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Memorial Paper in Honor of Jerzy Konorski

JERZY KONORSKI MEMORIAL ADDRESS

O. Hobart MOWRER

Department of Psychology, University of Illinois, Champaign-Urbana
Illinois, USA

Jerzy Konorski's name first became known to American psychologists as a result of the following circumstances.

Instrumental reinforcement of autonomic responses

In 1935, B. F. Skinner published a paper entitled, "Two types of conditioned reflex and a pseudo type". Here he analyzed the similarities and differences between classical Pavlovian conditioning and the type of learning commonly known in this country, especially as a result of the experimental studies of E. L. Thorndike and common observation, as habit formation. Skinner also pointed out that sometimes habits occur, not spontaneously, but only when a specific stimulus is presented, thus giving the appearance of being conditioned responses, when, in point of fact, they simply represent a form of discrimination learning. Here the subject finds that a given habitual, or instrumental, response will "work" only when a specific stimulus is present or other special circumstances prevail. This is what Skinner, in the 1935 article, meant by "a pseudo type" of conditioned reflex.

Without any apparent knowledge on Skinner's part, Konorski and a Polish colleague, Stefan Miller, had for some years prior to the appearance of Skinner's article also been interested in the similarities and differences between habits and conditioned responses. So soon after the Skinner article appeared, they published, in 1937, a critique, citing seven previously published related articles dating back to 1928. In many respects Miller and Konorski were in agreement with Skinner's analysis; in other respects they differed. They took particularly strong exception

to a rather oblique and incidental conjecture (on p. 67) in Skinner's article to the effect that responses mediated by the autonomic nervous system may be subject to modification under the same conditions as are relevant in habit formation. In fact, Konorski and Miller concluded their discussion with this emphatic statement:

According to the existing state of knowledge — and we dispose [sic] of no facts to the contrary — the conditioned reflex of the new type [by which they mean habits] is confined exclusively to striped muscles, while the classical type has no restrictions laid on effectors and includes among them, besides striped muscles, smooth muscles and glands. Skinner's imaginary case shows that he overlooks this restriction, saying that a salivary hypothetical reaction to a stimulus different than food (unconditioned), e.g., light is liable to be increased by food reinforcement. Being a glandular reaction, salivation cannot by any means be made a conditioned reaction of the new type [i.e., an instrumental response or habit].

The editor of the journal in which Skinner's 1935 article appeared, arranged for him to write a rejoinder which was published at the same time as was the Konorski–Miller critique. Here Skinner (1937) considerably modified his terminology and concepts (in ways which were to become basic to his 1938 book, *The behavior of organisms*), but he refused to reject the *possibility* that responses mediated by the autonomic nervous system may be instrumentally reinforced. He said: "That responses of smooth muscle or glandular tissues may or may not enter into Type R [learning], I am not prepared to assert" (p. 279).

It has long been commonly accepted that the Fakirs, of India, sometimes learn to control certain physiological (and normally involuntary) processes, and during the early part of this century many psychoanalytic writers postulated that emotional responses may function and be reinforced instrumentally. But for roughly three decades after the exchange between Konorski and Miller and B. F. Skinner, American psychologists, on the basis of rather scant but consistent experimental evidence, increasingly agreed that autonomic responses can be developed only by classical conditioning, not by instrumental training methods. In fact, even Skinner, in his 1953 book, *Science and human behavior,* wrote:

"Glands and smooth muscles do not *naturally* produce the kinds of consequences involved in operant reinforcement, and when we arrange such consequences experimentally, operant conditioning does not take place. We may reinforce a man with food whenever he 'turns red', but we cannot in his way condition him to blush 'voluntarily'. The behavior

of blushing, like that of blanching, or secreting tears, saliva, sweat, and so on cannot be brought *directly* under the control of operant reinforcement. If some technique could be worked out to achieve this result, it would be possible to train a child to control his emotions as readily as he controls his hands" (p. 114, italics added).

So it would appear that Konorski and Miller were correct in their original criticism of Skinner's 1935 article and hus had a significant impact on learning theory and research here in the United States, and elsewhere. But in the 1960's, there was to be a surprising turn of events. In the late 1950's, several investigators, using different techniques and working more or less independently, initiated a variety of experiments which were eventually to show that autonomic responses *can* be instrumentally reinforced and thus brought under "voluntary control". Although there is considerable difference of opinion as to precisely *how* this effect is produced, there are now a number of published reviews of the relevant literature, one of the most recent and incisive of which is that of H. D. Kimmel, entitled "Instrumental Conditioning of Autonomically Mediated Responses in Human Beings" (1974).

How did Jerzy Konorski, in his last and most definitive book, *Integrative activity of the brain: An interdisciplinary approach* (1970*a*), evaluate these developments? In this book there is a long chapter entitled "The Origin and Physiological Basis of Instrumental Movement". Here Konorski refers to "the long-debated instrumentalization of autonomic responses" and says that "recent data obtained in Miller's laboratory [Miller and Carmona 1967] seem to confirm the existence of such a mechanism" (p. 483). But then Konorski adds: "Since we have no evidence that [autonomic responses] have a special analyzer [i.e., a sensory projection area in the brain] analogous to the kinesthetic analyzer for somatic movements, we do not believe that they can be instrumentalized *in the same way* as are the motor acts" (p. 482, italics added). Thus, we find Konorski in agreement with Skinner's 1953 statement that autonomic responses cannot be "brought *directly* under the control of operant reinforcement". This position is in accord with the fact that some of the most successful instances of instrumental reinforcement of autonomic responses have involved the introduction of "artificial" sensory feedback from such responses, which often occur not only involuntarily but also unconsciously (see Shapiro et al. 1972). In other parts of his 1970 book, Konorski refers to images that can be called up voluntarily which may then, in turn, have the power to produce autonomic responses. This procedure, along with the occurrence of voluntary skeletal responses which may produce autonomic changes, is referred to in the Kimmel review as the "mediation hypothesis".

As already indicated, there is today no general agreement on how autonomic responses can be brought under voluntary ("instrumental") control, and one of the reasons seems to be that this result can probably be accomplished in more than one way. Various investigators have been quick to see and test the possible utility of instrumental modification of autonomic states in various functional, or psychosomatic, disorders. The results to date have been somewhat ambiguous (see Jonas 1972) but promising enough to prompt Kimmel to end his recent review of this field with the following comment:

"The rapidly growing interest and experimentation in the field of instrumental autonomic conditioning, from the not-so-distant past, when it was "not so much imaginary as impossible", to the present, when biofeedback machines for home use are being recommended, must go down as a most remarkable scientific about-face" (p. 333–334).

