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I. P. PAVLOV*

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It is both a great privilege and a great pleasure to take part in the centenary commemoration of the outstanding Russian physiologist Ivan Petrovich Pavlov. It is also a privilege to revive the memory of my work with this great scientist—work which, though now belonging to the past, I always recall with great affection.

I shall try first to survey briefly Pavlov's work and its significance for modern science, and then to give personal reminiscences of my association with him. I shall not deal here with the first phase of his work, which was devoted to the physiology of digestion and which alone was already sufficient to establish his permanent reputation, but with the second phase, in which he created a new branch of physiology—namely, the physiology of higher nervous activity.

To begin with, take a look at the study of physiology of the central nervous system at the stage it had reached by the turn of the century—that is, at the time when Pavlov was beginning his work. The rapid development of neurophysiology in the second half of the nineteenth century, achieved by physiologists and clinical workers such as Flourens, Hitzig and Fritsch, Ferrier, Goltz, Sherrington, Hughlings Jackson, Broca—to mention only the most outstanding ones—led to the establishment beyond doubt of the fact that mental processes depend on the function of the brain, more particularly of its highest and most complex part, the cerebral cortex. This does not mean that

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this fact had not been recognized earlier, but the research carried out by the scientists I have mentioned developed it in a direct and, so to speak, tangible manner.

On the one hand, the experiments of Hitzig and Fritsch, Ferrier, and others demonstrated that certain mental processes are localized to certain parts of the brain—a finding which was corroborated by clinical observation. On the other hand, the experiments of Flourens and Goltz indicated that after the removal of the cerebral cortex animals remained alive and in good health, but became, it was said, "soulless automata," "creatures without memory and mind." It must be remembered, moreover, that this work was going on in the full tide of nineteenth-century scientific materialism, when the theory of evolution on the one hand and the rapid progress of biochemistry on the other made it seem as though nothing could now impede the triumphant march of science towards the unravelling of all mysteries—even that of life itself. This, then, is the background of the events here reviewed.

It was becoming clear at the time that all our conscious activity and behaviour is controlled by the brain, and as such could be investigated, theoretically at least, by physiological methods, without reference to psychic phenomena.

This idea was already beginning to take hold on a very big scale. It was first clearly formulated by the prominent nineteenth-century Russian physiologist Sechenov in his booklet *The Reflexes of the Brain*. It is not surprising, therefore, to find that at the end of the nineteenth and the beginning of the twentieth centuries widespread attempts were being made to find an experimental approach to the problem in question. Of all the attempts then made two were developed further than any others and became part of the permanent structure of modern science. One line of inquiry was opened up in America at the end of the nineteenth century by Thorndike and later developed into that branch of psychology now known as "behaviourism." The other began several years later in Pavlov's physiological laboratory and gave rise to the physiology of higher nervous activity.

Although both these lines of investigation are concerned with one and the same field—namely, the objective approach to the study of animal behaviour—and although they now tend more and more to converge and coalesce, their origins and points of departure were widely different.

Thorndike, aiming at the objective investigation of animal behaviour, rejected introspective explanations. He was not

concerned with the problem of what the animal "thought" or "felt" in its activities, but concentrated on trying to discover the laws governing its response to external conditions and stimuli. But, although he was not concerned with the subjective analysis of mental processes, he was nevertheless a psychologist, not a physiologist. Consequently, the physiological explanations he gave from time to time to supplement his arguments, though ingenious and penetrating, were somewhat amateurish. They neither were an integral part of his system nor were they later developed or elaborated.

With Pavlov it was a quite different matter. He was a physiologist through and through. His approach to cerebral activity was purely physiological, and he came to its study from what might appear to be a remote field—the physiology of digestion. It cannot be too strongly emphasized that both his aims and his methods were strictly physiological. Consequently, whereas the behaviourists, who in their investigations discounted mental processes as a link between external stimuli and reaction, had nothing to offer in place of these processes, Pavlov deliberately substituted physiological mechanisms for psychic activity.

Aim of His Work

The aim of Pavlov's work was not so much the description and classification of the phenomena of animal behaviour—the aim, more or less, of the behaviourists—as the explanation of those phenomena with reference to the relevant cerebral processes.

