The gums were lanced, but on the fourth day it was heavy and dull, and on the fifth it died. No *post mortem* examination was made.

I have met with the following case of extreme rarity, which, if my idea of the pathology of this central opacity is correct, will illustrate the disease, as well as one point in the early development of the eye which I have brought forward, viz., the existence of the *membrana pupillaris* before and independently of the iris.

I have before mentioned that the choroid is early formed, that the iris and the ciliary body are the results of a fresh effort (as it is called) of the "*misus formativus*"; and we know that in certain rare instances the iris is entirely wanting, although it is probable that the *membrana pupillaris* exists. The case which I am going briefly to narrate is one of congenital absence of the iris (or *irideremia*), where there was, by coincidence, also a central opacity of the capsule.

CASE XII. A child was admitted at the Eye Dispensary about ten years ago with complete absence of the iris. It was six months old, and was brought because it was supposed not to see. The posterior chambers were fully exposed, and presented the red reflecting glare round the edge of the lens, usually seen in these cases; in the left eye there was a central spot of opacity upon the anterior capsule, which gave a very peculiar appearance to the eye. The mother was healthy. The child disliked the light, and shrank away from it. I saw it again after a considerable time, and found that it had become a little more used to the light, but otherwise its state was the same.

The following question will most probably occur to my hearers: If inflammation of the *membrana pupillaris* and its adhesion to the capsule are the cause of the central cataract, and if the inflammation of these parts is merely an extension of disease from the superficial membranes of the eye, why do we not more frequently see central cataract, when we so frequently see cases of acute purulent ophthalmia in newly born infants? The proportion of cases of acute purulent ophthalmia in infants to the entire number admitted to an Eye Dispensary or Hospital is probably about one in sixty; whilst the proportion of instances of central cataract is about one in several thousand cases. In reconciling this great disparity, we must remember that for the production of such a result as this deposit, there must be a coincidence of at least two distinct conditions. The inflammation must be unusually severe even for these cases, and it must have attacked the eye soon after birth, with a tendency to spread to the internal parts; and, secondly, the *membrana pupillaris* must be entire, and some vascular connections with the iris must remain. Now, when we combine the probable rareness of this latter condition with the chances which this individual has of suffering from purulent ophthalmia, we shall be able to understand the cause of the very great comparative rareness of the central capsular cataract.

Gulstonian Lectures:

ON

DIABETES AND SACCHARINE CONDITIONS OF THE URINE.

DELIVERED AT THE ROYAL COLLEGE OF PHYSICIANS,
FEBRUARY 25TH, 27TH, AND MARCH 4TH, 1857.

By ALFRED B. GARROD, M.D., Fellow of the Royal College of Physicians; Professor of *Materia Medica*, Therapeutics, and Clinical Medicine, at University College; and Physician to the Hospital.

LECTURE II (*concluded*).

WE will next endeavour to throw some light upon this obscure disease, and discover what is its real and intimate nature; in this pursuit we shall find that many and diverse opinions have from time to time been held, most of them having facts, obtained either from morbid anatomy or the symptoms presented during life, in their support. In passing the most important of these hypotheses in review, we shall have to detail the principal morbid appearances which have been found after death, and shall thus include the morbid anatomy of this disease.

The first, and perhaps the most ancient theory, refers diabetes primarily to a disease of the kidneys, either functional or organic. Such was the opinion of the ancient Greek writers and

of Galen, and within the last half-century it has been supported by Watt, Cullen, Mason Good, and many other high authorities. It is an hypothesis which must naturally occur to the mind, seeing that the most important symptom of the disease has direct reference to the renal organs; morbid anatomy also appears at first sight to favour this view, for the kidneys are generally found much hypertrophied, the vessels enlarged and often very congested. In some cases of diabetes where I have had an opportunity of testing this point, I have also found such a condition. At times, however, especially when the disease has not been of long standing, the kidneys have presented a normal appearance, as seen by Andral and Christison; and in certain instances, they have been even contracted. In four cases in which the kidneys were weighed, I obtained the following results.

