THE GENERIC NATURE OF THE CONCEPTS OF STIMULUS AND RESPONSE*

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I

In the description of behavior it is usually assumed that both behavior and environment may be broken into parts, which may be referred to by name, and that these parts will retain their identity from experiment to experiment. If this assumption were not in some sense justified a science of behavior would be impossible; but it is not immediately clear to what extent it is supported by our observations. The analysis of behavior is not an act of arbitrary subdividing, and we cannot define the concepts of stimulus and response quite as simply as "parts of behavior and environment" without taking account of the natural lines of fracture along which behavior and environment actually break.

If we could confine ourselves to the elicitation of a reflex upon a single occasion, this difficulty would not arise. The complete description of such an event would present a technical problem only; and, if no limit were placed upon apparatus, an adequate account of what might be termed the stimulus and the response could in most cases be given. The advantage would be that we should be free of the question of *what* we were describing. But when we insist upon a reproducible unit, as we cannot help doing if we are to have a science of behavior, the account of a single elicitation, no matter how perfect, is inadequate. For it is very difficult to find a stimulus and response which maintain precisely the same properties upon two successive occasions. The possible (and very rare) exceptions to this rule concern only very simple stimulating forces acting upon simple (and usually simplified) preparations. In the intact and unhampered organism (to which our laws must, eventually at least, apply) most stimuli are subject to the momentary orientation of receptors or to similar factors; and especially where the stimulus is selected through the action of prepotency (which is the case in the greater part of normal behavior), it is extremely difficult to give any clear account of how the stimulating energies are going to act. The reasons are not quite the same on the side of the response, since the stimulus-response relationship is not symmetrical, but the rule is equally well obeyed. Even in such a relatively simple example as the flexion reflex two successive responses will be found to differ widely if the character of the movement is closely enough examined.

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We are accustomed to deal with this problem by main force. We confine our study to a reflex in which the response is originally of a very simple sort or may be easily simplified (flexion, for example, or salivation) and in which the stimulus is of a convenient form, may be localized sharply, and is applied, rather than selected through prepotency. It is easier to restrict the stimulus than the response, since the stimulus presents itself as the independent variable, but we are able by technical means to control some of the properties of the response also. In this way we devise a sort of reproducibility; that is to say, we are frequently able to describe a restricted preparation in which a stimulus is correlated with a response and all properties of both terms are capable of specification within a satisfactorily narrow range.

For many purposes a preparation of this kind may be an adequate solution of the problem of reproducibility. As we shall see later, some degree of restriction is probably always required before successful experimentation can be carried on. But severe restriction must be rejected as a general solution. It necessarily implies an arbitrary unit, which does not fully correspond to the material originally under investigation because its exact character depends in part upon the selection of properties. Likewise, it is not a solution that can be extended to a very large number of reflexes. Above all, it suppresses, by virtue of the very act of restriction, an important characteristic of the typical reflex. It is with this last objection that we shall be especially concerned.

II

One way to show the inadequacy of the restricted preparation is to determine how much of either the stimulus or the response is

essential or relevant to the correlation between them. In a preparation of the flexion reflex we are able, by reason of certain technical restrictions, to state a correlation between two terms fairly completely described. But on the side of the stimulus we must admit that, so far as a mere correlation is concerned, the exact location that we have given is unimportant—that the correlation could be shown even though the stimulus were applied elsewhere within a rather wide range. Similarly, we need not specify the form of the energy (whether it is heat, for example, or pressure, or electric current) or the duration of its administration or its amount within rather wide limits. A reduction to terms of afferent nervous impulses would eliminate part of the problem of the form of energy but not that of the irrelevance of the other properties. On the side of the response, likewise, we need not specify the rate or degree of flexion; and if we have not simplified we can not specify the exact direction, or, having simplified, we cannot justify the selection of one direction as against others. Most of the properties of the two events in the correlation are, so far as the mere elicitation of the reflex is concerned, irrelevant. The only relevant properties are flexion (the reduction of the angle made by adjacent segments of a limb at a given joint) and a given ("noxious") kind of stimulation applied within a rather large area.

If we turn, then, from the exact reproducibility of stimulus and response to the criterion of simple elicitability, we arrive at nothing more than a correlation of two defining properties. In ordinary practice these properties alone maintain their identity from experiment to experiment. But it would be inconvenient to regard a reflex as a correlation of properties. We cannot produce one defining property at a given elicitation without giving incidental values to the non-defining properties which compose the rest of the event. A stimulus or a response is an *event,* that is to say, not a property; and we must turn, therefore, to a definition on the principle of classes. Accordingly, if we are to continue to regard the flexion reflex as a single entity, both the stimulus and the response must be taken (tentatively, at least) as class terms, each of which embraces an indefinitely large number of particular stimuli or responses but is sufficiently well defined by the specification of one or two properties.