It must be added, however, that the "about-face" to which Kimmel (1974) here refers is not quite a complete one, since no one has yet asserted and unequivocally demonstrated that instrumental autonomic learning occurs "naturally" (see 1953 quotation from Skinner) or, to use Konorski's words, "in the same way" as does the learning of motor acts (1970*b*).

Confusion and clarification with respect to instrumental learning

Let us drop back now to the second set of circumstances under which American psychologists became aware of the work of Jerzy Konorski and his associates. In 1948 Konorski published a small book (which he usually referred to as a monograph) entitled *Conditioned reflexes and neuron organization.* This book contains a chapter on "Conditioned reflexes of the second type" which has a short preface: "Dedicated to the memory of Dr. Stefan Miller, killed by the Nazis. This section of the physiology of higher nervous activity was elaborated in a common association with him of effort and thought" (p. 211).

At the outset of this chapter Konorski says that what he is calling "conditioned reflexes of the second type" are "the same changes which the behaviourist psychology has studied for many years under the group term of 'habits'"; and he cites both the positive (reward) and negative (punishment) parts of Thorndike's Law of Effect. In addition to giving examples of behavior being "stamped in" (reinforced) by reward and "stamped out" (inhibited) by punishment, Konorski here describes not only the positive discrimination learning which Skinner in his 1935 article referred to as "a pseudo type" of conditioning, but also what is today commonly referred to as "avoidance conditioning" or what Ko-

norski called "defensive conditioning". That is to say, he and Miller had found that if, for example, a dog is presented first with a conditioned stimulus of some sort and then an electric shock which causes the dog to flex a particular leg, a conditioned flexing of the leg will continue to occur for a long time if, following the response, the UCS is omitted, rather than being regularly paired with the CS, as in classical Pavlovian conditioning. Being at that time (1948) still under the influence of Pavlov's radical objectivism, Konorski said he was "unable to explain the causes of this phenomenon" except to speak teleologically and suggest that the dog keeps responding to the CS, if it is *not* followed by the negative UCS, "in order to rid itself of the stimulus which heralds the negative unconditioned stimulus" (p. 231). In this book, Konorski also advanced evidence for believing that sensory feedback plays a crucial role in instrumental learning or habit formation.

It appears that Konorski's 1948 book was not widely read in this country. At any rate, the Cambridge University Press, which published the book, reports (private communication) that only 399 copies were sold here. This supposition is confirmed by Konorski himself, in his autobiography (1974). Of the 1948 book he says:

"Its reviews were generally favorable, although living in Poland and having no relations with Western scientists I had no feedback about how it was accepted. My feeling was that while in England the book became quite popular and well known, in America it passed almost unnoticed, judging from the very sparse references to it in papers and monographs concerned with the problems of conditioning. My explanation of this fact is that at that time (1948) experimental psychology was strongly Skinnerian or Hullian, and physiological explanations of the mechanisms of conditioned reflexes were utterly unpopular. I suspect that either people did not read the book at all, not being attracted by its title, which to me seemed highly attractive, or else, if they had it in their hand, they rejected it" (p. 204).

If I had known of Konorski's *Neuron organization and conditioned reflexes*, I would have had special reasons for citing it (which I did not) in *Learning theory and personality dynamics* (1950). Instead I cited an instance of avoidance learning reported by Brogden et al. (1938) but not adequately analyzed by them (and some research of my own) to show that all one has to do to take the mystery (teleology) out of the persistence of a successful "conditioned defense reflex", to use Konorski's terminology, is to posit that if an initially neutral stimulus and a noxious one are closely associated in time, the CS acquires the capacity to arouse *fear*, and if a response produced by a CS, or danger signal, is never fol-

lowed by the UCS when the response occurs to the CS alone, then the occurence of this response *and its associated sensory feedback* serve as an all-clear or safety signal, reduce the fear elicited by the CS, and thus provide a form of secondary reinforcement, namely, relief from the fear aroused by the danger signal. Thus, instead of saying that conditioned avoidance responses occur *in order to* accomplish this or that, we see that they persist long after the UCS has ceased to occur *because* they reduce the conditioned, and very durable, secondary drive of fear, and thus fall into the category of habits in general (which may be reinforced either by primary or secondary drive reduction). Although such an interpretation is only common sense, it had been rendered inadmissible by Watsonian behaviorism and, of course, by the ultra-objectivism of Pavlov, which played an important role in Konorski's thinking at the time he wrote the 1948 book. As we shall presently see, Konorski was later to change his position with respect to this point of view.

The pragmatic soundness of — indeed the necessity for — the view of avoidance learning just suggested has been demonstrated in a variety of experiments in which laboratory animals, not to mention human beings, readily learn to make responses to a danger signal which are radically different from the behavior produced by the original noxious unconditioned stimulus (see, for example, Mowrer and Lamoreaux 1946, Miller 1948). Thus, at a relatively early point it was demonstrated that the classical Pavlovian conditioning paradigm does not provide an adequate explanation of avoidance learning, which must instead be regarded as involving both conditioning (of fear) *and* habit formation (due to fear reduction).

As early as 1936, John B. Wolfe demonstrated that, with Chimpanzees as subjects, poker chips which have been associated with the presentation of food take on secondary-reward value, as indicated by the fact that these "tokens" can be used, independently of food, to set up any of a variety of new habits. Thus, Clark Hull (1943) and many other behaviorists came to accept this type of secondary reinforcement but defined it (so as to avoid subjectivity) as "anticipatory goal reactions". It is now clear that whenever a primary drive — such as hunger, thirst, thermal discomfort, suffocation, or any other type of primary drive — is operative, any stimulus (or "situation"), if associated a few times with the reduction or elimination of primary drive, will reduce the "worry" or "apprehension" (really a type of fear) which accompanies the drive, thus acting, not as a danger signal, but as a promise or safety signal, and mediating a form of fear reduction commonly known as *hope*. (This phenomenon has been objectively demonstrated, among other authors, by Segundo and Galeano 1961, see also Beck and Brooks 1967).

