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Introductory Note

The extension of the concept of the reflex to the description of the behavior of intact organisms is a common practice in modern theorizing. Nevertheless, we owe most of our knowledge of the reflex to investigators who have dealt only with "preparations," and who have never held themselves to be concerned with anything but a subsidiary function of the central nervous system. Doubtless, there is ample justification for the use of relatively simple systems in an early investigation. But it is true, nevertheless, that the concept of the reflex has not emerged unmarked by such a circumstance of its development. In its extension to the behavior of intact organisms, that is to say, the historical definition finds itself encumbered with what now appear to be superfluous interpretations.

The present paper examines the concept of the reflex and attempts to evaluate the historical definition. It undertakes eventually to frame an alternative definition, which is not wholly in despite of the historical usage. The reader will recognize a method of criticism first formulated with respect to scientific concepts by Ernst Mach [in *The Science of Mechanics*] and perhaps better stated by Henri Poincaré. To the works of these men and to Bridgman's excellent application of the method [in *The Logic of Modern Physics*] the reader is referred for any discussion of the method *qua* method. Probably the chief advantage, first exploited in this respect by Mach, lies in the use of a historical approach. But the reader should understand that in the present case no attempt is made to give an exhaustive account of the history of the reflex. Certain historical facts are considered for two reasons: to discover the nature of the observations upon which the concept has been based, and to indicate the source of the incidental interpretations with which we are concerned.

I

It was Descartes[1] who first proposed a mechanism by which the characteristics of the living organism could plausibly be produced. He came

[1] Descartes' account is to be found in the *Traité de l'homme*, which differs in many respects from the earlier *Passions de l'âme* in its representation of the action of the nervous system.

A convenient account of Descartes as physiologist is given by Sir Michael Foster in his *Lectures on the history of physiology* (London, 1901). The quotations from Nicolas Stensen and the translations from Descartes are taken from this work.

very near describing its action as the true mode of operation of the animal body, but the criticism of his contemporary, Nicolas Stensen, probably expressed his intention correctly. "Descartes," said Stensen, "was too clever in exposing the errors of current treatises on man to undertake the task of expounding the true structure of man. Therefore in his essay on Man he does not attempt such a delineation, but is content to describe a machine capable of performing all the functions of which man is capable." This interpretation is borne out by the text of the *Traité,* where, although parts of the anatomy are again and again pointed out as suitable for the functions of the mechanism, and the machine and body are, indeed, almost identified, the reader is, nevertheless, invited only to suppose the truth of the details. Descartes' interest lay primarily in furthering his philosophical notions, and the invention which is usually taken as the earliest expression of the reflex was little more than an instrument of persuasion.

In designing a convincing model of the living organism Descartes faced a peculiar difficulty. Movement in itself was easily enough obtained, for there were many mechanisms available as sources of energy. There was, for example, the current explanation of muscular contraction upon a hydraulic analogy, which, in fact, Descartes adopted. But if the energy itself was conveniently accounted for, the direction and order of its release were, on the contrary, critical. In meeting this difficulty Descartes introduced a novel device—the mechanism of the stimulus, by means of which external forces released the movements of the machine. The stimulus distinguished the model of *la bête machine* from that of a mere activated doll. It enabled the model to simulate the appropriateness and the apparent spontaneity of the movements of the living organism. So far as Descartes' purpose was concerned, it was successful in supplanting certain metaphysical concepts as causal agents leading to movement. Heretofore, the supposition had been that an animal moved because of the action of, let us say, a "soul." Descartes proposed that the body be regarded as a system of stored energy, and he pointed to minute, hitherto unobserved forces which acted upon the organism in such a way as to serve as releasing mechanisms.

The principle of the stimulus was, of course, little more than a guess. With an enthusiasm for the new physics, Descartes contended that the movements of an organism were functions of the forces acting upon it, but he could in practice point to only the roughest demonstration of this relationship. Subsequent investigation of the reflex has revealed the extraordinary difficulty of identifying the stimulating forces correlated with particular movements. The information available in Descartes' time was so scant, and the principle so far-reaching, that one is tempted to regard the discovery of the stimulus simply as another example of the insight with which Descartes anticipated later thought. But this would be to overlook the influence of an unusual analogy.

Descartes sought a mechanical model of the living organism for the support of an argument. For other reasons, namely, for the sake of the entertainment which they afforded, suitable models (utilizing the action of a stimulus as a source of spontaneity and appropriateness) had already been constructed by the engineers of the royal fountains in France. Descartes describes two of these fountain figures and the action of their releasing mechanisms, which are operated unwittingly by the observers.

> For in entering they necessarily tread on certain tiles or plates, which are so disposed that if they approach a bathing Diana, they cause her to hide in the rose-bushes, and if they try to follow her, they cause a Neptune to come forward to meet them threatening them with his trident. Or if they pass in another direction they occasion the springing forward of a marine monster who spouts water into their faces or things of a like sort according to the caprice of the engineers who constructed them.

A contemporary engineer, Salomon de Caus,[1] published an account of the operation of similar figures, although he did not describe the two groups referred to by Descartes. The mechanical principles are few in number, but among them can be found all of those used by Descartes in his "reflex arc." In Descartes' proposed model the organ of sense is set in motion "even ever so little" by the external object and pulls upon a thread, which in turn, acting like a bell rope, opens a valve at a central reservoir, letting the contained fluid flow outward along a pipeline into the muscles, which it activates. With a plate or lever substituted for the organ of sense and a waterwheel or similar device for the muscles, the description applies as well to the fountain figures. So slightly does Descartes depart from the details of the fountain mechanism that its position as the prototype of his model seems unquestionable.

It was the accident of a convenient analogy which led Descartes to the discovery of an important principle, and so great a mutation was it in the evolution of human thought that it proved lethal. In spite of frequent assertions to the contrary, Descartes seems to have exerted no influence upon the development of the reflex. Instead, the discovery of the stimulus was made again, with great difficulty, as the culmination of a century of experimentation, and another century and a half had elapsed before the principle had again been comparably extended to the behavior of the total organism. This lack of historical influence may be variously explained. Descartes was, as Foster has said, a "retrograde" physiologist, who accepted the more con-

[1] De Caus, *Les raisons des forces mouvantes avec diverse machines . . . . de grottes et de fontaines.* The date (1624) is considerably earlier than that of the *Traité,* and the examples treated by de Caus are, in general, simpler than those described by Descartes.

venient theory, as against the more accurate, for the sake of a broader consistency. His interest was ultimately philosophical, even in his physiological explanations, and he did not attempt to discover the true action of the nervous system.

Descartes is important to an understanding of the reflex, not because of an organic connection with subsequent history, but as a symbol. The stimulus is an essential part of a mechanistic theory of behavior, whether the notion is arrived at through observation, as it was with Marshall Hall, for example, or argued from physical necessity or mechanical analogy, as it was with Descartes. Furthermore, the analysis of behavior which is accomplished in the mere descriptive phrase, "withdrawing the foot from fire," became a critical part of later method. But a further characteristic of Descartes' position must be noted: although he substituted the stimulus for a metaphysical concept in his description of the animal, Descartes could not eliminate metaphysical concepts from his description of man. Here he regarded the mechanical principles as at work, but under the control of the soul, which might suspend the physical necessities much as the engineer might modify the activity of the fountain figures.