	Right kidney.	Left kidney.
D. H., a very attenuated phthisical patient	4½ oz.	5 oz.
E. H., male	4½	4 oz. 6 drs.
H. B., female	7½	8
J. B., male	7	7½

The advocates of this view of the pathology of the disease have assumed that sugar is not present in diabetic blood, and suppose that in saccharine diabetes, the kidneys, being in a morbid state, not only secrete an increased quantity of urine, but also possess the power of *forming sugar* from the blood. Dr. Elliotson, who was inclined to favour this hypothesis, makes the following remark in his lectures: "The absence of sugar in the blood, and the very frequent absence of dyspepsia, or anything connected with the stomach, except the hunger (which the excessive loss will explain), makes it appear most probable, that the disease is situated in the kidney."

At the present day, it is scarcely worth pursuing this hypothesis further; for, as I shall show you, diabetic blood taken from either the arterial or the venous system is always rich in sugar, and the abnormal presence of this principle cannot be explained by the irritation or altered action in the kidney itself. Again, although the kidneys have for the most part been found altered in structure, yet increased function would be quite sufficient to account for this change; and, that it is so produced, is rendered probable by the fact before alluded to, that in recent cases, no enlargement or morbid appearances have been observed. Lastly, we find instances in which the kidneys have for a very long period of time secreted enormous quantities of urine, and yet no trace of sugar has been found in it; and other instances, when a strongly saccharine condition of the urine exists without any notable diuresis.

An enlargement or simple hypertrophy of one kidney may be artificially produced in the lower animals by the extraction of the other; an increase of work is accompanied by an increased volume; it has also been found that the frequent injection of saccharine fluids into the veins of an animal sometimes induces an inflammatory condition of the kidneys.

By very many physicians, both of the present and of former times, the origin of this disease has been located in the stomach; and the important names which are found arrayed in support of this view, show the favour in which it has been held.

The arguments and facts were very forcible, and until recently, almost irresistible. In many cases, the stomach has been found after death in a morbid condition, often preternaturally large, with considerable thickening of the coats, a vascular state of the mucous membrane, and other signs of irritation. When no very distinct evidence of disease could be brought forward, it was explained by supposing that the error had been functional only, of which the ravenous appetite and dyspeptic symptoms have been regarded as proof. The experiments made some twenty years since by Mr. McGregor have been likewise usually adduced by the advocates of this theory. This gentleman fully confirmed the discovery of the existence of sugar in the serum of the blood of diabetic patients, which had been previously announced by Ambrosiani and Maitland; and he afterwards proceeded to further researches which have usually been considered as favouring the doctrine of diabetes arising from an error of the primary assimilating process: whether they do so in reality, we shall now proceed to inquire.

Mr. McGregor, by the use of emetics, procured the partly digested matters from the stomach of a diabetic patient, and found that fermentation by yeast could be readily induced in them; the same, however, took place, though in a less degree, when a healthy subject was substituted. He next fed the two individuals for three days upon roast beef and water, having on the previous day administered to each an emetic and purgative, for the purpose of completely clearing out the alimentary canal;

three or four hours after the next meal, the contents of their stomach were procured by an emetic of sulphate of zinc, and after due preparation, submitted to the action of yeast; those from the healthy man gave no evidence of sugar, whereas in the vomit from the diabetic patient distinct fermentation was obtained. Mr. M'Gregor also obtained sugar from the alvine discharges of diabetic subjects. This discovery of sugar in the alimentary canal, when a purely animal diet had been taken, and therefore quite free from amylaceous or saccharine matters, has been held, as a proof, that in these cases the stomach possesses an abnormal power of forming sugar from nitrogenised principles: but I apprehend that we need not have recourse to this explanation; for if the blood contains sugar in any abnormal proportion, as has been fully proved to be the case in diabetes, the secretions from this fluid may also be rich in this principle, and among others, the gastric and intestinal juices; Mr. M'Gregor himself discovered it in the saliva.

Sugar is found in the serum derived from blisters applied to diabetic patients, and I have likewise discovered it in some blebs similar to those of pompholyx, which occurred in one of my patients labouring under saccharine disease. There appears to be some doubt as to its existence in the perspiration of such subjects; though Dr. Willis, in his work on urinary diseases, states that he has known the furfuraceous cuticular exfoliation of the legs to be distinctly sweet to the taste.