Thus, in 1947, I published a paper entitled "On the dual nature of learning — A reinterpretation of 'conditioning' and 'problem solving'". By this time, a number of American psychologists had gone on record as favoring the view that there are two fundamentally different kinds of learning, Pavlov's classical conditioning and Thorndike's habit formation. In the article just mentioned the "reinterpretation" consisted of an extension of the role of habit formation, or instrumental learning (Hilgard and Marquis 1940), to include many situations which had previously been interpreted as pure instances of Pavlovian conditioning. Thorndike had virtually ignored the phenomenon of conditioning, or "associative learning", as he called it, and had maintained that if it occurs at all, it is of relatively minor consequences. Hull, who had been much influenced by Thorndike (Hull 1935), made habit formation the basic form of learning and suggested how conditioning might occur as a sort of by-product (Hull 1943). And N. E. Miller, a student and long-time colleague of Hull's, has, as we have seen, worked long and hard to show that visceral, as well as motor, responses can be enhanced by reward. My position in the 1947 paper, was that conditioning and habit formation should be recognized as separate and distinct (but often interrelated) forms of learning.

But then, as time passed and new experimental evidence became available, I began to move toward a more monistic position: perhaps conditioning could be used to account for habit formation and its antithesis, inhibition. If fear and hope (and their opposites, relief and disappointment) could be conditioned to external stimuli, could they not also become conditioned to response-produced stimuli and thus account for both the facilitation and inhibition of motor responses? The case of punishment was quite straightforward: a well established habit occurs, is followed by punishment (from an inherently noxious stimulus or the conditioned phenomena of fear or disappointment), and fear gets conditioned to response-produced stimuli and thus produces conflict and perhaps, eventually, complete inhibition.

The explanation of habit formation in terms of conditioning seemed, at first, equally straightforward: a response occurs more or less randomly, from discomfort, or by passive manipulation, and is regularly followed by reward. As a result, the sensory feedback from this response will soon start acting as conditioned stimuli for secondary reinforcement, thus increasing the probability of the response's recurrence. Thus inhibition was produced, not by the "stamping-out" of direct S–R neural bonds, as Thorndike and his successors had assumed, but by the conditioning of negative feedback (notably, fear) to the proprioceptive and other types of stimuli produced by the occurrence of a punished response; and habit

formation, could be likewise interpreted as the conditioning of positive secondary reinforcement (or hope) to the sensory feedback from responses which had been followed by primary reinforcement such as food. I decided that the attempt made by Pavlov in 1932 to derive habits from conditioning principles was rather awkward; but I now felt that he, rather than Thorndike and Hull, was on the right track.

It may be that Pavlov was prompted to write the 1932 paper just when he did because, from 1931 to 1933, Konorski (and for a brief period, also Stefan Miller) was working in his laboratory, in Leningrad, and was primarily concerned with what Konorski called Type-II conditioning, namely instrumental learning, since he and Stefan Miller, from their first reading of Pavlov's 1927 book, had felt that conditioning (Type–I) was not the sole form of learning. In his recently published autobiography, Konorski (1974) says:

"There was no doubt that Pavlov highly appreciated the importance of our contribution to the field of conditioned reflexes, which, according to his own words, led to 'physiological understanding of volitional movements'. However, he strongly opposed our thesis claiming the existence of two types of conditioning and failed to see any difference between them. He was so sensitive about this point that when writing the above-mentioned paper for his journal we simply did not dare to use our own terminology and called type II conditioned reflexes 'motor conditioned reflexes' or 'conditioned reflexes of the motor analyser'. Both of these terms were misleading.

It should be noted that this negative attitude of Pavlov toward the specificity of type II conditioned reflexes had a detrimental effect on the development of the study of these reflexes in Russia. In fact, had Pavlov accepted this specificity, the situation would have been clear and the experimental work on this type of conditioning would certainly have developed in Russia as it did develop, quite independently of our work, in the United States, where type II conditioned reflexes were called 'instrumental' or 'operant' responses. However, when the greatest authority in this field stated that type II conditioned reflexes simply do not exist, this was decisive and meant special investigations along this line, with insignificant exceptions, were not undertaken in Russia. (p. 195–196).

Although, in my two 1960 books on the psychology of learning (Mowrer 1960*a*), I ended up with a position much closer to that of Pavlov (that all learning can, in the final analysis, probably be reduced to conditioning) than to that of Konorski (who, from the beginning of

his scientific career, was convinced that there is a fundamental distinction between Pavlovian conditioning and the type of learning particularly associated in this country with the names of E. L. Thorndike and B. F. Skinner). But, paradoxically, the work of Konorski and his many colleagues and students was uniquely helpful to me in arriving at this position.

By the time the 1960 books were in preparation, I had in some way learned of Konorski's 1948 book, *Conditioned reflexes and neuron organization;* and here I discovered that the methodology developed by Konorski and Miller in the late 1920's for the study of what they called type-II conditioned reflexes was different from the trial-and-error procedure used by Thorndike in studying habit formation. Instead of letting a hungry animal (frequently a cat) fumble around in a "problem box" until it happened to make the response or a series of responses which Thorndike had predetermined as adequate for the release of the subject from the box and access to food, Konorski used a procedure known in this country as "putting-through". For example, a dog would be put in a Pavlov stand and instead of being subjected to classical Pavlovian salivary conditioning, a light cord would be attached to one of the animal's forelegs, a conditioned stimulus would be sounded, the dog's leg would be passively flexed, and reward would follow. Soon the dog would be flexing its leg "spontaneously", whenever it was ready for more food, but these "interval" responses were ignored, and the dog would be rewarded only when the signal was given and the dog flexed its leg, without the necessity of its being flexed passively by means of the cord (Skinner's pseudo-type conditioning").

Throughout Konorski's 1948 book considerable importance is attached to the importance of response-produced stimuli in the development of type-II conditioned reflexes — or, as we would tend to say habits — which are performed upon cue. Here is an example of Konorski's thinking in the 1948 book:

"When we raise the dog's leg to a certain height and reinforce this act by food, a conditioned reflex is formed not only to this concrete compound of proprioceptive stimuli, but, in accordance with the law of generalization, to all similar compounds. Hence it follows that the dog does not necessarily perform the exact movement which we tended to inculcate, but rather different movements (lower, partially with other limbs, etc.), at the disposition of his motor system and within the "field of generalization' of the given reflex. However, if we do not reinforce such movements, but only those of a definite height, definite shape,

etc., gradually differentiation is formed, because only the *definite* compounds of proprioceptive stimuli are reinforced and in consequence only they become excitatorily conditioned. And it is this differentiation which effects the gradual shaping of an active movement in the form which the experimenter requires" (p. 217).

