tation, and that the excitability of the parts is not the same in the two sides of the brain as in the rest of the body, then it is very strange that hemiplegia will appear on the one side or the other, according to the mere excitability of the parts. Next to this, there is a fact that I have dwelt upon in a long lecture which I published in the beginning of the year, that a disease located on one side of the brain can produce paralysis on the same side. It may be in the medulla oblongata itself, or in the cerebellum, or in the pons Varolii, or in any other part of the brain. Well, with the old views, it is impossible to understand this unless you admit what has not yet been proved, except in one case to my knowledge, that there is no discussion of the pyramids; but if we prove that the pyramids are not the channels of the transmission of the will to the muscles, it matters not whether there is discussion on the one part or the other, so that in one case there is a paralysis in one side corresponding to the disease on the same side. It is contrary to the old views that paralysis occurs, not because of the destruction of the conductor, or will-centre, but because of the irritation existing where the disease is, acting sometimes on the one side, and sometimes on the other side, of the cells of the spinal cord and the medulla oblongata, with a sort of involuntary action, so as to stop their activity. There are a variety of facts, besides these, that I dealt with in my lecture, there are a great many cases in which there is a disease of the two sides, but paralysis in those cases appeared on the side where the greatest irritation existed. Of course, if paralysis may appear on the side where there is disease, it may appear where there is a disease on the two sides, but on the side where the greatest irritation exists. There are also cases in which hemiplegia exists on one side of the body, and then some time afterwards it appears on the other side, the first hemiplegia being cured, and nothing is found but the disease on one side of the brain. Was there some other disease that was not known? I do not know. There are many cases which support the view I maintain, which is not in accordance with the old views. Then there may be disease existing in one side of the brain, producing at one time paralysis of the arm on one side, and paralysis of the leg on the other side; or a disease which produces at the same time paralysis in one arm, and paralysis in the other. But here now comes a kind of case, and I have a good many of them, which is extremely demonstrative. In those cases there is paralysis of the body on one side, and paralysis of the face on the other side. These cases are very well known, and very well explained, perhaps, when the disease is in the pons Varolii, through the disease striking at the same time the roots of the facial nerves, or the trigeminal nerve on one side where it exists, producing paralysis of these nerves. This is perfectly plain; and of course when I have put forward the views I entertain as regards paralysis, I deal not only with a paralysis not depending upon the existence of the nerve, but its entrance into the base of the brain, as in those cases it is a disease of the nerve that exists and not a disease of the nerve-centre. In these cases, the disease may sometimes be in the cerebellum, sometimes in the ganglia at the base of the brain, sometimes in the posterior lobe, sometimes in the anterior lobe, and so on, so that the greatest variety of seat has been observed in those cases in which there was this paralysis. And what do we see in these cases? the paralysis on the same side as the disease in some of these cases, and paralysis in the opposite side of the body in some of these cases also. There are two cases also in which paralysis appears on the corresponding side of the face to the disease, and the paralysis in the body appears in the corresponding side; there are but two of these cases, and they are very interesting; but all the details are missing in one of them, so that I would not say that we should attach much importance to them. There are forms of paralysis which, although depending on the brain proper, have all the characteristics of paralysis depending on the cerebellum or on the spinal cord itself. There are a number of cases in which hyperæsthesia appears in the paralysed limb, while the same degree of hyperæsthesia in a few of these cases was said to exist in the opposite side; and, in those cases where hyperæsthesia existed on the side of the paralysis in the brain-disease, physicians have not paid attention to the sensibility of the supposed healthy limbs. But in two cases that I have seen, and in many cases published by Dr. Priestley, hyperæsthesia existed on the paralysed side, and anæsthesia on the opposite. There are many cases in which, with the lesion on one side of the brain, paralysis has existed in both sides. Of course it may be said that there was another lesion which had not been found. That is quite possible; but in cases of hæmorrhage of the brain, in cases of sudden paralysis, it is very likely, if the patient die soon, we may find that there was no other lesion, and that we are to consider paralysis as having been produced on the two sides by the irritation starting on the one side. There are many cases of paralysis of the lower limbs, and a number not so large by far of paralysis of the two upper limbs, occurring from disease in one part of the brain. I used to fight against the

