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Study of behavior: Science or pseudoscience

Jerzy Konorski

With comments by Bogdan Dreher, Charles Gross and Giacomo Rizzolatti

The manuscript printed below has been written by Prof Jerzy Konorski around 1970, a few years before his death in 1973. The manuscript has not been published before. It was recently discovered in Konorski's papers deposed in the Library of the Nencki Institute of Experimental Biology. In his critical review Konorski debates advantages and shortcomings of the physiological approach of Pavlov and purely behavioristic approaches advocated by Hull and Skinner. He supports close cooperation o behaviorists with neurophysiologists and neuroanatomists, with focus on the investigation of the neural mechanisms underlying behavior. Konorski's ideas concerning the integration of the study of behavior and neurophysiology anticipated contemporary path of neuroscience. Indeed, his approach, which at that time appeared somewhat controversial, is universally accepted by contemporary neuroscientists. By contrast, physiological theories of higher mental functions formulated by Pavlov as well as deliberately anti-physiological approaches of Skinner and Hull have all but disappeared from serious scientific discourse. However, the same problems such as strongly promoted self-importance of some branches of neuroscience, the lack of inter-communication between different branches and resulting lack of integrating ideas appear to emerge anew in each new generation of scientists. (Editors of Acta Neurobiologiae Experimentalis).

Key words: neural mechanisms, behavioral theories, psychological theories, separatism of science branches

INTRODUCTION

Before starting to write this paper I wanted to be certain of the precise meaning of the word "legacy''. In the Concise Oxford Dictionary I found that it is "a material or immaterial thing handed down by predecessors". Thus, I understand that I am here in a role of predecessor, expected to hand over my thoughts about the field of science I have been concerned with throughout my life to the present generation of scientific workers. Analyzing now those thoughts I realize that they are shaped by my past. This means that although my views concerning particular problems of brain functions and brain – behavior relations have drastically changed with the development of my own research and that conducted by other authors, my general idea have remained amazingly similar to, although not identical with, those I held 45 years ago at the beginning of my scientific career. Accordingly, my scientific "legacy" requires explanation of the origin and development or my scientific *Weltanschauung* [a Worldview]. This calls for some autobiographical facts which have been dealt with in detail elsewhere (Konorski 1973).

PAVLOV'S LEGACY AND ITS DEVELOPMENT

Very early in my life, in the third decade of this century, for some reasons which need not be explained here, I become interested in what at first glance appears to be a simple question: "how does the brain work?" This was why I began to study first psychology and then medicine, being particularly interested in neurophysiology, neurology and psychiatry. Unfortunately, on the basis of my studies I could not find either an answer to this questions – which obviously could not be given – or even a hint as to how it should be approached.

The turning point in my life occurred when in 1927 I found (together with my friend and colleague Stefan Miller) the French translation of Pavlov's fundamental work on conditional reflexes (Pavlov 1927). We began to study this book, as well as other papers from the Pav1ovian laboratories, and we realized that this was precisely what we were looking for. Soon we succeeded in organizing out own small laboratory on conditioned reflexes, and in a few years we obtained some data which aroused Pavlov's interest and appre-

Jerzy Konorski
Department of Neurophysiology Nencki Institute
of Experimental Biology Warsaw Poland

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The first page of Jerzy Konorski manuscript ciation. As a consequence he invited as to come to Leningrad to discuss with him our problems. I stayed in this city for two years working in Pavlov's laboratory, and in this way the path of my scientific career was "settled".

What did I learn when I became a "pupil" of Pavlov? Let me recapitulate the main theses of Pavlovian teaching, as it was called in that time in Russia.

Pavlov claimed that experiments on brain functions, executed in anaesthetized animals are not a "true" physiology of this organ for the following reasons. The brain is **the** organ developed for no highest control and integration of animal behavior on the basis of messages arriving to it from receptors. This is its real and unique function. Therefore, studying its activity in a situation in which this function is strongly reduced and simplified by anesthetic drugs, is unreasonable, and cannot teach us anything about how the brain works in normal conditions. The only way to approach the normally functioning brain is to make experiments on wakeful animals by presenting them with various types of natural stimuli and observing their responses.

It should be recalled that the concept of a "reflex", denoting simple inborn responses to particular stimuli, was already well established in physiology by the end of the 19th century. Therefore Pavlov also resorted to this concept as the basis of his investigations by introducing the term "conditional" reflex, in contradistinction to the "unconditional" reflex as dealt with in neurophysiology up to that time. In his writings Pavlov stressed very strongly that all **essential** properties of conditional reflexes were exactly the same as those of unconditional reflexes, except that the latter were inborn, while the former were acquired during the animal's life as a result of his individual experiences. Thus it was thought that the careful study of conditional reflexes and their properties should pave the way for an understanding of their physiological mechanisms within the brain. This idea underlies the origins of a new branch of science that was called by Pavlov "physiology of higher nervous activity".