Descartes reserved a field of action for the concept of soul, not because the physical facts were any more lacking in the case of man than elsewhere, but because of the pressure of certain metaphysical notions. Fragments of similar reservations still prevail. But the history of the reflex can almost be told by describing the progressive encroachment of the stimulus upon them. The line which Descartes drew between the fields of action of his physical and metaphysical concepts was a temporal one only. A movement might follow at one time the action of a stimulus and at another the action of soul. The later distinction which was first definitely established by Marshall Hall set in part an anatomical boundary. But both lines were drawn for the same purpose, namely, to resolve, by compromise, the conflict between an *observed necessity* and *preconceptions of freedom* in the behavior of organisms. In one form or another, this compromise accounts almost wholly for the aspects of the historical definition of the reflex which we are attempting to reconsider.

II

The concept of the reflex arose again from investigations which had already begun during Descartes' lifetime. They were concerned with animal movement and represented a sudden turn in the history of the concepts dealing with that phenomenon. It is a generalization sufficiently accurate for our purposes to say that the movement of an organism had generally been taken as coexistent with its life and as necessarily correlated with the action of some such entity as soul. The necessary relationship between the action of soul and the contraction of a muscle, for example, was explicit. As a consequence, it was disturbing to find, experimentally, that a muscle could be made to contract after it had been severed from a living organism or even after death. This, however, was the contention of the new physiology.

The demonstration that the volume of a muscle does not appreciably increase during contraction was made by Francis Glisson[1] in the middle of the seventeenth century. He was, as Fulton has shown, probably anticipated here by the Dutch naturalist, Swammerdam, whose influence was less immediately felt. Swammerdam's experiments on excised nerve and muscle were more clear-cut than those of Glisson on the intact limb, but either procedure was convincing and was something more than a mere disproof of the "animal spirits" hypothesis of muscular contraction. The experiment pointed to the existence of a "property of contractility" resident in the muscular tissue and independent of any remote source of energy. Contemporary and subsequent experimentation was of the same import. Glisson himself experimented upon intestinal and skeletal muscle after death, when metaphysical concepts had supposedly ceased to act. Swammerdam's experiments indicated a characteristic activity in excised nerve and muscle, and in 1700 Giorgio Baglivi, the Italian physician, reporting the contractions of isolated muscular tissue, emphasized that this was "without the soul's having any share in it, or even being sensible of it." The experiments published by von Haller in 1739 and 1742 permitted him to make the following claims:

> By my experiments I separated this irritable nature on the one hand from a mere dead force, and on the other hand from the nervous force and from the power of the soul. I shewed that the movement of the heart and the irritable nature of the intestines depended on it alone. I confined it entirely to the muscular fibre. . . . I also shewed that that force was something perpetually living, and that it often broke out into movement though no external stimulus such as could be recognized by us was acting. By a stimulus, however, it could at any time be called back from rest into action. In a movement produced through it I distinguished between the stimulus which might be very slight, and the movement called forth by the stimulus which might be very powerful.

The doctrine of irritability was the theoretical accompaniment of this experimentation. As a property assigned to living tissue, irritability was from

[1] For an account of the events antecedent to the explicit formulation of the concept of the reflex see: (*a*) Foster, *op. cit.*; (*b*) the historical introduction in J. F. Fulton's *Muscular contraction and the reflex control of movement.* Baltimore, Md.: Williams & Wilkins, 1926; (*c*) Ch. 1 in Verworn's *Irritability:* A physiological analysis of the general effect of stimuli in living substances. New Haven, Conn.: Yale Univ. Press, 1913; and, for many quotations from the sources, (*d*) F. Fearing's *Reflex Action:* A study in the history of physiological psychology. Baltimore, Md.: Williams & Wilkins, 1930.

the first clearly defined in terms of the experimental operations which revealed it. In his fundamental experiment, Glisson noted that the gall bladder and the biliary duct bring about a greater secretion when they are irritated. "He argues," as Foster has noted, "that they cannot be irritated unless they possess the power of being irritated. This power of being irritated he proposes to denote by the term *irritability*." Although the concept was not immediately freed of non-physical counterparts, it was essentially a physical hypothesis, which ultimately led to the science of the general physiology of nerve and muscle. Movement, far from being the objective manifestation of the activity of soul, had become an organic process subject to experimental investigation.

As Verworn has said, stimulation and irritability cannot be separated. Irritability, by its definition, implies the action of a stimulus. The doctrine of irritability, moreover, assigns an autonomy of function to the parts of an organism. These were the prerequisites for a formulation of the concept of the reflex. The first expression in harmony with the experimental material was made by Robert Whytt.[1] The genesis of the idea is apparent in a single sentence from that work. The observation is made that muscles will contract not only upon direct stimulation but *"whenever a stimulus is applied to the coats or membranes covering them, to the nerves which are sent to them, or to some neighboring or even distant part."*[2] Step by step, the point of stimulation receds from the locus of the phenomenon with which it is identified. When the stimulus has been spatially distinguished from the response, the inference of a conducting medium is necessary, and a further experiment by Wyatt, suggested to him by Stephen Hales, showed that the spinal marrow was a necessary part of this conducting path, which could not function if the marrow were destroyed. We shall need to return later to discuss the significance of these experiments and certain points of method which they exemplify.

Whytt, it is true, regarded the conducting force as a "sentient principle," which, if it was non-rational, was also non-physical. He thus followed the example of Glisson in his compromise with older concepts. Nevertheless, the observations themselves were independent of Whytt's interpretation. His psychical qualifications, moreover, were in this sense useful: that they permitted him to generalize his principle to various functions of the intact organism, free of the resistance which would have been encountered if the principle had been wholly physical. The application to the vegetative functions was easily made, for it had already been strongly foreshadowed. He

[1] "An essay on the vital and other involuntary motions of animals." Edinburgh, 1751.

[2] The quotation is from the second edition, published in 1763, page 267. The first edition omits "to the nerves which are sent to them."

extended his doctrine also to the field of action of some of the departments of the soul.

III

Whytt left the concept of reflex action very much as Glisson had left irritability: in the position partly of a description of observed fact and partly of a superfluous interpretation. What von Haller had done in establishing irritability as a physiological datum independent of any aspect of soul, Marshall Hall[1] now repeated on behalf of the reflex. Not only, he suggested, could muscular tissue contract solely by virtue of its property of contractility, but a given muscle *in situ* could be brought into action by a train of nervous events which were in themselves acting only by virtue of an intrinsic property. "This principle," he said, "is that termed *vis nervosa* by Haller, *motorische Kraft* or *vis motoria* by Professor Müller, and *excitabilité* by M. Flourens."[2] To describe the series of events which a single instance of this activity comprised Hall accepted the word *reflex,* and he spoke of the principle in general as "the reflex function."