We must now look more closely at the starting-point and sources of Pavlov's work. In contradistinction to Thorndike, Pavlov worked solely on the basis of the physiological data then available. To him the fact that conscious behaviour of the organism depends on the cerebral cortex and therefore can be investigated as an expression of its functions admitted of no doubt. The problem consisted solely in how these investigations could be carried out. It was perfectly clear to Pavlov that experiments in the extirpation of certain cortical areas or in the stimulation of the cortex by electric shock—which were then widely practised—however fruitful and important they might be, did not represent the true method of investigating the normal and, so to speak, everyday activity of this organ. The problem was to find an appropriate method and the best starting-point for studying its *normal* activity.

While the work of Sechenov provided Pavlov with the theoretical basis, the practical method had already been formulated in Pavlov's earlier work. In the course of examining the activity of the salivary glands he had come across the phenomenon of so-called " psychological salivation." This occurs when the animal salivates, not in response to the actual presence of food or other stimulants in the mouth, but in response to the sight of food or to the sound of food being prepared—in other words, to the " idea " of food.

Pavlov saw that this simple fact offered a basis for the study of normal cortical activity. For if salivation in response to the presence of food in the mouth is an innate reflex, or, as Pavlov called it, " an unconditioned reflex," so salivation in response to the sight of food or the other stimuli signalizing its imminent presentation may also be regarded as a reflex action—not innate, but acquired through the animal's experience. This is what Pavlov called a " conditioned " or " conditional reflex."

The unconditioned reflex occurs by way of existing nerve channels established in the phylogenetic development of the nervous system, and is mediated mainly by the lower parts of the nervous system. The conditioned reflex, on the other hand, operates as the result of intercentral connexions established during the individual animal's lifetime as a result of its experience—connexions which depend, according to experimental evidence, chiefly on the cerebral cortex.

Pavlov argued that if the first of these reflex actions can be studied successfully by physiological methods—and no one can question the validity of these methods for the purpose—there can be no reason to doubt the validity of the physiological method for the study of the second group —that is, the conditioned reflexes.

It is thus apparent that the cerebral cortex (along with some subcortical structures), in contradistinction to the lower parts of the nervous system, may be regarded as a creative organ, a place where new connexions, and consequently new forms of animal behaviour, are established. This is its biological role and the essence of its physiological function.

And so we might sum up Pavlov's chief merit in this first period of his work on conditioned reflexes in the following way: it consists not in the fact that he understood the possibility of a physiological approach to problems of behaviour, for that possibility was well understood at the

time and resulted from the general state of knowledge in the biological sciences; his great merit lay in bringing to realization what hitherto had existed only in the realm of possibility—a *method* whereby cortical activity might be effectively and fruitfully examined.

Here it should be stressed that the method he then worked out for investigating conditioned reflexes in salivation may still be considered classical; for, although many other methods have since been added, his own has never been challenged for precision and scope.

It would be impossible, perhaps even inexpedient, to attempt to give an account, even in general outlines, of the work done by Pavlov's school during its 40 years' activity. But I should like to touch on some general questions closely related to this branch of science, particularly those which have given rise to some misunderstanding.

Reaction to His Work

Almost from the very beginning of the development of the study of higher nervous activity there sprang up a vast literature devoted to criticism, positive and negative, of the subject—criticism of a philosophical rather than a physiological character. This criticism was concerned with such questions as the validity of these investigations, their scope, their general philosophical significance, and so on. This critical literature is perhaps no less extensive than the literature of the subject itself, and often tends to overshadow the question at issue. Indeed, it frequently happens that a purely factual lecture or communication on the study of conditioned reflexes is at once made the pretext for a discussion which has nothing whatsoever to do with the real subject of the lecture but is devoted to such questions as the relation of mind to body, materialism, idealism, and so on, until amidst all these issues the proper subject is completely lost.