I became a full adherent and a strong advocate of this approach, and was even proud to belong to a small group of people who took part in this noble enterprise. My own main contribution to this enterprise shared with my colleague and coworker Stefan Miller, was the discovery of type II conditioned reflexes, based on a different paradigm than the Pavlovian reflexes, and a thorough description of their properties and categories (Miller and Konorski 1928 (Engl. translation 1969), Konorski and Miller 1933, 1936). These reflexes were subsequently called "operant responses" by Skinner (1938) and "instrumental responses" by Hilgard and Marquis (1940). In the late thirties, when I became better acquainted with the physiology of the central nervous system, I became much more critical towards the Pavlovian theory of cerebral processes, since I had realized that this theory is completely incongruous with the general principles of neurophysiology. In order to understand this incongruity one must take into account that before Pavlov began his research work on conditioned reflexes, he spent about twenty years on a different subject – the physiology of the digestive tract. In this domain Pavlov won a worldwide reputation, crowned by the Nobel prize in 1903. When he afterwards drastically changed the line of his investigation, he was not properly acquainted with the achievements of modern neurophysiology, which was founded on Ramon y Cajal's studies on the histology of the nervous system (Ramon y Cajal 1909, 1911) and developed by Sherrington in his studies on the activity of the spinal cord (Sherrington 1906). Indeed, Pavlov did not try to adjust his concepts of the activity of the cerebral cortex to the general principles of functioning of the central nervous system, but attempted to build his theory of cortical processes on the basis of his own ideas which had emerged from experiments on conditioned reflexes.

The main difference between the two lines of reasoning, that of Ramon y Cajal and Sherrington on the one hand, and that of Pavlov on the other, was shortly this. According to Cajal and Sherrington the central nervous system is a huge nerve-net in which the nervous processes (impulses) are traveling from one neuron to the others by nerve fibers (axons), and transmit either excitation or inhibition by synaptic contacts. This transmission being strictly unidirectional conveys information, either from reception to the brain or from the brain to effectors, or between various structures of the brain. On the other hand, Pavlov claimed that the excitatory or inhibitory processes originating in the given points of the cerebral cortex by the operation of corresponding conditioned stimuli radiate all over the cortex and then concentrate back to their departure points (Pavlov 1927). On the basis of these notions Pavlov attempted to explain all the manifold properties of both positive and negative conditioned reflexes and their complex interrelations.

The critical evaluation of the Pavlovian theory of cortical processes advanced by him on the basis of his experimental data, and my proposal of a new theory of conditioning based on the same data but with reference to the general principles of nervous activity developed by Sherrington, were presented in my monograph "Conditioned Reflexes and Neuron Organization" [Konorski 1948 (next ed. 1968)]. The title of this monograph was meaningful. Whereas the Pavlovian theory of cortical activity based on radiation of excitatory and inhibitory processes over the cortex and their concentration at their points of origin could not be reconciled with the neuronal organization of the brain as it was conceived by Ramon y Cajal, the theory which I proposed in the monograph was explicitly based on this concept.

It is understandable that achievement of the goal of explaining the properties of conditioned reflexes by means of neural processes analogous to those, the existence or which was firmly established in the "lower" parts of the nervous system, greatly strengthened my conviction as to the soundness of the general Pavlovian idea that conditioned reflexes **could** be explained in neurophysiological terms. For, the only obstacle hindering this explanation so far, prevalent at that time among physiologists, was that the Pavlovian theory was quasi-physiological and quite alien to the real functioning of the nervous system. I was almost certain that if Pavlov had from the very beginning tried to interpret his data on the basis of synaptic concepts and to indicate the affinity of his work to that of Sherrington, then his ideas would have been understandable to neurophysiologists and fully acceptable. Therefore, I considered that it was a tragedy for the development of the Pavlovian work that at some definite turning point in the early twenties, he came to his unfortunate concepts of irradiation, concentration and induction of cortical processes and thus his further theorizing went astray.

MY ATTITUDE TOWARDS BEHAVIORISM

This being so it must be clear to the reader that I was in strong opposition to the behaviorism which, accepting in full scope Pavlovian empirical terms (such as conditioned reflex, generalization, extinction, etc.) and praising Pavlov as the greatest **psychologist** of our time, opposed any idea of explaining animal behavior in physiological terms. In order to present my issue let me take the view of two of the most influential animal behaviorists of my time, Skinner and Hull.

B.F. SKINNER'S VIEWS

In the early nineteen thirties, Skinner decided to establish a purely empirical scientific discipline which would deal exclusively with description and systematization of animal and human learned behavior without a tendency to explain it by reference to any basic processes (Skinner 1938). In the article most representative of his views (Skinner 1950), he discards any theories explaining learning either on the basis of mental events, or physiological processes occurring, or assumed to occur, in the brain. Here are the main points of his argumentation:

"*A science of behavior must eventually deal with behavior in its relation to certain manipulable variables. Theories – whether neural, mental or conceptual – talk about intervening steps in these relationships. But instead of prompting us to search for and explore relevant variables, they frequently have quite the opposite effect. When we attribute behavior to a neural or mental event, real or conceptual, we are likely to forget that we still have the task of accounting for the neural or mental effect.*"

And again: "*Research designed with respect to theory is also likely to be wasteful. That a theory generates research does not prove its value unless the research is valuable. Much useless experimentation results from theories, and much energy and skill are absorbed by them. Most theories are eventually overthrown, and the greater part of the associated research is discarded*".