admission of such a thing; I could not believe it. I could not realise that it was possible that a disease located anywhere in the anterior, the middle, or the posterior lobes, not to speak of the pons Varolii, medulla oblongata, or cerebellum—that any disease located in the cerebral lobes can produce paralysis of the lower limbs without acting at all on the face and the upper limbs. I have, however, been convinced that beyond doubt there are such facts; and, in the publication of facts, which I shall make fully, you will find details which will leave no doubt on this point. There is one kind of paralysis which is very much in opposition to the old views, and that is paralysis limited almost entirely to one limb. If disease exist in any part of the brain, and produce paralysis in one arm, you know perfectly well it is chiefly the arm which is affected in these cases. It is considered, so far as certain facts go, a demonstration of the view of Soissart and others that the brain in certain parts contains the will-power of the arm, and in other parts the will-power of the leg; that the anterior part of the brain serves to move the leg, and the posterior part serves to move the arm. There are, however, a great number of facts which give the lie to this conclusion. There are cases of paralysis occurring throughout the upper and the lower limbs indifferently, with the lesion located anywhere, and not in one particular spot with reference to the paralysis in each part of the body; so that there is no doubt whatever that paralysis in the leg and paralysis in the arm may appear, no matter where the lesion may be. I find the time has gone, so that I can only say one word more; but I will return to this in the next lecture. It is that facial paralysis may be caused by disease in a limited part of the brain, without any paralysis anywhere else; such cases are extremely common, judging from the number of facts I have collected.

THE PROGRESS OF FEVER PATHOLOGY.

Extracts from an Address delivered at the Annual Meeting of the Bath and Bristol Branch.

By HENRY F. A. GOODRIDGE, M.D., F.R.C.P.,
Senior Physician to the Royal United Hospital, Bath; President of the Branch.

[RECOGNISING the advantage of making these annual addresses fulfil a sort of *compte rendu* purpose, Dr. Goodridge selected for his subject, the progress of fever-pathology. There were at least three reasons, he thought, why the subject of fever should possess especial attractions to the medical mind. 1. The frequency of its occurrence. 2. The gravity of its import, and 3. The occult nature of the process. He then continued]: "Occult in times past it most certainly was, unsurpassed in this respect by any other disorder of the nosology; occult, it is to be feared, we must confess, the febrile process, in not a few nor unimportant particulars, still is. But has not the light of modern research which has illumined so many dark corners of medical science at length penetrated the thick darkness here? Have not the improved means and methods of investigation—the introduction of the various so-called instruments of precision into clinical and pathological use—nay, the great and important department of experimental physiology, which has of late so rapidly developed, borne some fruit in this quarter? It is just to these questions that we would now address ourselves; and I think we shall find that if the results achieved are not so successful, so brilliant, as are witnessed elsewhere, it is at least not because of any neglect of the advantages referred to, nor of any lack of able and zealous labourers in the field or of solid and enduring work done, but rather because of the intricacy and recondite character of the subject to be dealt with, the exceeding difficulty and complication of the many problems to be solved, and the manifold sources of fallacy which continually start up on the right hand and on the left, while prosecuting the several branches of the inquiry.

Deriving, as we do, the word fever from *febris*, and that from *fervere*, it might be supposed that the recognition of excess of heat as the most characteristic feature of the disorder was as old as the Latin noun itself, and co-extensive with its use. Yet this would be not quite correct. For, firstly, since the days of Hippocrates until quite recent times, authorities had frequently maintained that fever was a favourable circumstance, a salutary process, and they found that their etymology and their pathology coincided, or at any rate they made them coincide, for they derived the word from *februa*, which, as we well know from the name of our second month, at one time the final one of the Roman year, means to make pure. And, secondly, Boerhaave and his followers held that the increased frequency of the pulse was the great characteristic of the febrile state. The opinions entertained by the ancients upon the subject of fever will always form a curious and interesting chapter in the history of medicine. We cannot, however,