And so some people fulminate against the physiology of higher nervous activity, asserting either that it denies the existence of mind, that it attempts to explain mechanistically all our conscious activity, or that it over-simplifies the highly complex and subtle phenomena of mental life and tries to force them into the narrow framework of "reflexes," which these critics dismiss as elementary and primitive nervous functions. Others, on the contrary, extol the physiological method to the skies, dismiss with contempt years of psychological achievements, and maintain that

psychology is not a science at all and that physiology alone is capable of giving a complete and adequate explanation of all our mental activity.

For some people the so-called intrusion of physiology into a field regarded as the preserve of psychology amounts to nothing less than sacrilege against the dignity of the human spirit. Others, on the contrary, consider the possibility of a physiological and objective approach to our conscious behaviour as the crowning achievement of the human spirit and its ultimate triumph.

Physiology of Higher Nervous Activity

For these reasons it may be useful if we try to clear away some at least of the confusion surrounding these questions and to define what the physiology of higher nervous activity is—and what it is not.

First we must realize that the subject we are dealing with is a branch of the biological sciences and, particularly, a part of neurophysiology. It is an experimental science which, on the basis of the observation of certain physiological phenomena, attempts to draw conclusions regarding their physiological mechanism. It would be no more reasonable to doubt the validity of these conclusions than to doubt, for instance, the validity of the findings concerning the structure of chemical compounds arrived at by chemical experiments.

That, of course, does not rule out the possibility that our present assumptions concerning the cortical functions may prove erroneous and that we may have to revise them more or less in the light of subsequent knowledge. For it must be remembered that this study of the most complicated arrangement of organisms, the brain, is one of the utmost difficulty, and therefore, in Pavlov's phrase, " when you aim so high it is no disgrace to fail." In fact, it is certain that in the development of this science many of our concepts will be proved wrong and will have to be replaced. All such changes, however, must occur in the fire of experiment, on the basis of strictly relevant factual evidence, and certainly not as the result of barren and purely speculative disputes such as I have already described.

But while the direct evidence of the science of conditioned reflexes and the laws formulated on the basis of that evidence cannot be at the mercy of general philosophical disputations—though they can and must be liable to constant and the closest scientific scrutiny—the general

question of the application of these laws to various phenomena of animal, including human, behaviour is another matter altogether.

The problem under consideration may be expressed in this way: whether the laws discovered in experiments on conditioned reflexes in animals can be applied to human cortical activity and whether even now all cortical activity can be explained in physiological terms, or whether those laws are, as yet, applicable only to the most elementary cortical phenomena and their extension to the whole field of higher nervous activity is at present impossible.

Pavlov's standpoint was not unequivocal. On the one hand, endowed as he was with a highly critical mind, he realized that his work was only the beginning of the physiological analysis of cortical activity, and that, as he put it, "the mountain of the unknown will long remain enormously greater than the fragments we have managed to detach and study."

On the other hand, with the characteristic sweep of his genius he was always trying to embrace wide fields of phenomena both of animal and of human cortical activity and to subject them to physiological analysis, and he would often apply the laws discovered in his experiments to phenomena far removed from the material on which those laws were originally based.

Two Points of View

As I consider this point particularly important, I should like to examine it in greater detail. Up to now experiments in the field of higher nervous activity have been concerned almost exclusively with certain specific classes of phenomena, chiefly the elaboration of conditioned associations between various external stimuli and specific reflex activities such as alimentary activity, defence activity, and so on. In this way many important experimental results have been obtained and a number of laws governing higher nervous activity established.

If we take the view that these laws can be extended to all associations between various perceptions and experiences, and that associative function (both in the sense of the formation of associations and in the sense of their fading or inhibition) represents the only cortical activity of animal or man, then the simple conclusion is that conditioned reflexes are the elements of which all this activity is composed—in other words, to revert to Pavlov's figure

of speech, it suggests that we now know with reasonable certainty the substance of which the mountain of the unknown which looms before us is made.

If this were so, then the general scheme of cortical activity in both animal and man would be mastered, and our task would consist only in interpreting and deciphering the more complicated forms of behaviour and in reducing them to their most elementary terms.