The alternative to acknowledging this generic nature is to argue that every possible restricted correlation is an independent unit in itself. On this hypothesis there are practically an infinite number of flexion reflexes, corresponding to the product of the number of ways in which an affective stimulus can be applied into the number of particular responses that can be obtained through different methods of restriction. We may contrast these two views by saying that either a reflex is a broad term expressing the correlation of a class of stimuli with a class of responses (where the reproducibility of non-defining properties is unimportant) or it applies to any one of a group of particular correlations (where the terms have been severely restricted to obtain the reproducibility of all properties). In the second case we may still group our specific correlations together on the basis of a defining property without implying the functionally generic nature of either stimulus or response: even if there are practically an infinite number of flexion reflexes, for example, they all have something in common not shared by any other, in that their responses are examples of flexion. If we wish to assign the term reflex for the moment to a group of this sort, rather than to a particular example, our problem may be stated in the following form: is a reflex a correlation of classes or a class of correlations?

There is a statement of the subject that differs only slightly from the present (although it is much less flexible), in which what we have called the irrelevance of the non-defining properties of a stimulus is expressed by speaking of a group of stimuli, all of which are *equivalent* in the elicitation of a response. The kind of proof usually given for this view is based upon the fact that in the process of conditioning (Pavlov's type) a new reflex is created. It is then possible to prove the irrelevance of certain properties (or the equivalence of stimuli) in the following way. Let a conditioned reflex be established to a light, for example, which is so placed that only a limited region in the retina of one eye is illuminated. Then it may be shown that after the conditioning is complete a beam of light striking other parts of either retina will elicit the response. The effectiveness of the newly conditioned stimulus is independent of the property of location, and so far as the simple correlation of stimulus and response is concerned we need not specify its location in our description, at least within wide limits. We may also find that the

properties of brilliance, hue, shape, and size may not be significant over considerable ranges, and that the only important properties are, indeed, those denoted roughly by "spot" and "light." Here, then, we have a class of stimuli, defined by two properties, the members of which are equivalent so far as the elicitation of a response is concerned.

The advantage in using a conditioned reflex lies in being able to show that members of the group differing from the particular stimulus used for conditioning cannot be eliciting responses "on their own account" since they were not able to do so before the conditioning was set up. But unfortunately this proof is of limited scope. It is not easily applied to the case of the response and is of no value for unconditioned reflexes or conditioned reflexes the history of which is not known. An indefinitely large number of stimuli may, through the use of conditioning, be made to evoke the same response (a spot of light and a tone, for example, may both elicit salivation), but there need be no common property among them except that of being a stimulus, which is not in itself a property that will guarantee the effectiveness of an untried stimulus known to possess it. The "equivalence" of a spot of light and a tone is the product of an experimental procedure and is clearly not the equivalence found in the case of two spots of light; but the ability to elicit a common response does not distinguish between the two sorts, and no distinction is, as we have said, possible when we do not know the history of the organism.

A better proof, which is applicable to all cases, makes use of the secondary laws of the reflex (1). It is often true in the investigation of these laws that the *number* of elicitations of a reflex is important, as, for example, when we are measuring a rate. It is then possible to test the irrelevance of a non-defining property by showing that two responses, one of which possesses the property, the other not, contribute equally well to a total number. Suppose that we are studying the behavior of such an organism as a rat in pressing a lever. The number of distinguishable acts on the part of the rat that will give the required movement of the lever is indefinite and very large. Except for certain rare cases they constitute a class, which is sufficiently well-defined by the phrase "pressing the lever." Now it may be shown that under various circumstances the rate of

responding is significant—that is to say, it maintains itself or changes in lawful ways (2-10). But the responses which contribute to this total number-per unit-time are not identical. They are selected at random from the whole class—that is, by circumstances which are independent of the conditions determining the rate. Not only, therefore, are the members of the class all equally elicitable by the stimulation arising from the lever, they are *quantitatively mutually replaceable.* The uniformity of the change in rate excludes any supposition that we are dealing with a group of separate reflexes and forces the conclusion that "pressing the lever" behaves experimentally as a unitary thing.

An almost parallel argument could be made from the same data on the side of the stimulus, yielding a stimulus-class sufficiently well denoted for our present purposes by the term "lever." The proof by appeal to secondary laws is much stronger than the argument for equivalence of stimuli based upon the behavior of newly conditioned reflexes. It is of general validity and goes beyond the use of mere "ability to elicit" to a quantitative measure. Thus in our test case we could distinguish between the separate correlations of a single response with a tone and a spot of light by showing, for example, that the extinction of one of them does not modify the state of the other.

An exception may be taken to this last example on the ground that there will probably be *some* influence between the two, and this brings us abruptly to an important point. The argument on the basis of secondary laws would be unanswerable if it were as clear cut as we have given it, and it would decide the question clearly on the side of the reflex as a correlation of two generic terms rather than as a class of distinct correlations or any one member of such a class. But unfortunately the argument must be qualified, and in such a way as to strengthen the opposite view. For it is true that the non-defining properties are often not wholly negligible and that the members of our classes are consequently not exactly mutually replaceable. On the side of the response, of which we have less control, our data will not show this in most cases because of the present lack of precision. But it is certain that there are outlying members of a class which have not a full substitutive power; that is to say, there are "flexions" and "pressings" that are so unusual be-

cause of other properties that they do not fully *count as such.* It ought to be supposed that lesser differences would be significant in a more sensitive test. If we should examine a large number of responses leading to the movement of the lever, most of these would be relatively quite similar, but there would be smaller groups set off by distinguishing properties and a few quite anomalous responses. It is because of the high frequency of occurrence of the first that they are typical of the response "pressing the lever," but it is also because of this frequency that any lack of effectiveness of atypical responses is not at present sufficiently strongly felt to be noted.

