Part Second.

REVIEWS.

Die Krankheiten der Milz. Eine Pathologisch-Therapeutische Abhandlung. Von Dr C. R. Heinrich. Leipzig: 1847.

The Diseases of the Spleen; a Therapeutico-Pathological Treatise. By Dr C. R. Heinrich. Leipsic: 1847. Pp. 450.

The obscurity in which the physiology of the spleen has been so long enveloped, extends also to its pathology. Little is known either of its inflammatory affections, or the changes of structure to which it is subject. No doubt, many isolated and valuable contributions to this department of pathology have appeared from time to time in various periodicals; but, so far as we are aware, notwithstanding the immense advances our science has made within the last five-and-twenty years, no separate treatise has, as yet, been directed to a consideration of the nature, causes, and treatment, of diseases of the spleen. This desideratum our author has undertaken to supply, and, as the path is somewhat new, he modestly informs us that the work is not to be regarded in the light "of a complete and highly polished fabric, but as a wall, a fragment, on whose foundations future workmen may extend their operations." The work is arranged in two divisions; the first being devoted to a consideration of diseases of the spleen in general; the second to an exposition of its more special maladies. As a natural introduction to these, four chapters are devoted to a history of the development of the spleen throughout the whole series of animals, to its position and structure in man, to its functions, and to a history of its diseases.

As the subject is interesting, and but little known, we shall endeavour to present our readers with such an abstract of the work as may not only prove useful, but prompt to further inquiry; and, in the mean time, confine ourselves to the introductory portion.

The spleen lies deep in the left hypochondrium, and, in the healthy state, its presence and limits can only be accurately ascertained by means of percussion. It must have attained some size ere it projects beyond the ribs, and is appreciable to the touch. In our examinations of this organ, therefore, our principal means of determining its size and position are the pleximeter. But it must not be forgotten that there is no organ in the body which exhibits so many changes, both of size and weight, as the spleen. And this is true of it not only in the diseased, but also in the healthy state. From the measurements of Krause, it appears that the length of the spleen is from five to five and a half inches, whilst its breadth is from three
to four. Schlemm, on the other hand, states its normal length to be from four to five inches. In the child, it is more than a half less than in the adult. The most accurate tables of its weight are those given by Dr J. Reid, in this Journal, some years ago.

Much light has, of late years, been thrown on the structure of the spleen by means of the microscope, and more especially in the works of Müller, Henle, Vogel, Bardeleben, and Evans. From the researches of these authors, it appears that the spleen is made up of the following parts:—first, of a covering membrane; second, of the usual apparatus of arteries, veins, lymphatics, and nerves; third, of a peculiar stroma, or fibrous tissue; fourth, of spleen vesicles, which contain a pulpy substance, and fill up the meshes of the stroma.

The coats of the spleen consist of two very different but closely connected membranes, an external and an internal, proper to the spleen. The former is a direct process from the internal surface of the peritoneum, and, like other serous membranes, is constantly moist and smooth. The latter, and peculiar coat, is a whitish, tolerably firm membrane, and generally regarded as fibrous; but, according to the researches of the author, it is composed of cellular and elastic tissue, and, in proof of this, its great elasticity may be cited. It is quite peculiar to the spleen, gives the organ its form, and serves as a support to the soft parenchyma. Whilst the external surface, with the exception of the hilus, is entirely enveloped in a peritoneal covering, the inner surface closely adheres to that of the spleen. The proper tissue is not penetrated by the vessels as they enter the hilus; but it accompanies and surrounds them, penetrates with them into the parenchyma, and forms, by the films it sends off, a firm basis—the stroma, or fibrous tissue of the spleen.

On examining the peculiar substance of the spleen, it is found to consist of a firm, rigid, fibrous-looking base, and of a red, pulpy mass, filling the spaces of the former. The best mode of examining this stroma, or base, our author informs us, is to macerate a fresh human spleen for some days in acidulated water, cut it into thin slices, free it well from blood and similar matters, by means of a blunt knife, and frequent washing in water and ammonia. On placing a portion thus prepared under an object glass, of even moderately magnifying powers, it appears like a bundle of parallel running fibres, which interlace and cross each other in such a way as to resemble the trabeculae cordis, having very much the appearance of muscular fibres. The soft, loose, and red pulp contained within the meshes of the stroma, and which becomes brighter on exposure to the air, may be best seen in a slice of fresh healthy spleen. It requires no further preparation than cleaning it of any blood corpuscles which may obstruct the view. On placing it under a magnifier of from 200 to 300, there will then be perceived a more or less close aggregate of generally round, rarely oval, corpuscles, of from 1-300 to 1-350 of a line long, and 1-400 to 1-500 of a line broad, p. 13. Vogel asserts that these are hol-
lowed out in a cup-like manner, and Henle that they contain no nucleus; with neither of these opinions can our author coincide, because, while some of them here and there appear to contain a dark point resembling a nucleus, yet, on moving them about, it is evident that it is merely a granule attached to the surface. Furthermore, each vesicle contains within its smooth transparent membrane an amorphous finely granulated substance, which, on the addition of acetic acid, is evidently recognised as something peculiar. These corpuscles either lie or float singly in the surrounding fluid, or they are grouped irregularly together like a cluster of grapes. These bodies, of such various form and size in one and the same organ, our author can only regard as cell formations in various degrees of development and transformation, which are peculiar to the spleen and its cognate sanguineous glands. He agrees with Evans in thinking, that the secretion of their contents is the peculiar function of the parenchyma of the spleen.

Each spleen vesicle has, besides its artery and vein, an accompanying lymphatic. When the stomach is empty these vessels are scarcely visible; they become distinctly perceptible in the bodies of those dying suddenly, and who have partaken of either liquid or solid ingesta a short time previously; they are then turgid, and appear like white points. They are also perceptible in certain pathological states, as after typhus, the exanthemata, &c. They are more readily seen in the ruminating animals than in man; and the more distinctly the points appear, the more do the vesicles, and indeed the entire parenchyma, of the organ become enlarged. In such cases the vesicle may attain the size of a line in diameter, lying like a round corpuscle in the pulp of the organ, from which it may be readily detached. On puncturing one of these swollen vesicles it collapses, and evacuates a fluid muddy substance, rich in corpuscles, which for size may be compared to the blood globules, though not so regular in their form, and colourless; subsequently they more resemble the chyle or lymph globules. The spleen vesicle has now become a Malpighian corpuscle. The vesicles or cells of the spleen, and the spleen corpuscles, so named after their discoverer Malpighi, are, according to our author, essentially the same cell formations, and identical, differing only according to the degree of their development and condition.

The researches of C. H. Schultz and Simon have thrown much light on the state of the blood in the portal system generally; but our present knowledge of the physiological condition of that contained within the veins of the spleen, is, our author conceives, still very defective. He believes it to be different from that contained in the other veins of the body. The researches on this subject have, as yet, been rather scanty, and somewhat contradictory. The following summary comprises all that is known on the subject. According to Soemmering, the blood contained within the veins of the spleen is of a deeper dirtier colour than that of other veins. It is
probable that its specific gravity, like that of the vena portae and vena cava, is also somewhat lighter. (Thackrah.)