M. Bouchardat supposes that in the stomach of diabetic patients a peculiar secretion takes place; that a species of ferment or diastase is present, which possesses the power of converting starch into sugar; this, however, has been found to be a normal change in the duodenum, and induced probably chiefly by the action of the pancreatic fluid.

Dr. Macintyre was of opinion that the stomach is primarily in fault, and adduced, in support of his views, certain cases in which perforation of the coats of this viscus has been found after death, an effect he attributes to the altered character of the gastric fluid; but we know that this corrosion may happen in many other diseases; whether it is more common in diabetes has not been determined; but considering the great activity of the digestive functions which then so often occurs, it is not at all improbable.

Many have regarded diabetes as arising from some altered condition of the blood, from some error in what has been rather obscurely named the secondary assimilating process, or in those changes which matters introduced into this fluid undergo before they become a portion of the solid frame or are eliminated from the system.

Dr. Willis was the first who advocated this doctrine; and the following passage gives a summary of his views on the pathology of this affection. He states, that "diabetes is rather an immediate affection of the blood than of the kidneys, and thence derives its origin; for the mass of the blood becomes, so to speak, melted down, and is too copiously dissolved into a state of serosity; which is sufficiently manifest from the prodigious increase in the quantity of urine, which cannot arise from any other cause than from this solution and waste of blood." Many have been the supporters of such an opinion, among whom may be mentioned the name of the great Sydenham; but, at the present day, more definite explanation than that of a mere abnormal waste of the nutrient fluid is required to satisfy pathologists. It is an explanation, also, so general in its character, that it may be applied to various other diseases besides the one under consideration. The late Dr. Prout, who perhaps had more diabetic patients under his care than any physician before or since his time, and who did so much both in advancing the knowledge of organic chemistry, and in applying this knowledge to the study of medicine, held opinions as to the pathology of this affection which, however profound and plausible at the time they were promulgated, cannot, I think, be now regarded as correct. Dr. Prout considered—"1. That the assimilation of the saccharine principle is a distinct function; 2. That the cellular tissue represents the saccharine aliment; 3. That a secondary assimilating process, analogous to that in the digestive organs, is constantly going on in all parts of the body; and lastly, that an important function of the kidneys is of a disorganising character—that is, consists in the reduction of organised into crystallisable compounds." And it was his opinion that "the proximate cause of diabetes lies partly in the assimilating organs, and partly in the kidneys; and may be thus stated. When crystallised sugar is taken into the stomach by a diabetic individual, it is reduced to a low state, remains more or less unassimilated, and passes through the system to the kidneys, by which organs (being already crystallisable) it is separated unchanged. When organised

saccharine principles, as farinaceous matters, etc., are taken into the diabetic stomach, they are, in the first place, reduced to the form of low sugar, part of which is assimilated as in the healthy stomach, while another portion is modified or remains unassimilated. The portion that is assimilated is applied to the purposes of the economy: the portions modified and unassimilated pass together through the system to the kidneys, by which glands the portion modified is disorganised, and finally appears in the urine as crystallisable sugar, along with the portion originally remaining unassimilated in the stomach. The same remarks are applicable to gelatinous, and, in extreme cases, perhaps, to albuminous and oleaginous aliments. The secondary assimilating processes in diabetic individuals participate in the derangements of the primary processes just detailed; that is to say, the gelatinous tissues are either reduced to sugar, and thus not assimilated at all, or they are imperfectly assimilated, or they are malassimilated; in all which conditions the saccharine principle derived from the gelatinous and other tissues may be supposed to pass through the system to the kidneys, by which organs, like similar matters brought from the stomach, the various modifications of the saccharine principle are further disorganised, and converted into crystallisable sugar."