The hypothesis that the phenomena of the reflex arc are only aspects of irritability seems to have been original with Marshall Hall. Of the contributions which he claimed to have made it resisted most successfully the charge of plagiarism leveled against him by his contemporaries and was most often cited in his defense. The hypothesis involved, not a denial of the operation of non-physical concepts, but an exclusion of their operation from the reflex field. In Hall's own estimation, as Sherrington[3] has pointed out, "his chief advance lay in the doctrine of separateness in the central nervous system of the great sub-system for unconscious reflex action, and another great sub-system for sensation and volition." This estimate of himself has been more or less confirmed historically. He is, it seems fair to say, the acknowledged author of the almost immutable distinction between voluntary and reflex action and of the resulting negative definition of the reflex as a form of movement *un*conscious, *in*voluntary, and *un*learned. The emphasis which this brief account has thus far placed upon the relation of the reflex to non-physical concepts may now appear more reasonable. Hall's basic hypothesis

[1] Hall, M. On the reflex function of the medulla oblongata and medulla spinalis. London, 1833, *Phil. Trans. Roy. Soc.,* read June 20, 1833.

[2] From Hall's *Memoirs on the Nervous System. Memoir II.* On the true spinal marrow and the excito-motory system of nerves. London, 1837. Whytt had distinguished between the action of the will and the action of a stimulus, but by the latter he probably intended only "mechanical action" upon a muscle. Whytt also used the word *spontaneous* to aid in the distinction, but in the reverse sense! *Reflex* movements were for him spontaneous, since they are performed by the several organs "as it were of their own accord."

[3] Quoted by Stirling, W. *Some apostles of physiology.* London, 1902.

was simply a restatement of the relationship and must be understood accordingly.

Hall distinguished between four modes of muscular action. *Volition,* he said, acts through the cerebrum, is spontaneous, and affects the muscles in a direct line through the spinal marrow and motor nerves. *Respiration* acts in the same way, is also spontaneous (sic), but its seat is in the medulla. *Involuntary* action is the response of a muscle to direct stimulation (the phenomenon of irritability). The fourth mode (the *reflex function*) involves the spinal marrow and differs from volition and respiration in that it is neither spontaneous nor direct in its course.

> It is, on the contrary, *excited* by the application of appropriate stimuli, which are not, however, applied immediately to the muscular or nervo-muscular fibre, but to certain membranous parts, whence the impression is carried to the medulla (spinalis), *reflected,* and reconducted to the part impressed, or conducted to a part remote from it, in which muscular contraction is effected.

We may neglect the second class, which seems to have been included because of the current knowledge of the respiratory center but was exclusive of neither voluntary nor reflex action, and the third (unfortunately called involuntary), which was only a statement of irritability. For the distinction between the two remaining classes (volition and reflex action), we may consider Hall's available evidence.

Hall defined volition as a form of movement which was (*a*) spontaneous, and (*b*) dependent upon the integrity of the cerebrum. The second characteristic was not in itself sufficiently distinguishing. Hall was familiar with the segmental nature of the spinal functions, and it was a fair supposition that the differential activity found in so uniform a structure might be accentuated in the higher segments. If it was a matter of observation that characteristic movements of the animal disappeared upon ablation of the cerebrum, this was also true for any given part of the cord. Spontaneity, on the other hand, was the more critical factor in the delimitation of volition. But spontaneity, as Hall used it, described in effect only those movements for which no appropriate stimuli could be observed, and the word seems to have had no other meaning. The distinction between reflex and voluntary action rested, then, upon the possibility or the impossibility of the experimental demonstration of stimulating forces.

To the support of his distinction Hall mustered a variety of other facts. He noted that drugs (such as opium or strychnine) discriminate in their action between voluntary and reflex activity, that the brain sleeps, while the spinal marrow never sleeps, and so on. He was, of course, describing an observable phenomenon, which we recognize as the differential functioning of separate reflex system.[1] The observations do not, however, indicate an essential difference between brain and cord, especially since the brain-cord distinction is seldom strictly respected physiologically. Hall's appeal to physiology, like that to anatomy, lacked cogency. His distinction rested primarily upon the single item of spontaneity.

In defining volition as the hypothetical antecedent of movement for which no corresponding stimulus could be observed, Hall left the concept open to extensive modification, for it was implicit in the nature of the reflex that it should, in the course of its growth, disfranchise volition. So far as it concerns behavior, the history of the reflex has been, in fact, essentially the account of the discovery of stimuli and of the concurrent passage of the corresponding behavior from the field of volition into the field of reflex action. Furthermore, in opposing volition and reflex action as mutually exclusive terms, Hall identified the reflex with scientific necessity, and volition with unpredictability. This was the pattern for future controversy, of which we may note two instances.

In 1853 Pflüger,[2] as is well known, questioned the reflex nature of the movements of the spinal frog on the basis of unpredictability. In separate instances of the flexion reflex, he pointed out, the movement of the leg varies widely, although the stimulus is held constant. On the basis of the observed variability, Pflüger postulated a spinal mind, his famous *Rückenmarkseele*. Note that the experimental justification for mind (as for Hall's volition) was the absence of demonstrable necessity, that the function of the non-physical concept was, as heretofore, to account for variability. Refutation of Pflüger's criticism needed only a demonstration that the observed variability was itself a function of collateral stimulation. The necessary observations were first supplied by von Uexküll, and the principle (*Shaltung*) has been elaborated by Magnus. Briefly, they have shown that a given response may be modified through proprioceptive stimulation arising from the posture of the animal. The effects which Pflüger observed in the spinal frog are consequently subject to adequate prediction, and, in this particular instance at least, the variability has disappeared. With it disappeared also its corresponding *Seele*.

Shortly before the beginning of the century Pavlov was engaged in the investigation of the activity of the digestive glands. For much of this activity it was possible to identify the necessary antecedent events (the mechanical

[1] An exhaustive account of the discriminating action of drugs upon separate reflex systems is available in R. Magnus's *Körperstellung*. Berlin: Springer, 1924.

[2] Pfluger, E. *Die sensorische Functionen des Rückenmarks der Wirbelthiere nebst einen neuen Lehre uber die Leitungsgesetze der Reflexionen*. 1853. This controversy, of course, extended well beyond Pflüger's personal participation. It was, in fact, the continuation of a philosophical reaction against Hall's concept and was only accentuated by Pflüger's interpretation of his experimental findings.

or chemical changes acting directly or reflexly upon the glands). The greater part of the normal secretion, however, was unfortunately not under the control of the experimenter. The reader should not be surprised that this was called "psychic" secretion. Pavlov undertook the investigation of this activity. His findings are too well known to call for more than the briefest comment here. Essentially, it was the discovery of the operation of "substitute" stimuli. The nature of the process of substitution (conditioning) and the use of the principle as a method are not important at this point. We may emphasize, however, the aspects of the discovery which exemplify the usual course of reflex investigation. *Given a particular part of the behavior of an organism hitherto regarded as unpredictable* (and probably, as a consequence, assigned to non-physical factors), *the investigator seeks out the antecedent changes with which the activity is correlated and establishes the conditions of the correlation.* He thus establishes, as we say, the reflex nature of the behavior. In traditional practice, upon the demonstration of such a correlation, non-physical concepts dealing with the same subject matter are discarded.