I think that Skinner's reasoning underlying his decision of discarding the physiological approach in the analysis of animal behavior is faulty. As a matter of fact, almost all natural sciences aim to explain the hidden mechanisms of empirical data by proposing hypotheses, tested by new empirical data, which either confirm or reject them. I do not see any reason why the sciences of behavior should stand aside from this general rule of scientic development.

Skinner's main argument against the explanation of animal behavior with reference to central nervous processes occurring in the brain, was that nobody could see these processes. This was why he considered that the term CNS (central nervous system) the behavioral scientists dealt with, was in fact the "conceptual nervous system", since its functions were concluded on the basis of stimulus-response relations (Skinner 1938). But this contemptuous denotation is unjust and incorrect. Skinner is certainly fully aware that the behavior of animals does depend on the function of the central nervous system, in particular of the brain. He also knows that the general principles of functioning of this system are basically understood, and therefore the goal of scientific research consists in determining the arrangement of nervous connections which can account for particular types of stimulus-response relations. Skinner also knows that Sherrington's fundamental work on the central mechanisms of spinal reflexes was based precisely on stimulus-response technique, since direct recording of the activity of spinal neurons was in his time impossible. Nevertheless, the "conceptual spinal cord" proved to be a reality and the principles of its activity were on the whole confirmed by later studies directly observing the activity of spinal neurons by means or electrophysiological methods.

The Skinnerian approach to behavioral data seems to me the same as if a chemist were to describe the empirical properties of the chemical compounds and their reactions, completely neglecting the molecular theory of the matter, which explains the occurrence of these reactions. The similarity between such an imaginary approach of a chemist and Skinner's approach to the behavior of animals is even closer, if we realize that the molecular theory arose much earlier than scientists could dream of "seeing" molecules and atoms, and therefore it was purely conceptual.

What has been offered by the above described approach to our knowledge? Since its principal task is the empirical study of behavioral responses, it is reduced to a more collection of facts, irrespective of their significance for understanding the central mechanisms by which they are controlled. It happens that some of these facts may be of prime importance from this point of view, but their proper evaluation must be done only by those people who are concerned with the elucidation of central mechanisms controlling animal behavior.

The above discussion on the validity of Skinner's general approach to the study of behavior should not in the least detract from his important methodological contributions in this field. Indeed, Skinner's methods are used in almost every behavioral laboratory and they play a significant role in the hands of those who aim at understanding animal behavior by physiological mechanisms.

C. L. HULL'S VIEWS

A quite different point of view was represented by another prominent American psychologist, Hull (1943). For Hull animal behavior was also the principal aim of his research work, but he attempted to systematize it by means of a number of concepts called "constructs", which intervene between the stimulus and the response and are chosen in such a way as to account for empirical data obtained in behavioral experiments. Here belong such notions as habit strength, reaction potential, inhibitory potential, and others. But Hull makes it clear that these constructs have no physiological meaning and are proposed because, according to his view, "the major neurological laws" were (in his time) not ripe "to constitute the foundation principles of a science of behavior". It is well known how popular Hull's system was among behaviorists and how much it has promoted the experimental work on animal behavior.

From my point of view the system proposed by Hull cannot by definition be quite satisfactory just because of its abstract character. After all, in the time when Hull undertook his life work, the "major neurological laws" were fairly well understood. At least they were sufficiently developed to establish a theory of behavior which, although imperfect, could certainly fullfil the same role as was played by his abstract constructs. Therefore these constructs, based **only** on the empirical material from the field of conditioned ref1exes, might become misleading at the time when they should be translated into neurophysiological concepts.

Moreover, to my mind the Hullian system played a negative role in the development of our science. It was this. When reading Hull's principal treatise, one notice that his knowledge of neurophysiology was extensive and that he did emphasize that his system was transient and should be substituted in the proper time by a neurophysiologically based system.

Unfortunately, this admonition was not obeyed by his followers. Therefore, when neurophysiology in the next years grew immensely and its discoveries **could** throw much light on the mechanisms or behavioral responses, psychologists continued to stick loyally to Hull's constructs, changing and improving them, but drawing no profit from neurophysiological discoveries.

Thus, will-nilly, a most disappointing situation occurred in the science of behavior. Instead of coming closer and closer to neurophysiology, experimental psychology became, on the contrary, even more remote from it than in the time of Thorndike and Hull. The science of behavior proclaimed itself to be a self-contained scientific discipline, fully independent of brain physiology, and even reluctant to have any bonds with it. And this unhealthy situation still exists, its clearest manifestation is the current use of the term "behavioral science", and the refusal of its adherents to study behavior within the scope of brain physiology. Since they are also strongly opposed to introspective notions largely used by classical psychologists, the discipline exists in a vacuum, being isolated both from physiology and psychology.

MY ATTITUDE TOWARDS TRUE PSYCHOLOGY

As it was argued in the preceding section of this article, the line of research conducted on the basic of either Skinnerian or Hullian approach in directed toward an extension of our knowledge of behavior as such, without any attempts at understanding the cerebral processes on which this behavior is based. Therefore, this line of research has been denoted as the science of behavior. However, the investigators studying animal behavior along this line also use another term, no less popular than the previous one, namely they call this field "objective psychology". It is easy to see that this term is illogical, since psychology is by definition a field of science concerned with mental events, that is those events of which we are aware from our subjective experience or introspection. In opposition to this the science of behavior is purely objective and rejects any references to psychic events. Thus the term objective psychology should be avoided, since it includes a typical ***contradictio in adjecto***.