at present further advert to them, but must content ourselves with the single remark, that if ever in matters medical speculation could be said to have run riot, assuredly it would be in reference to fever. And as in some other matters, so here; what men found they could not succeed in explaining to their satisfaction, they at last came to deny the existence of altogether. The school of Broussais, or the pathologico-anatomical school of his period, rejected all essential fever or fever that was not demonstrably dependent upon a local inflammation, or that had not, as it was termed, a material basis; thus, I hardly need remind you, typhoid or enteric fever was with them a dothineritis, intermittent fever a periodical gastro-enteritis, or an inflammation of the spleen, and so on; and well do I remember that, when proceeding in 1846 to the final examination for my M.B. degree, no candidate was thought to be safe who was not well posted up in the arguments in defence of the essentiality of fever. Nobody, I suppose, nowadays expects to be challenged on this matter.

In tracing the development of modern views of fever, we have not *really* then (and this will more distinctly appear immediately) to go back much further than a quarter of a century ago. Certain events are conspicuous as having played an important part in the process. I will briefly review them.

First of all, of course, was the introduction of the thermometer as a clinical instrument. Until the time of De Haen, the ordinary method employed for estimating the heat of a fever-patient, was the application to his skin of the hand of the observer, than which, we know, none could be more fallacious. De Haen, using the mercurial thermometer, and with precautions previously neglected, carried out by its aid the first systematically conducted observations which laid the foundation for the regular employment of thermometry at the bedside. He ascertained, what at first seemed paradoxical, that during the cold stage of intermittent fever, when the patient is shivering almost intolerably, the temperature of the interior of his body exceeds to a considerable amount the normal standard, and becomes as high as in the succeeding hot stage. De Haen flourished about the middle of the last century. He did not, however, recognise the wide bearing of his own discovery, nor the consequences deducible therefrom, but only found therein a weapon wherewith to fight the iatro-mechanical physicians, whose theory that fever was due to friction was then rife, the Dutch professor himself being all the while satisfied that fever was one of those things which it was not vouchsafed to mortal man to know. His contemporaries and the following generation were alike supine, and so De Haen's observations almost passed into oblivion, until first Gavarret in 1839, and then to a much more considerable extent G. Zimmermann in 1851, brought them to light again. The latter demonstrated the great practical value of the thermometer and of temperature observations, and subsequently Wunderlich may be said to have fully established the same. While then, on the one hand, the thermometer has only confirmed an observation as old as the days of Galen, that "*calor præter naturam*" is an essential symptom of fever; on the other, this instrument has furnished the means of determining with exactitude the presence or absence of this pathological condition, of measuring the intensity of the same, and of registering the fluctuations, even to minute fractions of a degree, which, according to the specialities of each case, the febrile heat may undergo. In fact, the thermal history of a case of fever has become capable of accurate study, and can be even graphically represented as in the temperature charts with which we are now so familiar. The march of each one of the principal varieties of fever has been found to be definitely characterised in respect of its pathognomic feature, its pyrexia; and thus the thermometer comes to confirm or correct a diagnosis otherwise made, or may itself alone render any diagnosis possible. But to expatiate, as it would be easy to do, upon the advantages of the clinical thermometer, surely were a work of supererogation, when probably nearly every gentleman whom I have the honour to address is carrying one in his pocket.

We can readily understand that at a period when erroneous views prevailed respecting animal heat in general and its sources, correct ones could hardly exist as to the origin of the preternatural heat of fever. A false physiology could scarcely lead to a true pathology. But as the heat of the body is now known to depend mainly upon chemical processes—oxidation processes—continually taking place in its tissues, the preternatural heat, which was at once assumed to be due to increased production as its principal cause, was taken in conjunction with another fact, also well known to the ancients, viz., that in fever the body wastes, or, as subsequently more accurately determined, fever-patients lose more in body-weight than non-febrile persons under otherwise equal circumstances, and the conclusion was arrived at, that in febrile diseases an increased consumption of the tissues of the body constantly takes place. This doctrine, although partly anticipated by others, was