IV

The subsequent development of Hall's formulation could by its very nature approach only one end, namely, the hypothesis that the total behavior of the intact organism might be described in terms of the reflex. This extension was possible only upon the demonstration of necessary correlations in a large body of residual behavior, most of which was mediated by the distance receptors of the head segments. The work of Pavlov may therefore be taken as historically fundamental. His evidence was decisive, if necessarily incomplete. It led to two achievements. The principle of conditioning supplied the extended range of stimulation needed to account for the complex behavior of the total organism, and the demonstration of the reflex activity of the cortex laid siege to the last stronghold of the old anatomical distinctions. The extension of the concept has been further facilitated by the work of Magnus upon reflexes concerned with the maintenance of posture. Magnus, like Pavlov, broadened the field of operation of the stimulus by discovering a large number of specific stimulus-response correlations, and, again like Pavlov, he attacked the anatomical distinctions by demonstrating the reflex nature of the activity of the higher, although chiefly subcortical, centers. The reflex as a concept in the description of behavior has received its most extended systematic support from behaviorism.

The adequacy or inadequacy of the reflex in the description of total behavior seems to be beyond immediate experimental demonstration and, in any event, is beyond the scope of this paper. Some of its implications also need not greatly concern us, as, for example, the fate of the non-physical concepts which are deprived of their field of operation. We may regard the ultimate validity of the concept of volition (as, indeed, of that of the reflex) as beyond any immediate estimation. We are concerned with the reflex as a working concept. What is its nature and how shall it be defined? In particular, we have set ourselves to resolve certain difficulties of definition imposed by the extension to total behavior, where volition (or the practice which it represents) is important for its effects. But perhaps we have reviewed enough of its history and may turn directly to a statement of the argument.

In the history of the reflex one positive characteristic has always been given by the facts—the observed correlation of the activity of an effector (i.e., a response) with the observed forces affecting a receptor (i.e., a stimulus). The negative characteristics, on the other hand, which describe the reflex as involuntary, unlearned, unconscious, or restricted to special neural paths, have proceeded from unscientific presuppositions concerning the behavior of organisms. When Marshall Hall decapitated his famous newt, he pointed quite correctly to the reflex activity of the parts of the headless body, to the observed fact that movement followed, inevitably, the administration of specific stimuli. But his assumption that he had imprisoned in the head of the newt the source of another kind of movement was irrelevant and unsupported. The fact before him was a demonstrable necessity in the movement of the headless body; his failure to observe similar necessities in the movement of the intact organism was the accident of his time and of his capabilities.

Tentatively, then, we may define a reflex as an observed correlation of stimulus and response. When we say, for example, that Robert Whytt discovered the pupillary reflex,[1] we do not mean that he discovered either the contraction of the iris or the impingement of light upon the retina, but rather that he first stated the necessary relationship between these two events. So far as behavior is concerned, the pupillary reflex is nothing more than this relationship. Once given a specific stimulus-response correlation, we may, of course, investigate the physiological facts of its mediation. The information there revealed will supplement our definition, but it will not affect the status of the reflex as a correlation. These are matters, however, which will bear a more detailed treatment, for they present many problems.

V

The notions of both stimulus and response were, as we have seen, essential to the principle of irritability, so that the correlation which we are emphasizing was already present (in its most easily observed form) in the older con-

[1] Disregarding the supposed discovery by Galen and Descartes.

cept. The reflex emerged as a separate principle when a correlated stimulus and response could be spatially distinguished, and we have already commented upon Whytt's insistence upon the possibility of a spatial differentiation. The observation of a correlation between two spatially discrete activities led at once to the inference of a series of intervening events, to the inference, that is, of conduction. Subsequently, the investigation of the events intervening between a stimulus and its correlated response became the particular field of reflex physiology. We shall need to review certain characteristics of its method, and we may turn first to the procedure by which the anatomical structures underlying the mediation of a reflex are identified.

Even for so early an investigator as Robert Whytt convenient material from the physiology of the nervous system was available in interpreting reflex phenomena. Rough descriptions of the activity of end-organ and effector and of the conducting action of nerve were at hand. The investigators of the reflex appropriated this current knowledge but began immediately to refine the references to anatomy. Whytt, as we have already seen, first demonstrated that the necessary relationship between stimulus and response (or, as he expressed it, the "sympathy between different muscles or other parts of the body") was lacking after destruction of the cord; "from whence it seems to follow," he added, "that the nerves. . . . have no communication but at their termination in the brain or spinal marrow." His conclusion, if it was not strictly logical, was made extremely probable by Bell's subsequent differentiation between the functions of the anterior and posterior spinal roots. Although Bell did not expressly subscribe to the reflex doctrine, his experiments are more to the point in its support than in his generalization to "sensation and the power of motion." The "diastaltic arc" of Marshall Hall applied Bell's discovery more explicitly to the principle of the reflex.

The diastaltic arc (eventually spoken of as the reflex arc) embraced an "esodic" (afferent) nerve, a spinal center, and an "exodic" (efferent) nerve. It was an *anatomical* term. The experimental evidence for its close correspondence to the reflex was of the sort we have noted: the impairment of reflex function after anatomical injury (Whytt), the fractional functioning of surgically isolated parts of the arc (Bell), and so on. Subsequently (especially in the neuron theory), the argument was extended to microscopic levels. By similar procedures the gross location of the central part of a given arc is determined. The practice is essential to the doctrine of the segmental action of the cord and was utilized early in the history of nerve physiology (independently of reflex theory) in the localization of higher centers, for example, the respiratory center. Typical examples in reflex investigation may be found in the work of Magnus on various mid-brain preparations. The basic assumption is that, when the ablation of a particular part of the nervous system impairs or abolishes a reflex function, the ablated structure is essential to the reflex and includes part of the arc. That inference, of course, is not unavoidable. Moreover, in the interpretation of such experiments the probability of operative artifact must unfortunately be regarded as proportional to the degree of specificity desired. For the broader inferences, however, such as that of the participation of the spinal cord, the probability of serious artifact is negligible.

The notion of the reflex arc as the anatomical counterpart of a reflex has been generally accepted. An end-organ (or a nerve trunk acting in that capacity), an afferent nerve, an interconnection between nerves in the cord or brain, an efferent nerve, and an effector are usually regarded as essential to the mediation of a reflex. We shall have no occasion to go beyond these rather general assumptions. The method which we have referred to is not peculiar to the reflex, and we shall not need to estimate it more closely. We may note, however, that the description of a reflex in functional terms (as a correlation of stimulus and response) is always prior to the description of its arc. In any available procedure the anatomical inference must always be drawn from an experiment in which the integrity of a *function* is critical.

