

what is prepared in the preceding stage, in other words an equilibrium step. Now Zazzo replies that these definitions are too limiting, and that naturally if one defines the stages in too limiting a way it will be easy to say afterwards that there are no general stages. He proposes, therefore, a much more elastic definition of stages.

I will simply reply that these are degrees of the possible structuration of stages, and, that, of course, one can be content with one or two degrees within the five. One can very well conceive of a series which can be called stages which would conform only to the first criterion, that is to say, to the constant order of succession. One can very well imagine another series of stages where there would be at the same time order of succession and integration. One can very well conceive of a third series of stages where there would be the first three requisites and not the two last, etc. I do not think that these criteria constitute a kind of *a priori* framework. The criteria I have presented are what can be found in a field where stages are clearest; there is no question of generalizing these requirements for all stages in all fields.

ZAZZO:

That reply seems to me very satisfactory because it admits differing definitions for different levels, and I think that links up with what Tanner said in his paper.

PIAGET:

I now come to the big problem: the problem of the very existence of stages; do there exist steps in development or is complete continuity observed? Now, Tanner, upholding the position which he has adopted during all the meetings of our Group, shows that in somatic development one finds only continuity. He therefore naturally tends to generalize this idea and in opposing the idea of relative discontinuity in other fields he makes an objection which seems to me extremely serious: it is that when we are faced macroscopically with a certain discontinuity we never know whether there do not exist small transformations which would be continuous but which we do not manage to measure on our scale of approximation. In other words, continuity would depend fundamentally on a question of scale; for a certain scale of measurement we obtain discontinuity when with a finer scale we should get continuity. Of course this argument is quite valid, because the very manner of defining continuity and discontinuity implies that these ideas remain fundamentally relative to the scale of measurement or observation. This, then, is the alternative which confronts us: either a basic continuity or else development by steps, which would allow us to speak of stages at least to our scale of approximation.

I think Bertalanffy's paper brings to this point documentation of great value. Bertalanffy takes his examples from fields as different as architecture, the history of automobile technique and the evolution of species in order to demonstrate that this is a general problem. In the evolution of species it is remarkable that one has great difficulty in finding intermediate types between two species. Stable species are found, as if there were no evolution; one observes only very few transitional forms which would enable evolution to be demonstrated. Thus the solution proposed by Bertalanffy, using the ideas of equifinality and equifinal steps, is that the intermediate types are unstable, whereas the species themselves are steady open states. In the case of species there would, then, be equilibrium steps for certain morphogenetic organizations, whereas the cases of transition would be unstable.

This conception of Bertalanffy's seems to me to correspond exactly with what Inhelder and I have found in the field of cognitive stages. On the one hand we find stages which characterize a certain proportion of individuals at any given age. On the other hand, we always find sub- or intermediary stages, but as soon as we try to pin these intermediary stages down we enter a sort of cloud-dust of sub-intermediaries because of their instability. Other organizational steps are relatively more stable and it is these that one can consequently consider as 'stages'.

The usual reproach made about stages like those I have been reminding you of is that in making them continuity is neglected. This is exactly Tanner's argument; and generally, every time we constructed a series of stages in one field or the other of the cognitive functions, Inhelder and I came up against the same objections of exaggerating a discontinuity and neglecting a continuity which we might have found if we had made a finer analysis.

This year, however, in the work of our Centre for Developmental Epistemology in Geneva I met with the reverse objection from one of our collaborators; that is to say, he systematically saw discontinuity everywhere where I tried to show him transitions and continuity from one stage to another. The problem was one which interested our Centre and which goes perhaps rather beyond the interest of psychologists: it was the problem of what is called in logical empiricism the analytic and the synthetic. The question was to establish whether there exists between analytic and synthetic statements a complete opposition (Carnap) or a continuity of transition (Quine). It was for this reason that we carried out the psychological experiment (amongst others) on counting and verifying $3+2=5$, that I described above. The idea was to take a statement considered by all the logicians to be strictly analytic, but which I

wanted to show actually began in the child by being experimental and consequently synthetic.