True psychology, as a science of describing and analyzing mental events on the basis of self-observation, is an old discipline which does not need any defense. Usually two arguments are used against its validity. One is that quite often the reports obtained from introspective observation by one person are unreliable, and other people cannot confirm them. But this is quite often true of objective observations too, when people looking at the some object do not agree about its visual properties. After all this is why behavioral studies are often performed by resorting to independent observations by two or more persons in order to obtain more reliable results. In fact, we do know how unreliable and misleading human perceptions can be and how cautious we must be to evaluate them properly. The other argument against introspection as a scientific method is, that while objective observations are, at least theoretically, "public", that is a number of people may observe the some object, the events "perceived" by subjective observations are by definition "private". Of course this is true, but it should not belittle their value. After all, an investigator who would like to test the generality of a particular subjective experience can present the stimulus by which it is elicited, say show a picture evoking particular emotions, to a number of persons of a given group, and record what they are feeling while looking at it.

Finally, it should be noted that subjective observation is used in a very important field of science called psychophysics, which has allowed us to discover important properties of human perceptions. If we trust the observer's reporting what he has perceived, why not trust him when he reports what he has imagined?

All of those things seem so commonplace to everybody possessing common sense that I feel even embarrassed to mention them. Yet, I do know quite a lot of "psychologists" who strongly oppose the scientific value of introspection, and if pressed about this issue use a hypocritical maneuver, claiming that they base their statements not on subjective observations of other people, but on their objective verbal reports. By the way, this hypocrisy is quite naive, because if one is, for instance, interested in the classification of human emotions, he could hardly say that his interest lies in the classification of a definite class of verbal reports.

Another important question concerning descriptions of mental events is whether it is legitimate to attribute these events to higher animals. Certainly the naive misuse of such descriptions by laymen and even some scientists, who attributed a number of human experiences to animals on the basis of superficial and uncritical observations, led to denying any kind of subjective experiences to animals. This approach is again unscientific. For, if a species of organisms called *Homo sapiens* and possessing a highly developed brain, for some mysterious reasons is endowed with subjective experiences, then it must be admitted that higher animals, whose brains are very similar to those in humans, should also possess this peculiarity. Moreover, if behavioral and autonomic manifestations of various drives, such as hunger, fear, rage etc., are in higher animals analogous to those in man, then there is no reason to negate analogous emotional experiences in those animals. If anybody would oppose this opinion on the grounds that subjective experiences in animals cannot be directly proved, we could show him that the same is true of all human beings, except himself. Yet I do not think that it would be reasonable for scientists to hold such solipsistic views.

To sum up it is now fashionable among some groups of psychologists to discredit introspective methods of observations, considering them unscientific. Some of these psychologists, especially those engaged in experiments on animals, are consistent in that they are interested **only** in animal behavior, based on pure objective observations, and develop the theoretical systems in which subjective experiences do not intervene. Some of them, for instance, Tolman (1932) use terms denoting such experiences like expectancy, they regard them, however, as intervening variables. Others, especially those concerned with human psychology adopt a hypocritical attitude, pretending they do not care about mental events, but are interested only in external responses, among them verbal reports.

Despite the value that "objective psychology" may have had in bringing scientific rigour to the study of behavior earlier in this century, it seems to me that this attitude is in the present period of development of brain physiology particularly harmful. For all naturalistically minded scientists, whether they are by profession physiologists or psychologists, are aware of the fact that mental events do depend directly on the cerebral processes and that no such events can exist without their occurrence. In fact, both the methods of destruction of various parts of the brain and of recording evoked potentials in wakeful animals make the correlations between mental events and cerebral processes increasingly better understood. Although some decades ago psychologists **could** claim that the skull was a black box and that we could not even imagine what is going on inside, now this black box is illuminated by the light coming from direct penetration into it either by the surgical knife, or drugs, or implanted electrodes. Therefore, our long-lasting dream of fixing correspondence between mental processes and cerebral processes does not seem unrealistic and is even not very far from realization. But this can be done only when psychologists stop being ashamed of their own scientific domain, when they return to studying mental evens and help to bridge the gap between those events and neural processes. The sooner they do so, the better it will be for the development of our knowledge on the correlations between mind and brain.

SYNTHESIS

The above considerations, originating of course from my scientific development lead me to specify the following possible domains of investigations, which are directly or indirectly connected with brain functions.

(1) The domain dealing mainly with subjective experiences. The method or investigation is here introspection that is observation of events occurring in our minds. This area was extensively studied in the nineteenth and the beginning of the twentieth century. The monumental treatise by William James (1950) is a classic specimen of this discipline.

(2) The domain dealing with subjective experiences plus their external effects. This is the typical human psychology of today, if it is not biased by aversion to introspection. The works on emotions or motivations are good examples of such studies since these are the phenomena in which subjective experiences are virtually inseparable from their autonomic and behavioral effects.