In its simplest form, the concept of the reflex arc satisfies the need for continuity between stimulus and response, but the arc must serve as a locus not only for a communication between end-organ and effector but for modifications in the form of the communication. The statement of any reflex (for example: "the flexion of a limb following electrical stimulation of the skin of the foot") implies the possibility of a quantitative description of both stimulus and response. The statement thus expresses the observed correlation of two events, but by describing these events it describes also the special conditions of the correlation. A given stimulus and its response differ, for example, in time of inception, in duration, and in the form and amount of energy; and these modifications and conversions must be accounted for by the intervening events.

By procedures of the sort we have already described, the characteristics of a reflex have been assigned to particular parts of the arc. The gross conversions of energy have, of course, been referred to end-organ and effector, part of the elapsed time to afferent and efferent nerve, and so on. By a process of logical and surgical isolation, however, a certain group of the conditions of a reflex correlation have been shown to be independent of the activity of end-organ, effector, and nerve-trunk. These are the special characteristics of reflex conduction. They have been classically described by Sherrington in *The integrative action of the nervous system*. In Sherrington's list each characteristic is expressed as a difference between nerve-trunk and reflex-arc conduction, which means simply that certain of the inferences noted above have already been made. In reproducing the list here, we shall reword it in order to emphasize the nature of each item as an observed condition of a correlation of stimulus and response.

(1) There is a latent period between application of the stimulus and appearance of the end effect; (2) the duration of the response is greater than the duration of the stimulus; (3) if stimuli are applied rhythmically (between certain limiting rates), the rhythm of the response does not closely correspond with the rhythm of the stimulus; (4) the intensity of the response does not vary rectilinearly with the intensity of the stimulus; (5) a single brief stimulus is often not effective, but succeeding stimuli following closely upon it are; (6) afferent and efferent paths cannot function interchangeably; (7) repetition of a stimulus (with certain time specifications) evokes progressively weaker responses; (8) the strength of stimulus just sufficing to elicit a response is variable; (9) (*a*) a second stimulus is ineffective for a short interval after a first and subnormally effective for a succeeding short interval, (*b*) two stimuli at separate points of stimulation may facilitate each other, (*c*) a stimulus may act to produce the absence of a response, (*d*) injury to the nervous system may temporarily destroy or weaken the effectiveness of a stimulus which is subsequently found to be normally effective; (10) the effectiveness of a stimulus depends upon the integrity of the blood supply; (11) the effectiveness of a stimulus is partially or wholly abolished by anaesthetics.

Although all the characteristics except (6) and parts of (9) are represented to some extent in nerve conduction, the degree to which they are present in reflex conduction cannot be explained by reference to nerve-trunk alone. It has been assumed, therefore, that the characteristics represent the functioning of a special structure, which has been called the *synapse,* and which has been hypothetically located at the interconnection between neurons. Since these interconnections are grouped together in the gray matter of the cord and brain, the experimental practice associated with the other parts of the arc may be used in testing the hypothesis. It is possible, for example, to show that the synaptic characteristics are present in conduction through the cord when an afferent nerve is stimulated close to the cord and the response taken electrically from the efferent root. Again, the characteristics can be shown to vary with the temperature of the cord, but they are almost wholly independent of the temperature of either afferent or efferent nerve. Examples could be multiplied indefinitely. Moreover, as Sherrington has shown, the location of the synapse at the interconnection between neurons receives considerable support from the histology of the nervous system, from studies in nerve degeneration, from the physical chemistry of surfaces of separation, and from various other sources.

Reflex physiology seeks a physico-chemical description of the events peculiar to the mediation of a stimulus-response correlation. It regards the synapse, therefore, as a physico-chemical system. Theories of the details of that system have been of various sorts. Keith Lucas[1] sought an explanation of synaptic phenomena in terms common to nerve-trunk conduction. In his theory the synapse was regarded as a region of impaired conduction, and its description in physico-chemical terms waited only upon the description of the conduction of the nervous impulse. Sherrington has recently regarded the synapse as the locus for the depositing of excitatory and inhibitory substances or states.[2] For Lapicque the synapse is the boundary between neural structures of independent chronaxies. We shall have no need of evaluating theories of this sort, nor shall we find it necessary, in the light of our brief examination of method, to justify or discredit the hypothesis of the synapse itself. We are interested, not in the validity of that concept, but in its nature. Here we are led to one conclusion.

Our present information concerning the synapse is derived wholly from observed instances of reflex conduction. There is nothing in our description of the synapse which has not already served to describe experimental data, but we translate our descriptions of data into the laws of the synapse for convenience of expression. The synapse, that is to say, described in terms of its characteristics, is a construct. *It is the conceptual expression for the conditions of correlation of a stimulus and response, where the incidental conditions imposed by a particular stimulus and a particular response have been eliminated.*

There is nothing in the physiology of the reflex which calls in question the nature of the reflex as a correlation, because there is nothing to be found there which has any significance beyond a description of the conditions of a correlation. It may be objected that, should reflex physiology succeed in describing the synapse as a physico-chemical system, the synapse would be no longer conceptual. Actually, the description would be translated into concepts of another order, which would possess the tremendous advantage of being common to all the physical sciences. But we are here very close to certain fundamental questions of scientific method which we shall not attempt to answer.

The physiological study of the reflex supplements and restricts our definition. It begins by identifying and describing certain of the events which intervene typically between stimulus and response, and it then arbitrarily restricts

[1] Lucas, K. *The conduction of the nervous impulse.* London: Longmans, Green, 1917.

[2] Sherrington, C. S. Remarks on some aspects of reflex inhibition. *Proc. Roy. Soc.*, 1925, *97 B*, 519-544.

the use of the word *reflex* to correlations which employ that kind of event. Physiologically, the word implies the participation of at least two neurons with a synaptic junction. The best practical criterion is irreversibility of conduction, which is not in any degree a characteristic of nerve-trunk conduction and is therefore the clearest evidence of the operation of a synapse. This restriction in the use of the word *reflex* excludes (1) the movement of the organism solely under the influence of mechanical forces (for example, the movement of the paw of a dog when it is "shaken"); (2) the activity of an effector in response to direct stimulation, a distinction which is more often of importance in dealing with the internal economy of the organism; (3) those responses mediated by other types of nervous system than the synaptic; and (4) those correlations between discrete activities which are mediated by non-nervous mechanisms, as, for example, by hormones.

The advantage of these qualifications (even of the third, which is sometimes felt to be a notable difficulty) is that they insure a uniformity of material in the investigation of the reflex. It would be impossible to state with any degree of specificity a law describing the course of reflex fatigue if it were to apply to the exhaustion of such diverse mechanisms as a neural structure and a concentration of hormonic substance. The restricted definition limits the application of the principle of the reflex, but within boundaries which may be justified upon independent grounds.