However, despite this, my collaborator maintained that if the changes were slight they were nevertheless always made by sudden reorganization and discontinuous steps. He was a logical empiricist who insisted on the distinction between the analytic and synthetic, and I did not manage to convince him that there was continuity. In another field I should perhaps easily have convinced him. In other words, these notions of continuity and discontinuity depend not only on the scale of measurement but also on the general system of interpretation.

I will conclude, then, by saying that stages of development appear to me to be a reality, but differ from one field to another; they are more or less defined, more or less precise and accentuated according to the fields. As regards general stages common to all fields of development, I am in some doubt. I can neither affirm nor deny their existence. One can only decide by successive approaches, which would consist in establishing a series of correlations, correspondences and parallelisms—work which is almost entirely still to be done. Lastly, the idea of stages seems to me necessarily linked with the idea of equilibrium, or steady states.

GREY WALTER:

The definition of 'stage' is essential to our whole philosophy about children. We can't escape the general axiom that in dealing with an organism there are bound to be thresholds; there are bound to be stages below thresholds at which nothing happens, stages above thresholds when something does happen. But the difficulty arises when one has many thresholds, that is many processes each with a different threshold, in which a series of step-functions may overlap. Piaget seemed to me to be saying that in studying the *whole behaviour* of a child, there may well be so many thresholds that the various jumps superimpose and produce a smooth curve. It seems to me that one of our tasks is to see if we can dissect out the various step-functions and thus isolate particular psycho-physiological functions in the child for further investigation. The origin of some of our misunderstandings may be that some of us are looking at individual functions—for example, the existence of a particular type of brain activity in the EEG, or the existence of some drive in sexual behaviour—whereas others are looking at whole behaviour in which there is a blurring of these otherwise critical stages of development.

TANNER:

I was going to say very much the same thing. Piaget acknowledged the force of the argument that something which appears as a stage

may do so because it has been measured on a coarse scale and that if the scale is made finer one may discover there is continuity. In my turn I agree that if you go still further down in refinement, down to the cellular level, you again come to step-functions. Thus there are some behavioural levels where stages appear, other levels, for example of physical measurements, where continuity appears, and then the cellular level where ultimately there is again discontinuity. It is chiefly for this reason that, as I said in my comment on Piaget's paper, I don't feel the question of stages is a very real one. I think that stages are useful only in certain operational situations and that it would probably be a mistake to discuss as a question of philosophic importance whether stages do or do not exist in the disembodied or decontexted sense.

PIAGET:

I should like to make a quotation. It is from Henri Poincaré and he said 'Scientific research consists in making what is discontinuous continuous and in making what is continuous discontinuous'!

INHELDER:

Certainly in the field of cognitive functions stages have no absolute significance. What actually does a stage signify if not a change of a qualitative order, a sort of metamorphosis? We can speak of stages only when we observe a real change in behaviour in a defined situation. Such a transformation can be marked by a change in the way of thinking. It may happen, for example, that a child for a shorter or longer time tries to find a solution to a problem by the method of trial and error. He gets close to the correct solution through a series of successive adjustments, without, however, achieving success. Then almost abruptly he changes his tactics and reasons in a perfectly logical way with a feeling that it has become self-evident. It is then that we are faced with a new form of thought qualitatively different from the previous form. This new method or tactic will itself serve as a basis for new trials and errors in connexion with more complex problems. This metamorphosis of thought is particularly clearly seen in respect of ideas of conservation.

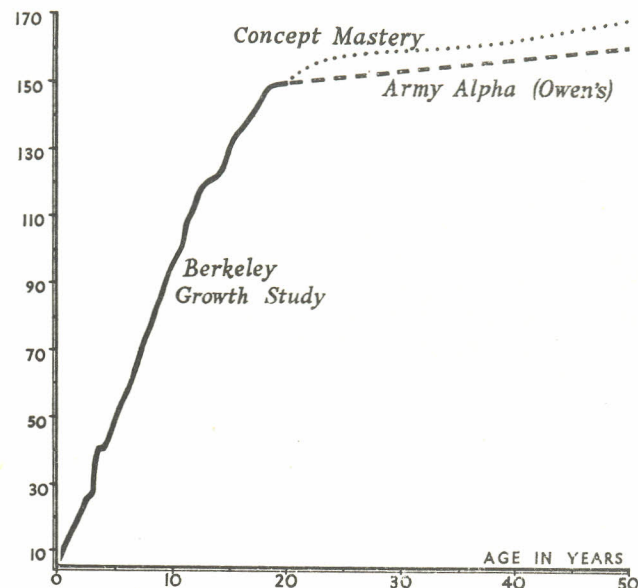
BERTALANFFY:

