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SAMUEL JAMES MELTZER[1]

DR. SAMUEL JAMES MELTZER was born in Curland, northwestern Russia, March 22, 1851. He received his preliminary education in a Real Gymnasium in Königsberg and his later training in the University of Berlin where he graduated in medicine in 1882. After taking his medical degree he decided to make his career in America, as the country which in his opinion had the best form of government. He had not sufficient means to make the journey and was therefore obliged to secure a position as ship's surgeon on one of the transatlantic vessels. On arriving in New York it was necessary in the beginning to devote his time mainly to building up a practise sufficient to support his family, but almost from the beginning he made arrangements also to give part of his time to research. From that period until his death on November 7, 1920, in his seventieth year he was a tireless investigator. When in the course of time the opportunity came to him from the Rockefeller Institute to give his time entirely to research he did not hesitate in making his decision. At a considerable financial sacrifice he abandoned his medical practise to devote himself to the kind of work that he most loved and most valued. By his good work and his high character he attained a position of honor and distinction in American medicine and endeared himself to his fellow-workers in all parts of the country. His productivity was remarkable. The list of his published papers includes over two hundred and forty titles, distributed among some forty-eight scientific journals of this country, Germany and England. These papers contain contributions to the subjects

[1] Read before the Federation of American Societies for Experimental Biology, Chicago, December 28, 1920.

of physiology, pharmacology, pathology and clinical medicine together with a number of lectures and general addresses. That he was an investigator of recognized standing in these several branches of medicine and was regarded as a valued contributor to so many scientific journals of the first rank is a striking demonstration of the breadth of his interests and knowledge. He was a member of twenty or more national scientific or clinical societies and in all of them it may be said he was prepared to take his part as an expert in the reading and the discussion of technical papers.

He served as president of the American Physiological Society, the Society for Experimental Biology and Medicine, the American Gastro-enterological Society, the American Society for the Advancement of Clinical Research, the Association of American Physicians and the American Association for Thoracic Surgery. The membership in these societies is composed of trained specialists. It is their custom to choose as their presiding officer only those who have made contributions of distinction to the subject to which the society is devoted. It seems to me unique in the modern history of medicine for one man to have received such special recognition from technical workers in so many different fields.

While his activities covered this large range he was interested primarily in physiology. "I belong," he said in a recent paper "to those who believe . . . that the knowledge of physiology is of special importance to clinical medicine." His work in this field entitles him certainly to be ranked among the foremost American physiologists. In attempting to present some estimate of the results of his labors I must limit myself mainly to his physiological activity. Indeed in this subject alone his papers are so varied that it will be possible to bring under review only what seem to be his major contributions. His first appearance as an investigator is recorded in a brief note in the *Proceedings* of the Berlin Physiological Society, May 14, 1880. In this note it is stated that Professor Kronecker exhibited a dog in which Herr Cand. Med. Meltzer had cut the nerves going to the mylohyoid muscle and thus demonstrated the importance of this muscle in the initial stage of swallowing. At a later meeting of the society in the same year Kronecker presented the full results of an investigation carried out by Herr Cand. Med. Meltzer under his supervision on the "Process of Swallowing." This paper was published subsequently by Kronecker and Meltzer in the *Monatsbericht der Königl. Akademie der Wissenschaften zu Berlin,* 1881. In this important contribution the mechanism of swallowing was given an entirely new interpretation which has since been generally accepted and is known as the Kronecker-Meltzer theory of deglutition. Meltzer had attracted Kronecker's attention while a student in his course. Out of this acquaintanceship developed an invitation to engage in a research and eventually a warm friendship between the two men that lasted throughout life. Meltzer's career was thus determined while still a student of medicine. Kronecker's influence attracted him to physiology and set his feet in the paths of research. The investigation in which they collaborated was important and original—just what part each contributed it is not now possible to discover, but it is interesting to find that this initial venture into research furnished a motif which can be detected recurring again and again in Meltzer's subsequent work. A companion paper upon "Die Irradiationen des Schluckcentrums und ihre Bedeutung" was published by Meltzer alone in 1883. It is a very suggestive paper on account of the careful analysis it contains of the far-reaching and curious effects in the central nervous system of the act of swallowing and also because in it Meltzer announces certain views upon the importance of the inhibitory processes which subsequently formed the basis of his theory of inhibition, and remained with him throughout life as a sort of compass by which to set his course on his voyages of discovery. He calls attention in this work to the fact that reflex excitation of the inspiratory muscles is accompanied by reflex in-