We have tried to emphasize an essential continuity between reflex physiology and the special science of the description of behavior. We must not, however, fail to recognize a well-grounded distinction between the two fields, which is based primarily upon a difference in immediate purpose. The one seeks a description of the reflex in terms of physico-chemical events, the other a description of behavior in terms of the reflex. It is assumed that the word *reflex* refers to the same thing in both instances. Historically, however, the investigation of the physiology of the reflex proceeded, as we have seen, almost independently of the description of behavior and was advanced almost to its present status at a time when the possibility of a quantitative description of behavior was considered too remote for scientific consideration. The concepts of reflex physiology, consequently, cannot be transposed to the description of behavior without modification, which will in most cases be found, however, to proceed naturally from an analysis of the physiological method, as, for example, in the discussion of the synapse given above. Moreover, many assumptions which are unimportant and therefore tacit in reflex physiology are critical in the description of behavior and must be given an explicit and independent statement. This is true of the procedure by which a single reflex is isolated, as we shall see later. Again, a number of problems which arise in the description of behavior have no parallel at all in reflex physiology. Some of these matters we have already touched upon; others will enter at one time or another into the following discussion.

VI

We may summarize this much of the argument in the following way. A reflex is defined as an observed correlation of two events, a stimulus and a response. A survey of the history discloses no other characteristic upon which a definition can legitimately be based. The physiological investigation does not question the correlative nature of the reflex, for its data and its concepts deal essentially with the conditions of a correlation; but heterogeneous instances of correlations which would be embraced by the definition, read literally, are excluded by the physiological refinements of usage. It now remains for us to deal more specifically with the reflex in the description of behavior. What is the description of behavior, and how does the reflex, as a correlation, enter into it? Here (the reader may again be warned) we shall be concerned not so much with the validity or the adequacy of the concept as with its nature and the method peculiar to it.

Lacking some arbitrary distinction, the term *behavior* must include the total activity of the organism—the functioning of all its parts. Obviously, its proper application is much less general, but it is difficult to reach any clear distinction. The definition of the subject matter of any science, however, is determined largely by the interest of the scientist, and this will be our safest rule here. We are interested primarily in the movement of an organism in some frame of reference. We are interested in any internal change which has an observable and significant effect upon this movement. In special cases we are directly interested in glandular activity, but this will usually concern us only secondarily in its effect upon movement. The unity and internal consistency of this subject matter is historical: we are interested, that is to say, in what the organism *does*.

But the description of behavior, if it is to be either scientific or satisfying, must go further. As a scientific discipline, it must describe the event not only for itself but in its relation to other events; and, in point of satisfaction, it must *explain*. These are essentially identical activities. In the brief survey at the beginning of this paper it was occasionally necessary to regard the stimulus as a newly discovered cause of movement for which various conceptual causes had previously been designed. In this way we represented a real aspect of the history of the reflex. But we may now take that more humble view of explanation and causation which seems to have been first suggested by Mach and is now a common characteristic of scientific thought, wherein, in a word, explanation is reduced to description and the notion of function substituted for that of causation. The full description of an event is taken to include a description of its functional relationship with antecedent events. In the description of behavior we are interested in the relationships within a regressive series of events extending from the behavior itself to those energy changes at the periphery which we designate as stimuli. We stop here in the regression only because further steps are beyond the field of behavior. The two end events, the behavior and the stimulus, have, moreover, a particular importance, because they alone are directly observable in an intact organism, and because they limit the series. With the relationship of these two end terms the description of behavior is chiefly concerned.

The reflex is important in the description of behavior because it is by definition a statement of the *necessity* of this relationship. The demonstration of the necessity is ultimately a matter of observation: a given response is observed invariably to follow a given stimulus, or exceptions to this rule may be independently described. In its extension to total behavior the principle generalizes the statement of the necessity observed in a particular reflex, the form of the expression remaining essentially the same. That is to say, the hypothesis that "the behavior of an organism is an exact, if involved, function of the forces acting upon the organism" states the correlation of a stimulus and a response, both of which remain wholly undifferentiated. It is, in this sense, the broadest possible statement of a reflex, but it is not an observed correlation and is therefore a hypothesis only.

It is, nevertheless, solely the fault of our method that we cannot deal directly with this single correlation between behavior as a whole and all the forces acting upon the organism stated in the hypothesis. Quantitative statements of both stimulus and response and a statistical demonstration of the correlation are theoretically possible but would be wholly unmanageable. We are led, for lack of a better approach, to investigate the correlation of parts of the stimulus with parts of the response. For the sake of a greater facility (and in this case the very possibility) of description, we turn to analysis.

Originally, the use of analysis was quite accidental and unrecognized, but it has, nevertheless, always been necessary. The early observations were possible only after it had been achieved in some form or other. This is not difficult to understand if we remember that the correlation which we call a reflex rests ultimately upon observation. In an intact newt, to return to Hall's experiment, it would have been very nearly impossible to observe a correlation between the movement of the tail and the application of a probing needle, because the movement of the tail was also correlated with other stimuli and the action of the probing needle with other movements. *In the isolated tail,* however, one kind of movement followed a given stimulus and was absent in the absence of the stimulus. The correlation was obvious and therefore observed.

Marshall Hall and his few predecessors divided the behavior of an organism into parts by the expedient method of dividing the organism. This became, in general, the method of reflex physiology, although, for obvious reasons, the division of the nervous system supplanted the division of the whole organism. The best-known group of reflexes to be studied in surgical isolation are those surviving in the body of the organism after section of the cord just below the bulb. This is the "spinal" preparation, which has been the basis for the greater part of physiological investigation, notably that of Sherrington. Other common reflex systems are the decerebrate, in which the medulla and the cerebellum remain intact, and the various mid-brain and thalamic preparations, as, for example, those of Magnus. A further extension of the method involves the surgical or physiological exclusion of end-organs, as by extirpation or anaesthetization (for example, of the labyrinth), or by section of afferent nerves. The common object of these procedures is to permit the investigation of a particular response in relation to a controlled variable, independent of other variables also related to that response.

But the same result may be obtained in another way. The experiment may be so designed that the undesired variables do not vary. The distinction between the two methods will appear in the following example from the work of Magnus. Certain postural effects in a mid-brain animal are correlated partly with the position of the labyrinths relative to the earth and partly with the condition of flexion or extension of the muscles of the neck. The correlation between the posture and the state of the neck muscles can be studied alone if the labyrinths are cocainized or extirpated. But Magnus was also able to obtain the isolation by designing his experiments in such a way that the position of the labyrinths relative to the earth did not change. Perhaps the best examples of this method, however, are to be found in the work of Pavlov. Here the organism is intact and the very active receptors of the head segments fully functional. By controlling light, sound, odor, and other conditions in the experimental chamber, it is possible to observe in isolation the correlation between a given response and a selected stimulus. Placing an animal in a dark room, that is to say, is equivalent for purposes of isolation to blinding it, to sectioning the optic tracts, or to destroying the visual projection areas in the cortex, and has the great advantage over these surgical methods of being relatively free from unknown artifacts.