On examining this explanation of the cause of the disease, we shall discover that there are many points of which no demonstration can be afforded. Modern chemistry does not recognise a distinction between the different forms of sugars spoken of by Dr. Prout, nor has any proof been given of the assimilation of sugar by the animal economy, in the sense just alluded to; that is, of its being ever built up into animal tissue. From what we now know of the changes occurring in the animal economy, it would appear that they are mostly of a disintegrating nature; that, on the other hand, it is to the vegetable organisation that we owe the construction of complex principles from the simpler combinations; and, lastly, it would be scarcely worth while to go into much detail in refuting this view, as important recent researches have indisputably proved that sugar is produced in the system by a special formative organ.

Others have imagined that diabetes is induced, not from abnormal formation of sugar in the animal economy, but from a loss of destructive power over this substance occurring in the system; that is to say, the sugar, either taken as such into the stomach, or formed from amylaceous matters, does not undergo the ulterior changes as in health; and hence the blood becomes impregnated with this principle, which is afterwards eliminated by the kidneys. Dr. Bence Jones remarks, when speaking of amylaceous digestion, that "the changes which occur in health may be represented by the following series—starch, dextrine, sugar, vegetable acid, carbonic acid;" and that diabetes may be represented by supposing the changes stopped short at sugar; and that, from some cause, the conversion of the sugar into the vegetable acid and carbonic acid does not take place. Dr. Jones, at the same time, makes the same objections to this theory, to which we shall presently allude.

M. Mialhe of Paris, assuming the theory just propounded to be correct, has endeavoured to proceed a step further, and to explain why such an arrest of change takes place—why sugar, which is evidently formed in the healthy body, ceases to undergo the next series of changes. His theory is as follows: that, in health, the blood is alkaline; and that this condition is necessary, in order that the sugar constantly introduced into it may undergo the proper changes, and be resolved into carbonic acid and water; that a diabetic condition is induced whenever this alkalinity is diminished to such a point that the transformations just spoken of are no longer possible. This condition of the system, he believes, may be induced by various causes, as suppression of the perspiration, which is naturally acid; the too free use of acid drinks, and of acid fruits; the exclusive use of an azotised diet, containing much sulphur and phosphorus, giving rise to a large quantity of sulphuric and phosphoric acids, and thus neutralising the alkaline bases of the blood.

There are many points about M. Mialhe's hypothesis which are at first satisfactory to the mind. It explains readily what really appears to be a frequent cause of diabetes, namely, the occurrence of suppressed perspiration, and the taking of large quantities of beer and acid fluids; but, when closely examined, the theory will, I believe, be found very defective; for no proof has as yet been given that diabetic blood is less alkaline than the same fluid in health: and this would certainly be the case in a marked degree if the non-destruction of sugar were due to this cause; and, again, therapeutic treatment founded on this basis has certainly, in most instances, signally failed. How happy would it be for the diabetic community

vere M. Mialhe's doctrines correct! How readily could a sovereign remedy be found for their malady! In our next lecture on the therapeutics of our subject, I shall have occasion to bring forward evidence of the real value of the proposed alkaline treatment.

I may remark, before quitting this part of the subject, that, in the system of diabetic patients, other oxidisable substances, where the products of the decomposition are readily discovered, appear to undergo decomposition: for example, acetate and tartrate of potash are resolved into carbonate of the base; and salicine, when administered, can be discovered in the urine as saliculous acid—an effect due to oxidation. For this last fact, I am indebted to Dr. Ranke.

