PRESIDENTIAL ADDRESS

ON

PHYSIOLOGY AND THE HEALING ART.

Delivered at the Opening of the Winter Session of the Ulster Medical Society, November 1st, 1900.

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PHYSIOLOGY was formerly said to be the basis of rational medicine. It is a saying which has grown somewhat worn and old, but I fear the amount of conviction which it carries to the breast of the average medical man has not increased with its age. These are sentiments which perhaps I ought not to re-echo, but no real harm is ever done by fairly looking at facts in the face. All the less likely is this to happen in the present instance, because I believe the saying is in reality more true to-day than when it was first uttered. The intimate connection has only been obscured by other developments; the dissociation is more apparent than real.

ASSOCIATION NOT SUFFICIENTLY INTIMATE: PROBABLE CAUSES.

In seeking to trace the origin of this feeling to its cause it seems to me that first and foremost should be placed the great and rapid development of the subject of physiology which carries its devotees far into fields where none but the specialist can be expected to follow them. This minute research is necessary if truths of moment are to be wrung from the secrets of nature. Nevertheless it is a process with which the man of practice cannot always sympathise. It is only when those truths are brought back, and take their place in the guidance of treatment that his interest is specially awakened.

Another circumstance to which I would assign a place in obscuring the link of connection between pure physiology and practical medicine is connected with the study and treatment of acute infectious disorders. These in themselves, always of great interest to the medical man, have been rendered doubly so by the great achievements of the science of bacteriology. The diagnosis of such maladies by the discovery of the micro-organisms, which give origin to them, and the study of the life-history of these germs themselves have filled a gap that could not have been filled by any other means. In this way the science of germs has come to occupy an engrossing position in men's minds, and to a similar extent has overshadowed, and in a sense crowded out the older subject of physiology. But even here, where most obscured, the knowledge yielded by the latter must come in when the process of diagnosis is complete, and the curative treatment, if it is to be rational, is undertaken.

I should, however, be very sorry to convey to your minds that I do not fully appreciate the great good which has been done by the study of bacteriology. I only wish to recall your attention to the fact, that there is still a solid substratum of physiological basis to the healing art, whose culture it is unwise to neglect, if we are, as I hope it is the aim of all of us, to build up a stable, well-proportioned, and durable scientific structure.

One other cause I should like to indicate, as a factor, which I think in some measure accounts for the rather small degree of sympathy which too often exists between physiology and the practice of the healing art. I will do so by asking you a question. Do clinicians sufficiently avail themselves, or are they placed in a position in this country to sufficiently avail themselves of the resources brought within their reach by the application of physiological methods to the discovery and treatment of disease?

With this question I will leave you for the present, and will ask you to follow me while I endeavour to bring before you a few of the advances which have recently been made in a physiological field intimately associated with the every-day practice of your profession.

RECENT ADVANCES IN PHYSIOLOGY OF GASTRIC DIGESTION.

Somewhat more than ten years ago an elaborate institute for the experimental study of medicine was established in St. Petersburg. This institute consists of numerous depart-

ments, is in short the Pasteur Institute of the capital of Russia. Two of these are devoted to the prosecution of physiological research—one in the field of chemical, the other more strictly in the field of experimental physiology. The director of the former is the distinguished Professor Nencki, of the latter the no less celebrated Professor J. P. Pawlow. It is to some of the work which has emanated from Pawlow's laboratory that I wish more especially to draw your attention. During the whole of the ten or more years which have elapsed since its foundation the laboratory, consisting of the director with a staff of assistants and numerous workers, has devoted itself exclusively to a systematic study of the processes of digestion. The first results of this combined work were given out in collected form, about three years ago, as a series of lectures to Russian physicians, and soon after published in the Russian language. The book was translated into German by Dr. Walther, one of the assistants of the laboratory, about a year later, and thus was made more available to the scientific world generally. It is quite a small work, less than 200 pages, but it literally teems with results of the most valuable kind to the practitioner of medicine. The volume was supplemented a few weeks ago by a further fasciculus of fewer than 50 pages.