Saunders, Tiedeman, and Gmelin believe that, as regards coagulability, there is no difference between splenic and other blood. Bardeleben concurs in this, and also states, that even under the microscope its blood corpuscles appeared to be the same as that of other blood. Soemmering and Heusinger, on the other hand, affirm that it coagulates more slowly, and that in the dead body the portal blood is always found in a fluid state. Thackrah, Heusinger, and Schultz agree in stating, that the clot of blood obtained both from the vena portae, and the splenic vein of horses and dogs, is always very soft, and very soon dissolves. Home and Heusinger have shown that the serum is large in proportion to the clot. Heusinger found that, in dogs killed immediately after being fed, the proportion of serum was greater than after a ten hours' fast. Rolof and Soemmering have stated in a general way, that the blood contained in the veins of the spleen is more watery than other blood. Heusinger found the serum of a red colour in dogs. In like manner, Thackrah and Schultz found the serum of the portal blood more or less reddened from dissolved colouring matter, and the former of a much higher specific gravity than that of other veins. According to Heusinger, the serum contains less albumen and gelatine. And, according to Thackrah, the serum of portal blood coagulates neither so completely nor so rapidly as that of other venous blood. According to Soemmering, the blood of the splenic veins contains a large quantity of alkaline salts. On this circumstance, Frerichs has lately founded an hypothesis, in which he supposes one part of the function of the spleen to be, to furnish to the liver by means of the veins a richly alkalized blood, and another, to give off by the lymphatics a poorly alkalized lymph. Lastly, Soemmering affirms, that this blood is poor in oil, and Heusinger states that he has never found fat globules in it. The contrary, however, was long ago affirmed by Dumas, and probably correctly, at least the portal blood has been found by Schultz and Simon, when compared with other blood, to be very rich in fat. It has also been observed microscopically by Simon. The want of a proper quantitative analysis of the portal blood, and more especially of that of the spleen, is still a sensible gap in our physiological knowledge. When furnished with that, it is to be hoped such a step will be gained, as to lead to clearer and more comprehensive views of the true nature of the spleen.

Having disposed of the structure of the spleen, our author next proceeds to consider its physiology. In treating of this subject, said John Bell many years ago, we must be indulged in some speculation; indeed, it is privileged ground; and truly the privilege has not been neglected. No organ of the body has given rise to more strange and wild physiological speculations than the spleen. By some it has been regarded as a mere useless appendage, filling
a vacant space in the body, or, at most, acting as a counterpoise to the weight of the liver; others, mounting a step higher, considered it the seat of the soul—the cause of the venereal appetite; while Sir Anthony Carlisle could give it no higher a function than that of a mere oven to heat the great end of the stomach, and thus assist in the process of fermentation. Leaving such absurdities, we pass to more rational views regarding it. Our author has no hesitation in placing the spleen in the chylopoetic system, and considers its structure so far determined by recent histological researches, as to permit no doubt of its being a lymphatic gland. Comparative anatomy likewise supports this view, as the spleen is wanting in that class of animals in whom no lymphatic system can be anatomically demonstrated; and further, its size bears proportion to the development of this system. We shall give the author's own words as to the part the spleen plays in the assimilating process. After describing the contents of the thoracic duct and their appearance, he proceeds:—"There is almost constantly mixed with the white contents of the thoracic duct a tolerable number of ready-prepared blood globules. Their number varies under different circumstances, but is very much the same under similar conditions of the organism. This fluid is of a much darker colour than that seen in other lymphatics; it is sometimes of a grey colour, or greyish red, sometimes of a rosy hue, and at others of a blood red. This red colour is most striking in the herbivora, increases considerably towards the end of the duct, and is still more marked when its contents are coagulated under the influence of atmospheric air, and when the serum begins to escape in consequence of the contraction of the clot. On inquiring into the cause of this colour, which has not yet been satisfactorily explained, it must evidently be referred to a double origin. A chief part of it is evidently owing to the access of the oxygen of air, a process which takes place most completely during respiration in the lungs; and thus, partly by a physical, partly by a chemical process, is completed the transformation of the tolerably advanced and matured lymph globule into the red blood corpuscle. This chief and concluding act of haematosis, must, as a necessary preparation, have been preceded by another of a purely chemical nature, by means of which the red colouring matter is separated, to constitute a covering to the contents of the compound chyle globules, made up of fat and albumen, and by means of which the fatty and watery contents of the portal blood undergo a considerable metamorphosis. The liver and the lungs are the organs in which takes place the chemical transformation of fat, which is thrown off by the former in the shape of bile, in the latter as oxidized carbon."—P. 23. It appears to him, as already suspected by Hewson, that it specially belongs to the spleen, though also to the mesenteric glands, to separate the blood pigment, and thus co-operate in regulating the watery contents of the new accession (aufnahme) of chyle. The influence of the spleen on the formation of pigment is, he thinks, further strengthened by
the fact that red-blooded animals only possess a spleen; and that, in animals in which the spleen has been extirpated, the gall-bladder has been found to contain only a small quantity of colourless bile.

The views of Oesterlen are quite opposed to those of all other authors. According to him, the spleen, like all other glands, is made up of two principal parts; first, a crowded heap of peculiar corpuscles, or so named cytoblasts; second, of fat cells. In connexion herewith he regards the action of the sanguineous glands, on the composition of the blood, as a chemical transformation of the fatty matter of the serum; the blood is in them partly freed of this fatty matter, but retains by the solution of the cytoblasts, which are always the last to be added to the fluids, a larger quantity of protein compounds. Our author holds this theory to be untenable, both on anatomical grounds, and also that in dogs and rabbits killed by frequent and large doses of olive oil, the lungs, liver, and kidneys, were regularly found to have undergone fatty degeneration; whilst in the parenchyma of the spleen, though subjected to microscopic observation, not a drop of oil could be discovered. Whence he argues, that were the spleen a fat-transforming organ like the liver, which undoubtedly throws off fat in the form of bile, or like the lungs, in which the oil undergoes the process of combustion, then deposits of fatty oil should be found in its cells as well as in those of these organs. The spleen he believes to be as important a preparatory organ for the liver, as the latter for the lungs. Many physiologists have indeed considered it as a bile-secreting organ, without, however, being able to give any satisfactory reason for their so doing. Heinrich lately put forth another view. Looking only at the relation of the spleen to the lungs, he regards the spleen as the lungs of the abdomen, a water lung. The true relation of this organ to the other blood-preparing organs, may be said to be a combination of the above two. Without the spleen there is no normal activity of the liver. Without the spleen no normal respiration. The course of the thoracic duct, from its origin in the superficial net of lymphatics in the spleen and the neighbouring mesenteric glands, to its opening into the venous circulation, is the peculiar workshop of the blood globules, which first become visible here in consequence of the union of the splenic lymph with the chyle globules. Tiedeman and Gmelin found no fibrine in the chyme, but discovered it in small quantity in the thoracic duct. Rees found the specific gravity of the fluid contained in this duct in man, to be not more than 10:24, whilst that of the serum of the blood in general, was at the least 10:52. Let the product of the abdominal lymphatic glands be ever so highly organized, yet haematosis is only fully completed in the necessary process of respiration. It is during the process of circulation through the lungs that the new blood first gets rid, by combustion, of its superfluous quantity of fat, whereby its normal quantity of fibrine, which is a higher organisation of albumen, is retained, and the

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blood corpuscles are first observed in their proper form and number. The author does not enter upon the morphological part of the inquiry regarding the development of blood corpuscles; but expresses his surprise that, while modern physiology has made this the subject of its special attention, the connexion of the spleen therewith has been most strikingly neglected. He then refers to the researches of Kölliker, and quotes the following as his results regarding the mode in which the blood corpuscles are developed in the full grown mammalia.