Here a question occurs to me. We have the stages as analysed by Professor Piaget. On the other hand, we can compare the curve of brain growth (Vol. I, Fig. 1, p. 37) with the 'age curve of intelligence' (see Fig. 1) based on the Berkeley Growth Study. It seems that these curves of brain growth and mental growth show a remarkable similarity. Would you say that the smooth Berkeley curve is caused by a smoothing-out of Professor Piaget's stages?

FIG. 1

A PROPOSED AGE CURVE OF INTELLIGENCE,
BIRTH TO 50 YEARS

Based on data from the Berkeley Growth Study, the Terman Gifted Study, and Owen's Iowa Study (after Bayley, 1955).



INHELDER:

Yes, I suppose it is a cumulative effect.

The effect of pseudo-regularity in curves of development can result from the way the tests are grouped. It has been noted that different ideas of conservation, of an arithmetical, geometrical, physical, etc., order are not formed at exactly the same moment. The processes involved are parallel, but not synchronous because one finds more difficulty in structuring one aspect of reality than others. These overlaps are one of the reasons why for many years I refused to believe in general stages of cognition. It was only later on after I had noticed the surprising concordance of structural order in the mechanisms of thought that I was able to show steps within cognitive functions. These steps are characterized by structures.

These structures in the concrete thought of the child or in the formal thought of the adolescent always represent the *optimum* of his operatory capacity. Naturally, during each day the child goes through oscillations of thought, and both the adolescent and the adult are far from reasoning formally all the time. The attainment

of a cognitive stage merely means that an individual under optimal conditions becomes capable of behaving in a certain way which was impossible for him before.

PIAGET:

This is the same point that Bowlby raised when in his reply to my essay he said 'I wonder if Piaget accepts the idea 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 and in others none at all, and that in respect of any one activity we may shift from one level to another?'

Well, I fully accept this idea. Our cognitive functions are certainly not uniform for every period of the day. Although I am mainly engaged in intellectual operations, I am for example at an operatory level for only a small part of the day when I devote myself to my professional work. The rest of the time I am dealing with empirical trial and error. At the time when I drove a car and my engine went wrong it was even empirical trial and error on a very low level, as you can imagine. Every moment I am indulging in pre-operative intuition. At other times I go even lower and almost give way to magical behaviour. If I am stopped by a red light when I am in a hurry it is difficult for me not to link this up with other preoccupations of the moment. In short, the intellectual level varies considerably, exactly like the affective level, according to the different times of the day, but for each behaviour pattern I think we shall find a certain correspondence. For example, for a primitive emotion a very low intellectual level, and for a lofty aesthetic or moral sentiment a high intellectual level. We shall always have this correspondence between the two aspects.

BERTALANFFY:

I think we all agree that we do not invariably find stages, but in certain phenomena we find more or less continuous curves, and in others, distinctive steps. The growth curve of body-weight in a fish, for example, is quite smooth and without inflection. In the rat, however, a lower mammal, the curve is in general similar, but a more detailed analysis reveals that it is actually composed of two 'growth cycles', the transition from the first to the second cycle being relatively sharp and corresponding to the beginning of puberty. Finally, in man there is a very apparent growth-cycle added to the basic curve at puberty—the increase in growth-rate that Tanner calls the adolescent spurt (see Vol. I, p. 36). I want to emphasize that such steps are real. In physiological phenomena in the rat, for example, you find continuity with respect to certain characteristics and with respect to others you have breaks. For example, make an allometric plot

FIG. 2

RELATIVE GROWTH IN THE ALBINO RAT
(A) SIMPLE ALLOMETRY. (B) CHANGES IN ALLOMETRY

The discontinuities in relative growth appear at a body weight of circa 100 g., i.e. the time of puberty. A similar break is found in the growth-in-time curve of the rat. The figures give only regression lines: complete data and statistical analysis in Bertalanffy and Pirozynski (1952, 1953); Racine (1953).