hibition of the expiratory muscles and vice versa, and he goes on to make the suggestion that a similar relationship must prevail in the case of all antagonistic muscles such as the extensors and flexors of the limbs. Some ten years later Sherrington gave the necessary demonstration that this interrelation does hold with the muscular antagonists, that the contraction of the one is accompanied by the inhibition of the other and he designated this relationship under the term of "reciprocal innervation." Meltzer meanwhile had been accumulating instances of this combined action of excitation and inhibition, but he neglected at that period to apply a distinctive name to this kind of correlated activity. There can be no doubt that when it is possible to label an idea with an appropriate designation its currency in the scientific world is greatly facilitated. In his paper on "The Self-Regulation of Respiration" read before the American Physiological Society in 1889 and published in the *New York Medical Journal* and under a different title in the *Archiv. für Physiologie* he describes experiments intended to show that two kinds of afferent fibers exist in the vagus nerve, one exciting and the other inhibiting inspiratory movements. He used this fact to modify the Hering-Breuer theory of the self-regulation of the respirations by assuming that the expansion of the lungs stimulates both groups of fibers. The resultant effect, as in the case of the simultaneous stimulation of the motor and inhibitory fibers to the heart, is a dominance of the inhibitory effect, thus cutting short the inspiration and bringing on an expiration. But after the inhibition ceases the excitatory fibers, which, like the acceleratory fibers of the heart have a long after action, come into play and start a new inspiration. In his first general paper on inhibition this idea of a combined action of opposing processes is extended by the citation of numerous instances taken from physiological literature and is expanded into a general theory which makes inhibition a universal property of irritable tissues.

> "I entertain and defend the view that the phenomena of life are not simply the outcome of the single factor of excitation, but they are the result of a compromise between two antagonistic factors, the fundamental forces of life, excitation and inhibition."

That is to say, whenever a tissue is stimulated two different processes are aroused, one leading to functional activity and one to a suppression of activity. As to the nature of these processes very little is said. He was not satisfied with the Hering-Gaskell conception that excitation follows or is an accompaniment of catabolic changes while inhibition is due to processes of an anabolic or assimilative character. He goes only so far as to assume that both processes are concerned with the kinetic and potential energies of the system, that excitation facilitates the conversion of potential to kinetic energy while inhibition hinders or retards this conversion, like the turning off or on of a stopcock. Nor was he satisfied with Sherrington's term of reciprocal innervation to describe all of the phenomena he had in mind. While this phrase is a suitable designation for the relationship between physically antagonistic muscles such as the flexors and extensors it is less appropriate in other cases, for example the simultaneous phases of contraction and inhibition exhibited in peristalsis. In later papers he suggested first the term *crossed innervation* borrowed from von Basch, but subsequently adopted the designation of *contrary innervation* as more applicable to the whole series of phenomena which he was considering. This process he believed is universal in its action—it is "manifest in all the functions of the animal body." Moreover his experience and observation as a practising physician led him to believe that "a disturbance of this law is a factor of more or less importance in the pathogenesis of many disorders and diseases of the animal body." In this way he would explain in part at least the muscular incoordination in tabes and the gastric crises of that disease, as well as gastric and intestinal colic in general. If the orderly sequence of a peristaltic wave is

disturbed so that the advancing wave of contraction meets a contracted instead of an inhibited area conditions are present which may well bring about a distension sufficient to account for the pain of colic. He gives many other illustrations of pathological conditions which may find a plausible explanation on the assumption of a disorder or disharmony in the law of contrary innervation. How far Dr. Meltzer was correct in the applications of his theory it is not possible to say. In all probability some of the specific instances that he cites in support of his views are amenable now to other explanations. But it is a fact, I believe, that he was much in advance of his earlier contemporaries in the emphasis he placed upon the significance of inhibition in the general activities of the body. The story is far from being told but it may be said that physiological thought since 1883 has tended more and more toward some such general conception of the rôle of inhibition as was in Meltzer's mind. For him at least it was a rewarding theory, it played, as he expressed it, a dominating part in all of his researches. One can not wholly appreciate his work nor understand his position on controversial points unless this attitude is born in mind. His theory of shock for example to which he held tenaciously was that "the various injuries which are capable of bringing on shock do so by favoring the development of the inhibitory side of all the functions of the body." There is a shifting of the normal balance toward the side of inhibition.