Here was grist for my mill! Motor responses become habitual because the sensory feedback produced by these responses takes on secondary-reinforcing, or hope-producing, properties. And although Konorski maintained that what was going on here was basically different from conditioning as Pavlov conceived it, he was, as it seemed to me, providing excellent evidence that type-II conditioning, or habit formation, is dependent upon type-I conditioning, the position which Pavlov himself had tried to demonstrate, though not very convincingly, in his 1932 paper. That Konorski gave response-produced stimuli an important role in the establishment of habits should not have surprised us, for, as we have seen near the outset of this paper, he and Miller, in their early controversy with Skinner, took the position that "Since we have no evidence that [autonomic responses] have a special analyzer [i.e., a sensory projection area in the brain] analogous to the kinesthetic analyzer for somatic movements, we do not believe that they can be instrumentalized in the same way as are the motor acts" (see p. 3–4). By implication, Konorski is here saying that sensory feedback from a motor response is essential to its development as a type-II conditioned reflex, or habit.

In the first of my 1960 books (p. 292 ff), I therefore drew heavily upon Konorski's research. In his 1948 book Konorski had said that "the proprioceptive stimulus constitutes an indispensable complement to the conditioned compound" in the formation of type-II conditioned responses, which implies that this type of habit formation would be impossible if the foreleg used in Konorski's experiments were deaffernated. And in 1958, two of Konorski's associates (Stępień and Stępień) published a paper entitled "The effects of ablations of the sensori-motor cortex of instrumental (Type II) conditioned reflexes", in which they state:

"When brought to the experimental chamber, the dogs behaved quite adequately; they stood correctly on the stand awaiting the conditioned signals. They manifested a very clear and prompt general alimentary reaction to the conditioned stimuli consisting in turning towards the food-tray and salivation, but they were completely unable to perform the movement of putting the leg on the food-tray" (p. 311).

From about 1962 onward, I received all the research reports in English which were published by Konorski and his co-workers; and it

was evident that, as a result of further investigation, habitual responses were not quite so totally dependent upon response-produced sensory feed-back as had been previously supposed. In 1970 Konorski published a paper entitled "The problem of the peripheral control of skilled movements", which he sums up by saying:

"After deafferentation of a limb, the animal is still able to perform instrumental responses with this limb, if these responses are either very simple or well trained. On the other hand, if the response requires precision, it cannot be performed by the deafferented limb. Probably training of skilled movements in the young also requires afferent input from the limbs concerned" (p. 49).

And in the 1970 edition of Konorski's *Integrative activity of the brain,* he speaks even more emphatically on this score. He says:

"The major issue emerging from this discrepancy is that whether or not a proprioceptive stimulus generated by the instrumental response becomes a consummatory food CS, this does not influence in any ostensible way the very process of instrumental conditioning. This is in clear opposition to all theories (including our own original theory) which claim that the proprioceptive feedback of the performed movement is essential for instrumental conditioning as a sort of secondary reinforcement" (p. 415).

Now I was by no means dismayed, or even surprised, by these findings, for in the 1960 books, I had already anticipated a difficulty with placing the entire process of habit formation and inhibition upon the conditioning of hope or fear, respectively, to response-produced stimuli. The theory presented no special problem as far as punishment and the resulting inhibition of a response were concerned. A previously punished response starts to occur, this sets up a flow of sensory impulses which have been associated with punishment and, as a result, are now conditioned stimuli for fear, and the subject finds that the quickest and best way to eliminate the fear thus generated is to inhibit the previously punished response.

But before a habit can provide sensory feedback that arouses hope or positive secondary reinforcement, it must be in some way *selected,* from innumerable other possible responses, and *initiated.* Once chosen and initiated, the sensory feedback from the response in question can provide both hopes and fears to guide it to its proper culmination or to inhibit it. And I think Konorski over-states the case when, in the passage last quoted, he seems to completely denegrate the role of sensory feedback in the inhibition or guidance of motor responses (see Mowrer 1960*a*, p. 279–281, and 1960*b*, p. 283–288). But the phenomenon of res-

ponse selection and initiation cannot, it would seem, be entirely dependent — or, in fact, dependent at all — upon peripheal mechanisms.

In order to deal with this problem at all adequately, I had to have recourse, in the 1960 books, to the concept of *image,* which was operationalized as *a conditioned sensation* (see McMahon 1973). Such a concept has important uses, not only in the present context, but also in accounting for sensory preconditioning and various other instances of "mediation", especially in connection with language and thought. In conditioning type I, we may conjecture that the first thing that occurs when the CS is presented is an anticipation or *image* of the impending UCS, and that in conditioning, type-II, an image of the correct response precedes and selects and initiates the response itself. Not only do we mentally "plan our day"; but each and every action, before it occurs, is visualized, considered, and decided upon or rejected, unless these actions have, by long usage, become more or less automatic, i.e., subject to the control of lower centers of the brain — a point of view not significantly different from or an advance beyond Wiliam James' "ideo-motor" concept as put forward in the chapter on "Will" in *Principles of Psychology* (1890).

In *Integrative activity of the brain* Konorski arrives at the same conclusion. For example, on page 193 he says:

"As far as the psychological evidence is concerned, we know that imagining a certain movement leads either to its overt performance or at least to its latent performance, manifested by the EMG record. The idea that the image of a movement is an agent eliciting that movement was clearly stated by James (1890), who called this phenomenon 'ideomotor action', and who provided extensive evidence to show that it is true" (p. 193).

Konorski then quotes three long paragraphs from James, and then adds:

"These quotations show how clearly James saw the mechanism of voluntary movements, a mechanism at which he had arrived on the basis of astute introspection. It is most encouraging to know that we have come to exactly the same concept by quite different considerations — namely, through the physiological analysis of these movements" (p. 194).

In his Autobiography, Jerzy Konorski says that while still an undergraduate at Warsaw University, a consuming and enduring desire developed in him to learn "how the brain works" (p. 185) but that it was not until he and his close friend and fellow student, Stefan Miller, who had much the same interests, had read Pavlov's book, *Conditioned*

reflexes, that they saw a creative and feasible method of approaching this objective. First, foremost, and always, Konorski pursued this objective by means of behavioral experiments; but in his final, culminating work, he brought to bear upon the task of understanding the "integrative action of the brain", additional sources of knowledge: observation of patients in a mental hospital near Warsaw where he first obtained employment after becoming a physician; observation of patients in a Russian military hospital with various head injuries during World War II; informal instruction in neurophysiology from Dr. Liliana Lubińska, who had intensively studied this subject in Paris and who, in the Fall of 1933, after his return to Warsaw from Pavlov's laboratory, became Jerzy Konorski's wife and talented collaborator. Interestingly enough, as his scientific career developed, Konorski also became an astute introspectionist.