The practical merits of both these methods are obvious; but we are concerned with a broader aspect of analysis. For the physiologist, the isolation of a reflex is a preliminary matter of method and is relatively insignificant. In the description of behavior it is of first importance. How legitimate, then, is the process of analysis, and what is the nature of its product?

Let us deal entirely with the flexion reflex in the spinal dog, as a familiar and convenient example. We have already analyzed, of course, when we have once named, so that we must go back for a moment to the behavior as a whole. Without regard to its correlation with stimulating forces, behavior, as we have seen, is simply part of the total functioning of the organism. The problem of analysis at this level is common to physiology and anatomy. We shall not need to solve it, but shall assume that for purposes of description the body of an organism may be divided into parts (that we may speak, for example, of a leg), and that the functioning of a particular part may be described in isolation (that we may speak, for example, of the flexion of a leg). Moreover, we shall assume that the forces acting upon the organism may be analyzed and described in the manner common to the physical sciences. Our own problem lies beyond these assumptions.

In the flexion reflex our first experimental datum is the nearly simultaneous occurrence of the flexion of a leg and, let us say, the electrical stimulation of the skin of the foot. If we measure both events very carefully and repeat the stimulation, we obtain a second flexion which closely resembles the first, and we find that we may corroborate the observation, within limits, as often as we like. We call the observed correlation a reflex and, for convenience of reference, give it a special name, the flexion reflex.

The question then arises: *what is the flexion reflex?* If we try to answer by describing in detail a stimulus and a response, we meet embarrassing difficulties. We find that the exact degree and direction of flexion may vary with many factors. We find, for example, that it was very important for our original measurements that the torso of the animal had a particular position, that the contralateral leg was, say, unsupported, and so on. But we cannot specify these incidental conditions in our description without destroying its generality. Thereupon we shall probably resort to surgical methods. Theoretically, at least, we may pare down the structures underlying the flexion reflex until the collateral variables are no longer effective. But we can never be sure that the reflex which we have thus carved out of the behavior of the organism would not have been grossly otherwise if our operative procedure had been different. We are not sure, that is to say, that what turns up at the end of our process of isolation is the flexion reflex. There is another method open to us. In the flexion reflex we are dealing essentially with a group of correlations showing many characteristics in common. They involve the same effectors acting roughly in the same way and stimuli which resemble each other at least in their gross anatomical reference. We may, therefore, if we wish, *construct* a flexion reflex by a statistical treatment of many of these separate correlations. We may, in other words, determine and state a correlation between the characteristics common to all our observed responses and the characteristics common to all our observed stimuli, and we may name this construct the flexion reflex. But the resulting description of this statistical entity will likewise depend upon our choice of observations and upon our method of analysis.

We have been proceeding, of course, upon an unnecessary assumption, namely, that there *is* a flexion reflex, which exists independently of our observations, and which our observations approximate. Such an assumption is wholly gratuitous, but it is remarkably insistent. It arises in part from the nature of the reflex. If we remain at the level of our observations, we must recognize a reflex as a correlation. But the immediate uncritical reaction to a definition on that basis is that a correlation, in point of satisfaction, is not enough. There is an urge toward solidification, clearly evident throughout the history. We turn instantly to the reflex arc for material support. Although our knowledge of the critical part of the arc is, as we have seen, derived wholly from the observation of a correlation, we much prefer to regard the characteristics of the correlation as properties of the synapse rather than to retain them as characteristics of a correlation. Under the same pressure, then, but with less justification, we are led to assume that there are isolated reflexes concealed in the behavior of an organism, which by proper investigatory methods we may discover, and in the description of behavior to state the corollary of this proposition, namely, that behavior is the sum or the integration of these units.

Here we are touching upon the subject of a widespread current controversy, but we may, by virtue of what we have already said, dispose of the matter briefly. Let us phrase two typical questions. Is a reflex a unitary mechanism? Is behavior a sum of such mechanisms? Then, if by reflex we mean a hypothetical entity which exists apart from our observations but which our observations are assumed to approach, the questions are academic and need not detain us; if, on the other hand, we define a reflex as a given observed correlation or as a statistical treatment of observed correlations, the questions are meaningless, for they ignore the process of analysis implied in the definition. A reflex, that is to say, has no scientific meaning apart from its definition in terms of such experimental operations as we have examined, and, so defined, it cannot be the subject of questions of this sort.

There is a certain practical advantage, it is true, in regarding a reflex as a unitary mechanism—an advantage, as Mach might have said, which may have given rise to the practice. It it only when we misconstrue a purely practical device and take it to be an integral part of our definition that the possibility of theoretical misunderstanding arises. Our sample questions deal necessarily with the reflex defined in terms which we have seen to be well beyond any observational justification. As Poincaré has said of a similar issue, *"ces questions ne sont pas seulement insolubles, elles sont illusoires et depourvues de sens"*.[1] A common mistake in the present case has been to suppose that, because an answer is lacking, the principle of the reflex is somehow impeached. As we have repeatedly noted, the validity of the reflex as a scientific concept is not here in question. The reflex remains, as it has always been, an observed correlation of stimulus and response.

VII

It remains for us to consider how a reflex as a correlation is dealt with experimentally. The first step, as we have seen, is the isolation of a response and the identification of its correlated stimulus. In practice, the demonstration of the correlation is usually left at an elementary level. It is based upon the appearance of the two events together and their failure to appear separately. As an experimental datum of this sort, a reflex may be given the expression

$$R = f\ (S) \qquad [1]$$

where R is a response and S a stimulus. Theoretically, the exact nature of the function is determinable, although for any present purpose corresponding values of S and R are obtainable by observation only. Choosing convenient measures of both stimulus and response, we may vary the strength of S and observe variations in the strength of R. This is common practice, although very little has been done toward determining how a given R varies with its corresponding S. One characteristic of the relationship is the threshold: for values below a given value of S, $R = O$. There are also temporal aspects of the function, which have been investigated under the headings of latency and after-discharge.

Threshold, latency, after-discharge, and the order of variation of S and R are thus descriptions of the correlation we call a reflex. They may be investigated with only one elicitation of the reflex or, at most, with a single set of corresponding values of S and R. There is a second field of investigation, however, which is concerned with variations in any aspect of a correlation, as they may appear in the comparison of successive elicitations. If, for example, we select a value of S and repeat the elicitation of the reflex at a given rate, we shall observe a progressive decrease in the value of R. Or, again, if the interval

[1] Poincaré, H. *La science et l'hypothèse.* Paris, 1903.

between two successive elicitations be made brief enough, the second R may be of greatly reduced magnitude or wholly lacking. Here are significant variations in the value of the terms in Equation [1]. They do not challenge the necessity of the relationship expressed therein (as they might well do if they were less orderly), but they do require that, in the description of a reflex, account be taken of *third variables*. We may indicate the required change by rewriting our equation as

$$R=f\,(S,A) \qquad [2]$$

where A is a variable designed to account for any given observed change in the value of R.

