - physiological, neurological, sociological, cognitive and affective. *to find terms*

It was thought that one of the main difficulties in an attempt to find a common language, was the different levels of conceptualization used in the various fields. For instance, Piaget's stages of cognitive development were probably of a different order from the analytic concepts of libidinal development. Dr. Bowlby thought that perhaps one of the reasons for the difficulty was that psycho-analysis had not yet evolved a model of the developmental process.

Regarding Piaget's ideas of the importance of equilibrium as a common factor in all fields of development, Dr. Sutherland queried whether this was in fact as important as the phenomenon of integration. What was most difficult to describe, he thought, was the critical experience of humans in conceiving of themselves as a whole person.

General Factors of Development

In this section of Piaget's paper, the team were in agreement with his theory that the interaction of genetic, environmental, etc. factors were as important, if not more so, than the factors themselves. Piaget's idea that affective development could be explained in terms of enriching-impoverishing was not considered to be valuable as a concept.

Dr. Bowlby thought it premature to accept Piaget's suggestion that parallels should be sought between stages of development in different fields until we had studied particular functions of emotional development in much more detail. The experience of learning theorists in attempting to explain human development had shown the futility of such an overall approach without sufficient data on particular functions.

Mr. Hildebrand said that Piaget, having described cognitive development according to his theories of stages of development, hoped that similar theories were in existence or could be developed about stages of development in the social, affective and neurophysiological fields, and wished to explore their interaction. However, it was generally agreed that there is not at present an adequate theory of motivation and



its development for such studies of interaction between the various fields.

Mr. Heinicke drew attention to the work he had done with Bales on the development of structure in small groups of men, which had shown that if a group were given an intellectual task, and established a group structure in the first session, this structure endured, even if challenged, in the second session. If groups failed to become structured in the first session, however, the reasons for such failure had not been explored.

Dr. Sutherland suggested that it was just in this point of 'failure' that analysts might approach the interaction of different fields of development. In the sausage experiment quoted by Piaget, the failure of the child to understand space perception might be due to some emotional factor and it may be at this point of failure or error that integration might be discovered between various fields of development, e.g. if cognitive development is blocked analysts might be able to say why.

#### Cognition

Mr. Heinicke raised the question as to whether cognition had been adequately defined in Piaget's paper. For instance, can perception of social relations be described as a cognitive ability? Dr. Bowlby thought a relevant question on these lines, might be how is it that cognition of some emotional factor such as jealousy, brought about by therapy, appears to affect motivation in other fields.

Piaget seemed to have assumed in his description of cognitive phases that when a child reaches a certain stage, all cognitive behaviour is at this stage, but this did not explain how people appeared to act at different levels of cognition - e.g. a mathematician operating at a high level of cognition in one intellectual field yet apparently operating at a very low level in the field of perception of social relations, or even in other intellectual fields.

Another point considered worth exploring, was whether Piaget conceived that one could pass through a certain stage of cognitive development 'badly' and yet reach another and higher one. Piaget apparently was concerned in his description of cognitive stages with an 'ideal' subject, and not with individual development, and considered the intellectual function as a unitary one, operating on the general principle of assimilation, which was paralleled in biology.

Although analytical clinical work was mainly concerned with personal idiosyncracies, it was thought that the work of Fairburn, and Colby's recent book "Energy and Structure in Psycho-analysis" were relevant to the considerations of Piaget's paper.

The role of cognition in instinctive behaviour was then discussed. Piaget apparently defined cognition as structured behaviour, and motivation as the affective function related to such activity. But if cognition is defined as structured behaviour, then behaviour patterns, being structured, would be defined as cognitive. Yet if it was agreed that sucking or a more complicated pattern such as that of nest building in birds have initially no cognitive function, one could not say that structuration was a criterion for defining cognition. However, if birds 'improved' in their nest building from year to year, one might then say that some cognitive function was involved, just as the behaviour patterns used by infants in sucking are extended to other fields because of the growth of cognitive ability.

Mr. Ambrose suggested that one definition of cognition could be that of a gradual increase of organization used in the past, transferred to the present. It was thought that the acquisition of new behaviour patterns, by humans might be due to ~~the~~ organization of a cognitive variety. Dr. Hinde's views on the role of cognition in behaviour patterns in birds would be valuable.



29.2.56.

-3-

Finally the question of intention, and of the differentiation of ends and means was discussed. Dr. Bowlby thought that experiments on babies to find out when such a differentiation takes place would help to explain the role of cognition in the ~~acquisition~~ <sup>emergence</sup> of behaviour patterns.

Molly Townsend.



Finally the question of intention, and of the differentiation of  
ends and means are discussed. Dr. Doolittle thought that experiments on  
mice to find out what a differentiation factor would be to  
explain the role of cognition in the motivation of behavior systems.

July 1950

## Comments &c

enough to become with our language, activities, and behavior.  
some phenomena are mentioned, necessarily, before we can begin  
to deal with the problem of intention. The first is the fact  
that we are not alone. We are surrounded by other people, and  
it is in this social context that we must understand the role of  
intention. The second is the fact that we are not alone in our  
thoughts. We are surrounded by other people, and it is in this  
social context that we must understand the role of intention.