(3) The domain dealing exclusively with behavior. While Skinner (1938) arbitrarily chose motor responses of animals for his investigation, Hull (1943), being closer to the physiological approach in the study of behavior, included also autonomic responses occurring in classical conditioning.

(4) "Pure" brain physiology dealing with cerebral processes in anaesthetized or immobilized animals, studied by electrophysiological methods. Of course, this domain has no direct relation to behavioral sciences. I have called it analytical neurophysiology.

(5) The domain dealing with animal behavior (including autonomic responses) studied from the **psychological** point of view, that is, with reference to the cerebral processes controlling this behavior. This domain is now developing with tremendous speed and efficiency, as judged by the number of journals, increasing each year so abundantly that it is hardly possible to follow them. It is called physiological psychology (the old term proposed by Wundt 1910), neuropsychology, brain and behavior study, all these names being synonyms.

(6) The domain which includes the interdisciplinary study of brain functions, utilizing facts obtained from various sources. On the one hand, it makes use of the data collected by analytical neurophysiology, data which teach us about the functional properties of nerve cells, the interconnections between various parts of the brain, as well as connections between the brain and peripheral organs. On the other hand, it deals with all relevant evidence both from the field of animal behavior and introspection - those phenomena which are **controlled** by cerebral processes. I have called this aspect of brain functions integrative activity of the brain (Konorski 1967), because to my mind the discipline dealing with this activity is the true psychology of the brain which Pavlov attempted to establish.

Of course, the last domain specified here is closest to that called brain and behavior study because the only difference between the two is that it does not neglect introspective observations in both normal and brain-damaged human subjects. In my last book (Konorski 1967) I emphasized how valuable may be the reports of some intelligent patients, suffering from visual agnosia, who are able to describe their mental deficiencies, which might otherwise completely escape our observation.

Now, having all these domains in mind, we may easily use that the domain concerned with the pure study of behavior is at least superfluous, if at all justifiable. In fact, as far as **human** behavior is concerned, according to our introspection it is inseparable from mental events and in consequence it is a part of human psychology. As a consequence of the growing possibility of explaining these events by nervous processes occurring in the brain, human behavior will be more and more intelligible from the physiological point of view.

Even simpler is the situation with regard to **animal** behavior. This behavior is studied in special experiments which allow us not only to observe motor and autonomic conditioned reflexes elicited by particular conditioned stimuli, but also to directly intrude into the brain in order to elucidate which parts of this organ, and in which way, are involved in the formation and occurrence of these reflexes. Accordingly, the study of animal behavior has already become part of brain physiology, and its separation from that discipline is completely artificial.

I do realize that those scientists who were brought up on different scientific ideas from those advocated in this article, namely in the belief that the study of behavior should constitute a separate field of science, independent of kindred fields, will not be convinced by my argumentation. I think that this is because people engaged in the pure study of behavior are *ipso facto* not physiologists, but “behaviorists”, and do not realize the explanatory power of brain psychology with regard to animal behavior. But I address this article to those students who are not yet biased by their previous behavioristic training and consequently have their minds open to other ideas, provided that these ideas are reasonable.

COMMENTS

Reflections on the article of Jerzy Konorski entitled “Study of behavior: Science or pseudoscience” - by Bogdan Dreher

In my opinion, the article “Study of behavior: Science or pseudoscience” must have been drafted within a year or two of publication of Konorski’s second major monograph (Konorski J., 1967, Integrative Activity of the Brain. Chicago University Press, Chicago, IL). I believe that the article was intended as a sort of clarifying addendum to both his major monographs.

To start with we probably need a few words of background in relation to Konorski’s first English monograph (Konorski J., 1948, Conditioned Reflexes and Neuron Organization. Cambridge University Press, Cambridge, UK). The fact that the monograph was “Dedicated to I. P. Pavlov and C. S. Sherrington in the hope that this work will do something to bridge the gulf between their respective achievements” strongly affected its reception.

On the ‘Pavlovian’ or at least official Soviet side of the bridge, the publication of the monograph almost coincided with a big conference in Moscow organized by pseudo-Pavlovian political rather than scientific establishment. All Soviet prominent scientists engaged in research on higher nervous activity were present and an ‘unanimous’ support for Pavlov’s concepts became obligatory. As a result, a number of first-class scientists, such as Orbeli, Beritoff (Beritashvili), and Anokhin, were denounced as revisionists and strongly criticized. No won-der that in this atmosphere Konorski's book drew bitter criticism and a complete disapproval. The Soviet attacks were followed by almost 'Red Guard-like'[1] ('scientific renegades', 'revisionists', 'servants of capitalism') attacks in the Soviet dominated Poland and Konorski's scientific position became quite precarious.

On the Western, that is, 'Sheringtonian' side of the bridge, the reception of the monograph was more positive. Indeed, the monograph was favorably reviewed and in England at least it became quite popular and well known. On the other hand, in USA, the book passed almost unnoticed and there were only very sparse references to it in papers and monographs concerned with the problems of conditioning. In his autobiography published *post-mortem* in 1974 (History of Psychology in Autobiography, Prentice-Hall, Inc., Englewood Cliffs, NJ) Konorski comments that: "at that time (1948) experimental psychology in America was strongly Skinnerian or Hullian, and physiological explanations of the mechanisms of conditioned reflexes were utterly unpopular".