First, The blood corpuscles are formed from the colourless cells of the lymph and chyle.

Second, These cells originate in vessels of the smaller and medium diameter, in consequence of free nuclei becoming surrounded with granules, which melt together to form a membrane.

Third, In the vessels of medium diameter, along with this formation of lymph corpuscles, there is also another similar one, which originates in the growth of those which have been previously formed.

Fourth, There are observed in the thoracic duct two kinds of lymph corpuscles,—a larger and a smaller. The first, it is likely, become dissolved in the blood; the latter, it is highly probable, are transformed into blood corpuscles, in consequence of their nuclei disappearing, and their cells becoming filled with colorising matter.

Fifth, Hence, in full grown mammalia, the completely developed blood corpuscles are colourless non-nucleated cells.

In addition to the active part the spleen takes in the preparation of the blood, our author also attributes to it what he calls a passive action, or that it acts as a diverticulum to relieve other organs from the too great quantity of blood which in various states of the body may be thrown upon them. This is a very old view, and was long ago supported most ingeniously by Rush and others. Some have thought that it acted as a diverticulum for particular organs, Chausnier for the stomach, Tiedeman and Gmelin for the liver. According to their researches, this organ becomes enlarged from three to four hours after a meal, in consequence of the great quantity of blood sent to it through the vena portæ; but the organ being incapable of dilating, the superfluity goes to the spleen through the splenic vein. Dobson, on the other hand, regarded it as a diverticulum for the whole body. According to him the spleen attains its maximum size from four to five hours after a meal, and this he attributes to the general plethora the system acquires from the food; and, as our vessels cannot contain more blood at one time than another, the superfluity goes to the spleen until it is carried off by the urine, &c.

Broussais, who generalised his notions, regarded diverticuli as of two kinds—temporary and permanent; among the former, he placed the supra-renal capsules, thymus and thyroid glands; among the latter, the spleen. From all we know of the functions of the spleen, then, we think its functions may be reduced to the three
following heads; 1st, it may be regarded as a diverticulum; 2d, as an elaborating organ; and, 3d, that its lymphatics in some way modify the chyle in the thoracic duct. One great argument against either of the latter views is, that the spleen has been, and may be removed with apparent impunity. We must confess, however, we have no great faith in any such experiments. At most, they merely prove that the spleen is not indispensably necessary for the various changes effected upon the blood and chyle; but they by no means prove that it does not concur in these. The results of some experiments, indeed, rather tend more strongly to confirm the latter. Appearances, such as are observed in certain diseases of the spleen, and denoting a want of haematin, have been witnessed by Bardeleben, Assolant, Schmidt, and others, after extirpation of the organ.—(To be continued.)

Parkes, Milroy, and Giacomini, on Asiatic Cholera.—
(Continued from page 608.)

A Disquisition on Pestilential Cholera; being an Attempt to Explain its Phenomena, Nature, Cause, Prevention, and Treatment, by reference to an Extrinsic Fungous Origin. By Charles Cowdell, M.B., M.R.C.S. London: 1848. 8vo. Pp. 210.

In our last Number we entered at considerable length into the history of the past and present epidemics of cholera; into its symptoms, morbid anatomy, and pathology. We have now to direct the attention of our readers to what is known of its mode of transmission, and of its treatment.

MODE OF TRANSMISSION OF CHOLERA.

When a disease of so fatal a nature as cholera attacks a town or district, destroying numbers of persons, the vulgar are very apt to consider that it is contagious, or infectious, and that an avoidance of those affected is a necessary precaution against danger. It almost invariably happens, under such circumstances, that many well authenticated examples of the disorder, apparently arising in this way, are at once found, which are stated with such confidence as to make it a matter of some difficulty to resist their influence. They are also often accompanied with a minuteness of detail and circumstance, that for a time bear down all opposition. The force of arguments so obtained, is always great in proportion to the rarity of the disease, and the panic of the people. Such arguments are, in all cases, nearly the same, and are derived, first, from the coincidence between the irruption of the disorder in previously unaffected places, with the progress of armies, the arrival of ships, of caravans, of fugi-
tives, pilgrims, or individuals; second, the coincidence between the occurrence of cholera in individuals, and their contact with others actually labouring under the disease; and, third, the immunity afforded by seclusion in the midst of an unhealthy district. No doubt, most remarkable examples of these coincidences and immunities may be obtained, and this in considerable number, from the history of the last epidemic. We willingly confess that a study of them is very likely to convince any one of the contagious nature of the disease, whose mind is not fully alive to the inherent fallacy of all such arguments, and to the difficulty there is, in such inquiries, of rigidly determining the relation between cause and effect.

On the other hand, all those who have witnessed the disease on a large scale, and especially Indian practitioners, are opposed to the idea of contagion. The principal arguments which they have brought forward are, first, that the medical men and attendants on the sick have not generally been attacked in undue proportion; second, that the disease, on many occasions, does not spread under circumstances of free intercourse; third, that quarantine regulations have failed in preventing its advance; and, fourth, that its duration, in many places, is very short. We do not consider it necessary to enter into details on each of these heads; we have only to assure our readers that numerous incontrovertible facts fully establish their correctness. One that has always seemed to us the most forcible, is the particular course which cholera often follows. For instance, the north-west passage of the epidemic of 1817—from Calcutta to this country; and, in more limited epidemics, its northward or southward progress, although the same intercourse in every direction has taken place between the inhabitants. Thus, Dr Parkes, in describing the epidemic which occurred at Moulmein in 1842-43, observes:

During this progress from the north towards the south, cholera, as already stated, attacked chiefly or exclusively the towns and villages stationed in low marshy places, on the banks of rivers, or on the shores of the sea. It did not extend inland, and the Burmans were accustomed to escape it by leaving their houses and travelling into the jungle. Directly the first death occurred in any village, the men deserted their fishing, or their paddy-fields, and, betaking themselves to their endless forests, preferred the chances of famine and the dangers of the jungle, to the risks of exposure to the attacks of cholera. They universally stated, that, though they were left without food by this flight, and were exposed to the burning noonday rays, and to the heavy tropical dews at night, yet cholera invariably left them after the second or third day’s march inland. The Burmese did not believe the disease to be contagious: they attributed it to the malice of “nats,” or demons, and they treated it chiefly with the cold affusion, in which they placed some confidence, and with vegetable stimulants and aromatics, of which they possess a very great variety.—P. 160.