On the side of the stimulus, on the other hand, small differences may be demonstrated. Since we may here control the values of our non-defining properties, we may mass the effect of a given example. Thus we can show that in the flexion reflex fatigue from one locus of stimulation does not result in complete fatigue of the reflex from another locus. Here we have segregated particular stimuli into two groups on the basis of the property of location, and have shown the relevance of the property to the course of a secondary change. A similar and very important example of the use of segregation arises in the behavior of the intact organism in the process of discrimination. Suppose we have established a conditioned response to a lever, as in the above example. Upon any one occasion the stimulus is, as we have seen, any member of an indefinitely large class of stimuli arising from the lever and the surrounding parts of the apparatus. It is possible to control some of the properties of these members. For example, the lever may be made to stimulate either in the light or in the dark, so that all properties which arise as visible radiation can be introduced or removed at will. We require to show that they are not wholly irrelevant. This may be done by setting up a discrimination, so that the strength of the response to the lever-plus-light remains at a given (say, nearly maximal) value, while the strength of the response to the lever alone declines to another value (say, nearly zero). Although a discrimination of this sort is in part the development of a distinction that did not originally exist, it can be shown that some significance originally attached to the differentiating property (7-10).

In either of these cases if we had allowed the stimulus to vary at random with respect to the non-defining property, we should have

obtained reasonably smooth curves for the secondary process, according to our present standards of smoothness. It is only by separating the stimuli into groups that we can show their lack of complete equivalence. But once having shown this, we can no longer disregard the importance of the property, even in the absence of grouping. A similar argument would apply, of course, if our criterion were simply ability to elicit. Here the relevance of non-defining properties (or lack of equivalence) can perhaps be shown only at near-threshold states of the reflex, since the measure is all-or-none and therefore crude, but we cannot assume that at other states a similar relevance would not be detected with a more sensitive measure. In neither case have we a clear indication that the argument for a generic definition is wholly valid.

In regarding every discrete correlation as a separate entity, both of the above proofs may be explained away by appeal to "induction"—a process through which a change in the state of one reflex is said to induce a similar change in the state of another. The apparent mutual replaceability of a number of flexion reflexes in the course of a secondary change is explained by holding that something done to one of them (fatigue, for example) is done to others also through induction. The principle is obviously designed to deal with the effects we have just appealed to, and it has the advantage that where the argument for equivalence or a generic term falls short of complete experimental support, the argument for induction is strengthened: for it might be expected that a mutual influence of this sort would be only partial, as it proves to be, and would, moreover, depend upon the degree of community of properties, as it can be shown to depend. On the other hand induction is under the present circumstances clearly an *ad hoc* device, and its use should lead us to suspect the view that every particular correlation is a discrete and autonomous entity.

We have, in short, no clear basis for choosing either of these two views, and the decision we are likely to make is free to follow our personal prejudices. If we are interested in the physiological events mediating a reflex we shall very probably want to deal with severely restricted preparations and we shall be willing to explain away the proofs for the generic nature of the reflex by bringing in the device of induction. If, on the other hand, we are interested in the be-

havior of the intact organism, where restriction is much more difficult and in many cases impossible unless the material is seriously disturbed, we shall be anxious to prove the irrelevance of non-defining properties and shall want to define our unit without respect to them. But it ought to be clear from our failure to find a valid proof for either of these extreme views that the truth lies between them. There is no reason why a clear definition of a unit of this sort is not possible in our present state of knowledge. The problem of definition is, after all, an experimental one, and the entities that we are to use in the description of behavior are experimental entities. We have placed ourselves at a great disadvantage in trying to find among our data evidence for a preconceived term, when our primary concern ought to be simply with putting the data in order; and we may well suspend for a moment the question of the nature of these terms and turn directly to an examination of the available experimental material.

III

1. One fact that seems to be sufficiently well established is that there are defining properties. Nothing we have considered of the importance of non-defining properties modifies this in the least, nor are we prejudging the present issue, since a property may be taken, as we have seen, to define either one reflex or a class of reflexes. A defining property appears on the side of the response in the first step toward what is called the discovery of a reflex. Some aspect of behavior is observed to occur repeatedly under general stimulation, and we assign a name to it that specifies (perhaps not explicitly) a defining property. Our control over the response is almost exclusively of this sort—specification. We have the refusal of all responses not falling within the class we have set up. Since we are completely free in this first choice, it is easy to select a wrong defining property, but the following steps cannot then be taken successfully. When a defining property has been decided upon, the stimuli that elicit responses possessing it are discovered by exploration. One stimulus may be enough to demonstrate the sort of correlation sought for, but (either deliberately or through lack of control) the properties are usually varied in later elicitations and other members of the class thus added. Subsequently the defining property of the stimulus is inferred from the part common to the different stimuli that are thus found to be effective.