The other viewpoint (which I am rather inclined to accept) is as follows. Experimental work on animals, as well as man, in the field of higher nervous activity concerns, as I have stressed, only some particular groups of relatively simple phenomena. And there is no doubt that these groups have already been physiologically analysed more or less thoroughly, or are in the process of being so analysed. Of course, it is extremely important, and very useful, to apply the knowledge we have gained in this work to the phenomena of everyday behaviour—not only of animals but also of children and adult human beings—and to foresee the effects of various experiences on individual behaviour and welfare. This, after all, is the same procedure as that followed by the physicist who, having discovered and analysed a particular phenomenon in the laboratory, tries later to recognize and identify it in the world outside the walls of his laboratory.

But just as a scientist of, say, Galileo's day would have erred in claiming that he understood why the sky is blue or what is the mechanism of lightning—and any explanation he might have offered would necessarily be inaccurate and pseudo-scientific—so, it seems to me, those physiologists are equally in error who maintain that they understand those very complex phenomena of our cortical activity which have not yet been submitted to physiological analysis. Thus, while it is unquestionably sound and useful to winnow out from our behaviour those facts which are susceptible of physiological study, and while we should try to explain from this standpoint as much of normal and pathological behaviour as possible, we must avoid giving pseudo-scientific explanations for those facts which have not, up to now, lent themselves to physiological interpretation. It seems to me that such " explanations " are all the more harmful because they give the impression that everything has been explained and made clear in a field where there is in fact still very much to be done.

If we accept this view, and try to place Pavlov in his historical perspective, I should say that his role can be

compared with that of Lavoisier in chemistry and Galileo and Newton in physics.

Just as Lavoisier, and not the alchemists, pointed the way to modern chemistry, and Newton rather than Aristotle showed the way to Einstein and Rutherford, so, I believe, the work of Pavlov will prove to have opened up the true paths towards the objective investigation of animal behaviour, and he may well be regarded by future generations as the man who laid the permanent foundations of this vital branch of human knowledge.

His Work Compared with Other Lines of Approach

But now let us come back from the days of Newton and Lavoisier to our own time, and from physics and chemistry to neurophysiology, and let us see what is the position of the physiology of higher nervous activity in relation to other main lines of investigation of the central nervous functions, represented by two leading neurophysiologists of this country, Sir Charles Sherrington and Professor Adrian.

There is no great difficulty about comparing the Pavlovian line of research with Sherrington's. It is particularly interesting for the historian of science to note that, although Pavlov's research on conditioned reflexes and Sherrington's work on spinal reflexes were undertaken quite independently of each other, the approaches to their respective fields were the same. For both of them the concept of reflex action was the basis of their research, and both of them tried to deduce the central nervous mechanism of animal activity by applying well-defined stimuli or combinations of stimuli, and measuring the animal's responses to them. For both of them the organism was, so to speak, a highly complicated device capable of an extremely wide range of responses to an extremely wide range of stimuli. They differed only in that, while Sherrington investigated the ready-made mechanisms of this device, Pavlov was concerned with those mechanisms which the experimenter himself had brought about. This far-reaching parallelism in their lines of investigation was of course not accidental; it arose from the fact that both of them had their roots in nineteenth-century biological materialism.

On the other hand, it is rather more difficult to draw a comparison between Pavlov's line of research and that of Adrian on the physiology of the brain. As is well known, Adrian's approach to the problems of brain physiology differs significantly from that of Pavlov. By means

of modern electrophysiological methods which consist in recording action potentials in the brain, Adrian and all who follow him attack this organ, so to speak, in a *direct* way and try to discover what is going on in the brain during its activity. Adrian is therefore not concerned, as Pavlov was, with hypothetical mechanisms of the brain deduced from external responses of the animal, but deals with the brain activity itself as recorded in action potentials.

Here perhaps lie both the strength and the weakness of this approach. The strength consists in the fact that any scientist much prefers to deal with factual evidence than with even the most fully elaborated and attractive hypotheses and theories. The weakness lies in the fact that, owing to the high complexity of the organ and its function, the evidence we obtain is as yet largely unintelligible. Indeed, so far, it represents nothing more than some fragments of a magnificent and extremely complicated mechanism, but we are still unable to find from these fragments how this mechanism functions.