first definitely stated by Virchow in his classical essay on fever in 1854, and with an important addition was embodied in the following formula, which formed the basis of the Goulstonian Lectures, delivered the following year by that great and good man, for the loss of whom to science and humanity the tear has hardly ceased to flow, Dr. Parkes of Netley. "Fever," said Virchow, "consists essentially in elevation of temperature, which must arise from an increased tissue-change, and have its immediate cause in alterations of the nervous system". Forthwith the appeal was to facts, and the new theory was submitted to the test of strict clinical and experimental investigation. The results we shall soon come to consider. The temperature of the human body being the resultant of an equilibrium between production of heat and loss of heat, it is obvious that the same effect may be brought about in opposite ways. Accordingly, the late Professor Traube of Berlin maintained that the elevated temperature of fever was the effect not of increased production of heat, but of diminished loss of heat. The diminished loss itself he attributed to irritation of the vaso-motor nerves, under the influence of the fever-exciting cause, producing spasmodic contraction of the small arteries of the periphery, and consequent anæmia. In regard to the well-pronounced and rapidly developed cold stage of intermittent fever, with all the phenomena indicating that the supply of heat from the interior to the periphery is for the time being restrained, there can be no doubt that the aggregate discharge by radiation and conduction of the heat of the body is diminished. But clearly Traube was too exclusive. His theory was found to be inapplicable to fever in general, as we shall see further on; and even in respect to the cold stage, thermometric observations have shown that the temperature rises before the shivering and the vascular spasm and cutaneous anæmia have at all occurred. Yet Traube did good service in drawing attention to diminished heat-loss as concerned in the elevation of temperature of fever, and thereby checking a corresponding exclusiveness that seemed likely to arise on behalf of the other factor, heat-production.

An event which marks an epoch in the history of physiological science, for with it is connected the great discovery of the vaso-motor nervous system, is scarcely less notable in the history of fever doctrine. It is needless to say that I refer to Claude Bernard's famous experiment twenty years ago, of dividing the sympathetic nerve in the neck of the rabbit. We have just seen that Traube invoked vaso-motor nerve-agency to help him out with his theory; but, in regard to this experiment, we know that one of the most striking phenomena observed to follow upon the operation is a rapid rise of temperature in the corresponding ear and half of the head of the animal. Now, this has been very differently interpreted. By the distinguished French physiologist it has always been maintained, and in his recently published "*Leçons sur la Chaleur animale*" he vigorously defends the point, appealing to the latest researches of himself and others in proof, that there is absolutely increased production of heat, created heat, within the sphere of the divided sympathetic, and that this increase, at first local, becomes general—"la température du sang, avec la fièvre d'abord locale, finit cependant par se généraliser". It was but a short step for those who shared this view to propound concerning fever, as in fact was soon done, that it consisted essentially in a paralytic condition of the sympathetic nervous system. On the other hand, there have been, and still are, numerous authorities, and among them too those who hold neuro-pathological views of fever, who deny that any heat is produced in this experiment, and attribute the rise of temperature observed to the local afflux of blood consequent upon paralytic dilatation of the vessels, in fact, to simple importation of heat by the circulation. They contend, moreover, that so far from the temperature of the whole body being raised as a consequence, a slight but distinct lowering of it is detectable. Thus, while with the one party this experiment is of supreme value as elucidatory of the pathogenesis of fever, with the other it is of no such value at all.

The discovery in physics, within the last few years, of the law of conservation of force (as it is termed), since it has thrown important light upon the mechanism of production of heat within the body, has contributed in no inconsiderable degree to the progress of fever-pathology.