There must be defining properties on the sides of *both* stimulus and response. Otherwise our classes will have no necessary reference to real aspects of behavior. If the flexion reflex is allowed to be defined simply as the class of all reflexes having flexion as a response (or as a reflex having for its response a class defined by flexion) there is nothing to prevent the definition of an infinite number of reflexes upon similar bases. For example, we could say that there is a reflex or class of reflexes defined by this property: that in the elicitation the center of gravity of the organism moves to the north. Such a class is experimentally useless, since it brings together quite unrelated activities. But we must be ready to show that all flexions are related in a way in which all movements of the center of gravity are not, and to do this we must appeal to the observed fact that all flexions are elicitable by stimuli of a few classes. As soon as this relation is apparent our tentative response-class begins to take on experimental reality as a characteristic of the behavior of the organism.

It is difficult, however, to say precisely what defining properties are. We frequently define the stimulus by the very doubtful property of its ability to elict the response in question rather than by any independent property of the stimulus itself. Thus, in the behavior of the unhampered organism with respect to some object in its environment, we often cannot describe the actual stimulating energies, but we assume that, whenever a response is elicited, some member of the class of effective stimuli has acted. Similarly in the flexion reflex the basis for our definition of the property "noxious" is probably only the effectiveness of a certain form of energy in eliciting a response. It is always implied, of course, that a parallel definition in terms peculiar to the stimulus can be given. An exception is the case already noted of the type of conditioned reflex in which we cannot define the stimulus except by ability-to-elicit or by appeal to the history of the organism. Fortunately we do not reverse the direction of this argument and define a response as any behavior elicited by a given stimulus. Behavior is less under experimental control than environment, and it would be more difficult to detect a significant correlation in that direction. But at this

level of analysis the response is seldom clearly defined in *any* way. A rigorous definition without regard to non-defining properties is, in fact, probably impossible because, as we have seen, the defining property can be made to fail by taking extreme values of other properties. Nor are the actual members of either class ever exhaustively investigated; so that it may be said that these broad terms are defined neither by specification of properties nor by enumeration.

2. Aside from avoiding a wrong defining property, which will not yield a correlation with a single stimulus-class, we have a certain freedom in specifying the response. By including other non-defining properties in our specification we may set up other and less comprehensive classes, for which corresponding stimulus-classes may be found. The latter will be less comprehensive also, since, as we have seen, the stimulus-class that we arrive at is always closely adjusted to the response. For example, if we begin with "flexion in a specific direction only," we obtain a stimulus-class embracing a smaller stimulating area. Now, there is nothing to prevent our taking such a restricted unit at the start, so long as for any such class a stimulus-class may be found, and if a restricted unit is taken first the very broadest term can be arrived at only by removing restrictions.

Our second experimental fact is that within the class given by a defining property we may set up subclasses through the arbitrary restriction of other properties. This procedure yields a series of responses, generated by progressive restriction, each member of which possesses a corresponding stimulus in a more or less parallel series. At one end we approach as a limit the correlation of a completely specified response and a stimulus which is not necessarily strictly constant but may be held so experimentally. If at this point both terms are in fact unit classes, one part of our problem vanishes, since with a perfectly restricted preparation there is no practical difference between a class of correlations and a correlation of classes. But this state is, as we have argued, probably always impractical and in any event never fully representative. Our interest in it here is as an ideal limit. The other end of the series, the unrestricted class, we have also seen to be ideal, so that any experimentally valid unit must be sought for among the partially restricted entities lying between these extremes.

In speaking of a *series* generated by restriction we are, of course, using too simple a term. Our technique of restriction must respect the defining property, but that is our only important limitation. Through the selection of different non-defining properties we may set up different restricted entities within a single class; for example, in restricting the flexion reflex by fixing the locus of stimulation, we may obtain separate entities by selecting different loci. There is no unique set of non-defining properties peculiar to a given defining property, and we have to deal, not with a single series, but with a complex set of ramifications from a single virtual source, approaching as limits an indefinite number of different completely restricted entities.

Part of the difficulty of definition that we encounter in dealing with a single defining property (point 1 above) may disappear in the partially restricted preparation. Usually the first restrictions are designed to protect the defining property by excluding extreme cases. They clarify the definition and add weight to the expressed correlation with a stimulus-class. In general, as we progressively restrict, our description comes to include more and more of the two events and is consequently so much the more successful. At the same time a greater and greater restriction of the stimulus-class is demanded, so that the increase in the validity and completeness of the correlation is paid for with added experimental effort.

3. Our third fact is induction, which it is now possible for us to demonstrate without raising the question of a unit. We have seen that it is possible to obtain various kinds of entities within a single class through the restriction of non-defining properties and that many of these may exist at the same time. They are experimentally real and operable, and there can be shown between two given examples some degree of mutual influence of the sort we have already examined. A change taking place in one of them is found to have taken place also in the other. The only important rule of induction that we need to note is that the extent of the mutual influence is a function of the degree to which the entities possess their non-defining properties in common. We shall not review other information in any detail. The literature is very large, especially if we include (as we rightfully may) all work on discrimination. It is an important field of analysis, although its relation to the problem of the definition of a reflex has usually not been made clear.