I think that the following comparison might be helpful in suggesting the relation between Pavlov's and Sherrington's lines of research on the one hand and Adrian's on the other.

Imagine that our task is to discover what is going on in a certain huge and complicated secret factory without being provided with clues. On the one hand, we could do it by examining both the raw material supplied to the factory and the products delivered from it. If we were allowed to do this we might change the raw material in order to see what changes there would be in the products, or even destroy some parts of the plant in order to see how that would affect production. From all this evidence we could form some idea how this factory works. Of course these conclusions would be only hypothetical, and must change according to our further knowledge of the factory. But, at the same time, in this way we could have a fair picture of what this factory was intended for. On the other hand, we could find a way into the factory and watch the machinery at work. This, one would imagine, would bring us nearer to a comprehension of the functioning of the plant, but as there would be no guide to explain to us the logic and sequence of the processes of production it might happen that we should be at a loss—seeing all the details but unable to understand the whole.

And so, it seems to me, genuine progress in our understanding of that most astonishing and perhaps also the most mysterious of all mechanisms on earth, the brain, will be reached when these two lines of investigation, from outside and from inside, converge and coalesce—in other

words, when we are able to express in Adrian's terms what is going on in the brain when the conditioned reflex is established or elicited and, vice versa, which behaviour patterns correspond to the particular patterns of brain activity recorded by particular action potentials.

Some Memories of Pavlov

I should like now to recall some of my memories of Pavlov from the time when I was working with him in the early 'thirties.

When anyone has achieved as much as Pavlov, and has left a legacy as significant both in scope and in ideas as his, we are naturally curious to know how and why he did it—to know what were the psycho-physiological qualities that made such achievements possible.

He is universally recognized, of course, as a genius. No one who had ever had the opportunity of meeting him could fail to be aware of it even without knowing who he was. And, what is more, that first impression did not fade when one came to know him better—as so often happens with the great of this world. After working under him for two years I still felt for him the same admiration he inspired when I first met him. If anything, that admiration had grown and become deeper. And others who worked with him have told me they had the same experience.

But merely to say he was a genius is to use an intellectual abbreviation, to give a rather too general idea of his achievements and of the impression he made on his contemporaries. For the word does nothing to explain how his work was done and in what particular qualities lay the secret of his unique position in his field.

His Surgical Skill

To begin with one of his most immediately striking qualities, I should like to remind you that he was a brilliant surgeon. Of this I have, unfortunately, only second-hand knowledge. By the time I came to work with him he had already given up performing operations. I have been told, however, that it was an extraordinary experience to watch him at work. His surgical precision, style, and unfailing certainty were fascinating. But it was far from easy to act as his assistant during an operation. For one thing, he operated mainly with his left hand, which made it extremely awkward for the assistant. (Pavlov, by the way, could use either hand with equal skill; he wrote, for example, with

his right.) The other factor which made work with him difficult was his speed. Assistants simply could not keep pace with him. This was inclined to throw him into fits of temper which he could not conceal, and which made his unhappy assistants nervous at times to the point of sheer clumsiness. I have read in some memoirs of Pavlov that, once when he was demonstrating the rather complicated so-called "Pavlovian pouch" operation on the stomach in the presence of a foreign guest, the guest thought that Pavlov was still in the first stage of the operation when it was, in fact, almost finished.

Some idea of the surgical skill which he showed at the very beginning of his career may be gathered from the fact that the discovery of the secretory nerves of the pancreas had to wait many years for confirmation simply because no one succeeded in performing the necessary operation—although Pavlov himself demonstrated it quite frequently to his students.

To a physiologist surgical skill is, of course, of the utmost importance, because it enables him to bring his ideas to realization. We know definitely that many of Pavlov's achievements were due to his surgical technique, especially in the first stage of his research, when he was working on the digestive glands.

His Capacity for Work

Another important facet of his personality was his extraordinary capacity for work. At the height of his powers he was utterly tireless. The whole of the work carried on in his laboratories, where several dozen research workers were employed, was done not only under his direct control but also with his personal participation. He himself once said, "Up to the age of 75 I didn't know what it was to be tired"; and I am sure that this was not an overstatement, for he had no liking for exaggeration.