From the chapters of the first of these publications, which deal with the subject of gastric digestion, I purpose to draw a few facts, which will serve to exemplify the nature of the work accomplished in Pawlow's laboratory. You can then judge of their practical bearing upon the science of medicine.

OLD METHODS FAULTY.

For a long time past, nobody had been quite satisfied with the methods by which gastric juice was obtained, in order to study its properties. This, as you will remember, used to be by means of simple fistulae leading through the abdominal wall into the stomach. The chief fault of this method consisted of the fact that gastric juice could never be obtained in any quantity, unmixed with food. In order to make real progress it was necessary to possess a method by which the juice could be collected (1) at all times, (2) in pure condition, (3) in exact quantity, (4) while the stomach functioned normally, (5) and from an animal in perfect health.

Fig. 1.—A, B, line of incision; C, flap for forming stomach pouch of Pawlow.

PAWLOW'S IMPROVEMENTS.

Pawlow achieved these desirable ends by an improvement of the old method, in which a partially isolated portion of the fundus of the stomach was converted into a little pouch which retained its nervous and vascular connections, and whose mouth was led to an aperture in the abdominal wall. The deeper end of the pouch, as well as the opening into the main portion of the stomach, were securely closed. By the application of rigid aseptic principles of surgery, no trouble was experienced in securing rapid and complete recovery of the animals. By means of a glass tube perforated with numerous small apertures, and led into the pouch, gastric juice of the purest quality could then be collected in almost any desired quantity. Figs. 1 and 2 give diagrammatic representations of the way in which this pouch is formed.

Fig. 2.—V, cavity of stomach; S, Pawlow's pouch; A, A, abdominal wall.

SOME OF THE RESULTS.

The first interesting fact confirmed by this method was that no secretion whatever occurs when the animal is fasting, but that a copious flow of juice can be set up by the mere act of chewing and swallowing food. This flow commences after a latent period of about five minutes has elapsed, never less than four and a half minutes, and is not due to the entry of food into the larger stomach. The latter was proved in an ingenious way—namely, by making an opening into the œsophagus in the neck, dividing the tube, and causing both ends to heal to the edges of the surface wound. After recovery the animal could be made to chew and swallow food as before, but the food came out at the opening in the neck, and unless specially led into the lower segment of the œsophagus, did not enter the stomach.

It was then seen that this "make-believe feeding" caused precisely the same secretion of gastric juice, and that the flow could be kept up as long as the animal continued to chew and swallow. Another modification of the method consisted in making the œsophagotomy as described, together with an ordinary fistula leading into the stomach. The juice excited by "make-believe feeding" could thus be collected from the whole organ, and not alone from a shut-off segment.

QUERIES WHICH PAWLOW SET OUT TO ANSWER.

Having secured an admirable means of studying the work of the gastric glands, Pawlow set out to answer the following amongst numerous other interesting questions relative to their functional activity:

1. Is the amount of juice secreted by the stomach in any way proportioned to the quantity of food eaten?
2. What is the way in which the juice is poured out? Is all or most of it discharged in one gush at the beginning of digestion, as soon as the meal is partaken of, or on the contrary by degrees, and in declining quantities suitable to the gradual digestion and removal of dissolved food from the organ?
3. Does the digestive power of the juice—the amount of pepsin which it contains—vary in any way with the different phases of digestion, or is it constant throughout?
4. Has the nature of the food any influence on the quality of the juice; or are the gastric glands in any way able to adapt themselves to the kind of food presented them for digestion?

The answers to these questions, with some of the evidence, I shall endeavour to present to you seriatim, and in as few words as possible.

RELATION BETWEEN AMOUNT OF FOOD AND QUANTITY OF JUICE.

Taking the first question as to whether there is any quantitative relation between the juice secreted and the amount of food partaken of; the facts on which the answer is based are shown you in the following table;

TABLE I.—Relation between the Quantity of Gastric Juice secreted and the Amount of Food taken.

