8. Removal of a Fatty Tumour.—U. S., æt. 73, admitted in April 1870. He suffered from a fatty tumour the size of a saucer, situated under the skin over the back of his neck. As the tumour was becoming very troublesome and increasing in size, he was anxious to have it removed. This was done, and the patient made an excellent recovery.

9. Removal of large Fatty Tumour.—Mrs A., æt. 71, admitted 28th August 1871, suffering from a fatty tumour, the size of a child’s head, situated on the inner aspect of the thigh. The skin over the tumour had ulcerated and caused a painful and troublesome condition of the growth. The tumour was cut out, and the patient recovered well from the operation, being dismissed on the 20th of September 1871.

In addition to these cases, I might add several successful operations for strangulated hernia in old people, one of the patients being seventy-eight years old, and one successful case of lateral lithotomy in a gentleman aged seventy-four; but these latter results are not so very uncommon in surgical practice.

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**Part Second.**

**REVIEWS.**

*Clinical Lectures on the Diseases of Women.* By Sir James Y. Simpson, Bart. Edited by A. R. Simpson, M.D., etc. Edinburgh: Adam and Charles Black: 1872.

We have now before us the third and last volume of Sir James Simpson’s collected writings, and it is a large, handsome, interesting book. The first volume contains the most important contributions to science which Simpson made. They were mostly published while he was yet young. The second volume has interest chiefly on literary and historical grounds, and has very little in it that can be called contribution to science. It is occupied with the history of the American discovery of anaesthesia and of his own promulgation of chloroform as an anaesthetic agent, and laboured defences of his claims as a discoverer. It contains the foundation of his wide fame. The third volume contains his own statement of his views as to gynaecological theory and practice. It is well known that Simpson had an unrivalled reputation as a physician and surgeon in the diseases of women. Crowds of women came to him seeking advice; and we find in this volume an account of much of his therapeutical proceedings for their relief. So great was his renown in this department that we are compelled to enter, at once, a caveat against allowing it to influence judgment on his writings and proceedings. Re—
nown and crowds of patients only imply the faith of the patients in the power of the physician; they imply only reputation and notoriety. It is common to suppose that reputation of this kind cannot be produced without a good substantial basis; but this natural supposition is a gross mistake. No man ever fostered the growth of his reputation more than Sir James Simpson, and he reaped the natural fruits of his care and zeal. We speak now without any special reference to the author and practitioner under review, and assert the well-known fact, that the widest and, in some senses, largest reputation may be based on nothing good or substantial; and we draw, as critics, the conclusion that renown, reputation, and crowds of clients give only a very slight and frail presumption in favour of real success in treating and curing, while they unmistakably show the presence of cleverness, worldly wisdom, ingenuity, and power to please and satisfy.

Simpson attracted universal attention to his plans of diagnosis and treatment. In diagnosis he did excellent service by showing the applications and value of the uterine probe. While this was his chief it was not his only contribution to this department; but we pass on to consider his practice. Before doing so, we must candidly tell our readers that we do not expect to find much of advancement of practice in any man's work. He who knows medicine or looks over its history will find how little any one has ever done in this department. Proposals are abundant; methods of cure are easily described and affixed to the descriptions of disease; but time generally either reduces them simply to nothing or shows that they are injurious. We have not space to enter on the philosophy of this grand subject—a philosophy so discouraging to our great therapeutic discoverers who rapidly cover and again cover the pages of the Lancet or the Practitioner with fancied new and beneficent treatments and cures. Besides the tremendous difficulties in the proper potential science of therapeutics, there is one great preliminary difficulty which it will probably take ages to overcome—our ignorance of the nature, and then our imperfection in the diagnosis, of disease. Simpson was true to himself; but very far wrong in his fundamental notions on this subject. His erroneous philosophy is probably the best excuse for his persistent introduction of wonderful new cures—his polypharmacy. His patients were only a short time under his care when they had surgical and medicinal treatment of great variety, sheaves of prescriptions delightful to credulous patients and to druggists. Leptandrin, furfurin, salts of nickel,—what not? It has been said of many such remedies that nothing is known of them but the names; we might go farther, and say that not even the names are known. Yet they cured and cure apparently just as well as other things.

Simpson's remarkable statement is this:—"Ask the medical practitioner what the nature of the difficulties is which he most frequently encounters in practice. If he will properly analyze the
subject, he will confess to you that his difficulties most usually con- 
sist in exactly determining what the special disease, or diseased 
action, in his patient may be—not in determining what he should 
employ for its cure, provided he were once perfectly certain of its 
precise nature and precise seat and extent. The real difficulties 
of the practitioner do not commonly consist of doubts about the 
nature and details of the therapeutic measures which he ought to 
use; but they consist of primary doubts and uncertainties as to 
what the diseased action is against which he is to turn his diversi- 
fied therapeutic resources. They are doubts not about therapies, 
but about diagnosis.”—(P. 3.)