Fortunately, in a few years' time, on the Soviet side of the bridge, the attitude to Konorski changed quite dramatically. Again I will cite here Konorski's 1974 autobiography: "1955 was the year in which, two years after Stalin's death, the "thaw" began, when Khrushchev came into power and dissociated himself sharply from the Stalinist period, denouncing it somewhat euphemistically as "the cult of personality." This was immediately reflected in all fields of cultural life in the USSR and even more so in Poland. In my own field the pseudo-Pavlovian indoctrination vanished completely, and I stopped being a revisionist and a servant of capitalism. On the contrary, I became even more popular than before, because my earlier protagonists were now able to openly take my side, whereas my antagonists were simply ashamed of their previous conduct and tried to apologize". Konorski continues: "Now the attitude of Soviet scientists toward me changed almost overnight. They became friendly and began to invite an improvement in our relations, feeling that bygones should be bygones".

Soon after, there was also a very pleasant surprise on the Western side of the bridge. Thus, in late 1957, Konorski was sent by the Polish Academy of Sciences to the United States to become acquainted with scientific centers concerned with brain research. The visit was sponsored and organized by Robert (Bob) Livingstone at that time Director of Basic Research on Neurological Sciences and Psychiatry, National Institutes of Health in Bethesda. During this visit, Konorski learned that contrary to his expectations his ideas were not unknown in the United States. Konorski did not encounter a 'mob' of scientific adversaries and he was able to establish many scientific as well as personal friendships. It was clearly a sign of growing realization of the value of Konorski scientific insights that in the late 1950-ties and 1960-ties at least three of Konorski's papers were published in such prestigious journals as *Science* or the *Proceedings of the National Academy of Sciences.* It was quite clear that a new era had already begun in America, an era of increasingly close intellectual cooperation between brain physiology and anatomy and behavioral sciences. Eventually, this cooperation, combined with rapidly developing pharmacological, biochemical, biophysical and new 'moleculo-genetic' approaches to the nervous system and learning resulted in fairly unified concept of Neuroscience.

Nevertheless, in his 1974 autobiography Konorski wonders: "I am very curious to know what will be the final fate of the book (Integrative Activity of the Brain. Chicago University Press, Chicago, 1967): will it eventually win general recognition, which I think it deserves in spite of its shortcomings, or will it have no important impact on the further development of behavioral sciences. I am rather afraid that the latter fate may prevail because the investigations concerning the mechanisms of conditioning are still in the hands of experimental psychologists, who simply do not care about the physiological interpretation of the phenomena of animal behavior and have quite different frames of reference from those applied in my book".

By the late 1960s–early 1970s, the experimental contribution of Pavlovian school was widely respected and even admired in the so-called West. However, there were no serious scientific adherents of Pavlovian theory of cortical processes. Thus, in my opinion the critique of Pavlov's legacy contained in the article addresses historical rather than any contemporary scientific debates.

Clearly however, the intellectual defeat of concepts based on 'Hullian' system or orthodox 'Skinnerism' among so-called experimental psychologist was not yet apparent. Although by that time (the late 1960s–early 1970s), Clark L. Hull was long dead, he had a number of influential followers and continuators. One of them was Neal E. Miller, Professor of Psychology at Yale University and later on, Professor of Psychology at Rockefeller University. At that time, Miller's approach appeared to bridge a substantial gap between behaviorism and so-called personality psychology. Furthermore, his work on biofeedback and susceptibility of autonomic nervous system to classical Pavlovian conditioning attracted a lot of attention and generated high hopes for the development of successful treatments of psychosomatic or even not outwardly psychosomatic cardiovascular diseases. Miller was not entirely unfamiliar with work conducted in the Department of Neurophysiology of Nencki Institute of Experimental Biology. Indeed, one of the prominent 'Nenckian' students of emotional behavior and neurophysiological mechanisms underlying motivation and emotional behavior, Elzbieta Fonberg, spent a part of her postdoctoral fellowship in Miller's lab in Yale. Neal Miller on his way to the 18th International Congress of Psychology in Moscow (August 1–7, 1966) visited Elzbieta Fonberg. Despite the fact that July-August is a peak of vacation season in Poland, Miller's talk (in which he described some of his biofeedback approach) was very well attended. Konorski presided over the talk and the discussion was very vigorous. However to my recollection of the event, and contrary to Konorski's generalization concerning the attitude of the followers of Hull, Miller did not 'continued to stick loyally to Hull's constructs' and was quite accommodating in relation to Konorski's neurophysiological interpretations of the findings presented.

Konorski's uneasiness in relation to Skinner's influence on experimental psychology was not entirely unjustified in the late 1960s–early 1970s. At that time, Skinner occupied one of the most prestigious chairs in Psychology (Edgar Pierce Professorship of Psychology at Harvard) and received a number of prestigious scientific awards (e.g. National Medal of Science or Gold Medal Award of American Psychological Foundation). Almost until his death in 1990, that is, many years after Konorski passed away (September 1973), Skinner was intellectually active and continued teaching and inventing clever devices and experimental procedures with which he was able to quantitatively assess the behaviors. To this day, Skinner's theory of learning is often referred to and is believed to have important implications for education.