Flesh diet ...	100 grams	=	26 c.cm.	} Juice
	200 "	=	40 "	
	400 "	=	106 "	
Mixed diet...	300 c.cm. milk	}	=	42 "
	50 grams flesh			
	50 grams bread			
	Double the above quantities	=	83.2 "	

The figures are taken from experiments in which a dog was "make-believe" fed with varying quantities, in the first instance, of a flesh diet, and in the second of a mixed one, the juice being collected from the whole stomach. On looking at these you will see that the chewing of 100 grams flesh caused a flow of 26 c.cm. of juice, of 400 grams almost exactly four times that amount. Again a given quantity of mixed diet produced a secretion of 42 c.cm., double that amount gave rise to almost precisely double the quantity of juice. The gastric glands therefore work in health with the greatest precision, apportioning their labour with beautiful exactness to the magnitude of the task before them. Increase the unit mass of any food by a given multiple, and within ordinary limits, the glands will respond with a like increase of gastric juice.

CURVE OF SECRETION OF GASTRIC JUICE.

Next comes the answer to the question relating to the manner in which the secretion of juice is spread out over the whole period of digestion.

The reply is taken from figures yielded by two experiments in which a dog was twice fed with 100 grams of flesh each time, the amount of juice poured out by the stomach pouch being measured hour by hour.

TABLE II.—Hourly Rate of Secretion of Gastric Juice (Two Experiments; Dog Fed with 100 grams Flesh in each).

Hour	Experiment 1.	Experiment 2.
Hour 1	11.2 c.cm.	12.6 c.cm.
" 2	8.2 "	8.0 "
" 3	4.0 "	2.2 "
" 4	1.9 "	1.1 "
" 5	0.1 "	1 drop
Total	25.4 "	23.9 c.cm.

Here you will see that the greatest secretion occurs during the early periods of digestion, but that the juice continues to be discharged in declining quantity as long as any food remains to be dealt with. The glands, therefore, do not pour out all their fluid at once and leave matters to themselves, but "stand by," paying out more and more with strict economy as long as the needs of the case demand, and the same result repeats itself with stereotyped exactness, as often as the same conditions are fulfilled. The course, therefore, which gastric secretion runs, follows definite and purposive laws, and is by no means a matter of blind chance. Fig. 3 represents in curve form the facts given in Table II.

Fig. 3.—Curve of secretion of gastric juice after a meal of flesh. (Two experiments).

DIGESTIVE POWER VARIES WITH STAGE OF DIGESTION. And now comes the reply to the third query. If the glands are thus capable of varying their rate of secretion in so remarkable a manner, according to the needs of digestion, are they not likewise able to extend similar variations to the digestive power—the pepsin content—of the juice which they secrete? I speak only of pepsin content because Pawlow has shown that the acidity of the juice, as it pours from the glands, is always constant and varies only because it comes into contact with more or less of an alkaline mucus, which latter in health should not be present.

The digestive power of the juice was determined by causing it to act on short columns of coagulated eggwhite, contained in capillary glass tubes broken into small pieces. The length of column dissolved gives a reliable means of measuring the activity of the juice. The results obtained from two experiments on a dog fed each time with 400 grams of flesh are given below:

TABLE III.—*Digestive Power of Gastric Juice (Quantity of Pepsin) at Different Stages of Digestion (Two Experiments; Dog fed with 400 grams of Flesh).*

	Experiment 1.	Experiment 2.
Hour 1	6.0 mm.	5.8 mm.
" 2	4.5 "	4.1 "
" 3	3.4 "	3.4 "
" 4	3.5 "	3.0 "
" 5	3.8 "	3.8 "
" 6	3.0 "	3.1 "
" 7	3.6 "	3.5 "
" 8	3.9 "	4.5 "

You will see from these figures that the juice secreted at the beginning of the meal has, in the case before us, the greatest digestive power—that is, contains the most pepsin—when presumably it is most required, and that the proportion gradually declines as the need for it likewise decreases during the following stages. The glands then have also the power of varying the quantity of pepsin which they discharge in the juice according to the same laws by which they vary the total amount of secretion poured out. Fig. 4 illustrates the same in curve form.

Fig. 4.—Curve showing digestive power of gastric juice hour by hour; food 400 grams flesh. (Two experiments).

So far then we find ourselves dealing with a mechanism whose adaptation to its work could not be more perfect nor more beautifully conceived.