Within the last few years, most important facts have been brought forward, throwing considerable light upon the nature of the glycogenic function of the animal body—facts which must necessarily tend, ere long, to give us more correct views of the nature of diabetes. It is chiefly to M. Claude Bernard that we owe these important additions to our knowledge. It had generally been contended, in spite of many and trustworthy statements to the contrary, that the whole of the sugar found in the animal body, both in health and disease, was derivable from saccharine or amylaceous principles; and that no sugar could be produced from albuminous, gelatinous, or other nitrogenised matters. This, however, has now been demonstrated to be erroneous. M. Bernard has shown that all animals, including man, possess the power of forming sugar; that this function is located in the *liver*; and that, as the other organs of the body are impregnated with the products of their secretions, so the liver of the healthy animal always contains this principle, which can be easily extracted therefrom. He has likewise shown that, in health, it is *only* from this organ that it can be obtained. To prove this, we have merely to take a portion of hepatic tissue in a fresh state; bruise it thoroughly in water; boil, in order to coagulate the albuminous matters, and afterwards filter. The liquid portion gives unequivocal evidence of the existence of sugar, by the potash and copper tests before spoken of; and, when fermented with yeast, yields carbonic acid and alcohol. From a portion of a bullock's liver weighing 82,150 grains, M. Bernard obtained, in one experiment, a quantity of sugar corresponding to 651 grains in the entire organ, or 1.79-100 of the weight of the liver. He has also examined the livers of several men who had been executed, or had died suddenly from accident; and in all cases found this organ rich in sugar, the percentage varying from 1.10 to 2.14. The investigations on this important subject have been extended over almost every class of animals; and sugar has been found in the livers of members of the articulatæ, mollusca, crustacea, fishes (cartilaginous and osseous), chelonian, saurian, ophidian, and batrachian reptiles; in various birds, and in a great number of mammiferous animals. Some differences are discovered in the relative amounts of sugar yielded by the livers of different animals, and in the cold blooded it is less than in birds and mammalia. These experiments have now been repeated by many observers, and it appears placed beyond doubt that the livers of animals killed when in a healthy state are rich in saccharine matter: at the same time, the other organs are destitute of this principle. It will be observed that the food of the animals whose livers have been subjected to this analysis is of very varied character; in some, the diet is purely vegetable; in others, entirely animal; and, in a third class, of mixed nature; yet sugar has been found alike in all, and in an equally large amount in carnivora and in herbivora. To prove that the sugar is not obtained from any saccharine or amylaceous matters in the food, but is simply derived from nitrogenised principles, M. Bernard has fed dogs for six and eight months exclusively with meat, and still found the same proportion in the liver. The experiment has likewise been repeated on owls fed for three months on bullock's heart, and with the same result. To demonstrate even more conclusively that the sugar is formed in the liver, M. Bernard has made the following experiment. He has kept a dog without food for thirty-six hours, and afterwards fed it copiously with cooked sheep's head. After three hours, when in full digestion, the animal has been killed by pithing, and the blood collected both from the *vena portæ* and also from the hepatic veins; that from the *vena portæ*, or before entering the liver, has given no trace of sugar by any of the tests; but the blood from the hepatic veins gave an abundant precipitate with the copper test, and also fermented on the addition of yeast. To make the experiment, if possible, still more conclusive, the meat with which the animal had been fed was examined, and proved not to yield a trace of sugar.

M. Bernard has proceeded a step further, and has shown

that, within the liver itself, after removal from the body, sugar is capable of being formed. To demonstrate this, he has carefully removed the organ from a full grown, vigorous, and healthy dog, which had been killed about seven hours after a full repast of tripe, by pithing the medulla oblongata; he then, before the blood had coagulated, passed a continuous stream of water through the liver, by attaching a gutta percha tube to the *vena portæ*, and connecting it with a tap. After the blood had been washed out, and the water escaping from the hepatic veins colourless and free from albumen, the washing was continued for a short time. It was found that the strongly coloured water which flowed from the hepatic veins at the commencement of the experiment was saccharine, but that, when the colour disappeared, sugar could no longer be discovered in the fluid. The tissue of the liver was afterwards examined by the method I have before mentioned, and a complete absence of sugar proved by the negative results given by the copper and fermentation tests. However, on leaving the liver for twenty-four hours, distinct evidence of this principle was obtained. The sugar thus found could only have resulted from its formation within the twenty-four hours from some of the insoluble matters left in the organ after the first washing. This experiment, therefore, proves, if proof be wanting, that sugar can result from albuminous or other principles present in the liver. M. Bernard has found that, after a short time, no further saccharine formation goes on, but the ordinary decomposition ensues. It has been ascertained that a high temperature completely checks this formation of sugar. If, for example, a washed liver be divided into two equal portions, and one half exposed to the air, without any preparation, but the other half boiled, and afterwards exposed for a few hours, sugar will be found in the first half, but no trace in the second. It appears, from these researches, that one most important function of the liver is to secrete sugar from portal blood, and this *irrespective* of the nature of the food, and for a time even altogether independent of it. This function of the liver appears to be totally distinct from its bile-secreting office, and, as has been observed in some of the lower animals, not to be isochronous with it. The knowledge of the glycogenic function of the liver enables us to explain much which was before obscure in the physiology of respiration: how, for example, one and the same material—namely, sugar—destined for the supply of this process, is provided for in animals of every species, and with every variety of aliment. This discovery has also taught physiologists that there may be internal as well as external secretions; that is, secretions where the products, in place of being eliminated from the body, are thrown directly into the circulating fluid; the liver throwing *bile outwards* (a fluid containing no saccharine matter), and *sugar into* the blood itself. May not this fact give some clue to the function of certain organs, as the spleen, suprarenal capsules, thymus, etc.—glands destitute of excreting ducts? In order that the liver should secrete sugar, certain conditions are necessary; remove these, and the glycogenic function ceases. For example, if the pneumogastric nerves are cut in a rabbit or any other animal, and life is preserved for some hours, no sugar is afterwards found in the liver, because the glycogenic function of the liver ceases when the nervous influence is cut off, and that which previously existed in the organ is removed and destroyed by the changes constantly in operation. This even happens when the animal is freely digesting amylaceous matters at the time of the operation. Our knowledge of this fact enables us to explain why, in acute and severe diseases, the urine of a diabetic patient often becomes less saccharine, or even free from sugar; and why, also, the principle is not ordinarily found in the liver, except death has occurred suddenly, even when the patient was previously diabetic.