The most important of his contributions in later years will be found in three series of researches, one dealing with the action of adrenalin upon the blood-vessels and the pupillary muscles; one with the inhibitory action of magnesium sulphate and the antagonistic effect of the calcium salts, and one with the development of his method or artificial respiration by pharyngeal and intratracheal insufflation. The first series consists of eight or nine papers, mostly in collaboration with his daughter. They showed in this work that the temporary action of adrenalin upon the blood-vessels may be converted into a long-lasting effect, in the case of the ear-vessels, if these vessels are first denervated by section of the vaso-motor fibers in the sympathetic and the third cervical nerve. A more striking result still was obtained for the iris. In the mammal subcutaneous injections of adrenalin in moderate doses have no effect upon the size of the pupil, but if the superior cervical ganglion is first excised then, after a certain interval, subcutaneous injections bring on a marked and long-lasting dilatation. His explanation of these phenomena was made in terms of his theory of inhibition. Whether or not his views in regard to the relations of the cervical ganglion to pupillary dilatation will stand the test of future experimental work it is to be noted that the observation itself constitutes a significant instance of a kind of independent physiological activity on the part of a peripheral ganglion. The bearing of these facts upon the prevalent conception of the rapid destruction of epinephrin in the tissues was brought out especially in a paper with Auer in which it was shown that if adrenalin is injected into a ligated limb and an hour or so afterward the ligature is removed the dilatation of the pupil quickly follows, thus demonstrating that for this long period the adrenalin had remained unaffected by the tissues. Incidental results of this series of experiments were his discovery of the use of the frog's eye as a biological reagent for the detection of small concentrations of epinephrin and the rapidity of absorption in intramuscular as compared with subcutaneous injections.

The work upon the inhibitory and anesthetic effects of magnesium salts gave rise to no less than twenty five papers, most of them published in collaboration with one or another of his associates but chiefly with Dr. Auer. The peculiar inhibitory action of magnesium sulphate had attracted his attention as far back as 1899, and he reported upon it incidentally in a communication to the American Physiological Society. But in 1904–05, influenced again by his general conception of the importance of the inhibitory

processes he took up with Auer a careful physiological study of its action. The results were most interesting and important. When given subcutaneously in certain doses the magnesium sulphate produces a condition of complete unconsciousness and muscular paralysis or relaxation, which is reversible, in the sense that when the animal is given proper care it recovers. Later he was able to show that out of this condition of profound depression or inhibition the animal may be restored to complete consciousness and motility with miraculous suddenness by the intravascular injection of small amounts of calcium chloride. No one who was fortunate enough to see this demonstration as given by Dr. Meltzer will forget its dramatic effect upon his audience. A healthy vigorous rabbit was brought quickly to a condition of complete immobility and apparent death by the magnesium sulphate and then even more suddenly raised from the dead and restored to its normal tranquil existence by the injection of some calcium chloride. Meltzer and his collaborators investigated various phases of this action of magnesium sulphate and all of the results obtained tended to strengthen in his mind the conviction that in magnesium he had discovered the element in the body that is especially concerned in the processes of inhibition. The antagonistic action of the calcium although exhibited in such a striking way was not in his opinion specific. His own experiments in connection with the results reported by other observers led him to the general view that calcium serves to balance the abnormal activity of the other kations, potassium, sodium and magnesium, whether this abnormal action is in the direction of excitation or of inhibition. Modern work upon the physiological significance of the inorganic constituents of the body fluids which was begun in Ludwig's laboratory, but was given its main inpetus by the striking contributions of Ringer had concerned itself chiefly with the salts of potassium, sodium and calcium, which alone seemed to be sufficient to maintain normal conditions of irritability. Meltzer's work has shown that magnesium also has its place in this ancient balance of powers through which the functional activity of protoplasm is controlled. One can understand that in arriving at these results he must have felt that he was approximating at least a demonstration of the correctness of his general conception of the rôle of inhibition in functional activity. In this as in all of his experimental work Meltzer was eager to give his results a practical application to the art of medicine. The possibilities of the use of magnesium salts as an anesthetic agent in surgical operations were tested with some success on human beings and more important still its efficacy in controlling the spasms of tetanus has had a wide and promising application.

His last extensive series of researches dealt with anesthetization and artificial respiration through pharyngeal and intratracheal insufflation. Something like twenty-eight papers, most of them in collaboration with pupils or assistants, were devoted to this subject. His interest in this topic seems to have been stimulated by the fact that in his use of magnesium sulphate for anesthetic purposes the chief danger lay in the inhibition of the activity of the respiratory center. To meet this difficulty he undertook a study of the methods of artificial respiration. The initial paper in 1909 by Meltzer and Auer described a method of artificial respiration by continuous insufflation of the lungs through a tracheal catheter. It was found that by this means not only could an animal be kept alive without the action of the respiratory movements to fill and empty the lungs, but that it furnished also a convenient and efficient method for anesthetization. The use of this method in animal experimentation and especially its use in human surgery of the thorax and facial region was apparent and on many occasions Meltzer sought to make known its advantages and to ask for an adequate trial of its merits at the hands of the practical surgeons. The method has found some acceptance and the application of the principle involved will no doubt be extended in the future as the technique of thoracic surgery improves. It was