4. In turning to induction we have necessarily taken up new criteria. Classes or subclasses may be demonstrated simply by showing correlations of stimuli and responses and by listing the properties of these events, but the influences exerted by one restricted entity upon another are felt principally in the course of secondary changes. Our fourth point is that, in the measurement of these more advanced aspects of a correlation, movement along a series in the direction of a completely restricted entity is accompanied by an increase in the simplicity and consistency of our data. If we are measuring fatigue, for example, we shall not obtain too smooth a curve if our stimulus varies in such a way as to produce at one time one direction of flexion and at another time another; but as we restrict our stimulus to obtain a less variable response, the smoothness of the curve increases.

This is not really a separate point but rather a special case of point 3. In such a secondary process as fatigue or extinction we are examining the effect of one elicitation upon another following it. But this is only induction, since we are not yet assuming any kind of identity from one occasion to another. We look for this effect to follow the main rule of induction: it will be a function of the degree of community of properties. In a completely restricted preparation we should therefore have complete induction, since two successive elicitations would be identical. Each elicitation would have its full effect upon a secondary change, and the curve for the secondary change would be smooth. But if we are using only a partially restricted entity, successive elicitations need not have identically the same properties, and secondary processes may or may not be advanced full steps through induction. From our third point, therefore, we could have deduced a form of the fourth, namely, that an improvement in data follows from any change that makes successive elicitations more likely to resemble each other.

5. If induction followed properties quite literally and without prejudice, its study would not add anything to our knowledge of the relationship between two entities that we could not infer from a comparison of properties alone. But properties are not all equally important so far as the induction between two members is concerned. The structure that we have set up has so far been based solely on community of properties. Any distinction whatsoever

between responses has been allowed, so long as the test of correlation with a stimulus-class was forthcoming. Now, it may have been noticed that an attempt to distinguish between two response-classes upon the basis of some property has failed. It may not have been possible to find two corresponding stimulus-classes that elicited them separately. But if we have one stimulus-class corresponding with two response-classes, we cannot be sure of confirming either correlation on a given occasion to the exclusion of the other. We must conclude, therefore, that the property upon which the two classes have been distinguished is not effective. This variable importance comes out clearly in the study of induction, and it is important enough to be stated separately as our fifth point.

The most general form of the rule, in agreement with the present ordering of experimental data, is as follows: practically complete induction may prevail between two entities differing even widely with respect to some non-defining properties. As we have just seen, it may be stated in relation to our second point in this way: some non-defining properties do not establish subclasses. A more limited expression, which takes the form of a qualification of point 4, is as follows: as we proceed with the gradual restriction of a preparation, noting a corresponding improvement in the consistency of our data, the point at which an adequate consistency is reached does not coincide with the final complete restriction of all properties of the preparation. The proofs for this very important rule (especially the proof by appeal to secondary laws) have been given above in arguing for the generic nature of the reflex, and we shall not need to repeat them. We are now, however, including some non-defining properties in the terms to which they apply, and we therefore avoid the objections that were previously raised. In fact, it will be apparent that we have based our selection of non-defining properties upon just the criterion appealed to in those objections—namely, completeness of induction.

This is a practical rule, which does not pretend to go beyond the limits of our present degree of precision. But its main features are too well marked to be seriously disturbed under limiting conditions. A practical consistency may appear at such a relatively unrestricted level—and, as one might say, so suddenly—that extrapolation to complete consistency appears to fall far short of complete restriction.

It would be idle to consider the possibility of details that have at present no experimental reality or importance. It may be that the location of the spot of light or the identity of the muscle-fibres that contract as the lever is pressed are somehow significant up to the point of complete specification; but we are here interested only in the degree of consistency that can be obtained while they are still by no means completely determined. This consistency is so remarkable that it promises very little improvement from further restriction.

As a matter of fact, when we have reached the point at which orderly secondary changes appear, we cannot go beyond it with further restriction without destroying this desired result. In the example of the lever, we may obtain smooth curves by restricting up to a certain point only; if we further limit the response by excluding all examples except those of one given kind (pressing with a certain muscle-group, for example), we destroy our curves by eliminating many instances contributing to them. The set of properties that gives us "pressing the lever" is uniquely determined; specifying either fewer or more will destroy the consistency of result that we have obtained. This follows naturally from the nature of our control over the response-specification and refusal to accept.

IV

These, then, are the important aspects of the analysis of behavior that bear upon the definition of a unit. We have listed them, not in relation to a definition, but in the order in which they appear in actual experimentation. But the problem of definition has now been practically solved. We have arrived at a structure of entities having an experimental foundation, and we have only to decide to what part of it we are to assign the term reflex.