Of late years, the different facts and arguments opposed to the contagious nature of cholera, have undoubtedly gained much in force and extension; and governments, in consequence, have begun to see the impolicy and inutility of quarantine regulations as preventive
measures. Dr Milroy sums up his argument on this subject with the following *résumé* of the facts in its support:—

When it is remembered that nine-tenths, I might rather say ninety-nine out of every hundred of the medical men in India, entirely reject the idea of the disease being propagated by infection,—that it has over and over again broken out in places remote from, and having no direct communication with, those where it chiefly prevailed,—that the attendants upon the sick are not a whit more liable to be attacked than others, a fact quite as true in Europe and America as in the East Indies,—that the pestilence every now and then unexpectedly bursts out in some district previously healthy with amazing fury, sweeps off its thousands, and then, in the course of a week or so, ceases altogether, sometimes after a thunder-storm, at other times without any appreciable cause,—that, in its migratory course, it has frequently appeared in numerous points of a large and scattered city at the very same time, while, in other instances, the distance of a few hundred yards has made all the difference between a region of almost inevitable death and one of complete exemption, and even of health, notwithstanding that uninterrupted communication existed all the while between the two; and when, too, we call to mind the indisputable fact that, upon no one solitary occasion have quarantine and other preventive measures of a like nature, however stringently and perseveringly employed, ever yet succeeded in keeping out the disease from any country;—that the Russian government, in 1831, having found their utter inefficacy, speedily abandoned all attempts of the sort;—that the Austrian Emperor formally declared that "he had committed an error in adopting the vexatious and worse than useless quarantine and cordon regulations against cholera," frankly admitting that he did so before the nature of the disease was properly understood,—that Prussia, too, having in vain had recourse to the same expedients, was forced to give them up,—that, in our own country, the government, intimated, in the speech delivered from the throne, if not their positive disbelief, at least their emphatic incredulity as to the importation of the disease from the continent by shipping or otherwise,—that one of the latest acts of the Central Board of Health in London was to announce that cholera patients should be as freely admitted into our public hospitals as any other sick,—that the Board of Health in Ireland candidly admitted that "they were not able to trace the disease to any communication by which it might have been introduced into the neighbourhood of Dublin,"—that the leading physicians and surgeons in Paris drew up a formal memorial, declaring their disbelief in its infectiousness, and that the French Academy of Medicine adopted and confirmed this opinion,—that the government of the United States, too, at first tried the effects of quarantine protection, but quickly abandoned it, the chief medical men in New York, Philadelphia, and other leading cities of the Union having pronounced against it;—when, besides these numerous and forcible reasons, we think of the singular exemption of some countries in Europe from the disease, for one, two, and even four years after the general visitation in 1831-2, and even after the pestilence had crossed the Atlantic, and made its power to be felt over nearly the entire extent of the New World; and all this, too, certainly not from any unusual stringency in the quarantine laws of those countries, but from some hidden cause quite beyond our ken,—can any one, after impartially thinking upon all these things, reasonably entertain a doubt as to the utter inadequacy of personal infection to account for the career of cholera, or hold to the folly and wickedness of ever again attempting to arrest its march by measures which have been proved to be wholly valueless? As well might we ascribe the blasting of our crops to direct transmission of the morbific cause from plant to plant—although unquestionably some forms of blight are capable of being propagated by immediate contact of the healthy with the diseased—and seek to protect them from the unseen foe by building a lofty wall around the threatened fields, as hope to keep out a disease like the epidemic cholera or influenza, by sanitary cordons or quarantine restrictions.—Pp. 33-35.
What, then, it may be asked, is the true mode in which cholera is transmitted? Dr Milroy, having got rid of contagion, says that it is propagated in the same mysterious manner as the influenza, or the blight which attacks vegetation. Dr Parkes, on the other hand, assumes the existence of a certain morbid agent, or virus, the nature and origin of which is unknown, but which is propagated under certain conditions. These conditions he considers to be, first, the comparative independence of the virus of the influence of temperature. He says comparative, because hot climates are acknowledged to be the most common foci of the poison;—second, the extraordinary influence exerted by the soil upon the virus, which sometimes seems to be contained only in the lowest strata of air; at others, possesses greater volatility, and passes with rapidity over large tracts of country;—third, the affinity of the poisons for moisture, although there seems to be a point of saturation beyond which moisture checks the development;—fourth, the remarkable predilection of cholera for all places where human beings are thickly crowded together, and where the effluvia from the excretions are consequently abundant.

It finds its conditions of development, not in any unusual atmospheric vicissitudes, but in the ordinary local and customary conditions. At Moulmein it prevails in one part of the town for months before it invades another; the only reason for this preference seems to be, that this part is nearest the river, and is the lowest, dampest, and most thickly populated quarter. It does not attack the residents on the high grounds, and when it spreads from its original point of seizure, the conditions which allow of such spread are to be recognised in the occurrence of meteorological phenomena previously wanting. So also at Madras; the disease is heard of at a station ninety miles off; a few days afterwards it appears in Madras itself; the atmospheric phenomena are not very different from those of years unmarked by its prevalence; but there is a certain degree of moisture in the atmosphere, and a wind blows directly from the station in which the disease had shortly before been prevalent. It is very fatal in the Black Town, crowded with Hindoos, and in the quarter of the Musselmauns collected round the palace of their Nuwaub, both places dense with a dirty and offensive population; it is less severe in the fort, quartered by English soldiers and tolerably clean; it is not seen at all in the houses of the English residents, scattered for miles along the chief roads and the shores of the sea. In both cases, we have the usual evidence of a poison exhaled from unknown sources, and existent in the atmosphere, entering a town, and propagating itself more or less rapidly according as it meets with moisture, animal and vegetable effluvia, and perhaps other terrestrial exhalations, which form the conditions which it demands for its development and increase.—Pp. 173-4.

Whether, then, we regard the hypothetical virus of Dr Parkes as an existing entity or not, the circumstances which he has pointed out as favouring its operation on the human economy are not the less deserving attention.

We had written thus far, when the work of Dr Cowdell fell into our hands. He considers the morbific cause to which we have been alluding, to be in its nature fungous, and endeavours to show that it is developed in the same manner, and produces the same effects as the fungi generally. In successive chapters he discusses the argument under the following heads. 1st, The aptitude of fungi for the
habitats assigned to them as agents in pestilential diseases. 2d, Such of the most remarkable among the known effects, produced by organized beings of the order protophyta (Fungi, Algae) as are illustrative of the argument. 3d, The capability of fungi to produce the phenomena of pestilential disease, as exhibited in pestilential cholera. 4th, The circumstances which probably concur to produce from the germs of fungi the effects of pestilential disease,—with facts to prove that a variety of nidus causes a variety of fungus. 5th, That the prevention or destruction of fungous germination, and the consequent catalytic action, is effected by the very medicinal agents, recommended, on the highest authority, in the prophylaxis and early curative treatment of pestilential cholera.

Dr Cowdell has exercised considerable ingenuity in the construction of his hypothesis, and might have strengthened it much more by a reference to the original authors who had written upon the subject, instead of merely consulting abstracts of their works in the pages of the British and Foreign Medical Review, Braithwaite's Retrospect, &c. We must, however, inform him that the idea is not new, and that he will find a masterly exposition of it, as applied to contagious and epidemic diseases, by Professor Henle, in his Pathologische Untersuchungen, published in 1840. But, notwithstanding all the talent and research which have been lavished on this speculation, Dr Cowdell has failed to exhibit greater proofs of its correctness now, than when Henle wrote, eight years ago. It is true that the blight among plants, the muscardine among silkworms, and tinea favosa, aphthae, and several other diseases in man, may spread by the propagation and development of fungous germs. In all these cases the microscope furnishes us with ocular proof of their existence. Such has not yet been the case in cholera, typhus, and similar disorders; and consequently, with whatever ingenuity analysis may be drawn between them, this view of their origin can only, in the present state of science, be considered as pure hypothesis.