"Integrative activity of the brain" and its evaluation

Although the author of *Integrative activity of the brain* extensively discusses both research findings and theory concerning type I and type II conditioning, he does not hesitate to consider events which, so to say, go on within a person's head. In order to illustrate how Konorski combined his various skills and far-ranging scientific knowledge, I invite the reader's attention to the following paragraphs:

"Comparing the diagram representing the connections directed toward the emotional field with diagrams representing the connections leading from that field to other analyzers, we may observe that all of them are strictly bidirectional. This means that if a given stimulus-object elicits by association a definite emotion, this emotion will also elicit the image of the same stimulus-object. For instance, if the visual perception of a given food gave rise to a strong appetite for it, hunger aroused by other factors will be likely to evoke the image of that food. Analogous examples can be given in respect to sexual emotions, fear, anger, and so on.

"This bidirectional character of connections linking the emotional units with gnostic units, combined with the long-lasting and inert character of emotions, discussed in Chapter I, leads to a peculiar phenomenon based on the positive feedback which may be called *emotional avalanche.* Let us explain this phenomenon by giving the following typical example. Suppose that we have recently experienced a dramatic accident, and after it is over try to have some rest or go to sleep. Since, however, the given emotional state, although in a much attenuated degree, is still in operation, it easily evokes through E→V connections the image

of the accident. This, in turn, through the V→E connections makes the emotion more intense, and this intensified emotion leads to the strengthening of the corresponding visual image. Thus, in a few minutes the emotion rises to the same intensity as at the time of the accident itself, and we find that we are unable to get rid of it without the help of a strong tranquilizer. Such an emotional avalanche is certainly familiar to everyone, and may concern any emotion — sexual drive, anger, despair, fear, and so on.

"There is no doubt that many psychiatric cases of long-lasting extreme aggressiveness, pathological anxiety, or depression are due to this mechanism of emotional avalanche. The fact that, even after the appeasement of the hyperemotional state by the tranquilizing drug, this avalanche immediately regenerates when the drug is no longer effective, is easy to understand if we take into account its self-augmenting mechanism. Therefore, the only possibility of annihilating this state is either to eradicate the given emotion by psychotherapy or, if it is impossible, to sever the reverberating circuit leading to its regeneration. This latter is probably provided by prefrontal lobotomy, after which the patient is still able to experience various emotional states, but they lose their self-regenerating character.

"A special case of the emotional avalanche is provided by the phenomenon of 'intractable pain'. As was previously noted, this condition arises after a long-lasting severe pain and consists in its becoming totally independent of the original source evoking it. This is because the frequent attacks of insufferable pain become bilaterally associated with the fear of the pain; as a result, fear elicits pain by way of association, whereas this pain in turn increases fear. The complete cure of this state by frontal lobotomy was commented upon in Section 4" (Konorski 1970, p. 258–259).

Although Konorski makes no reference to the possibility, it seems not unlikely that the effectiveness of electro-convulsive shock treatments for depression, when they produce positive results, may do so by virtue of silencing continuously reverberating circuits of the kind described, not chemically or surgically, but electrically.

My first knowledge of *Integrative activity of the brain* came from a long and enthusiastic review entitled "The brain and the mind of man" which Alberta S. Gilinsky (1969) prepared for *Contemporary psychology*. This reviewer says, in part:

"It is still possible for the integrative activity of the brain of one man to produce a big scientific work uniting separate disciplines on a problem of first importance. With *Integrative activity of the brain,*

Jerzy Konorski, M. D., Professor of Neurophysiology at the Nencki Institute of Experimental Biology in Warsaw, has done it. In sharp contrast to the constricting ideas of contemporary behaviorism and molecular analysis, here is freedom cultivated and bearing fruit. The most interesting thing about Professor Konorski's approach is the degree to which he has seized the problem regarded by everyone as impossible: the problem of elucidating the relations between mind and the brain".

"The audacity of the attempt in itself is extraordinary. The achievement is magnificent. Here is a powerful explanation of the function of the brain as it controls perception, imagination, memory, and voluntary movements — a detailed and absorbing account of the way the brain organizes behavior to enable the organism to cope with its environment" (p. 224).

And Gilinsky concludes her review with these remarks:

"This is theory construction at its best. Konorski arrives at a theory of how the brain works that is not only plausible and consistent with an immense variety of data from many sources, but satisfies us that it can talk about real things — everyday objects and life-size problems. Instead of confining itself to the too frequent trivia and artificiality of the laboratory, this system ranges far afield. It is relevant to the business and the pleasures of life — the art as well as the science of living" (p. 228).

When I was invited to prepare this address, I wrote to the University of Chicago Press, and obtained copies of 12 other English-language reviews, besides Gilinski's, of Konorski's 1967 book (Andrew 1968, Delgado 1968, Flynn 1969, Gross 1968, Grossman 1968, Lieberman 1972, Ochs 1968, Pampiglioni 1968, Smyth 1971, Sołtysik 1970, Walter 1968, and Wiliams 1968). These reviews vary considerably in length (Grey Walter's is the longest — 10 pages) and in enthusiasm; but they are all respectful and regard Konorski's book as useful, if not momental. And for each of these 12 English reviews, there must have been one or more reviews in a variety of other languages.

Several of the reviewers comment on the influence of both Pavlov and Sherrington upon Konorski; but it is Konorski's own view, as recorded in his Autobiography, that is most vivid and interesting in this connection. Upon returning to Warsaw, after the period of work with Pavlov (1931–1933), Konorski says:

"It was quite clear to me that the Pavlovian and Sherringtonian concepts of the functioning of the nervous system could not be reconciled with each other, and it was even impossible to find a "dictionary" which would translate one set of notions into the other. Simply, one of

the two theories should be rejected in toto, and the facts so far explained by the rejected theory should be reinterpreted in the framework of the other theory.

"By that time [1933–1939] I had no doubt that it was Pavlov's theory that should be rejected. The more I pondered Pavlovian explanations of various facts in the field of conditioned reflexes and tried to analyze the explanations, the more I discovered inconsistencies and contradictions in the Pavlovian interpretation of the facts. Thus, the idea grew in my mind to try to explain the whole bulk of experimental work collected by Pavlov's school by the Sherringtonian principles of functioning of the central nervous system" (p. 198).

How does Konorski handle the mind-body problem, i.e., the process whereby electro-chemical events become converted into mental events or consciousness? It is my own feeling that in *Integrative activity of the brain,* the author never tackles this problem, head-on-and perhaps for good reason. One of the reviewers, Charles Gross, says:

"The transit units communicate with higher-level units in the same sensory system. The lower-order exit units may give rise, *inter alia,* to 'targeting' or 'orientation' reflexes, reflexes controlling sensory input, and reflexes to noxious stimuli. The higher-order exit units are 'gnostic units'; and they form the anatomical substrate of cognition and association".

