The second criterion used is that of homology of action. Most mutant genes are hypomorphic : they fail to bring about the same process as successfully as the non-mutant. If two determinants can be shown to be affecting different processes, or different stages of a process, they are regarded as belonging to distinct genes.

The third criterion is that of independence in mutation, *i.e.* change. If mutations can occur independently of one another, they must be changes in distinct genes.

The great success achieved in associating genetically inferable linkage groups with cytologically visible chromosomes has led to the attempt to relate genes within the group to chromomeres or bands visible within the chromosomes. This would give a fourth criterion—visible separation—for distinguishing genes ; but a number of observations have cast doubt on the validity of this method of inference.

Finally, we could define a gene, *a priori*, as a unit of self-reproduction ; but no means are available for using such a criterion in practice.

The criteria of transmission, action, and mutation lead to identical inferences in a great majority of cases. In a few they do not. Sometimes the special properties of polyploidy or crossing-over serve to explain the disparity, but a number of difficult cases still remain. The reason for this may well be that our inferences of the genotype are limited by the necessity for making all observations on the phenotype. We have no means of direct observation of the genotype : all inferences in genotype must be inferred from differences in action as expressed in the phenotype. Thus the unit of action is the fundamental one in that different changes within such a unit will have similar effects on the phenotype, and recombinations within the unit will not be detectable.

Now the action of any piece of genetic material must depend on the circumstances in which it is acting, and these circumstances are clearly changing during development. A determinant detectably acting at one stage may not be detectably acting at another, so that genes will appear to have their characteristic times of action. It is also conceivable that a gene may act as one unit in some circumstances, and as several units in others, so that whether we regarded it as one or several units would depend on whether we looked at the organism as a whole or whether we compared different organs or stages. Several cases which may well be examples of such a relation are now known.

If this interpretation is correct, we should be led to picture the chromosomes as having a constant physio-chemical structure, and indeed a constant genetical potential throughout development ; but at the same time as having an effective action which could vary with the changing circumstances of development, and by so varying, alter in its content of those genes which we could infer from comparative studies of the phenotype.

The Philosophical and Experimental Aspects of Psychology

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The kind of philosophy that is important to an experimental science is that which is a technique for the removal of muddles created by words. The lack of philosophical interests in experimental scientists is liable to create a situation in which the activity of theorising is far lower in quality than the activity of designing and carrying out experiments. This situation very largely exists in experimental psychology as a result of the history of the subject

as an offshoot of philosophy. This has led to a separation of experimental psychology as an activity carried out in psychological laboratories from philosophical psychology which is studied in departments of philosophy and which tends neither to be informed by nor to act on experimental activities.

A typical muddle in psychology is such a question as : " Does green exist in the outside world or only in the mind ? " This is a typical example of a question of linguistic use masquerading as a question of fact. The sensible question is as to whether we shall decide to use the word " green " in one of two ways. This is a purely linguistic question of no great importance ; in ordinary speech it is used in both ways, and there are various means of making the distinction between these uses for the purposes of more exact thought ; *e.g.* by referring to " physical " green and " phenomenal " green. No philosophising is necessary to warn the experimental psychologist from this particular problem since he has been trained to avoid it or any question that looks like it. A more clearly understood theory of theories might, however, be of value to him in avoiding other muddles of the same general character. A certain amount of what passes as theorising by experimental psychologists is the creation of such muddles. An experimental psychologist may, for example, put forward such a theory as that " consciousness is the integrated action of the organism " without realising that he is merely putting forward a novel (and obviously inconvenient) way of using the word " consciousness " and not a hypothesis which could be supported or refuted by experimental evidence. Nor does there seem to be any good ground for maintaining that there is a discipline of " philosophical psychology " in which such problems may be taken seriously. Whether we are philosophers or experimental psychologists, all that we need to do is to show the linguistic character of such problems and consequently their meaninglessness when treated as questions of fact. Then we can pass on to the task of creating a body of theory in psychology comparable with the theory of any other science, that is, to create a language in which to co-ordinate its experimental findings.

Psychology, like other branches of science, deals with terms referring to a physical world and with terms referring to conscious experience. Let us call these System A and System B respectively. We cannot specify the field of psychology by saying that it deals entirely with propositions belonging to System A ; indeed the school of behaviourists had the ideal of a psychology whose terms should belong entirely to System B. In practice, the psychologist, like other experimental scientists, is concerned with co-ordinations into which terms from both System A and System B enter.

Many of the problems of theoretical psychology are closely parallel to theoretical problems in the natural sciences. There is, for example, the old problem of the status of such mental faculties as " will ", " imagination ", etc., which has reappeared in more recent times as the problem of the status of the " mental factors " derived by the procedure of factor analysis. This is much the same problem as that of the status of the electron, and is not essentially different from that of the status of the physical object. In the co-ordination of our experience we use substantives ; these stand for just those properties that are contained in our experiences. When we ask whether there are real objects referred to by the substantives, we are asking a question too vague to be meaningful. There are further real questions. For example, we may ask of mental factors whether they are located in a specific part of the brain or whether they are inherited as Mendelian factors. These are questions to which the answer can be found by research, but at no point of such research shall we have proved that we are dealing with " real " or " unitary " mental entities. That is a pseudo-problem, as is the question of whether there is a " real " electron or a " real " physical tree.