[Dr. Goodridge then passed in review the clinical and experimental investigations undertaken in connection with the doctrines of increased production of heat, and of increased tissue change, in fever, and proceeded]—

We come now, in the last place, to the dependence of fever upon alterations of the nervous system; and this is just the most debatable portion of our subject. We anticipated it somewhat in our notice of Claude Bernard's discovery of the vaso-motor nerves. We then remarked that a theory of fever had originated directly from that discovery; and, although but one of several theories that assign to the nervous system an important rôle in the production of that disorder, it

would appear to be the simplest and most definite of them, and perhaps, on this account, has very much concentrated upon itself the attention of experimental inquirers. According to this theory, fever is due to vaso-motor paralysis. The appeal has been to the experiment of dividing, in animals, the spinal cord at the level of the fifth or sixth cervical vertebra, to which proceeding, of course, closely correspond severe accidental lesions to the cord in this situation in the human subject. It was found, then, as the result of this experiment, that in animals of some size a rapid rise of temperature often occurred, exceeding that of fever; but in small animals, cooling was the rule, unless the surrounding air was warmed or they were artificially clothed, in which case the temperature rose, as in the former instance, and might attain to even a fatal elevation. By the advocates of the vaso-motor paralysis theory, these results were at once attributed to the disconnection of the vaso-motor nerves from their intracranial centre by the section of the cord, and the consequent withdrawal of their inhibitory influence upon the combustion processes of the body—in short, they regarded the condition thus brought about as very much identical with fever in the clinical sense. The cooling which occurred in small animals, they explained by excessive radiation of heat from the surface, itself the consequence of paralytic dilatation of the cutaneous vessels. In *man*, since Sir Benjamin Brodie's historical case, several similar ones have been recorded, in which inordinate elevation of temperature has ensued upon crushing injury to the cervical cord; and there are those here present, I doubt not, who have seen such. On the other hand, there have been as notable cases of depression of temperature, after the same injury. Mr. Hutchinson, in his excellent lecture on the subject, as regarded from a physiological stand-point, gives it as his opinion, that the condition of the heart makes the difference. "It certainly never happens", says he, "that the temperature rises in these cases unless the heart's action is vigorous. If the pulse is only of moderate strength, the patient cools". But to apprehend more distinctly, in reference to the claims of the vaso-motor paralysis theory, what are the essential effects of section of the cervical cord, let us briefly note what alike *a priori* reasoning, physiological experience, and clinical observation combine to teach respecting them. Of course, in a transverse section, not alone the bands of vaso-motor fibres, but all the other constituents of the cord, at the same time, will be divided. First, then, there occurs a sudden and great diminution of arterial tension, the direct result of vaso-motor paralysis—arterial tonus, as it is called, is abolished—more or less venous stasis arises from the quiescence of the muscles, and the heart's action, by its own partial paralysis, is retarded; in fine, the power of the heart and the efficiency of the circulation are greatly reduced. Secondly, there is paralysis of the muscles, to the extent of about five-sixths of the body; the muscles and the liver we know to be the most heat-producing organs of any; and while non-paralysed muscles, even though at rest, produce some heat, paralysed muscles produce next to none. And, thirdly, the respiration is impaired; the breathing is diaphragmatic; duskiness of complexion is, I believe I am correct in saying, a common feature in accidental cases in man; and, as a consequence also of the disturbance of the circulation, oxygen is imperfectly supplied to the organism. Hence, to sum up, allow what may be for the withdrawal of inhibitory influence, the result is a very serious curtailment of those processes upon the maintenance of which the production of animal heat depends. The real condition brought about, indeed, is one of particular interest; and the discovery of it affords, I think, one of the most telling instances of the value of animal experimentation, that method of research against which of late so much agitation has arisen. The one great characteristic difference, then, between an animal whose cervical cord is divided, and a normal one, is, that the former has lost all power of regulating its heat according to external conditions. Let us hear the description given by our distinguished pathologist, Dr. Burdon Sanderson. "A healthy dog, accustomed to the temperature of summer, may be exposed to that of a winter's day without the slightest variation. After the cord is divided, the same animal must be clothed, even when in a warm room, else it cools too much; while, if the room is only a very little too warm, it passes into a state of intense pyrexia. To prevent either result, depression on the one hand or collapse on the other, the surface-loss has to be accurately adjusted to the thermogenesis by artificial means; for the animal has lost its powers of making any adjustment for itself". The warm-blooded mammal has been reduced, in fact, to the condition of a cold-blooded animal. Thus, the man with his spinal cord crushed (supposing it to be completely crushed) at the level of the fifth or sixth cervical vertebra, has lost his power of regulating his heat; his temperature is no longer stable, but labile, dependent upon that of the surrounding medium. But is this the condition of the fever-patient? Far from it. The latter still regulates his heat, nearly as efficiently, indeed, as a healthy person does, only for a higher temperature; the one regulates for 98.6 deg. Fahr., the other for say 104 deg. Fahr.,