M. Bernard was led, by a train of reasoning which has since proved to have been erroneous, to discover a remarkable fact, one which has excited much attention among physiologists: I allude to the artificial production of saccharine urine by the irritation of the floor of the fourth ventricle. The experiment is thus performed. A healthy animal, either a rabbit or dog, is chosen, and the urine shown to be free from sugar by the different tests for this substance; a small instrument is then introduced through the walls of the posterior part of the cranium, in such a manner that the floor of the fourth ventricle can be reached; and this portion of the nervous structure is irritated by means of a fine point, and in such a manner as only to injure the upper stratum of fibres of the medulla oblongata, which constitute the floor of this ventricle. After a short time, usually from half an hour to an hour, if the operation has been successfully performed, the urine becomes sac-

charine to such an extent that sugar can be readily discovered by all the tests. This abnormal condition remains for several hours, sometimes for a whole day; at the same time, the quantity of the renal secretion is not unfrequently augmented. It was at first thought that the appearance of sugar in the urine in such experiments was due to the irritation being directly propagated by the pneumogastric nerves to the liver, but this has since been found not to be so; and these nerves, in respect to the glycogenic function, act simply as incident nerves; and, when divided in the neck, saccharine urine can be produced, not by irritation of their peripheral extremities, but by causing the influence of the irritation to be propagated to the medulla from their central extremities, and thus to the liver through the medium of the spinal cord and splanchnic nerves.

Before quitting the subject of M. Bernard's important investigations, it is necessary to state, as pertaining most closely to our subject, that no greater amount of sugar was found in the liver of animals fed on amylaceous substances, than when an exclusively meat diet was employed. He deduces from this, that no more sugar then enters the hepatic blood. I do not think this is necessarily a true deduction. We must remember that the animals upon which the experiments were made were in a state of health; in disease, the results might be different. M. Bernard thinks that the amylaceous and saccharine matters are converted by the liver into oleaginous substances, and in support of this opinion brings forward many facts.

Reviews and Notices.

THE JOURNAL OF PSYCHOLOGICAL MEDICINE AND MENTAL PATHOLOGY. Edited by FORBES WINSLOW, M.D., D.C.L. April: 1857.

THIS is a capital number of this interesting quarterly. The great work of M. Morel, on the *Degeneracy of the Human Race*, is reviewed at length by the editor, in an article which gives an admirable view of the author's ideas on this very important subject. We think ourselves that M. Morel cannot substantiate his position that there are now more numerous agents which act as degenerators of the race than at any previous period: in fact, we think that an extended view of the subject points in the opposite direction. Nevertheless, his work furnishes an immense amount of matter which will startle the psychologist.