Then there was his astounding memory. I do not know whether, by the time I knew him, his powers of memory had been diminished at all by his age, but I do know that even then he had a better memory than any of the people who worked with him. He could remember not only the names and patronymics—according to the Russian custom (*imia i otchestvo*)—of all his present and past students (an achievement he liked to boast about from time to time), the names of the dogs used for experiments, and the particular experiment performed on each dog, but he also remembered practically everything he had ever heard. And, in

any case, he certainly remembered everything he wanted to remember. He never made notes, but he never forgot to return records he had borrowed and had promised to bring back by a certain time, or to be present at an experiment he had promised to watch. I believe that he alone was capable of grasping the whole scope of the research work done by his school, and he stored in his memory an astonishing number of facts.

His Quality of Mind

But all these qualities, admirable and essential as they were for achievement of Pavlov's order, were not enough in themselves to determine the essential quality of his genius. There still remains something more, something extremely difficult to define, which, for want of a more exact word, we can only call "quality of mind." Pavlov's "mind" was extraordinarily rich and many-sided.

It is well known that the work done by his school was animated and guided by his thoughts and ideas—a fact which was always gratefully acknowledged by his pupils in their papers. And his own articles and lectures always bore the imprint of his personal concepts, hypotheses, and syntheses, some of which formed the basis for entirely new lines of investigation undertaken by his followers. And yet all this written material represents only a small part of what he had to say. The richness of his ideas, the quickness of his thought, his extraordinary vision and inventiveness—all this could be fully appreciated only by those who came in contact with him and had the opportunity of listening to him or talking with him. He was always ready for a discussion, and whenever he was not engaged in experiments he was prepared to talk over either problems closely connected with the experimental work of the laboratories, or more general questions such as the potential scope and practical applicability of the study of conditioned reflexes. Such discussions—an everyday feature of his laboratories—created a highly stimulating atmosphere for collective work, and gave everyone the satisfaction of sharing in the work of the school as a whole.

Pavlov came upon his discoveries by the process which Newton so modestly described as "continuous thinking on a subject." The modesty of this definition may suggest that anybody who is prepared to exercise his mental faculties in "continuous thinking" may do so at will. This of course is not so, for continuous thinking on a subject

is precisely that which many—if not most—people cannot do. In Pavlov's case I had the impression that thinking was something he just could not help doing—a passion which he could not resist. And this passion was fed because thinking came so easily to him, and was so fruitful in new ideas, that, to use his own term, it was constantly being "reinforced" by the positive results of his discoveries.

It was characteristic of Pavlov that he had no liking for unsolved problems and would rather find a temporary solution than no answer at all. It was indeed particularly interesting to follow the process by which he gradually modified and developed his ideas until they satisfied him. And here, if I may digress, I should like to recall an incident which I myself witnessed. One of Pavlov's collaborators, a woman of a rather not very critical mind, came once to ask him for an explanation in connexion with an experiment she was engaged on. Pavlov, busy at the time with something else, got rid of her with an obviously incorrect and even somewhat naive explanation. When, later, he was discussing her work with her, she quoted his "explanation." Pavlov was furious and demanded, "Who told you such rubbish?"—then roared with laughter when he learned that the "rubbish" was his own words.

Pavlov showed a great deal of elasticity in the formation of his concepts. He was ready to admit mistakes and to renounce ideas—even when they were well established or dear to him—if he suspected that they might be wrong. Even when he was fervently defending a certain concept and when it appeared he was professing it dogmatically, he never entirely suspended his faculty for self-criticism and even scepticism. He could see himself and his school with detachment and he was able to say that for certain problems "we are already too rusty" and that others would have to try to solve them. That was why he liked to see his pupils go their own ways and break away from the routine of the school. He knew how to give them wise encouragement, but he also knew how to subject them to sharp and vehement criticism. And so far as his school was concerned, he saw to it that all work published under its auspices remained in harmony with his teachings.