REGULATION OF THE JUICE TO THE NATURE OF THE FOOD.

But the fourth question is perhaps the most interesting of the whole in the series which I have selected. Has the nature of the food any influence on the quality or quantity of the juice? Are the glands capable of adapting their secretion to the kind of food presented them for digestion, and if so, how is this met?

The answer to the first part of the question is undoubtedly in the affirmative, the variations applying not only to the quality and quantity of the fluid, but likewise to the course and duration of its outflow. It would occupy too much time to quote all the experiments on which the facts are based, but these are the chief facts themselves.

Weight for weight the greatest amount of juice is poured out on flesh diet, the least on milk, bread being intermediate.

The total acidity is greatest with a flesh and lowest with a bread diet.

The digestive power—the pepsin content—is greatest with the juice poured out on bread, least on milk, the proportion being as follows: Bread-formed juice, 44; flesh-formed, 16;

and milk-formed, 11. Milk, therefore, in agreement with what is known from everyday experience, makes least demands on the activity of the glands for its digestion.

Nor do the peculiarities end here. The manner in which the juice is secreted and the period at which it attains its maximum digestive power are likewise different for each class of food-stuff. This is exemplified in the following table:

TABLE IV.—*Influence of the Kind of Food on the Quantity and Quality of the Gastric Juice.*

	Quantity of Juice in c.cm.			Digestive Power in mm. of egg white.		
	Flesh.	Bread.	Milk.	Flesh.	Bread.	Milk.
Hour 1	11.22	10.6	4.0	4.94	6.10	4.21
" 2	11.30	5.4	8.6	3.03	7.97	2.35
" 3	7.60	4.0	9.2	3.01	7.51	2.35
" 4	5.10	3.4	7.7	2.87	6.19	2.65
" 5	2.80	3.3	4.0	3.20	5.29	4.63
" 6	2.20	2.2	0.5	3.58	5.72	6.12
" 7	1.20	2.6	—	2.25	5.48	—
" 8	0.60	2.2	—	3.87	5.50	—

Quantities of Food—Flesh, 200 grams; Bread, 200 grams; Milk, 600 c.cm.

The time of maximum flow occurs therefore with a flesh diet either in the first or second hour, and both are nearly equal, with a bread diet in the first, and with a milk diet in the second hour. The period of maximum strength is reached in the first hour with flesh, in the second or third with bread, and towards the end of digestion with milk diet. Fig. 5 represents the curve of secretion of gastric juice on different diets, and Fig. 6 that of its digestive power at different stages under the same conditions.

Fig. 5.—Curves of secretion of gastric juice with different diets.

Fig. 6.—Digestive power of gastric juice hour by hour on different diets.

FIXED MODIFICATIONS OF THE DIGESTIVE FLUIDS TO SUIT PARTICULAR DIETS.

Intimately connected with the foregoing is a further important question, namely, whether the digestive system is capable of permanently adapting itself to a special form of diet or not. Thus, given a continued preponderance, say of proteid in the diet, with a paucity of fat and carbohydrate, or on the other hand, a more than usual proportion of carbohydrate, with a scarcity of fat or proteid, or lastly an undue amount of fat;

what power have the digestive organs of meeting these possibilities? The answer is that a condition is gradually established whereby a steady increase takes place in the amount of special ferment which is necessary to meet the requirements of the particular diet, while at the same time a decrease in those which are superfluous sets in. Such adaptation is, however, not accomplished through the gastric glands but by means of the pancreas. The main importance of the matter lies in the fact that once having established a dietic adaptation of secretion, it cannot be undone in a day. Great alterations in a dietary, therefore, even when urgently demanded, can only be advantageously introduced by degrees.

Fig. 7.—A, ordinary curve of gastric secretion, 250 grams flesh; B, curve from direct introduction of food, 150 grams flesh; C, curve from "make-believe" feeding with same; D, summation of B and C.

FLOW OF JUICE FROM "MAKE-BELIEVE FEEDING."

And now one point more, before I leave the results which I have selected from this admirable series of researches. It is I think perhaps the neatest determination made in the whole set of experiments. This is no other than an actual estimation of the value of the combined act of chewing and swallowing in terms of gastric juice.