The two extreme views with which we began may be related to the present result without difficulty. The extreme generic view is that a stimulus or a response is the whole class given by a defining property. But we have seen that this is probably never sharply defined without appeal to secondary properties, and its members are never exhaustively investigated. As a structure it may become prodigious: in the behavior of the intact organism the number of subclasses that could

be set up through discrimination is often practically infinite. This kind of unit yields a sort of reproducibility (that of its defining property), but it is not enough to insure uniform secondary processes. It is not, in short, an experimental concept, and although it might be well to give it a name ("surreflex" for example), we ought to reserve the term reflex itself for an observable entity. For the same reason we cannot accept the definition proposed by the extreme particularist; the fully determined entity approached with the technique of restriction is also, as we have seen, ideal. The material that we actually observe, and which exhibits significant uniformity, is the behavior of the preparation restricted to the point of giving simple and consistent data. Here, if anywhere, it will be convenient to apply our term.

We may restrict a preparation for two quite different reasons, either to obtain a greater precision of reference for our terms (so that our description of a response, for example, will describe it more completely and accurately) or to obtain consistent curves for secondary processes. The increase in precision gives a greater authority to our statement of a correlation, which is desirable; but it will not help us in deciding upon a unit. It leads ultimately to a completely restricted entity, which we have seen to be usually unreproducible and otherwise impractical, and to obtain a unit we should be forced to stop at some arbitrary level, for example, at a compromise between precision of reference and the experimental effort of restriction. Our second criterion, the orderliness of secondary processes, gives us, on the other hand, a unit which is in no sense arbitrary. As we have seen, the appearance of smooth curves in secondary processes marks a unique point in the progressive restriction of a preparation, and it is to this uniquely determined entity that the term reflex may be assigned. *A reflex,* then, *is a correlation of a stimulus and a response at a level of restriction marked by the orderliness of changes in the correlation.*

In certain respects this is not as simple a definition as one might wish for. It means that since many equally consistent preparations may be set up within a single class, there will be a large number of reflexes passing under a single name. This may seem to rob the principle of the reflex of much of its simplicity, but it is a necessary consequence of the complexity of the material, which cannot be

changed by theoretical considerations. If we shall not be able to refer unequivocally to a single experimental entity with the term "flexion reflex," at least we may know that this has never really been possible. A great deal of misunderstanding has arisen from the practice of naming reflexes, which an insistence upon a supplementary list of specifications may avoid.

Likewise, it is not necessarily true that the entities resulting from this definition are so uniform that a law based upon one example will have complete generality. A certain latitude is allowed by our present degree of precision. It is not always easy to prove from the degree of orderliness of a change that a significant property is not varying at random, although the presence of such a variable will probably affect the shape of the curve for the change. Aside from this matter of precision, it is also probable that preparations having different controlled values of a given property will yield different curves. In the case of restriction through the removal of properties (where this is possible), we have a series of preparations of increasing simplicity and of increasing ease of control but not necessarily of increasing constancy. We should not expect an increase in smoothness along such a series, but it is probable that the nature of a curve will show a change. These are, however, experimental questions and our only present task (formulation) has been sufficiently fully carried out. Our definition is not, in any event, dependent upon the generality of the laws obtained with a single example, although the greatest possible generality is obviously desirable.

In deciding upon this definition we choose simplicity or consistency of data against exact reproducibility as our ultimate criterion, or rather we temper the extent to which exact reproducibility is to be demanded and use the consistency of our data in our defense. This would be only good scientific method if we were not forced to it for other reasons. To insist upon the constancy of properties that can be shown not to affect the measurements in hand is to make a fetish of exactitude. It is obvious why this has so often been done. What is wanted is the "necessary and sufficient" correlation of a stimulus and a response. The procedure recommended by the present analysis is to discover the defining properties of a stimulus and a response and to express the correlation in terms of classes.

The usual expedient has been to hold all properties of a given instance constant so far as this is possible. In a successful case all properties *seem* to be relevant because they invariably occur upon all occasions. (It is almost as if, faced with the evident irrelevance of many properties, we had invented the highly restricted preparation to make them relevant.) In giving a complete account of an arbitrarily restricted preparation, we describe at the same time too little and too much. We include material that is irrelevant to our principal datum, so that part of our description is superfluous, and we deliberately ignore the broader character of the stimulus and the response. The complete description of one act of pressing a lever would have very little usefulness, since most of the information would be irrelevant to the fact of elicitation, with which we are chiefly concerned, and would tell us nothing about the set of properties that yield a consistent result.

We do not, of course, avoid nor wish to avoid restriction. It is an indispensable device, for it has the merit of holding a defining property constant even though the property has not been identified. Until we have discovered a defining property, it is necessary to resort to restriction to guarantee ultimate validity. And since, as we have seen, it is often difficult to designate defining properties clearly, especially where extreme values of other properties interfere, some measure of precautionary restriction is usually necessary. It is not often obvious that it is being used. We have spoken of the number of ways in which a lever may stimulate a rat and the number of ways in which the rat may respond. We should find it very difficult to define either of these classes without considerable precautionary restriction of essentially non-defining properties—concerning the size of the lever and so on. The use of a uniform lever from experiment to experiment is in itself a considerable act of restriction and is apparently necessary to assure a consistent result.