more or less as the case may be. The smallest experience of the treatment of fever by the cold-bath, the true antipyretic method, will satisfy any one on this point; for to the overpowering of the regulation-force of the patient our utmost efforts have to be directed. As to the supposed increased discharge of heat from the surface of animals with divided cord, Dr. Murri of Florence alleges that he has proved the direct contrary. Whilst the temperature in the rectum was normal or excessive, he found that of the skin unnaturally low. For the reasons that have been given, this is quite conceivable; at all events, it is clear that the result of section of the spinal cord cannot be used to uphold the vaso-motor paralysis theory of fever. Not a few pathologists, however, have adopted this theory. A reference to Dr. Murchison's grand work will show that he ranks among them. Claude Bernard, notwithstanding that he contends, as we have seen, for the thermogenetic effects, general as well as local, of division of the sympathetic in the neck, finds that section of the cord presents difficulties; and so he falls back upon the hypothesis of vaso-dilator excitation to explain the phenomena of fever, the existence of vaso-dilator nerves not having yet been demonstrated. Billroth, Liebermeister, and others, trace the essence of fever to some alteration of the nervous centres which preside over the regulation of the heat of the body, the nature of this alteration being obscure, but its effect being to disorder the regulation, so that it ceases to be for the normal degree of temperature, and comes to be for an abnormally high one. But it would seem to be no longer the question, whether this theory or that of the participation of the nervous system in the genesis of fever be correct, but rather whether the nervous system has anything at all to do with it. "It is no longer necessary", writes Dr. Sanderson, "to occupy space in discussing the theory, according to which there is a direct physiological relation between the nervous system and the chemical processes on which the production of heat depends; for it has now been shown experimentally by Dr. Murri that, after severance of the cord, the temperature rises even when the thermogenetic function may undergo a great diminution". Dr. Sanderson does not give any details, nor does he say that he has himself experimentally verified Dr. Murri's observations; but I think that, while wishing to pay all honour to the Italian physician, we may take the liberty of asking for some confirmation at the hands of a German, French, or English authority, before conclusions deliberately arrived at by many eminent men are thus summarily set aside. In an abstract of Dr. Murri's publication, I find it stated that he first divided the cervical cord of dogs (which produced a cooling), and then injected foetid pus under their skin, whereupon their temperature rose quite as much as after injection of the same in animals not operated on. Surely this would be something like an *experimentum crucis*; and would be additionally interesting from the fact, repeatedly ascertained, that, in cold-blooded animals, fever cannot be produced. In certain small mammalia, Dr. Sanderson seems to imply, the effect, as regards thermogenesis, of increased tissue-change in fever is balanced, or even more than balanced, by that of arrested food-supply; if that be so, it may be the explanation here also; otherwise, upon the view of this pathologist, that fever originates in the living tissues, and is from first to last a disorder of protoplasm, it is difficult to understand why in the frog, which of course has protoplasm in its tissues, and is quite able to produce heat, nothing, in however low a degree, answering thermally to fever in the higher animals, can be artificially produced. How successfully the process of inflammation has been studied in the batrachian's tissues, we are all aware.

And now, gentlemen, I must bring to a close this very imperfect sketch of the progress of fever-pathology. It was part of my original intention to include some notice of the causes, that is, remote causes, and of the effects of fever; but it soon became evident that, in doing so, I should exceed my limits. In respect of the latter, the effects of fever, I rather regret the omission; for very considerable advances in our knowledge have been made here; we stand on securer ground, and the subject is of the highest practical importance.