in recognition of the importance of this work that the American Association for Thoracic Surgery asked him, a physician and laboratory worker, to serve as their first president. It was natural that this work should have led him to consider the whole matter of artificial respiration in its relations to resuscitation after accidents of various sorts. His general paper in the *Medical Record* for 1917 giving a history and critical anlysis of the methods of resuscitation is an interesting and valuable contribution. He gives experimental data to prove that his device of intratracheal insufflation is the most efficient method of artificial respiration both for man and animals. But he realized that it is a method which requires special knowledge and training for its successful execution, and his broadening acquaintance with and interest in the practical aspects of resuscitation led him to experiment with the less efficient and less safe method of pharyngeal insufflation. He was a member of the three national commissions on resuscitation and served as chairman of the third commission. In connection with the duties of this service he devised a simple portable form of apparatus for pharyngeal insufflation which can be used with very little previous instruction and he demonstrated, with entire success I believe, that this form of apparatus is much more efficient than any of the so-called manual methods of resuscitation, or than any of the special machines for this purpose, pulmotors and lungmotors, which have been exploited commercially during the past few years. It was, I imagine, a sore disappointment to him that he was not able to convince his colleagues on the third commission that this apparatus met all the requirements for industrial and military use. It is probably the simplest and best instrument yet devised for artificial respiration as applied to man, and in institutions or industrial establishments where the need for artificial respiration may arise frequently and where special individuals may be instructed in its use it can be employed to great advantage. But it does require some little amount of training to use it properly—the average uninstructed man or woman can not be trusted to apply it intelligently, and for this reason the commission felt that it was wise to urge adoption of a manual method as the form of first aid which may be applied most successfully under ordinary conditions.

While the researches that I have attempted to summarize represent his most important contribution to physiological science, Dr. Meltzer kept in close touch with the progress in almost all branches of experimental medicine. He gave evidence of this interest in the publication of occasional papers on various topics or in articles of a general character. Shock, cardiac arrhythmias, therapeutics of self-repair, hemolysis, thyroid therapy, edema are among the subjects upon which he wrote, but probably the most original and helpful of his general papers is his well-known Harvey Lecture, 1906, on "The Factors of Safety in Animal Structure and Animal Economy." He applied this engineering term in a convincing way to describe the reserve powers possessed by many of the mechanism of the body. Doubtless the general conception involved had occurred to many others, but no one before him, so far as I know, had developed the idea so comprehensively and made of this provision a leading factor in the adaptation of the economy to its environment. The happy phrase that he employed served to precipitate the loose thought upon the subject, and its frequent recurrence since in medical literature is proof that the conception which it expresses has found wide acceptance in scientific circles. It is evident that his own thoughts were turned in this direction by the work of Chittenden upon the minimum protein diet. While he accepted, of course, the facts demonstrated by this observer in regard to the possibility of maintenance upon a low protein diet he was not willing to believe that a minimum diet is also an optimum diet in relation to the various metabolic stresses to which the body may be subjected. The experiences of the great war may serve to show that he was correct in taking this position.

To do full justice to the influence exerted upon contemporary medical research by Meltzer's work would require a careful analysis of the entire medical literature of the period, for, as I have tried to indicate, his sympathies were very broad and his activity was great. In some measure, either as interpreter or contributor, this influence was felt at many of the points of contact between medical science and medical practise. The border land between these subjects was in fact his special field of work. He had the spirit and ideals of the scientist, and knew at first hand what research work really means. He had experienced the labor and care and devotion required of those who aspire to increase knowledge. On the other hand he had a personal realization of the difficulties and necessities of medical practise and so was especially fitted to act as a sort of liaison officer between the two great wings of the medical army, the investigators who have the difficult task of discovering new truths, and the practitioners who must learn to apply these truths to the preservation of health and the protection from disease. No one in our generation, I venture to say, was more useful in this country in bringing about a helpful and sympathetic understanding between the laboratory worker and the physician. As a physiologist he enjoyed the best opportunities and training of his period. He was equipped with the methods and technique that the subject owes to the great masters of the latter half of the nineteenth century. The more modern methods of physics and chemistry which seem to be essential for the new generation of physiological workers he did not possess, but he did not let this deficiency discourage him nor diminish in any way his activity in research. He had the wisdom to understand that the armamentarium with which he was provided was adequate for the accomplishment of much important and necessary investigation. He was no faint-hearted seeker after truth. There never was a time, I fancy, in his active life when his mind was not full of problems that he wished to solve and which he intended to solve in part at least with the aid of his experimental methods.