Dr. Webster's papers on Foreign Pauper Lunatic Asylums have culminated this quarter in a most interesting account of the great Pauper Lunatic Colony of Gheel. It appears to us that the publication of this paper will do much toward silently revolutionising our present system of detaining pauper lunatics in large asylums, conducted on principles analogous to those only to be found in our workhouses and prisons. At Gheel, 800 pauper lunatics are distributed among the 8,000 inhabitants of this district of the Low Countries—not in a few large buildings, but dispersed in twos and threes among the different households. The domestic system of treatment is here carried out to its full extent. The lunatic, instead of forming one of a herd shut up in wards and confined in dismal airing courts, partakes freely of the *family life* of the people; restraint in the form of walls, and locks, and bolts, and a separation of the sexes, are unknown: nevertheless, the average number of cures is higher than in the expensively built asylums of England. Every medical man interested in the treatment of the insane should make a point of reading this most interesting paper.

A very instructive Report on American Institutions for the Insane is extracted from the *American Journal of the Medical Sciences*. The other articles which make up this journal are, the Physiology and Psychological Phenomena of Dreams and Apparitions; the Psychology of Spinoza; on Civilization and Insanity, by Dr. F. Parigot, late Inspecting Physician to Gheel—an admirable paper; Physiological Psychology; on Marriages of Consanguinity; Pathology of Insanity. We have not seen so good a number of this quarterly as the present one for a long time.

ILLUSTRATIONS OF THE PATHOLOGY OF CANCER. By JOHN ZACHARIAH LAURENCE, F.R.C.S., Surgeon to the Northern Dispensary. pp. 59. London: T. Richards. 1856.

THIS little work is a reprint of some papers which originally appeared last year in the *ASSOCIATION MEDICAL JOURNAL*. Of the able manner in which the author has discharged the difficult task undertaken by him, our readers have already had ample opportunity of judging. It therefore only remains for us to notice the fact of the publication of Mr. LAURENCE's papers in their present form, and to recommend the book as a highly valuable contribution towards the elucidation of that obscure and difficult subject—the pathology of cancer. Great praise and hearty thanks are due to Mr. Laurence for his labours.

British Medical Journal.

SATURDAY, APRIL 25TH, 1857.

THE RECENT TRIAL OF ALFRED MATCHAM FOR MANSLAUGHTER.

IN our number for last week, we commented at some length upon a few points arising out of the trial of Alfred Matcham for manslaughter, reserving others for future consideration. In the mean time, however, we have had access to sources of information not previously available, and have thus been enabled to correct some of our conclusions, and to lay before the public an amplified and more accurate version of some matters which have hitherto been imperfectly detailed.

The facts of this case resolve themselves into three groups: those which relate to the medical history of the case; those connected with the trial; and those which are known as to the career and character of the man.

The medical history of the case, owing to the want of requisite knowledge of such matters on the part of the counsel for the prosecution, was very meagrely elicited at the trial. In brief, it amounts to this. A young and well formed woman, who had borne four children, again became pregnant. At the fifth month, about the middle of last June, some indisposition came on, which, in the defendant's voluntary statement, was represented as inflammation of the kidneys, of such a character that her recovery was thought impossible. Now, it must be remarked, that this assertion was only brought forward at the inquest after the result of the *post mortem* examination was known to Matcham, and that at the trial the husband had no recollection of any such alarming illness.

It so happens, however, that we have before us, in the shape of a bill delivered to the husband, for the attendance and medicines which were found necessary for what is called "the cure" of this nearly fatal disease, a refutation of the whole story sufficiently clear to satisfy any unprejudiced mind. This document is in the hand-writing of Matcham, and specifies two mixtures, a bottle of drops, a *lini*, pills, and a powder, as having been supplied on three days, the 17th, 19th, and 21st of June. No other item is charged for till the 17th of August, when the last act of the tragedy began. Can any further demonstration be wanted of the fabrication of this fiction, which so deluded the judge, and misled the jury?

At the seventh month, somewhat similar symptoms appear, and are interpreted as the commencement of labour.