To sum up, then: we have seen that the characteristic elevation of temperature of the body in fever is mainly due to increased production of heat; that, besides the increased production of heat, there is a disorder of nutrition, an abnormal disintegration of the tissues of the body, and particularly of the muscular tissue, evinced on the one hand by increased excretion of urea and of potash salts, of carbonic acid, and perhaps also of water, and on the other by progressive loss of body-weight; that the increased production of heat, occurring at a time when a principal source of normal heat-production—viz., the food ingested—is all but completely cut off, must have its chief origin in the abnormal disintegration of tissue; but that the converse may also hold good to a greater or less extent, there being thus action and reaction. That, however probable may be the hypothesis of the intervention of the nervous system, the connecting link between the entrance into the

organism of the fever-excitant, the pyrogenic matter (be this *contagium vivum* or what it may), and the onset of the characteristic phenomena, has not yet been demonstrated; in other words, the proximate cause of fever remains undetermined. Several minor *lacunae* and unsettled questions we have had occasion to notice.

The work we have passed in review, as before observed, is almost entirely that of the last five-and-twenty years. Can we doubt, if we may venture upon a forecast of the future, that the final quarter we have just entered of the enlightened nineteenth century will witness a vast accession to this work, if not its completion? Can we doubt that voids will be filled up, that dubious or weak points will be corrected or confirmed, that difficult problems will be solved, that obscurities will be cleared away, and that the present *disjecta membra* will be united and consolidated into a true theory of fever? Can we doubt this, when, to say nothing of the advanced cultivation of science abroad, its claims have now come to the front at home, and original research is being stimulated and encouraged (if we except the measure before Parliament) to an extent utterly unknown at any previous period? Can we doubt it, when we look upon the beautiful pieces of mechanism, the elaborate appliances and apparatus, now in the hands of the biologist, and designed for the service of his laboratory, which are being exhibited at South Kensington? Can we doubt it, when the fraternity of science is no longer limited to local societies, the gathering of a few kindred spirits in the place of their abode, or to provincial associations, such as our own body at one time was, or even to national associations, such as our own body is now proud to be, but has come to extend to international congresses; men of science of different race and language speeding along the iron-way from capital to capital, or traversing the wide ocean, in order to confer on the subject of their common pursuit and to aid its advancement? Can we doubt it, when the ancient prophecy of Inspiration would seem to be literally receiving its fulfilment: "Many shall run to and fro, and knowledge shall be increased".

ON THE PAST AND PRESENT TREATMENT OF INSANITY.

Being the President's Address, delivered at the Annual Meeting of the Birmingham and Midland Counties Branch.

By G. F. BODINGTON, M.D., M.R.C.P., President of the Branch.

THERE has long been, as I need hardly remind you, a contention as to the necessity or non-necessity for specialism and specialists. Now I am not about to inflict upon you a dissertation on this somewhat trite question. But I desire to draw your attention for a moment to the incontrovertible fact that specialities do actually exist, and that, whether we will or not, we have to encounter them, to deal with them, and to be influenced by them. The field of knowledge is so vast that no one mind is encyclopædic enough to compass it—nay, no human being is able in the short space of a life time to "try the heights and fathom the depths", even of a single science. Hence it happens that in the pursuit of learning, just as in the mechanical arts, we have division of labour. Division of labour tends to perfection of knowledge. To become an adept, one must concentrate effort on a single subject. This principle is as applicable in medicine as elsewhere, but in medicine it has, so to speak, run to seed in sundry directions. We have doubtless conditions and circumstances to which exception may be fairly taken in reference to some of the developments of specialism. Still, however justly it may, for many reasons, be decried, there yet remains in it, I cannot help thinking, a kernel of good. The division of labour upon which it is based, and the concentration of thought which it entails are advantages not to be lightly thrown aside. "These specialists", says Oliver Wendell Holmes, in his *Poet at the Breakfast Table*, "are the coral insects that build up a reef. By and by it will be an island, and for aught we know may grow into a continent".

Now it happens that I myself am engaged in a branch of practice more open than any other division of the field of medicine to the charge of specialism. Whether this particular division of the medical art is rightly or wrongly so decidedly separated, I do not now attempt to decide. For the present I leave the question open. But being an arch-specialist, not from choice, but necessity, it becomes me, I think, in addressing this meeting to notice the fact, and to offer a

word or two of apology or explanation in introducing a very special subject.