RATIONAL TREATMENT OF CHOLERA.

It is unnecessary, perhaps, for us to remark, that every possible kind of remedy, and plan of treatment, even the most opposite in their nature, have been proposed and employed in Asiatic cholera. This at once betrays the absence of rational indications based upon a knowledge of the pathology of the disease. It is not, then, our intention to draw up a catalogue of what the numerous individual experiences and ideas of practitioners have led them to recommend, but rather to place before our readers the views of our authors on the subject, and ascertain, if possible, how far they may reasonably be considered consistent with the known phenomena the disease presents.

According to Dr Parkes, cholera runs a certain course. When the algide symptoms have once shown themselves, a case cannot be cut short. Even in the mildest forms, warmth does not return altogether
for a long time; but, when the disease has reached its acme, the patient is invariably seen to remain for some hours in a peculiar state, during which time nature seems to be gradually repairing the injury which has been done. If respiration could be maintained—not the mere mechanical act of breathing in and out, but the chemical process in sufficient integrity to allow the blood to circulate through the capillaries of the lungs—nature would gradually bring about the cure. This is the great problem which medicine has to accomplish, and which, next to the discovery of some actual antidote to the poison itself, appears to be the most ready method of accomplishing the cure of cholera.—(Parkes, p. 204.)

According to Giacomini, the rational treatment consists in overcoming the phlebitis (venous congestion), causing suspension of the circulation. For this purpose various hyposthenies are indicated; but he says they are often useless, because there is a complete absence of assimilation in cholera patients.

The instantaneous dryness of the cellular tissue by the operation of the morbid matter absorbed, and the filled state of the veins are such, that, at a certain epoch of the disease, there are no means of causing any remedy whatever to pass by assimilation into the blood. The skin is as dead, and does not absorb, and what is introduced into the stomach only washes or encumbers the absorbing passages. Consequently the best indicated remedies are not digested or absorbed, the digestive organs and skin not lending themselves to this office, and so the most powerful resources of art are rendered of no effect, in fault of a proper channel whereby they may be introduced into the blood.—Annales, p. 333.

Dr Parkes makes exactly the same statement, saying—

The great difficulty in the treatment of cholera, and the cause of the contradictory and opposing statements which have been made respecting the value of particular medicines, is to be found in the peculiar action of the choleraic poison. This action, by arresting the circulation, and thereby rendering absorption difficult, opposing itself to the common method of administering remedies. After a certain period of the disease, medicines remain in the stomach, and do not pass into the circulation, or do so with great difficulty and slowness. At least this is to be inferred, both from the circumstance that in the advanced stage, calomel, acetate of lead, creosote, opium, turpentine, &c., have been found in the stomach hours after they have been taken, and that fluids taken to appease thirst, remain in and distend the stomach, if they are not vomited, and also from the evident languor and delay of the circulation,—states which are considered unfavourable for absorption.—P. 200.

As medicines, therefore, cannot with any good effect be given internally, other means must be adopted for overcoming the venous congestion. Of these the most powerful seem to be bleeding, cold to the surface, and injections into the veins. Let us examine what our authors tell us regarding each of these means of cure.

Bleeding.—Dr Parkes states that the benefit resulting from bleeding was generally more marked according as the disease was in its earliest stage, and according as it tended towards the several varieties of pseudo cholera.

In these latter cases the employment of blood-letting was sometimes followed by very striking results, particularly in those cases attended by a full
pulse, and severe general spasms. For example, I saw a stout European soldier one hour after admission into hospital: he was violently purged and vomited, and was labouring under the most severe and frightful spasms. They were general and quite tetanic in character; the pulse was hard and sharp; the skin warm. He had been treated with calomel and opium without benefit. I immediately opened a vein, and took away forty ounces of blood before the spasms ceased. I then gave him Tinct. Opii, 3j. and repeated it in an hour. The pulse immediately after the bleeding became fuller and less resisting, the vomiting, purging, and spasms ceased, a gentle perspiration appeared on the skin, and he recovered without another symptom of any kind. It was the most striking instance I ever saw of pseudo cholera being cut short.—P. 207.

In the advanced stage he does not think it so useful, although if it do no good, it seems not to be injurious, and occasionally relieves the painful dyspnœa and oppression at the heart. It is, however, very difficult to get blood at this period; it flows from the arm in drops, and warm fomentations are often necessary even to procure these. According to Giacomini—

Blood-letting, as a rule, ought to be practised largely, with a view of preventing the phlebitis, the dilatation of the veins, their engorgement and their immobility. We say, as a rule—for if we wait until dilatation be effected and permanent, the blood only flows drop by drop, and all that is obtained only serves to empty the inferior part of the vein opened, without producing any advantage to the patient—we say, the blood-letting ought to be large; for if the quantity extracted does not correspond to what is indicated, the bleeding is of no effect, and, as the disease makes progress, inexperienced persons attribute to the bleeding the exasperation of the malady.—(Annales, p. 333).

Bleeding, when it can be practised, therefore, seems not only theoretically valuable in order to remove the venous congestion, but, when employed judiciously, has been found practically beneficial.

_Cold to the surface._—Empirical practitioners, amongst whom we must place Dr Milroy, naturally conceive, that when so much coldness of the surface exists, heat is directly indicated. He says—

The first thing to be done is to have the patient at once stripped and enveloped in warm blankets. The application of bottles of hot water, bags of hot salt or bran to the feet, between the legs, and along the course of the spine, will always be useful in increasing the warmth of the general surface. This is a point of great importance; as the cutaneous circulation is all but arrested, and the blood is consequently accumulated in the internal visceræ. The sympathy between the skin and the alimentary canal is known to every one by experience. Cold feet will often cause severe pain in the stomach and bowels; and, on the other hand, indigestion and diarrœa are almost invariably attended with a chilly state of the surface. The removal of the exciting cause in either case will speedily relieve, or altogether dissipate, the superinduced symptoms. How important, then, it must be to act upon this therapeutic principle in a disease like cholera, in which the whole body is marbly cold, and the gastro-intestinal canal is so strangely and violently perturbed!—P. 43.

Yet all this seems perfectly hypothetical, and constitutes an admirable commentary on the inutility of empiric practice generally, which, instead of seeking to remove the pathological cause of the disease, loses time in vainly endeavouring to alleviate the individual symptoms presented. How opposite are the statements of Parkes and Giacomini. For instance, Dr Parkes says—

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Warm-baths, vapour-baths, and warmth applied in any way to the surface, never appeared to me to be of the slightest service in true cholera. The spasms were sometimes relieved, but the algide symptoms were almost invariably increased. The depressing effects of the warm-bath were sometimes marked and unmistakable. I have seen a man walk firmly to the bath, with a pulse of tolerable volume, and a cool but not cold surface, and in five or ten minutes have seen the same man carried from the bath, with a pulse almost imperceptible, and a cold and clammy skin. I cannot find in my notes a single case in which the warm-bath appeared beneficial. It is, indeed, unlikely that the attempt to restore warmth by these trifling means, when the grand source of animal heat is so fatally disordered, can ever be successful. Several writers have also recorded their belief in the inutility of this measure.