Despite the clear differences between Konorski's and Skinner's scientific *"Weltanschauung"*, those differences did not crystallize into a nasty personal animosity. Indeed, in 1969 in his 'own journal', Skinner published an English translation (On a particular form of conditioned reflex, Journal of the Experimental Analysis of Behavior, 1969, 12: 187–189) of Stefan Miller and Jerzy Konorski original paper in which they described for the first time the "conditioned reflex of the second type".[2] The original paper was published in French in 1928 (Sur une forme particuliére des reflexes conditionnels in Les comptes rendus des seances de la société de biologie. Société polonaise de biologie, June 1928, Vol. XCIX, 1155–1557). In the translator's note, Skinner recalls the debate between himself and Konorski and Miller on the concept of conditioned reflex of the second type vs. Skinner's instrumental conditioning. This debate was published in *The Journal of General Psychology* (1937, issue 16, pages 264–279). It is interesting to note that the debating papers were substantially longer than either Miller and Konorski's 1928 paper or Skinner's original paper (Two types of conditioned reflex and a pseudo-type. The Journal of General Psychology, 1935, 12: 66–67).

In addition, the translated paper is accompanied by Konorski's postscript giving his present, that is, late 1960s views. Konorski's postscript ends: "To sum up, we may come to the conclusion that almost every single thesis of the above paper is more or less erroneous. I consider this fact very fortunate, because it shows that further experimentation has led to an increasing clarification of our ideas concerning one of the most important problems in brain physiology: the intimate nature of type II, alias operant, alias instrumental, alias voluntary activities of the organism".

It is interesting to note in this context that criticizing Skinner's apparent disdain for formulating and testing hypotheses (*"Research designed with respect to theory is also likely to be wasteful... Most theories are eventually overthrown, and the greater part of the associated research is discarded"*). Konorski does not invoke Karl Popper's rejection of inductivist views on scientific method and replacing it with 'empirical falsification' approach (e.g. "Logic der Forschung", 1934, Julius Springer, Vienna, AT, translated by the author and published in English as "The Logic of Scientific Discovery", 1959, Hutchinson, London, UK; see also "Conjectures and Refutations", 1963, Routledge and Kegan Paul, London, UK). Karl Popper (later on, Sir Karl Raimund Popper) probably one of the greatest philosopher of science of the 20th century, fearing that Hitler might annex his native Austria, in 1937 immigrated to New Zealand. A few years later, in 1944, Popper has met and started to exert profound and strongly acknowledged influence on one of the most accomplished protégés and continuators of Charles Sherrington, the Australian neurophysiologist and later on co-recipient of 1963 Nobel prize in Physiology or Medicine, Sir John Carew Eccles. Konorski greatly admired Eccles's research program. It was presumably due to the support of Jerzy Konorski that one of the young 'Nenckians', Wlodzimierz (Vlod) M.H. Kozak was granted the Rockefeller Fellowship enabling him to work for a year (1960) in the laboratory of John Eccles in the John Curtin School of Medical Research at the Australian National University in Canberra. According to Eccles [see J.C. Eccles (1976) Under the Spell of the synapse. In: The Neurosciences Path to Discovery (Worden FG, Swazey JP, Adelson G, Eds). MIT Press, Cambridge, MA]. Popper encouraged him 'to make my hypotheses (electrical transmission across the neuro-muscular synapse) as precise as possible, so it would call for experimental attack and falsification'. Such precise formulation allowed Eccles truly successful falsification of his hypothesis and thus he provided a strong support for alternative hypothesis that synaptic transmission is chemically rather than electrically mediated.

Although, Konorski might have not been fully aware of elegance and power of empirical falsification as scientific methods, he always stuck to that way of conducting scientific research. Indeed, the analysis of the evolution of his ideas shows, that he has been changing or even abandoning his own deeply held views when experimental data of his pupils or other scientists clearly contradicted them. [3]

Notes

[1] I am well aware of anachronistic use of the term "Red Guard", which became known only much later, during Chinese Cultural Revolution of 1966–1976.

[2] I am grateful to B. Srebro for reminding me of Skinner's (1969) translation of Miller and Konorski (1928) paper.

[3] Both Krzysztof Turlejski and Bolek Srebro helped me in clarifying my thoughts and formulations.

Bogdan Dreher, School of Medical Sciences and Bosch Institute, University Of Sydney, Sydney, NSW, Australia

My conlcusion - a comment by Charles Gross

Professor Konorski's ideas on integrating the study of behavior with cellular neurophysiology certainly anticipated contemporary neuroscience. Indeed many of his points that once appeared polemical are now universally accepted by contemporary neuroscientists. The physiogical ideas of Pavlov and the anti-physiological ones of Skinner and Hull have all but disappeared except as historical curiosities.

Charles Gross, Department of Psychology, Princeton University, Princeton, NJ, USA

Comments on the paper "Study of behavior: Science or pseudodscience" by J. Konorski - by Giacomo Rizzolatti

The more I was advancing in the reading of this essay by Konorski the more I was impressed by the depth of his thought. The analyses of the work of Pavlov, Skinner and Hull, which represents the core of the essay, are of great historical interest and, most importantly, are very relevant to modern neuroscience.