Assigning the term reflex to the entities in this part of our structure means, of course, that the reflex is a generic term. That is to say, the "stimulus" and the "response" entering into a given correlation are not to be identified with particular instances appearing upon some given occasion but with classes of such instances. In this sense the generic view has been borne out as against the autonomy of the completely restricted preparation. This is perhaps the most

important characteristic of the definition. Freedom from the requirement of complete reproducibility broadens our field of operation immeasurably. We are no longer limited to the very few preparations in which some semblance of completeness is to be found, for we are able to define "parts of behavior and environment" having experimental reality and reproducible in their own fashion. In particular the behavior of the intact organism is made available for study with an expectation of precision comparable with that of the classical spinal preparation. (Indeed, if smoothness of curve is to be taken as an ultimate criterion, the intact organism often shows much greater consistency than the usual spinal preparation, even though the number of uncontrolled non-defining properties is much smaller in the latter case. That is to say, the generic character is more marked in reflexes peculiar to the intact organism.)

V

The generic nature of stimulus and response is in no sense a justification for the broader terms of the popular vocabulary. We may lay it down as a general rule that no property is a valid defining property of a class until its experimental reality has been demonstrated. This excludes a great many terms commonly brought into the description of behavior. For example, suppose that it be casually observed that a child hides when confronted with a dog. Then it may be said, in an uncritical extension of the terminology of the reflex, that the dog is a stimulus and hiding a response. It is obvious at once that the word "hiding" does not refer to a unique set of movements nor "dog" to a unique set of stimulating forces. In order to make these terms validly descriptive of behavior it is necessary to define the classes to which they refer. It must be shown what properties of a stimulus give it a place in the class "dog" and what property of a response makes it an instance of "hiding." (It will not be enough to dignify the popular vocabulary by appealing to essential properties of "dogness" and "hidingness" and to suppose them intuitively known.) The resulting classes will meanwhile have been shown to be correlated experimentally, but it ought also to be shown that secondary changes in the correlation are lawful. It is not at all certain that the properties we should thus find to be significant are those now supposedly referred to by the words "dog"

and "hiding," even after allowing for the vagueness inevitable in a popular term.

For reasons to be noted shortly the existence of a popular term does create some presumption in favor of the existence of a corresponding experimentally real concept. But this does not free us from the necessity of defining the class and of demonstrating the reality if the term is to be used for scientific purposes. It has still to be shown that most of the terms borrowed from the popular vocabulary are validly descriptive—that they lead to consistent and reproducible experimentation. We cannot legitimately assume that "riding a bicycle," "seeing one's friends," or "heartbreak" are responses in any scientific sense.

This restriction upon the use of the popular vocabulary in behaviorism is often not felt because the partial legitimacy of the popular term frequently results in some experimental consistency. The experimenter is more likely than not to hit upon experimentally real terms, and he may have some private set of properties resulting from his own training that will serve. Thus the word hiding may always be used *by him* in connection with events having certain definite properties, and his own results will be consistent by virtue of this definition *per accidens*. But it is a mistake for him to suppose that these properties are communicated in his use of the popular term. If no more accurate supplementary specification is given, the difficulty will become apparent whenever his experiments are repeated by someone with another set of private defining properties and will be the greater the wider the difference in background of the two experimenters.

We are here very close to a problem in epistemology, which is inevitable in a field of this sort. For the relation of organism to environment with which we are primarily concerned must be supposed to include the special case of the relation of scientist to subject-matter. If we contemplate an eventually successful general extension of our methods, we must suppose ourselves to be describing an activity of which describing itself is one manifestation. It is necessary to raise this epistemological point in order to explain why it is that popular terms so often refer to what are later found to be experimentally real entities. The reason is that such terms are in themselves responses of a generic sort: they are the responses of the

populace of which the experimenter is a member. Consequently, when the organism under investigation fairly closely resembles man (for example, when it is a dog) the popular term may be very close to the experimentally real entity. We may hit immediately upon the right property of the stimulus, not because we have manipulated it experimentally in the manner described above, but because we ourselves react in a measure similarly to the dog. On the other hand, if the organism is, let us say, an ant or an amoeba, it is much more difficult to detect the "real" stimulus without experimentation. If it were not for this explanation, the partial legitimacy of the popular term would be a striking coincidence, which might be used (and indeed has been used) as an argument for the admission of a special method (such as "empathy") into the study of behavior. In insisting that no amount of reality in the popular terms already examined will excuse us from defining a new term experimentally if it is to be used at all, we are of course rejecting any such process. Our rule that the generic term may be used only when its experimental reality has been verified will not admit the possibility of an ancillary principle, available in and peculiar to the study of behavior, leading to the definition of concepts through some other means than the sort of experimental procedure here outlined.