Dr. Meltzer was elected to membership in the American Physiological Society at its first annual meeting held in Philadelphia in December, 1888. From that time until his death he was perhaps its most faithful member in attendance, in the presentation of papers and in participation in the discussions and social intercourse. Other less heroic spirits might weary under the load of papers and seek respite and fresh air by frequent disappearances between acts, but this was never the case with Meltzer. He loved the meetings, he loved to listen to the papers and to take part in the discussions. He had something to say of value on almost every paper that was read. It is small wonder therefore that his position and influence in the society constantly increased in importance. He served as president from 1911 to 1913, but the older members know that before that time and since his advice was paramount in matters of policy as well as in the selection of officers. He was sincerely and deeply interested in the welfare of the society and believed in its importance as one of the major agencies concerned in the advancement of the cause of physiological research. What he had to say in regard to its policies was always said in the opening meetings and in the plainest of terms, and if in his opinion it was necessary to be critical of either persons or things he never hesitated to express what was in his mind. His courage in stating his position in matters in which some personal criticism necessarily played a part in the discussion has often aroused my admiration. He did not indulge in circumlocutions or euphemisms, but was entirely frank and direct. There could be no mistake as to what he thought and yet no matter how plainly and bluntly he might speak there was as a rule no offense taken, because it was evident to every one that what concerned him was not personalities but the principles involved. The American Physiological Society owes much to him for the sound policies and wholesome traditions which have characterized its his-

tory. I have not so much direct knowledge of the influence exerted by Dr. Meltzer in the numerous other societies of which he was a member. In the case of the Society for Experimental Biology and Medicine we know that he was its chief founder and for many years its primum movens—it was long known familiarly among scientific men as the Meltzer Verein. I have no doubt that in every organization with which he was connected his influence was always exerted on the side of the highest scientific ideals—no other position was possible for him. He was high-minded, courageous, sincere and optimistic. Age oftentimes lays a stiffening hand upon the scientific worker, causing him to shrink from the laborious routine of research, but with Meltzer there was never any indication of weariness or sense of failure. In spite of much ill-health and physical suffering in his later years he was full of hope and energy and determination in the pursuit of his scientific ideals and problems. Death came to him, as he would have chosen, while in his study and at his work. He was a good and faithful servant in the cause of medical research. Rewards came to him in the form of academic honors and membership in the most important medical and scientific societies, but I am confident that he found his greatest recompense in the joy of the work and in the affectionate appreciation of his many scientific friends.

W. H. Howell

THE RELATIONS OF PSYCHOLOGY TO MEDICINE[1]

A sufficient excuse for this discussion of an old theme is the notable rapid progress of both psychology and medicine, and the consequent changes in their actual and prospective relations. Fresh consideration of the question what should be the relation of psychology to medicine may benefit alike the sciences and the art concerned. The discussion may not be exhaustive; instead, it must be limited to an outline of the theme and the indication of those characteristics of the two principal subjects which are preeminently important as conditions of profitable working relations.

Medicine as an art strives to maintain or restore the health of the human being. The object of the physician's concern, his patient, ordinarily is both active and conscious. It is therefore desirable that the practitioner be thoroughly grounded in the facts and principles of human action and experience. Although this may seem self-evident, it has not been accepted generally in medical education. The history of medicine indicates that it has long sought to attain a reliable and adequate scientific basis for the practise. Naturally enough, knowledge of structure was first of all sought, and in consequence, the science of gross anatomy developed. Subsequently it gave rise to histology, cytology, embryology, pathological anatomy, and bacteriology, all of which are now recognized as essential morphological bases for the art of medicine. Paralleling the growth of the knowledge of structure, although somewhat more recently and more slowly developed, are the various sciences which deal with organic functions. Among these, human physiology was first chronologically, and first in importance to medicine. For several centuries it has grown steadily, gradually extending its inquiries to most of the important types of organic process. From it have arisen a number of special sciences of function and its alteration, as, for example, in immunology, pathology, and certain aspects of pharmacology. But strangely enough, physiology has failed to take possession of those large and important groups of phenomena in human life which are designated by the terms behavior, conduct, experience, and mind.

[1] Address of retiring vice-president and chairman of Section I, American Association for the Advancement of Science, Chicago, 1920.