The most interesting section is probably that devoted to Pavlov. Konorski first summarizes what he considers to be the foundation of Pavlov thinking. He writes: "Pavlov claimed that experiments on brain functions, executed in anaesthetized animals are not a "true" physiology of this organ for the following reasons. The brain is the organ developed for highest control and integration of animal behavior on the basis of messages arriving to it from receptors. This is its real and unique function. Therefore, studying its activity in a situation in which this function is strongly *reduced* and *simplified* by anesthetic drugs, is unreasonable. The only way to approach the normally functioning brain is to make experiments on *wakeful animals* by presenting them with various types of *natural stimuli* and observing their response (The italics are mine)

Using these premises (behaving, not simplified animals; natural stimuli) Pavlov discovered the "conditional reflexes" and, some years later, Konorski, described what is now known as "operant conditioning". It might seem absurd, but one century after Pavlov, what Konorski learned from Pavlov is often neglected. Enormous amount of money and energy are allocated for understanding "how human brain works" unraveling the microcircuitry, for example, of the mouse visual cortex. Is this really the most important field of research? Is not perhaps the study of intact individuals (animals and humans) the main avenue for understanding, as Pavlov maintained, the normally functioning brain?

The criticism by Konorski of the last part of Pavlov studies is of great relevance for the neuroscience of today. According to Konorski, Pavlov in expanding his studies of conditional reflexes neglected the "true physiology", the one based on the work of Cajal and Sherrington, and constructed a self-referential "physiology". Konorski writes: *"(Pavlov) claimed that the excitatory or inhibitory processes originating in the given points of the cerebral cortex by the operation of corresponding conditioned stimuli irradiate all over the cortex and then concentrate back to their departure points. On the basis of these notions Pavlov attempted to explain all the mani fold properties of both positive and negative conditioned reflexes and their complex interrelations."*

It is unbelievable how history repeats itself. Brain imaging has been *the* frontier in neuroscience for about a decade. This technique can still give important contributions to neurosciences, but with a caveat. It must be linked to neurophysiology. If this link is severed, brain imaging becomes, as it occurred with the late work of Pavlov, a self-referential method, a method that links mysterical blood flow phenomena, typically of oscillatory type, with poorly defined mental phenomena.

The criticism of the work of Hull is also surprisingly modern. For Hull animal behavior was the principal aim of his research, but he added a number of concepts called "constructs", which intervene *between* the stimulus and the response and are chosen in such a way as to account for empirical data obtained in behavioral experiments. Habit strength, reaction potential, inhibitory potential are some of these constructs. Konorski writes: "From my point of view the system proposed by Hull cannot, by definition, be quite satisfactory just because of its abstract character. After all, in the time when Hull undertook his life work, the "major neurological laws" were fairly well understood".

Remember the "boxology" of the "cognitive revolution"? The similarity between Hull concepts, disconnected from neuroscience, and those of cognitive psychology is impressive. The same conceptual "sin". Arbitrary, *a posteriori* interpretations of neurological facts with a tenuous link with the neurophysiological reality.

Extremely fair is the judgment of the work of Skinner. It is hard to say how much his stubborn refusal to look inside the nervous system influenced negatively the development of psychology of his days. However, as Konorski writes: *"The above discussion on the validity of Skinner's general approach to the study of behavior should not in the least detract from his important methodological contributions in this field. Indeed, Skinner's methods are used in almost every behavioral laboratory and they play a significant role in the hands of those who aim at understanding animal behavior by physiological mechanisms."* One cannot agree more.

Konorski starts his essay asking himself what could be his "legacy" to science. Here I want to write a personal note. When I was a post-doc in the Institute of Moruzzi in Pisa, I read Konorski's book "Integrative Activity of the Brain" (1967). Of course I knew the work of Hubel and Wiesel and that of Lettvin and Maturana, but Konorski's prediction of "gnostic neurons" was a kind of illumination for me. "This is how brains works", I thought. A few years later Charlie Gross and coworkers discovered that many neurons in the inferotemporal lobe have indeed the characteristics of gnostic neurons. As Charlie Gross recognized (Neuroscientist 2002) the notion of gnostic neurons was the idea driving his fundamental experiments on the physiology of the temporal lobe.

The idea that the secret of the brain can be revealed if we discover the "gnostic neurons" of the brain was the hidden force behind my own experiments. I approached the motor system with the ideas that there is something more in it that the capacity to "generate" movements. In other words that also the motor system should work in virtue of something like the "gnostic neurons" of Konorski. It was a very fruitful approach. It led us to prove that neurons in the premotor cortex encode the goal of motor act and not the movements, that a sector of premotor cortex encodes the peripersonal space, and eventually that there are neurons that are involved in understanding the behavior of others (mirror neurons). Mirror neurons are, after all, nothing else than "gnostic" neurons of the motor system.

Giacomo Rizzolatti, Dipartimento di Neuroscienze, Sezione di Fisiologia, Università di Parma, Parma, Italy; Brain Center for Motor and Social Cognition, Italian Institute of Technology, Parma, Italy