VI

Throughout this discussion we have kept to our intention of dealing with the reflex and its associated processes solely at the level of behavior. We have made no reference to intermediating events in the central nervous system, and, here as elsewhere, this has apparently not caused the slightest inconvenience. But the reader may feel that the present case has involved a special difficulty: the definition that we arrive at may seem to be logical or statistical rather than physiological. To take a simple example, what we should call the defining property of a stimulus is actually the logical product of all observed instances. This is easily said, so far as a consideration of the stimulus is concerned; but when we come to deal with what this means in terms of a central nervous system, it is much more difficult.

The answer of the student of behavior ought to be that this is not his problem. He is interested in a set of concepts adapted to the

description of behavior. The notion of a class or of a defining property is justified in a description of this sort because, so far as behavior is concerned, all problems arising from its use are soluble. That is to say, we have techniques available for demonstrating defining properties, for showing the relative importance of non-defining properties, and for measuring induction. The problem of discrimination, with its subsidiary problem of the establishment of new classes (or, in a broad sense, concepts), can be formulated equally well without reference to a central nervous system. And if these are real aspects of behavior (if nothing has gone wrong in our analysis), they must also be aspects of the activity of the central nervous system, which it is the business of the reflex physiologist to discover—through some other means, incidentally, than inference from behavior. This is a division of labor that ought to be as pleasing to the physiologist as to the behaviorist. A rigorous formulation of the present problem at the level of behavior should be the most desirable starting point for a physiological study and is a necessary condition for the eventual synthesis of the two fields.

VII. Summary

1. In breaking behavior and environment into parts for the sake of description, we cannot take a single instance of the elicitation of a response as a unit because it is not a fully reproducible entity. The usual solution of this problem through forced simplification is inadequate.

2. In a reflex preparation the observed correlation is never between all the properties of both stimulus and response. Some properties are irrelevant. The relevant properties are accordingly taken to define classes, and the reflex is regarded as a correlation of generic terms. The alternative view is that every possible correlation of a particular stimulus and a particular response is a unit in itself. One argument against this alternative, frequently offered in support of the notion of "equivalence of stimuli," is incomplete. A better proof of the generic view is based upon the secondary laws of the reflex. If smooth curves can be obtained in secondary changes while the stimulus and response vary in composition with respect to given properties, these properties may legitimately be regarded as non-defining. In the examples given, however, the non-defining proper-

ties can be shown in several ways to be not wholly irrelevant. This failure adds weight to the alternative view, but the device used to explain away the proofs for the generic view makes the particular unit equally doubtful as an autonomous entity.

3. The observed facts are: (*a*) that there are defining properties (not rigorously described) which establish gross classes of stimuli and responses; (*b*) that by specifying other properties we may set up other and less comprehensive classes in a progressive series or set of ramifications extending from the gross class to completely restricted entities (the latter not necessarily operable); (*c*) that between any two members of such a family we may demonstrate induction and show that it is a function of the degree to which the entities possess their properties in common; (*d*) that in restricting a preparation we obtain greater consistency of result because, from (*c*), we make two successive elicitations more likely to resemble each other; but (*e*) that some properties are largely irrelevant so far as induction is concerned, so that in the progressive restriction of a preparation a point may be reached beyond which further restriction does not yield an improvement in consistency and may yield the opposite.

4. These phenomena, properly considered, lead to a definition of stimulus and response. Both extremes of a series of preparations are non-experimental. There is only one other point in such a series uniquely determined: that at which smooth curves for secondary processes are obtainable. A reflex is accordingly defined as a correlation of a stimulus and a response at a level of restriction marked by the orderliness of changes in the correlation. If this is not an ideally simple definition, it is at least in accord with our data. It is based upon consistency of result rather than exact reproducibility of terms; and it utilizes restriction only in moderation.

5. The generic nature of stimulus and response is not a justification for the use of a popular term until it has been defined experimentally. The objection is not often felt because the popular term may have some legitimacy, due to the fact that the term is itself a generic response—of the populace. Its partial legitimacy is consequently no coincidence, nor an argument for the admission of a principle peculiar to the study of behavior that will allow the definition of concepts through other than experimental means.

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LA NATURE GÉNÉRIQUE DES CONCEPTS DU STIMULUS ET DE LA REPONSE

(Résumé)

1. Quand on divise le comportement et le milieu en parties pour cause de description, on ne peut pas prendre un seul exemple de l'élicitation d'une réponse comme unité parce que ce n'est pas une entité entièrement reproductible. La résolution ordinaire de ce problème au moyen de la simplification forcée n'est pas satisfaisante.

2. Dans une préparation réflexe la corrélation observée n'est jamais entre toutes les propriétés du stimulus et de la réponse. Quelques proprietés ne sont pas pertinentes. Les propriétés pertinentes sont ainsi prises pour définir les classes, et le réflexe est considéré comme une corrélation des termes génériques. La vue alternative est que chaque corrélation possible d'un stimulus quelconque et d'une réponse quelconque est une unité en elle-même. Une argument contre cette alternative, fréquemment offert pour soutenir la notion de "l'équivalence des stimuli," est incomplet. Une meilleure épreuve de la vue génétique est basée sur les lois secondaires du réflexe. Si on peut obtenir des courbes unies dans les changements secondaires pendant que le stimulus et la