Cold to the surface was a measure much more grateful to the patients than warmth. This might have been anticipated also from the way in which the bed-clothes are thrown off, so as to expose the surface freely to the air. The cold affusion, even in the last stage, two or three hours before death, sometimes caused the pulse to become again perceptible. Perhaps the application of cold to the surface may affect the respiration in some way; the gasping inspiration which the shock of the falling water generally induces, may influence the circulation in the lungs, like the first impression of the cold air on the body of the newly-born infant. But, unfortunately, after a short time, the reviving effects of the cold affusion disappear, and the case resumes its former course. The use of large fans and punkahs, causing a blast of air upon the body, seemed to me to be occasionally useful, and to be generally agreeable to the patient.—(Parkes, p. 209-211).

Again Giacomini observes—

What seems wonderful and incredible is, that the cold bath during the algide period should be immediately followed by heat of skin, elevation of the pulse, cessation of the cramps, and freedom of respiration, as I have observed this very morning (7th July 1836). In my opinion, therapeutics up to this time does not possess a more efficacious and prompt remedy wherewith to combat the cholera than the cold bath, the passage in the vessels being obstructed by vascular hyposthenics. It is important, however, that in this mode of treatment the cold applications should not be alternated with warm, as the employment of these last may become very hurtful.—Annales, p. 333.

Cold baths, repeated morning and evening, have done prodigies; not only the heat and pulse have reappeared by this treatment; but bleeding, impracticable until then, became possible, and the disease was overcome in the majority of cases the most grave.—(Ibid. p. 334).

We leave, then, our readers to judge whether an empirical or rational practice should be followed in the application of heat and cold.

Injections into the Veins.—We have already seen that medicines will not pass into the blood when taken by the mouth; their direct introduction therefore into the circulation by means of injection, although a bold practice, seems to be perfectly warrantable. The immediate effects produced by the saline injections of Dr Latta of Leith, and Dr Mackintosh of Edinburgh, under the idea that the salts in the blood were defective, have been described by all who saw them to be most extraordinary. They dissolved 3ss of muriate of soda, and 5iv of sesqui-carbonate of soda in ten pints of water, at a temperature varying from 106° to 120° Fah., which were injected slowly, half an hour being consumed in the process. After the injection of a few ounces, the pulse, which had ceased to be felt at the wrist, be-
came perceptible, and the heat of the body returned. By the time three or four pints had been injected, the pulse became good; the cramps ceased; the body that could not be heated was rendered warm, and all the other symptoms were alleviated. These magical effects, however, were not lasting. The discharges continued, and the evacuations became even more profuse; the patient now relapsed into his former condition, from which he could again be temporarily roused by a repetition of the injection; the amendment, however, was more transient, and death followed.

Dr Parkes says, that in some cases which he witnessed in India, he did not even see the temporary vivifying effects which generally followed the employment of these injections in Edinburgh; he therefore determined on trying some other agent.

I still thought that alkalis and salines seemed indicated by the evident occasional escape of the water and salts of the blood, and I fancied some benefit might result from an attempt to supply to the system a proteine compound; although, of course, I could form no conception of the probable mode in which such an addition could prove useful. I determined, therefore, to inject into the veins an alkaline solution of albumen. I shall now detail, as briefly as the subject will permit, the few cases in which these injections were used. In all these cases I believed the patients to be doomed to an inevitable death. I did not consider myself authorized to try an experiment of so serious a nature, on any man, while there yet remained a chance of his rallying under the ordinary treatment. Consequently, I must premise the plan was tried under the most unfavourable circumstances.—P. 219.

The solution injected was composed of sesqui-carbonate of soda 5iv; chloride of sodium, 5ij; the albumen of one egg; and four pints of water, at a temperature of 98° Fah. The flakes of coagulated albumen were separated by filtration, and the fluid was slowly injected in five very desperate cases. We regret that our space will not allow us to give the details of each. Suffice it to say, that there was, as in the case of the purely saline injections, a marked temporary improvement, but they all ultimately died. Dr Parkes observes—

My own impression is still somewhat in favour of this practice. All these five cases were of the worst kind; there did not appear to be a chance of recovery for any of them under the ordinary treatment, and yet one was certainly carried through the cold stage, and, if differently treated during the consecutive fever, might have been completely cured. I think also that the alkaline solution might have been made weaker with advantage, and other ingredients might perhaps have been added.

The operation should, I think, be again tried; it is very simple; it gives no pain, for in this stage of cholera the skin is almost insensible; and it can do no harm to the patients, who are, in fact, doomed to almost certain death. The possible supervision of phlebitis in an after stage should, I think, be disregarded; this disease is less formidable than cholera. The injection is not to cure cholera, but to restore and to sustain the circulation for some few hours, until the healing force of nature may repair the lesions of the blood and restore to the vitiated fluid its normal composition.—P. 237-9.

On the whole, we agree with Dr Parkes in thinking, that injections into the veins, perhaps somewhat modified, deserve a more extensive trial.
For other points connected with the treatment of cholera, we must refer to the different works published on the subject. The opposite opinions maintained with respect to the value of different drugs, are undoubtedly owing to practitioners having mistaken ordinary cases of diarrhoea, or the slighter forms of the disease, for the true algide cholera. To this subject we may return at some future period, in connexion with a little work we have just received from Dr Spencer Thomson on British Cholera.

Observations on some parts of Surgical Practice, to which is prefixed an Inquiry into the Claims, that Surgery may be supposed to have for being classed as a Science. By John P. Vincent, late Senior Surgeon to St Bartholomew's Hospital. London: 1847. 8vo.

Mr Vincent's work consists of two distinct parts, 1st, An introduction devoted to an inquiry into the claims of surgery to be classed as a science; 2d, Practical observations. In both departments there is a want of systematic arrangement, and a peculiarity in the author's style, a sudden transition from one subject to another, which renders it not a little difficult to follow him throughout his reasoning to its results; and in the practical part, which contains much that is worthy of attention, this peculiarity of style interferes considerably with its usefulness. Mr Vincent commences his inquiry into the claims of surgery to be classed as a science, by commenting on the vague ideas generally entertained as to what really is science; if we understand him aright, "science is the comprehension of truth in any department of knowledge over which the mind has the power of giving certainty to the results of its investigations," in contradistinction "to the knowledge of things derived from the senses and not from the intellect;" such knowledge, according to Mr V., being "neither fixed or certain, because it is not capable of being demonstrated;" and his opinion seems to be, that it is wrong in surgeons to assume, that the understanding cannot attain as great a degree of scientific perfection in their branch of natural knowledge, as it can in mathematics and arithmetic. Now we think that surgeons are in this instance right; there is evidently a great difference between fixed and absolute sciences, such as mathematics or arithmetic, and natural sciences, such as medicine and surgery. The numbers in arithmetic never vary in their relative value; and hence, in dealing with them, we can calculate positively on certain fixed and absolute results. No analogy, therefore, can be drawn between this and a science dealing with organized living bodies, "the vital actions and functions of which vary not only in different individuals, but even in the same individual under different circumstances." As regards the slight value which our author seems to attach to the knowledge derived
from the senses for advancing surgical science, we must bear in mind that this, after all, is the only source from which the surgeon in his department can gather the facts to be afterwards elaborated, by the intellectual process of reasoning, into great general principles. Again, though we agree with him, that surgeons are but too ready to rest on the prevailing opinions of the day, often without due inquiry as to the grounds on which these rest, we are of opinion, that great caution is necessary in rejecting, as mere matter of opinion, the information derived from others. Human life is too short, and the opportunities of individuals too limited, to admit of any one man constructing a complete system for himself; he must (exercising due discrimination) depend to a great extent for information upon the labours of others who have had greater experience than himself. In the words of a modern writer, "Any thing which I am told by a credible witness is information, and so is any thing which comes to my knowledge through any of my own five senses, and so is any knowledge that I gain directly by attending to the processes and feelings of my own mind; but any notion which does not come from one or other of these three legitimate sources, sensation, consciousness, or competent testimony, is good for nothing. It may be an ingenious hypothesis, or a plausible opinion, but it is not matter of fact, it is not information; till it assume a positive form it is not knowledge, and I have no security for its eventual truth."