In establishing the fixed rate of secretion, for the whole act of digesting a particular meal (which as a matter of fact under like conditions has been found to be always the same) we have determined the value of chewing and swallowing, *plus* the further exciting effect of the food after it enters the stomach. But we can estimate the value of these two factors separately, that of chewing and swallowing, in the way I have explained to you, by receiving the food from an opening in the oesophagus and not allowing it to enter the stomach. The other factor is known if we bring the same meal directly into the main stomach of the animal through an ordinary gastric fistula, and observe its effects on the secretion as shown by the stomach pouch. Food introduced in this manner is not nearly so powerful an excitant of juice flow as when it enters in the usual way. But the curious fact comes out that the sum total of the "make-believe-feeding" or swallowing juice, *plus* that caused by the mere introduction of food is exactly equal to that of the whole meal swallowed and digested in the ordinary way. Thus by having any two of the foregoing factors, we can readily determine the third, and in this way we learn that the whole of the earlier part of the secretion is an accompaniment of the act of chewing and swallowing, and not at all dependent on the mere entry of food into the stomach.

Now this "make-believe" juice, if we may so call it for shortness, is not only copious in amount, but of a very active character. It constitutes therefore a most important part of the whole. But although an accompaniment and apparently caused by these combined events, it is not really so.

VALUE OF APPETITE IN CAUSING SECRETION.

The same secretion can be excited by the mere sight of food, accompanied by an eager desire on the part of a hungry animal to have it. This implies that the food is such as will excite this keen desire, and without it the chewing and swallowing are wholly ineffective. Everything therefore which aids in creating the desire for food and in augmenting the satisfaction of having it has its material value in an increased flow of the best and most active gastric juice. Hence the importance of set preliminaries to the chief meal of the day. The customary ablutions, the change of raiment, the dismissal from the mind of business anxieties, the table

decorations, the plan of the meal, the savoury odours, and, last but not least, the expectation of finding an appetising repast, these are all matters of moment and have their fixed values in terms of active gastric juice.

NEED FOR CHEMICAL WORK IN CLINICAL RESEARCH.

I may now return to the query with which I left off—regarding the application of physiological methods to the study of disease.

It would be ungracious of me to find fault in any way with the excellent clinical work done in this country, even if I possessed the authority or had the wish to do so. That is not my purpose. I wish rather to indicate a line of work which might, I think, be more cultivated, and which I believe has great possibilities connected with it. It is work which will necessitate a considerable expenditure of time and trouble, but these considerations must not stand as serious obstacles in one's path when the end in view is important. The line of work to which I refer consists in the fuller application of the methods of chemical physiology to clinical work.

Some years ago a means of examining the contents of the human stomach was perfected by Ewald. This consists in the introduction of a soft tube at a given time after a test meal whereby the contents can be siphoned off or expressed out of the organ. The method necessitates a careful chemical examination of the juice obtained, and, when this is done, I am told it yields information of the most valuable kind regarding disease, which cannot be obtained in any other way. But is this method, which I think I may call a physiological one, adopted with sufficient frequency in our country? Are the chemical examinations which it entails carried out with sufficient thoroughness?

Again, in the majority of departures from health, the importance of quantitative variations in the composition of the chief excreta of the body outweighs in value the qualitative. And yet, are quantitative chemical determinations of these ingredients carried out to the extent, and with the degree of precision which their importance demands? Nay, more, are the means for such examinations adequately supplied by the numerous great institutions devoted to the treatment of disease throughout our country?

Further, it must often be almost imperative in such perverted conditions of body metabolism, as one meets with in the various kinds of diabetes, of leucocythæmia, of Addison's disease, in obscure fevers, aye, even in ordinary fevers, to possess a complete picture of the whole metabolism from beginning to end, from the ingesta to the egesta. It is only by thus seriously grappling with such problems that real progress can be achieved.

NEED FOR CHEMICAL LABORATORIES AS HOSPITAL DEPARTMENTS.