This paragraph presumably tells us *where* the transformation occurs but it still leaves open the question of precisely, or even approximately, *how* it takes place. Perhaps the harshest criticism to come from any of Konorski's English reviewers is that of Grey Walter, who says:

"All these processes [involving neural activity] can be followed in the bioelectrical effects that reflect the underlying chemical changes in the brain. Yet Konorski chooses to ignore both the electrosemiology and the biochemical transactions, which are the currency of brain business. Instead, he resorts to a succession of schematic diagrams that can only be descriptive and rarely lead to the formation of testable hypotheses" (p. 299–300).

In addition to his *magnum opus* and the earlier book *Conditioned reflexes and neuron organization,* Jerzy Konorski has published, alone or in collaboration, 184 scientific papers. The pertinent references to these paper have been organized chronologically and published under the editorship of Renata Głowacka (1974).

Upon inquiry from the publisher, I learned that the first printing of *Integrative activity of the brain,* in 1967, was for 3,000 copies, which

were not sold until 1970, at which time a small, second printing of only 1,015 copies was run off. The book has therefore not had the reception which Gilinsky — and several other reviewers — have predicted. In his autobiography, Konorski himself comments on the limited sale and somewhat dubious reception of this book. He says:

"Contrary to my expectations the reaction to the book was rather poor. I had a feeling that many of my friends and colleagues simply disliked it, or had not read it, or even did not know about it, because its advertising was inadequate. I had an impression that with a few manifest exceptions, the book was received coldly or even in an unfriendly way... .

"The great drawback of the book is that it is too concise, since within one volume I have condensed material which could be presented in two separate volumes. This makes the book difficult and requires very attentive reading and rereading. I was also told by my colleagues that some paragraphs are not sufficiently clear.

"I am very curious to know what will be the final fate of the book: 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 science. I am afraid that the later fate may prevail..." (p. 214).

Perhaps one of the reasons for disappointment with respect to Konorski's last book is that, without exactly saying so, it seems to imply a solution to the mind-body problem. Gilinsky, in her review, comes close to positively asserting that the book succeeds in this objective. However, most other reviewers are less impressed with the book's accomplishments on this score. Konorski speaks of various "gnostic analyzers" or perceptual centers, but to say *where* a certain mental phenomenon occurs is not to explain *how* it occurs. More or less successful attempts to establish the locus of certain types of mental activity in the brain have been in progress for a long time, but we still do not know how the electrochemical activities of the nervous system become transformed into, or mediate, consciousness. The Konorski book represents some advances in brain localization, organization, and neurological functioning, but the chasm between physical and mental events seems almost as wide as ever. The expression, "gnostic analyzer", is only a semantic innovation, not a solution to the age-old mind-body problem.

Difficulties and their resolution

In 1949 a development occurred in the Soviet Union which had far-reaching consequences for the scientific work and reputation of Jerzy Ko-

norski. When Konorski's just-published book, *Conditioned reflexes and neuron organization*, became known in Russia, it was bitterly denounced. Konorski was guilty of criticism on two counts with respect to Pavlovian theory. From the very outset of his association with Pavlov, there was, as we have seen, a difference of opinion between the two men — one a scientific giant and the other a youthful novice — with respect to the question as to whether there are two types of conditioning processes or only one. The question, to this day, has not been definitely settled, but there was no doubt concerning the truth of the matter in Pavlov's mind.

Konorski's other, in some ways more serious "deviation", involved his decision that Charles Sherrington's work provides a sounder basis for neurophysiology than do the theories of Pavlov (see p. 19). Near the end of his autobiography, on page 215, Konorski makes the following brief but interesting comment:

"When through Lubińska [Konorski's wife] I came across the Sherringtonian physiology of the nervous system based on Ramon Y. Cajal's notions of its anatomical organization, it was possible to project the studies on conditioned reflexes upon the actual network of the brain. I accomplished this task by writing my earlier monograph" (Konorski 1948).

All this made it unmistakably clear that Konorski was no "Pavlovian", and his subsequent behavior made it equally evident that he did not propose to become one. Yet Konorski always felt deep admiration and gratitude towards Pavlov. On page 214 of the autobiography, Konorski says that when, at an early age, he decided to devote his life to "the study of how the brain works", neither he nor his friend, Stefan Miller, had any idea of how to go about this. "At first I was completely in the dark, because I failed to come across those sources in which this question had already been coped with. The first beam of light came to me from Pavlov's work, which stimulated me to begin studies on this subject myself, in close cooperation with Stefan Miller".

The changes in East-West relations after 1955 were of the greatest importance for Konorski and what has come to be known as the "Polish School of Neurophysiology". They meant, first of all, that Konorski could now begin interacting with "the West" in a variety of ways that had previously been impossible. Konorski was able to make a personal visit to the United States, where he developed many friends and new scientific contacts. As a result, this country became, as Konorski phrased it, "some sort of 'intellectual market'" for the research findings of his laboratories. After this, he made numerous other visits, for conferences, lectures, and observation of what was being

done in American laboratories that was of special interest to him. There was voluminous exchange of scientific reprints; and, as a result, the work of Konorski and his associates became increasingly well known in a number of different scientific disciplines here. At the time of his first visit (in 1957), Konorski was pleased to find that even then he was by no means unknown: and during the ensuing years the research at the Department of Neurophysiology of Nencki Institute which was under his direction became, in a very real sense, integrated with what was going on in this country, although Konorski and his staff also continued to have cordial, though not particularly close, relations with many Russian scientists.

Something more along these lines will be said in the next section of this paper, but perhaps enough has been delineated here to incite in the reader a desire to get the comprehensive picture of the situation, as it is set forth, incisively and gracefully, in the autobiography.

Jerzy Konorski, man and scientist

Jerzy Konorski died, Semptember 14, 1973, three months short of the age of seventy. In May, 1974, Prentice-Hall, Inc., published Vol. VI of *A history of psychology in autobiography*, for which the editor, Gardner Lindzey, had solicited and received a chapter by Konorski. A few excerpts, especially pertinent to the foregoing discussion, have been quoted and give something of the tenor of this chapter as a whole, which, in many respects, is extraordinarily comprehensive and illuminating. But there are also some omissions. Konorski was deeply pleased and, in his Autobiography mentions, that during the 1960's he was elected a Foreign Associate of the National Academy of Sciences in this country; but, out of modesty, he did not indicate the many other honors and awards he had received. Following his death, accounts of Konorski's life and work appeared in a number of Polish Publications, which include many biographical sidelights and photographs. And Dr. Kazimierz Zieliński (1974), present Director of the Nencki Institute of Experimental Biology, has published in English a memorial essay which is especially useful because it gives a comprehensive picture of the high esteem in which Konorski was held, both nationally and internationally.