Those things which I have observed for myself, and those which others have told me, make up a solid basis of truth—a terra firma of fact; if I am dissatisfied with its narrow limits, I may fling myself over into the abyss of speculation, and, finding in every deep a deeper still, perish at last in total scepticism; or, I may try to soar upwards into a transcendental region, and, after fruitless efforts to be wise beyond my nature's capacity, be content to fold my weary pinions at last on the homely landing-place of common sense and tangible truth." In a practical natural science like surgery, we must look to facts, the knowledge of which we have derived from the evidence of our own senses, or from the positive information conveyed to us by others, as the materials on which our intellectual powers are to work,—the basis on which the superstructure of scientific principles is to be raised; but to trust for its advancement to a process of pure intellection unaided by the senses, is a long since exploded absurdity.

We believe, however, that our author's practical conclusions would be nearly the same in spite of his "pure intellection" theory; for his native good sense bursts through the mists of his philosophy in the following passage, which is strangely at variance with some of his previous reasoning:

The improvement of surgery upon scientific principles, must commence and proceed by investigating the more common instances of disease, by which there is afforded a larger field for making observations, [what! without appealing to the senses?] a wider latitude for determining the relations, and a
greater facility for obtaining the points of bearing the facts have with each other, than the consideration of rare specimens of disease can afford. In this way only is the greatest knowledge of disease to be acquired. The simple and elementary principles are to be first understood in learning any science. Surgeons, by attaching so much importance to rare cases of disease, rarely do more for the benefit of surgery than giving currency to ill-formed opinions.

Shortly after emitting this ray of light he again becomes misty, dreamy, and philosophical, and enters into speculations we consider it unnecessary to notice; and accordingly we turn to his practical observations as more congenial to our taste.

The practical department of the work embraces observations on a very considerable number of surgical diseases and cases; but they are not arranged under separate heads, nor in such a manner as to render them as useful as they might have been made for reference and consultation. The author's plan seems to be, to lay down certain general propositions regarding what he considers as the laws of vital action in health and disease, and then to apply these laws or principles to the elucidation of certain points in special diseases, which, he believes, exhibit their action. To be fairly understood, the book requires to be read through as one connected whole, and with a constant reference to the preliminary propositions with which it sets out. The first subject treated of is muscular action, considered in reference to fractures and dislocations. Mr Vincent very justly reflects on the want of attention paid to this subject, and remarks on the necessity of studying the functions of muscles in their associated actions, and also the connexion and sympathy between the actions of what are termed the antagonist muscles of a limb; some of his propositions on this subject are sufficiently startling and novel. Thus, at p. 10, he says:—

There is yet another law of muscular action that is to be constantly attended to in practical surgery. It is, that the relaxation of muscles is to be effected by attending to their position, when they are required to throw out their strongest exertions; and not, as usually is supposed, by approximating their attachments. It is a fact we might almost expect, as the result of the powerful influence of association under which they act, and which, whilst this combination exists, regulates the exercises of their forces. Thus the powerful gastrocnemius muscle exerts its greatest strength, when, in progression, it is acting to advance the body forward, by throwing its weight upon the toes or metatarsal part of the foot. To do this the foot is extended, which is the same thing as approximating the attachments of the muscle. Now, that case which is called dislocation of the foot backwards, and in which the tibia is presenting in front of the astragalus, offers an exemplification of this position. Of course the gastrocnemius has its lever of action increased in power as the foot lies extended, by the heel projecting so much behind, which advantage, joined to that of its habitual exertion when in this direction, forms very great opposition to the foot being brought to a flexed position; but yet this is not difficult, and the surgeon will sensibly feel the cessation of its action, the moment it is brought to a right angle with the axis of the tibia. This is owing to its being then in a state of least action, in the usual exercise of its powers.

The example here given does not, when fully considered, bear out the author in his conclusions. It is perfectly true, that, to reduce the luxation in question, the foot requires to be brought to
a right angle with the axis of the tibia; but what position of the leg
most facilitates this being done? certainly not the extended posi-
tion, when the fibres of the gastrocnemius are on the stretch (or
at what Mr V. would call their minimum condition of action); but,
on the contrary, the bent position, when the leg is flexed fully upon
the thigh, so as to approximate the points of attachment of that
muscle and relax its fibres. In our own experience, at least, both
in the form of dislocation alluded to, and also in oblique fracture of
the lower part of the tibia, this is the method by which we have
found reduction most readily accomplished. As regards the second
case given in illustration of this assumed law—namely, fracture
of the humerus below the insertion of the deltoid—we think there is
a very obvious cause for the fact he mentions; we mean, the power
exerted by the pectoralis major, latissimus dorsi, and teres major
muscles, tending to draw the upper portion of the humerus towards
the chest, so as to antagonize in a great measure the action of the
deltoid. The same peculiar views regarding muscular action mark
his observations on dislocations; but he gives many useful practical
hints as to the reduction of particular luxations, and as to the dis-
tinction between the circumstances in recent and old standing dis-
locations. Another proposition, which he attempts to lay down as a
general law, is, that in the animal economy there exists "a power
which can set aside the ordinary laws of matter." His meaning ap-
ppears to be, that nature, having adapted certain parts for special
functions, there is a natural tendency in these parts under all cir-
cumstances to carry on their natural functions; as examples of this
law, he adduces cases of openings in the perineum communicating
with the urethra, and the urine nevertheless continuing to pass by
the urethra instead of the more dependent opening; and especially
refers to the example of this after the operation of lithotomy, the
water passing by the natural passage before the perineal incision
has healed. As to the former of these examples, we readily admit,
that in many cases of fistula in perino, only a small quantity of
water comparatively speaking passes by the openings in the peri-
neum; but is this in opposition to the natural laws of hydraulics
and gravitation? is it not easily explained, when we consider the
narrow and tortuous character of the fistulous channels as compared
with the canal of the urethra? As regards the urine passing by the
urethra before the wound is healed after lithotomy, we must re-
collect, that the incision of the prostate heals before the external
incision; in the earlier stages, it generally depends upon swelling of
the lips of the prostatic incision or of the parts around it, and when,
in the first days after the operation, the urine does not come away
freely by the wound, it is by no means a very favourable symptom:
so that we think that, in this proposition also, our author's examples
scarcely justify his conclusion. In speaking of the powers of the
constitution to bear up under severe injuries, Mr Vincent strongly
recommends abstaining from operative procedure in the first in-
stance, merely watching the patient and throwing in stimuli, if the vital powers seem to flag, or if they are required for the purpose of arresting the progress of gangrene.