As it appears in such an experiment, A is properly either time or the number of elicitations at a given rate. The inference is commonly made that it represents a factor of another sort, which varies with time or the number of elicitations in the same way. In the first example noted above the phenomenon has been called reflex fatigue, which is regarded as a synaptic change—as the exhaustion of a substance or state, or as an increase in resistance, according to one's preference in synaptic theory. But in the description of behavior, where we are only secondarily interested in these physiological inferences, reflex fatigue is nothing more than an orderly change in some measured aspect of a given correlation. A law describing the course of that change, where the independent variable is time or the number of elicitations or some other condition of the experiment, is peculiarly a law of behavior. It may become a law of the synapse, by virtue of certain physiological inferences, but it has by that time passed beyond the scope of the description of behavior.

Nevertheless, if we are to follow current usage, a definition of reflex fatigue as an observed variation in *one* aspect of a correlation is too narrow, for we know from observation that, when such a change has taken place, the other aspects of the correlation have also changed. If we have observed, for example, a change in the ratio of a particular R and S, we may expect to find all other ratios, as well as the threshold, latency, and after-discharge of the reflex, likewise changed. It is usual, therefore, to regard the particular change which we chance to observe as a sample of a greater process. Occasionally, where a change in one aspect of a correlation is alone important (as in summation, which is chiefly a matter of threshold), the characteristic may possibly be defined in terms of a single change. But such a characteristic as reflex fatigue, or the refractory phase, or facilitation, is by intention a description of a group of concurrent changes.

If we are to speak in terms of these group changes, it is almost necessary to have a term describing the *state* of a correlation at any given time with respect to all its aspects. The physiologist, of course, may use the synapse for this purpose. When he has once described reflex fatigue as the exhaustion of a synaptic substance, for example, he may attribute a change in *any* aspect of a correlation to that exhaustion. Although he may observe and measure at one time a change in after-discharge and at another a change in the magnitude of *R*, he may reasonably consider himself to be dealing with the same process in both cases. Fortunately, there is also a term serving the same purpose at the level of behavior. If, in a given reflex, the threshold is low, the latency short, the after-discharge prolonged, and the ratio R/S large, the reflex is ordinarily said to be strong. If, on the other hand, the threshold is high, the latency long, the after-discharge short, and the ratio R/S small, the reflex is said to be weak. An attribute of *strength* is imputed to the reflex. The strength of the response, of course, is not meant; a weak response may indicate a strong reflex if it be elicitable with a very weak stimulus.

"Reflex strength" expresses in a very general way the state of a given correlation at a given time with respect to many of its characteristics. It is a useful term, for it permits us to deal with reflex fatigue, for example, as a *change in reflex strength,* without stopping to specify the particular changes which compose it. Nevertheless, its usefulness does not extend beyond this qualitative level. The concept is subject to a major objection, which holds as well for the parallel use of the synaptic state. We do not know, since it has never been determined, whether the changes which compose such a characteristic as reflex fatigue all proceed at the same rate. If the threshold, let us say, and the magnitude of *R* do not vary in precisely the same way, we are not justified in taking either as a measure of a supposed common variable, nor, indeed, in continuing to regard reflex fatigue as a unitary process.

The study of the reflex, then, leads to the formulation of two kinds of law. The first are laws describing correlations of stimulus and response. A reflex, as we have defined it, is itself a law, and of this sort. It has a considerable generality in spite of the specificity of its terms, but it must be supplemented by other laws describing the exact conditions of a correlation. Secondly, there are laws describing changes in any aspect of these primary relationships as functions of third variables, where the third variable in any given case is a condition of the experiment. These secondary laws may be dealt with in groups, according as they involve the same experimental third variable, and they may be spoken of, for convenience, as describing changes in reflex strength. In the behavior of intact organisms the apparent variability of specific stimulus-response relationships emphasizes the importance of laws of the second sort. Conditioning, "emotion," and "drive," so far as they concern behavior, are essentially to be regarded as changes in reflex strength, and their quantitative investigation may be expected to lead to the determination of laws describing the course of such changes, that is, to laws of the second sort.[1]

It is difficult to discover any aspect of the behavior of organisms which may not be described with a law of one or the other of these forms. From the point of view of scientific method, at least, the description of behavior is adequately embraced by the principle of the reflex.

Summary

The present analysis of the reflex as a concept in the description of behavior follows the method first formulated with respect to scientific concepts by Mach and Poincaré. It examines the source of the historical definition and points out the incidental nature of most of its criteria. Eventually, it offers an alternative definition and considers in detail some of the questions which arise from the nature of the concept so defined.

I. Descartes "discovered the stimulus" and designed a mechanism which could account for animal movement upon the basis of the appropriate release of stored energy. But he was interested less in describing the action of the nervous system than in supporting metaphysical contentions of the automaticity of animals. He advanced the stimulus as a substitute for soul, but only within a field which omitted the greater part of the activity of man.

II. The notion of the reflex developed, independently of Descartes, from the investigation of "irritability." The action of a stimulus was implicit in the concept of irritability, which also assigned an autonomy of function to the parts of an organism. The concept of the reflex arose quite naturally when a stimulus and its related response were to be spatially distinguished. Robert Whytt made the first historically effective observations.

III. It remained for Marshall Hall to clear the concept of psychical counterparts. This he did by setting up a distinction between reflex and voluntary action, which resulted eventually in the unfortunate historical definition of the reflex as a form of movement unconscious, involuntary, and unlearned. Volition, in Hall's sense, was essentially the hypothetical antecedent of movement for which no corresponding stimulus could be observed, a definition which served to identify the reflex with scientific necessity and volition with unpredictability.

[1] The second half of my thesis, of which this paper was the first half, describes experiments on "hunger drive" from this point of view.

IV. The history of the reflex has known only one positive characteristic by which the concept may be defined: the observed correlation of two events, a stimulus and a response. The negative characteristics, on the other hand, which describe the reflex as involuntary, unconscious, and unlearned, have proceeded from unscientific presuppositions concerning the behavior of organisms. The reflex is tentatively defined herein as an observed correlation of stimulus and response.

V. Reflex physiology undertakes to describe the events which intervene between a stimulus and a response. The physiological usage does not question the definition of a reflex as a correlation, for the synapse is only a conceptual expression for the "reduced" characteristics of a given correlation.

VI. The essence of the description of behavior is held to be the determination of functional laws describing the relationship between the forces acting upon, and the movement of, a given system. The reflex is, by definition, the precise instrument for this description. Its analytical nature is discussed, and existing methods of analysis are examined. Current objections to analysis are held to have no scientific meaning.

VII. The experimental study of the reflex may be divided into two parts. There is, first, the investigation of the characteristics of a correlation—latency, threshold, after-discharge, and the order of variation of *S* and *R*. Secondly, there is the investigation of variations in these characteristics as functions of third variables. The notion of reflex strength is useful in dealing with this second group. The question of third variables is of extreme importance in the description of the behavior of intact organisms.

From the point of view of scientific method, any law describing the behavior of organisms must be reducible to one of the forms herein discussed. The description of behavior, that is to say, is adequately embraced by the principle of the reflex.