When, on October 31, 1973, Professor Bruce Overmier, of the University of Minnesota, invited me to prepare and deliver this address, I had only Konorski's scientific writings and the memory of some brief personal contacts with him as source material. At the same time that I wrote to Dr. Zieliński to ask for his assistance, I also addressed a letter to Professor John P. Seward who, to my great pleasure, has chaired this occasion today. Happily, John was able to make available a quite unexpected and unique source of information concerning Konorski.

I knew that he had followed Konorski's work with interest and that, a decade or so ago, he had briefly visited the Nencki Institute. But what I did not know is that Dr. Wanda Wyrwicka, a long-time member of Konorski's staff, is now on the faculty of the University of California at Los Angeles. As Emeritus Professor of Psychology at that institution, John knew that Dr. Wyrwicka is a member of the UCLA Department of Anatomy; and shortly after receiving my letter he contacted her, asking if she could tell him anything that might be useful in the preparation of this paper. I now quote from Professor Seward's letter:

"She volunteered to try at once. Here is the result, pretty much as she gave it, though transcribed from rough notes:

"Jerzy Konorski was born Dec. 1, 1903, in Łódź, Poland. His father was a lawyer, but J. K. supported himself from his early teens. He attended the public high school, went on to the University of Warsaw, majoring in mathematics and social science, and in 1925 entered medical school.

"About a year later Pavlov's book, *Lectures on the functions of the brain,* was published, and with the aid of a Russian dictionary Konorski and a fellow-student, S. Miller, read it avidly. Impressed as they were, they were not satisfied with Pavlov's treatment of voluntary motor behavior. Together they planned a research project, persuaded a psychology professor to give them laboratory space, and in 1928 published the first experimental results on Conditioned Reflexes, Type II.

"In 1930 Konorski was appointed to the staff of a psychiatric institute near Warsaw. But he had already decided to devote his life to scientific research. He and Miller wrote to Pavlov; he invited them to come to Leningrad, where Konorski spent two years, from 1931 to 1933, in Pavlov's laboratory. Although the two men differed on points of theory their disagreements did nothing to lessen their mutual respect.

"Returning to Warsaw without a position, Konorski was granted space to carry on his research at the Nencki Institute of Experimental Biology. Came the war and the Nazi occupation of Warsaw. Konorski escaped because his former colleagues in Russia had invited him and his wife to Moscow to attend a commemoration of Pavlov. He spent the remaining war years as head of the physiology laboratory at Sukhumi, Georgia, by the Black Sea, working mainly on war-related problems. Food was so scarce it was decided to sacrifice the animals of the municipal zoo. In view of their importance as scientists, Konorski and his wife and co-workers were awarded an elephant's leg. It was during this time that Konorski started his first book, *Conditioned reflexes and neuron organization,* published in 1948 by the Cambridge University Press.

"Konorski returned to Poland in 1946. Warsaw had been destroyed and the Nencki Institute was housed in three rooms in Łódź, the city of his birth. The University of Łódź appointed him Professor of Neurophysiology.

"Back in Warsaw, as head of the neurophysiology department in the Nencki Institute, Konorski gathered a distinguished group of associates and students from the three fields of medicine, biology, and psychology. The result has been a steady flow of research papers under his editorship in the Institute's journal, *Acta Biologiae Experimentalis* (since 1970 *Acta Neurobiologiae Experimentalis*). Under an exchange agreement between Poland and the United States, Konorski and a number of his colleagues visited universities in this country, while a long list of American scientists, including psychologists, returned the compliment. Konorski himself was elected to the Polish Academy of Science. His 1967 book, *Integrative activity of the brain,* was in every sense a *magnum opus,* though he would have called it a stepping stone.

"Konorski was first of all a scientist. His greeting to Dr. Wyrwicka as a new staff member was, 'Here at the Institute science is our life; family matters come second'. (Konorski's own wife, a talented and well-trained neurophysiologist in her own right, shared his intellectual activities over the years and was with him at his death). However, he proved not to be a ruthless taskmaster. He respected his colleagues' independence, while his interest and advice were always available.

"I asked Dr. Wyrwicka why Konorski had stayed in Warsaw most of his professional life instead of accepting some attractive offer elsewhere. Her reply brought out a few more personal characteristics of the man. One factor was a deep attachment to his homeland. As a young man Konorski had been an enthusiastic mountain-climber. He had also absorbed Polish culture to the point of memorizing and reciting long passages from native poets. A second factor was his strong sense of responsibility to the investigators at Nencki who depended on him for guidance and support. Thirdly, there were his many friends in Warsaw. But more recently, Konorski admitted, his attitude had become more international: perhaps it didn't matter so much where one worked; it was the advancement of science that counted".

Here, in Dr. Wyrwicka's account, many facets of Jerzy Konorski's personality and character are illuminated, but the outstanding characteristic was his single-mindedness in pursuing a particular line of scientific endeavor for nearly 50 years. Many factors undoubtedly contributed to the steadfastness of Konorski's career aims and his outstanding accomplishments. He was innately talented, he was affable and witty,

and interpersonal relations were easy and pleasant for him; but Jerzy Konorski also had great courage and always chose scientific and personal integrity rather than any form of mere expediency. Although, as we have seen, this latter trait made life difficult for him at times, in the long run it was one of his greatest personal and professional assets.

Professor Wyrwicka refers to Jerzy Konorski's deep identification with and pride in the history and culture of Poland. But I have the feeling that certain events specifically connected with World War II created a special loyalty in him to Poland in general and the people of Warsaw in particular. Although much less well equipped than the advancing German Wehrmacht, the Polish army heroically defended Warsaw for three weeks, at the end of which time the municipal authorities decided that there was no point in further resistance and surrendered the city. The Nencki Institute was then located on the western outskirts of Warsaw and was mistakenly regarded as a military installation and completely demolished, as were other parts of the City. But enough buildings remained that life and business could go on more or less as usual.

