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Comments on Professor Piaget's Paper

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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'.

1. Professor Piaget's first question to me was whether there is not a danger of vitalism surreptitiously introduced by my attitude to the 'prioric' 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 things that conform to Kant's definition of the *a priori* — e.g. things that exist in our mind before any experience and which must be there in order to make experience possible — are not things that exist in the absolute. Nothing is really there *a priori*. All the forms and functions of our mental processes that really exist independently of experience are related to the form and function of our central nervous system and have developed in phylogeny just as have the form and function 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. Nobody in the world is less of a preformationist than 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 empiricism'.

2. 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 (see page 24). If I may substitute 'adaptive interaction' for 'equilibration', as I assume I may, the answer is

simply and emphatically yes. (I agree with Professor Bertalanffy's objection to the term equilibrium: see on page 94 below). 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 higher animals have developed under the pressure of natural selection in the service of that general regulative equilibration. 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' (p. 24) 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. But 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.

3. 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, though, 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. Exactly the same applies to all the 'laws' prevailing in the function of our brain.

4. The last question is whether there are any objective criteria for distinguishing, in cases of conflicting motivation, mere compromise solutions 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 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 (see page 69), 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 seem to be 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 neurophysiology 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 hindrance to mutual understanding not to use such terms.

Another example: at our last meeting I was trying to explain the controlled use of Gestalt perception in the study of animal behaviour (Vol. III, p. 122). 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 is similarly fundamental. 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 can be described tolerably well in the terminology of Von Holst's studies on central co-ordination, while the conception of the innate releasing mechanism which is only functionally determined cannot 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 a cooled 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 human, etc., etc. All injunctive conceptions merge, without any clear boundary-line, into neighbouring ones which have one or several part-constituent characters in common. 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 'bow-wow' we do not ourselves know whether we mean this dog, any dog, any mammal, any four-legged animal or perhaps anything alive. It is quite difficult to find out what part-constituent properties one enjoins oneself, if one wants to place 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 uses 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 perception, 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. The conception of the case-history, which formerly did not play any role at all in our daily work, now looms very large indeed. Conversely, I find some of our study group, particularly 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 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 endoderm 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 clearest and most 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. Bowlby has already proposed a view of psycho-physiological development which makes use not only of Goldschmidt's principle of harmonized reaction velocities (page 36 in his comments see on); 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 Archimedean 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 the 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 this 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 invaluably simplified 'model', we found that practically all disturbances of sexual function 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 need 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 whether the variability of developmental velocities in the child is any less than it is in the wild goose and I therefore emphatically agree with the objections to the typification of 'stages' in the development in humans. 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 between the 'stages' in the developmental processes he investigates and those that some one else is studying. Each of us ought to be searching constantly for lawful and harmonizing interactions between the processes he himself is working on 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 any particular 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.