I remember a conference at which a well-known Soviet histologist attacked Pavlov's theory of cortical localization on the basis of recent histological evidence. Pavlov defended his theory with extraordinary energy, ruthlessly demolishing his opponent's arguments. It seemed as if he

was as convinced as his audience that the counter-arguments he was putting forward were irrefutable. But those who knew him were certain that he would not forget his opponent's objections. And in fact, when at one of his weekly conferences Pavlov read a paper on conditioned reflexes (intended, I think, for an encyclopaedia) and was asked why he had omitted his theory of cortical localization, he replied that in this paper he wanted to deal only with things he was absolutely sure of, and did not think that theory sufficiently proved. And he proceeded to quote the histologist with whom he had argued so strongly.

This gift for self-criticism, combined with his creative powers, enabled him to see problems from so many aspects that all his scientific speculations had an air of uncommon wisdom. He had also a gift for plain, concise speaking enlivened with an enthusiasm which carried away his audiences, but he hated the use of words for their own sake or in order to secure a mere effect. Once he told one of his more loquacious assistants, "You are in the power of words." And even when I came to know him his speech was free from the repetitions characteristic of advanced years.

His natural charm, the love of scientific work which was radiated by his whole personality and infected others with his ardour, his wisdom and simplicity—all these qualities gave him a singular power over all who came into contact with him. And to these we must add a talent for teaching and a strong administrative sense which enabled him to co-ordinate the work of many collaborators in the one single, monumental achievement of his school.

Pavlov the Man

I have tried to touch on the most characteristic of those features of Pavlov's intellect and personality which, to my mind, made up his genius. I have no illusions about the fact that they are far from complete and that there are many facets of his complex personality which I have not mentioned. I also realize that the picture I have sketched of this great scientist may seem somewhat one-sided, for I have paid little attention to Pavlov the man. It would, I am afraid, have been quite impossible for me to do so. I should like only to add that of all the things I admired in him it was perhaps his humanity and simplicity that attracted me most. Those qualities manifested themselves in all he did—whether he was expressing doubt about his own attainments or glowing with enthusiasm and the

dynamic faith of youth after having obtained some unexpected results, or whether he was giving way to his weaknesses, quarrelling and obstinately sticking to his point even when his error was obvious—only to admit his fault when he had cooled off.

Conclusion

I should like to finish by quoting extracts from Pavlov's message to young scientists—words which I think are of interest to us all, and which perhaps express his personality better than anything I can say.*

"What would I wish for the youth of my country who devote themselves to science? First of all—consistency. . . . I can never speak without emotion of this most important condition for fruitful scientific work. From the very beginning of your work train yourselves to be strictly systematic in amassing knowledge. Learn the A B Cs of science before attempting to ascend its heights. Never reach for the next step without having mastered the preceding one. Never attempt to cover up the gaps in your knowledge by even the most daring conjectures and hypotheses. No matter how the colourings of this bubble may please your eye, it will inevitably burst, leaving you with nothing but confusion. . . . Study, compare, and accumulate facts. No matter how perfect a bird's wing, it could never raise the bird aloft if it were not supported by air. Facts are the air of the scientist. Without them you will never be able to soar. Without them your 'theories' are useless efforts. Yet while studying, experimenting, observing, try not to stop only at the surface of facts. Do not become an archivist of facts. Try to penetrate the mystery of their origin. Seek persistently the laws governing them.

"Secondly, modesty. Never think that you already know everything. No matter in what high esteem you are held, always have the courage to say to yourself: 'I am ignorant.' Don't allow yourself to be overcome by pride. On account of pride you will be stubborn where it is necessary to be conciliatory; you will reject useful advice and friendly assistance; you will lose your sense of objectivity. In the group which I am called upon to direct, atmosphere is everything. We are all harnessed to one common cause, and everyone furthers it to the best of his strength and ability. Frequently we cannot distinguish what is mine and what is thine, but through this our common cause only gains.

"Thirdly, passion. Remember, science requires your whole life. Even if you had two lives to give it would still not be enough. Science demands of man effort and supreme passion. Be passionate in your work and in your quests. . . ."

*Translation of Horsley Gantt.

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