However, after the City was well secured, the Gestapo moved in and started a systematic search for and extermination of all Jewish citizens. The way in which these people were dealt with horrified and outraged the Poles; and when I was in Warsaw in 1965, I saw books still on sale in several stores showing atrocities of various kinds, including the marching of columns of Jews into deep pits where they were machined-gunned to death, one on top of another, until the "mass grave" was full, and the operation was then moved on to another cite, all to the blaring accompaniment of German martial music reproduced over a system of loudspeakers. Since some Jews were for a time able to avoid detection in underground caves and tunnels in the main downtown Jewish section, the Germans made doubly sure that no one escaped by demolishing all houses, apartments, and stores in this area, reducing several square blocks to pure rubble. In 1965, although there had been much nearby reconstruction, this area had been left virtually untouched, as a kind of memorial to the Jewish community which once flourished there and had been so ruthlessly exterminated.

While the war was still in progress, there was at one point, a courageous but ill-fated up-rising of the people of Warsaw against the Nazis. As punishment, thousands of Poles were massacred in a public square, and then the whole city of Warsaw was subjected to such destruction as to render it completely uninhabitable, except for the suburb of Praga, on the right bank of the Vistula River which had just been liberated and was now held by the Soviet Union Army.

Most of the survivors of the Warsaw insurrection, and subsequent

shelling of the whole city, made their way to Praga, and it was from here that Warsaw proper was gradually reinhabited when the Germans retreated before the ultimately victorious Russians.

Jerzy Konorski must have felt an inexpressible gratitude that, by a quirk of circumstances, he and his wife had been able to spend most of the War years, safe and relatively comfortable, in the Russian city of Sukhumi, on the beautiful shores of the Black Sea. To make the contrast even more stark between their good fortune and the tragedies that befell many old friends in Warsaw was the fact that, just in advance of the systematic Jewish pogrom there, Dr. Stefan Miller and his wife, rather than be slaughtered by the Nazi, committed suicide.

I believe Jerzy Konorski therefore felt that he must consecrate the remainder of his life in such a way as to show his sorrow for those who had perished during the war and to help re-build a new Poland, scientifically and culturally, for generations to come. Not only were Dr. Konorski and Dr. Lubińska treated with special kindness by the Russians during the War; as soon as they returned to Warsaw, in 1945, arrangements were almost immediately made, as Dr. Wyrwicka has mentioned, for the provisional reestablishment, on a small scale, of the Nencki Institute of Experimental Biology in the relatively intact Polish city of Łódź, where, as previously noted, Konorski had been born and reared. Here the Konorski's, a few former colleagues, and a new generation of students were to live and work for a decade. During this time a miracle of reconstruction took place in Warsaw, one part of which was especially impressive. Prior to World War II, there had been in Warsaw what was known as the Old City, which consisted of some ancient brick fortifications, and a large cobble-stone square, edged by very handsome townhouses two or three hundred years old. These houses were, of course, all devastated by the Nazis but, amazingly enough, the original architectural plans survived the war, and one of the first things the people of Warsaw — and, indeed, of all Poland — wanted to do was to restore these mansions, along with two ancient cathedrals which had stood nearby.

By 1955, true to their promise, the people of Warsaw had also constructed a large, well equipped new building for the Nencki Institute of Experimental Biology back in Warsaw, which was located about the same place as the original building had been. Konorski and Lubińska returned from Łódź to Warsaw in that year, and Professor Konorski took up his post as head of the now greatly expanded Department of Neurophysiology. Later he was to become Director of the Nencki Institute as a whole. How could a man repay such a debt of gratitude?

What can I now say from personal contacts with Jerzy Konorski? As previously mentioned, a grant from the National Institutes of Health, made it possible for him to come to the United States for the first time in 1957 and to remain here three months, with visits to a dozen or more places where research of special interest to him was being carried out. One of these places was the Champaign-Urbana campus of the University of Illinois, and I was one of his hosts. He was in our home, gave a talk to my seminar, and we had many informal conversations. Konorski was then 54 years old, but because of his quick, vigorous way of moving and talking, he gave the impression of being much younger.

After a couple of days we were on a first-name basis, except that I was somewhat uncertain how J-e-r-z-y should be pronounced. Noting my perplexity, Konorski gave me the correct Polish pronounciation, but then quickly added: "But don't worry about it. The English equivalent of Jerzy is 'George' — so just call me 'George'"! The memory of that exchange has always remained with me, typifying this man's basic friendliness, informality, and sense of humor.

After the 1957 visit, Jerzy Konorski returned to the United States on a number of occasions, for professional conventions, lecture series, and further visits to laboratories where work of special interest to him was going on. Unfortunately, it was never my good fortune to see him on any of these occasions; and the last and only other time I saw and spent some time with him was in the Spring of 1965, when I was invited by Dr. Janusz Reykowski, of the Department of Psychology at the University of Warsaw, to give a number of lectures which he had arranged.

On the afternoon that I was to give a seminar to the 50 or 60 persons then associated with Konorski, I was escorted to his office, where he welcomed me most graciously; but he was now in his working environment, under a certain amount of stress, and with many responsibilities weighing upon him. (Although I did not know it then, it was during this period that *Integrative activity of the brain* was being written, a task which Konorski says, in his Autobiography, he found very taxing). However, there was still a moment of levity and fun when, a few minutes after my arrival in his office, we went to the room where the members of the seminar were already assembled. The first order of business was to sound a curious little Oriental noise-maker which had presumably been a gift to him and was now ceremoniously used to call the seminar to order.

After the seminar that afternoon, it was aranged that I should return to the Department of Neurophysiology for a tour of the laboratories the next morning. Professor Konorski could easily have designated someone else to take me on this tour, but despite the many obligations

that impinged upon him, he did it himself and gave me an explanation of each experiment which we saw in progress.

Dr. Wyrwicka has quoted Jerzy Konorski as saying that for himself and the persons associated with him, science must come first. It seems clear that he felt a special dedication was necessary on this score because of the great sacrifices that had been made by others to provide the facilities needed for him to pursue, for almost half a century, the consumate question: "How does the brain work"?

But Jerzy Konorski gave himself not only to science but to the international fraternity of scientists who shared in his quest. He has accordingly left to us a legacy, not only of scientific accomplishment, but also of personal affection and admiration on the part of many others. This Memorial, therefore, is a tribute to Jerzy Konorski, not only as an internationally famous scientist, but also as a man of great personal courage, steadfast personal loyalties, and a largeness of spirit which will be long remembered and cherished. Jerzy Konorski received much from his native land, Poland, and he gave much in return. But he was also a man who, through science, gave much to and received much from the world at large. We honor him, not only as a creative and dedicated citizen of his own country, but of the world. We are richer for his life, his labors, and his qualities as an extraordinary human being.

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O. Hobart MOWRER, Department of Psychology, University of Illinois Champaign-Urbana, Illinois 61820, USA.