I may safely say, he says, that for the many years I have followed up these principles, I have never had a case of traumatic gangrene arising from severe injuries that has not stopped. Of all the subjects that come before the surgeon for prompt and decisive conduct, this is, I am convinced, one of the most important. It is an opportunity for science to step in and save life; it is a demonstration of the ability to decide the agitated question of immediate amputation or not. It is the exercise of a convincing practice much beyond any proof derived from arguments. I am convinced, that by adopting this course of watching the expressions which the constitution shows during the first three or four days of the first stage of very serious injuries, and by throwing in brandy when the indications demand it, that there can be no occasion to amputate on account of traumatic gangrene.

He then animadverts on Baron Larrey's advocacy of immediate amputation, which he condemns as unjustifiable. We believe that Mr Vincent's experience as to the unfavourable results of primary amputations is the same as that of most hospital surgeons, and that his views are very much founded upon this unfavourable experience; but in reviewing this much agitated point, it would be well if surgeons, before condemning the advocates of either side of the question simply from their own experience, would duly consider the different circumstances under which the cases have been treated. Whilst the statistics of civil hospitals in large towns show a very great mortality after primary amputations, the statistics of military surgery, on the other hand, as exhibited in the works of Larrey and Guthrie, show a very great proportion of success in primary, over secondary amputations. We therefore think, that if surgeons were to employ themselves in examining into the causes of this difference of result, they would render a much greater benefit to surgical science, than by ranging themselves as uncompromising adherents of either party.

Mr Vincent makes some very useful observations on varix and varicose ulcers; in regard to the operations for obliterating the veins in varix, he observes—

I have always been at a loss to understand the reasoning which has led to the adoption of the practice of obliterating a vein in a varicose limb. Varix is not a mere disease of a part of a vein, which the tying of the tube, or the removal of the part, can get rid of. It is the derangement of a system of tubes designed for conveying the blood to the centre of circulation; and one condition for properly carrying this on is, that the space in which the fluid is to move should be duly capacious. The breaking up of one channel must throw more fluid into those that are left open; and as the relief of all tubes under disease is just in proportion to the undisturbed course which the contents of the tube are allowed to take, so I apprehend this practice is really calculated to keep the varicose state of the limb. I have seen cases where varicose veins of the leg have been tied, or divided, and where portions have been removed; but as soon as the patient got about afterwards, I have observed that the leg has been embelished with fully as many diseased vessels as before these several operations.
Mr Vincent might have added, that by throwing the returning current of blood upon the collateral venous branches, we not merely distend them, but, that distension gradually effaces their valvular structure, and induces a permanently varicose condition of those veins. The pathology of burns is next considered, and the nature of the different kinds of burns, with reference to the agents causing them, is very carefully and judiciously treated of. Though our author treats at great length on scrofula, we regret to say we have derived no information; and he seems to be quite unacquainted with the more recent researches on this subject. The principal methods of treatment he recommends are, sea air, attention to the cleanliness of the skin, the use of the flesh brush, and in certain affections of the eye, mild mercurials, not a word of cod liver oil, or iodine and its preparations; whilst as regards the local affections in this disease, his remarks prove him to be quite unacquainted with the true pathological condition of the fungoid testicle, and of the recent improvements in its treatment proposed by Mr Syme. Again, whilst he treats us to a long disquisition on the comparative advantages of peas and beans over glass beads for keeping issues open, he does not favour us with a single remark as to his experience in reference to excision of diseased articular surfaces in scrofulous patients. In his treatment of venereal diseases, Mr Vincent is one of the old school, a decided advocate for mercury; but we shall make no remarks on this subject, as our limits forbid our entering on such discussion. Mr Vincent has long enjoyed a very wide field for practical observation, and he appears to have reasoned much on the cases presented to him in his practice; but his work does not evince a knowledge of what has been doing in surgical pathology and operative surgery by others. He seems also to have observed many things through the distorting medium of peculiar views, and hence, as we have more than once noticed, his facts do not always bear out his conclusions; and, on the whole, though the book contains much valuable practical information scattered throughout its pages, we confess ourselves disappointed in it, as coming from a surgeon of Mr Vincent's acknowledged talent and experience.

A System of Surgery. By J. M. Chelius. Translated from the German, and accompanied with additional Notes and Observations. By John T. South, Surgeon to St Thomas' Hospital, &c. 2 vols. London: 1847. 8vo.
valuable and extensive notes of Mr South, in which he has largely embodied the results of his own experience, have undoubtedly rendered his translation of it one of the most complete treatises on surgery in the English language. It seems to us to be alike essential both to the student and practitioner.

Part Third.

MEDICAL NEWS.

MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.

SESSION XXVII.

MEETING IV.—Wednesday, January 5, 1848.—Dr Gairdner in the Chair.

1. Mr Spence read two cases in surgery, which are published at length in our last Number.

2. Professor Miller read a case of encysted calculus removed by lithotomy. This is also published in our last Number.

Dr Newbigging related the history of a case of encysted stone in the bladder, which had occurred, many years ago, in the practice of Sir William Newbigging. A man, thirty-eight years of age, underwent the operation of lithotomy. On seizing the stone with the forceps, the resistance to its extraction was such as to induce the belief that the opening in the prostate was too small, and this was accordingly dilated so far as was deemed safe. The resistance, however, still continuing, the finger of the left hand was introduced along with the forceps, when it was ascertained that part of the stone was imbedded in a sac, occasioned by the contraction of a portion of the bladder forming a ring around the calculus, which retained and prevented the stone being removed. While the forceps were made to maintain their hold, the ring, which had formed a deep fissure in the stone, was gradually, although somewhat forcibly, dilated by the finger; and, so soon as this was accomplished, the final extraction of the calculus was effected. Notwithstanding the manipulation, and the large dimensions of the stone, the patient recovered in the usual time, without one unfavourable symptom. The calculus is in the collection of the Royal College of Surgeons of Edinburgh.

A discussion now took place on Chloroform, in the course of which Dr Lowe remarked that he had found it useful in preventing certain insane persons from voluntarily rejecting food after it had been administered by the esophagus tube; and Professor Miller had found it, in one case, to relax a spasmodic stricture, so that a large bougie could be passed without difficulty or suffering.

3. Dr Bennett exhibited two kidneys studded throughout with small abscesses, and demonstrated, under the microscope, that the pus corpuscles they contained were the nuclei of cells.

MEETING V.—Wednesday, February 2, 1848.—Dr Alison, V.P., in the Chair.

4. Dr Seller read a paper, entitled "On the Signification of Fact in Medicine, and on the Hurtful Effects of the Incautious Use of such Modern Sources of Fact as the Microscope, the Stethoscope, Chemical Analysis, Statistics, &c."

The object of this paper was not to discourage the employment of such sources of medical facts as the microscope, the stethoscope, chemical analysis, statistics, and the like, but to enforce certain precautions against errors, and misapplications of their results, with a view to the greater efficiency of these powerful means of research. In the outset, the author stated that the representations which science affords of the facts, in particular of organic nature, though termed facts by courtesy, may, and often do, involve no inconsiderable amount of error. The admixture of error with the so-called facts, is great in