Gentlemen, I propose to lay before you to-day some considerations upon the past and present treatment of insanity, and the method of dealing with insane patients. I select this as the topic of my address because now for nine years I have been engaged in the management of an asylum, and for a much longer period have been more or less familiar with some of the phases of lunacy. I do not pretend to be able to deal with the subject as it deserves, nor to expound it according to your merits; yet, bearing in mind the considerations I have just enunciated touching division of labour, bearing in mind what is comprehended or implied in the word experience, and having in wholesome reverence the maxim, *ne sutor ultra crepidam*, I feel it to be the subject which I am least incompetent to discuss, and therefore most justified in bringing before you, however imperfectly, on the present occasion. Should it seem to any of my hearers that a subject so special is likely to offer but little of interest for this auditory, I would remind you that insanity comes in the first instance under the care of the general practitioner, almost invariably. Further, I have to say, for your comfort, that it is my intention to avoid the technicalities of asylum routine, and as far as possible to make my observations bear more or less directly upon those phases of mental alienation, and those practical points in the care and management of the insane which are apt to come within the cognisance of the general practitioner or consulting physician in the ordinary course of daily work.

In the *Lancet* for May 20th of this year there appeared a remarkable article entitled the *Physiological Treatment of Insanity*. I agree with the sentiments expressed, but I call the article remarkable, because it is wonderful to my mind that at this day it should be necessary to call back attention to the propriety of treating insanity on a physiological basis, or that it should be requisite to point out the futility of dealing with it by other than physiological methods. Why, even Hippocrates recognised insanity as a bodily disease, and located it in the brain. *Τῇ δὲ αὐτῇ καὶ μαινομεναὶ καὶ παραφρονοῦμεν*. For by this same organ (that is, the brain) we are mad and become delirious, he says. He enunciated a pathology of madness, and laid down rules and maxims for its treatment, all referable to the view that it depends upon disordered physiological processes. Yet in the latter half of the nineteenth century, one of the leading organs of medical opinion in England writes these words: "Strangely, many able students and practitioners of medicine do not perceive the natural and indissoluble bond which holds these two departments together"—that is to say, the departments of psychology and physiology. Such is the language of the writer in the article to which I have referred, and I have little doubt it conveys an accurate impression, for many corroborative circumstances have come under my own notice in the course of my experience. Yet it is a stranger thing that it should be so, for, so far as I know, all our most trusted modern teachers inculcate physiological treatment.

Dr. Copland, to go back a generation, in his *Dictionary of Practical Medicine*, has an article curiously entitled "The Physiological Pathology of Insanity"; and in the Remarks more generally applicable to the Treatment of Insanity, therapeutical measures are advised which clearly indicate that treatment upon a physiological basis was fully recognised at the date of the publication of the *Dictionary*, 1844. Dr. Prichard, writing about ten years earlier, under the head of the *First Practical Indication in the Medical Treatment of Insanity*, speaks in these terms: "The first indication is to remove or lessen that diseased condition of the brain on which we have reason to believe that insanity depends as its immediate cause"; and again, under the head of the *Second Practical Indication in the Medical Treatment of Insanity*, he says, "The second indication for the medical treatment of insanity is to restore and maintain, as far as can be done, a healthy condition of the physical or natural functions, and to obviate or remove disorders in other parts of the system which may be connected or coincident with the diseased condition of the brain". What teaching can be more physiological than this? Esquirol, too, writing in the same decade, when discussing the moral treatment of insanity, insists that moral means are agents which exercise a direct influence upon the brain; not on the mind, be it observed, but on the brain. In laying down the principles of physical treatment, he points out that we must not fail to use hygienic and pharmaceutical means which will operate upon the brain, or upon those parts of the organism in which the primitive irritation arises. In short, his method of treatment is thoroughly physiological. Coming to our own time, we find amongst writers the tendency to found treatment upon physiology rather than upon psychology increasing day by day. The correlation of mind and brain is day by day more fully accepted, I may say demonstrated, and mental pathologists and therapeutists follow in the train of the mental physiologists. Let me quote one or two writers in proof of this.