And yet what clinical hospital in our land is adequately equipped to afford its staff the necessary amenities for carrying out such a series of investigations? I do not imply that there are no facilities for such work, but they are all too few, and the laboratories are not, as they should be, integral parts of every large hospital, just as much as well-equipped operating theatres are at the present time. That it is by no means impossible to carry out such a scheme is shown by the fact that it is adopted, for example, in the Johns Hopkins Hospital in Baltimore, and in many of the clinics in Germany.

CHEMISTRY IN THE MEDICAL CURRICULUM.

And now I think I come to the kernel of the whole matter. Is the training which our medical students for the most part receive, in the subject of chemistry carried out along lines that will enable them to meet such conditions as I have indicated in the later stages of the curriculum and in after-life? Are our young medical men afforded sufficient opportunity and encouragement to draw them into such fields of inquiry?

The education of our students in this branch of science—I say it with all humility—has not, to my mind, received the attention which it deserves. Till very recently, it was quite the exception for examining bodies, university or other, to require any knowledge, however elementary, of quantitative chemical analysis, and even now there are, so far as I know, only two which insist upon it.

Our students are well versed in the detection of the ingre-

dients of simple and complex chemical mixtures, many of which contain metallic elements never normally found in the human body, or likely to occur in it as poisons. But the important practical matter of quantitative estimation has been wholly omitted, and with it the invaluable training in experimental methods of precision which even a short course of this work entails. Nor has it been found impossible by those who have tried it to graft such a course of instruction on to the old scheme, by omitting here a little and there a little of unnecessary or less useful work, without prolonging the time which a student ordinarily devotes to practical chemistry.

As an indication of the necessity for alteration in the direction I suggest, I have often mentioned the fact that quite a small number of students, when they come to work at physiological chemistry, know the meaning of the simple word "titration," and curiously enough I have found my remarks very forcibly borne out by discovering not long since, in an otherwise excellent translation of an important German medical work, that this same word is all through the book rendered by the English word "trituration."

But I do not wish to lay the blame entirely at the door of the chemists. The same accusation till comparatively recently held good against the physiologists, with this difference as I think, that these latter have more quickly appreciated the requirements of the case, and have striven as far as possible under present conditions to meet them. Behind both sets of teachers however lies the Council of General Medical Education, which has not awakened to its responsibilities in this matter.

NEED FOR POST GRADUATE RESEARCH SCHOLARSHIPS.

Such arrangements and possible legislation as I have foreshadowed can, however, only be designed to meet the needs of the average student. For specialisation and also for research the young medical man must obtain a fuller training. How is this to be acquired when as a rule he is under the necessity of toiling hard to earn a living? Well, a good deal can always be done by an earnest worker in the unoccupied hours with which the young practitioner is usually blessed.

But the progress of medicine should not be left to take its chance in this haphazard fashion. Hitherto money prizes and scholarships have been almost exclusively applied to other and perhaps very desirable objects. It cannot, however, be too quickly recognised by those able and willing to promote the advancement of medical knowledge that here is a field most deserving of support from which an urgent appeal for assistance is now issuing throughout the length and breadth of the land.

NEED FOR MORE WELL-EQUIPPED LABORATORIES.

But even these provisions will not fully meet the needs of the case. Achievements such as I have exemplified in the case of one department of the St. Petersburg Institute are only possible by the aid of combined work. For this purpose well-equipped and fully-manned laboratories must be provided by some means or other, and all honour be to such men as Lord Iveagh, Mr. Thompson-Yates, Mr. R. G. Dunville, and others who have nobly come forward with their wealth to aid in this work. Philanthropists of this type are, however, far too few. Nor should the cause be left entirely in their hands. The Government of our great empire, which has hitherto shown too little sympathy with such aspirations, must awaken to its duties in this respect if our country is to take the place which its responsibilities impose upon it in the advancement of a great, noble, and humane science.

Meanwhile we must each and all of us do what little we can, and "be satisfied with success in even the smallest matter, and think that even such a result is no trifle."

MEDICAL STUDENTS IN GERMANY AND SWITZERLAND.—In the last (summer) semester the number of students of medicine in the universities of Germany and Switzerland were as follows: Munich, 1,421; Berlin, 1,067; Vienna, 1,033; Leipzig, 561; Würzburg, 549; Freiburg, 509; Berne, 343; Graz, 340; Erlangen, 331; Strassburg, 315; Zürich, 315; Bonn, 306; Geneva, 305; Heidelberg, 301; Giessen, 293; Tübingen, 297; Greifswald, 275; Breslau, 245; Königsberg, 246; Marburg, 239; Innsbruck, 234; Halle, 215; Göttingen, 213; Jena, 188; Kiel, 185; Lausanne, 160; and Rostock, 124.

AN ADDRESS

ON

SOME OF THE SEQUELÆ OF CRANIAL INJURIES.

*Delivered at the Annual General Meeting of the Shropshire and
Mid-Wales Branch of the British Medical Association.*

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It is a most difficult matter for a country practitioner to keep notes of cases passing through his hands, even if he limit his note-taking to the most interesting. The cases, indeed, which prove of most interest in their termination are often the least so at the commencement. My own notes of the past twenty-three years are scattered, but the facts that a large number of cases of cranial injuries have been under my own care, and that cases in which a fall from a horse, a carriage accident, a blow on the head, each produced some different brain lesion, have induced me to collect from other sources histories of cases of cranial injury followed by epilepsy, moral perversion, diabetes, and insanity.

I have collected as many cases as possible from unassailable sources, with their complete histories, and then deduced as well as I could those facts that should be useful in prognosis. The cases have been collected without any reference to the seat of injury, my only request being for cases of cranial injuries resulting in epilepsy, diabetes, or insanity, all of which except insanity I have personally met with. I shall preface each series with a short history of a case that has come under my own care.

The first series of cases are of a most interesting nature, as they are all the result of frontal injuries, and represent a condition that is most difficult of treatment, and consequently of prognosis. These cases were collected, like the rest of the series, with no reference to the seat of injury. The following are six cases of moral perversion following injuries to the head:

CASE I.—A young married woman, with one child, was thrown out of her trap on to the frontal eminence. She was unconscious for six days, and very frightened for many months after. She then developed nymphomania, and took a violent dislike to her own people, but was otherwise sane. This condition lasted many months, but was eventually overcome by travelling, and she is now as devoted to her husband and child as ever.

CASE II.—A boy, aged 12, suffered a depressed fracture of the frontal bone. No operation was performed. Prior to the accident he had a most exemplary character both at home and at school. On getting up he took to stealing things of all descriptions, useless or otherwise, and would hide them. Previously good tempered he became irritable, with violent outbursts of temper.

CASE III.—A girl, aged 10, previous to sustaining a compound depressed fracture of the frontal bone, had been bright and sharp. There were no symptoms of concussion. An operation was performed and the fragments raised. This child was afterwards idle, untrustworthy, destructive, untruthful, and took to stealing.

CASE IV.—A boy, aged 12, suffered a depressed fracture of the frontal bone with hernia cerebri. He had epileptic fits for five months afterwards, then his character altered, and he developed stealing propensities.

CASE V.—A man, aged 35, with no family history of mental disorder, received a very severe blow on the left frontal region. The outer table was fractured, and he was unconscious for a quarter of an hour. He was not again rational until his removal to an asylum. So-called cerebral irritation set in, he was incoherent and confused, also acutely maniacal and destructive. He recovered gradually.

CASE VI.—A man, aged 56, with no family history of mental disorder, sustained a fracture of the frontal bone by a kick from a horse. He was unconscious for one hour, and then began to wander about, became threatening, refused his food, and developed mania. He went to an asylum, and in five months was discharged cured. No operation was performed.

These cases are noteworthy, as all resulted from blows over the psychic area.

TRAUMATIC DEMENTIA.

The second series of cases, those of traumatic dementia, are extremely difficult to discuss as the histories are not complete. My idea being to get the region of the cranial injury, and I must make the best out of the material provided.

CASE VII.—Skull injury; region not stated. Mania.

CASE VIII.—Fractured skull; some months later cephalalgia, *petit mal*, and *grande mal*. He killed his wife and shot himself. He recovered from this wound, but afterwards cut his throat.

CASE IX.—Cranial injury, followed by severe headache. He became insane, and killed his wife.