Just so!! For a threatened abortion, give furfurin; for a cancer, 
burn with sulphate of zinc; for a headache, give sulphate of nickel; 
for anaemia, give manganese; to remove adhesions, give a local mer-
curialization; to cure a fibroid, give bromine; to cure sterility, split 
the cervix uteri; to cure phthisis pulmonalis, give oily inhalations; 
to cure coccodynia, operate, and never mind though a bit of the 
knife is broken off and remains in the bone; try to remove a hæma-
tocele by the local use of mercury or iodide of lead, etc. (No doubt 
the etc. is as effectual as the mercury or iodide of lead.) Here is 
the grand medicine of the nineteenth century. If we know the 
disease or diseased action, we have no difficulty in finding the remedy. 
Would that this were other than really an egregious unintentional 
hoax on medicine! Would that therapeutics were waiting for ad-
vance of diagnosis! It is too true that diagnostic science is far from 
being far advanced; but truly therapeutics are as yet at an infinite 
distance behind it. We are not now speaking of that great depart-
ment of medicine, the homely empirical therapeutics, founded not 
on science, but on public and professional confidence and know-
ledge of human nature, which demands the highest wisdom in the 
practitioner, and which, if it is not to degenerate into quackery, re-
quires truth in the inward and outward parts.

There is a sense, however, in which Simpson’s statement is true; 
but it is not the sense which he meant his words to convey, and it 
is a sense which only plunges medicine in deeper disgrace (not dis-
honour) than the intended one. This sense may be given in a kind 
of paraphrase of Simpson’s words. Practitioners generally are edu-
cated well enough to know the difficulties of diagnosis; they know 
that a good physician has a considerable power in this way; and 
they are conscientious enough to feel the want of it, or to doubt 
their own power. But when they have made up their minds as to 
the nature of the disease, then their ignorance or bad education 
comes fully into play; they have no difficulty in prescribing; they 
ply their leptandrin or furfurin, or nickel or mercury, without fear or 
scruple. Poor and honest fellows! Ignorance is the mother of 
superstition. Their remedies do as well as any charm or incanta-
tion, or as an “Ave Maria, ora pro nobis,”—and that is truly doing 
a great deal, doing a good deed. The medicine-man of Central 

VOL. XVII.—NO. IX. 5 N
Africa is a conscientious creature, just as he is in Scotland. He has no doubts about his diversified therapeutic resources. Patients in Central Africa pay their fees and swallow their drugs just as in Scotland. Such is polypharmacy as it still exists in Great Britain, and is expounded in the end of the nineteenth century by great teachers. Such is the real basis of the success of homœopathy, hydropathy, allopathy, and of every pathy;—want of science, want of scientific appreciation, credulity, want of feeling of the want of science and of scientific method, want of respect for the laws of nature; medical superstition, the same in Great Britain as in Central Africa, and equally useful in both places, but more excusable in the latter.

Simpson has in this volume a great deal of matter, chiefly of curious historical interest, on the subject of cancer. Any one who knew the man would expect that cancer cures would be some time or other a subject of his study. As might have been expected, it has come to nothing. The chapters which he devotes to the matter are worthy of perusal as a contribution to an important part of the history of medicine, and one which does not do the profession much credit.

Fibrous tumours of the uterus, a grand gynaecological topic, he discusses very fully, yet far from completely. He had an overweening faith in treatment by medicines and otherwise in this disease; and he was very unsuccessful in operating for the removal of such growths. So much was this the case that he latterly dis- countenanced surgical interference. It is well known that in the meantime our only rational hope of removal of these tumours is by surgery, and that recently much success has been obtained in this way, both in this country and on the continents of Europe and America.

In his lectures, as originally published in the *Medical Times and Gazette*, he has no recognition of pelvic peritonitis, now called perimetritis, but dwells at length on pelvic cellulitis, now called parametritis. But it is evident that under the influence of his assistant Dr Lauchlan Aitken, and of a book which he does not name, but which is easily recognised by his description of it as "a monograph only recently published," he had greatly changed his mind on the subject of perimetritis and parametritis. He takes laboured exception to these new names given by Virchow, but this is now of no avail, for the names are meantime adopted, as any one who knows the modern literature of the subject can testify. The names, however, are matters of small importance, and not worthy of long discussion. The new chapter which he now gives on perimetritis fits badly with his old account of pelvic cellulitis. He ought either to have completely studied the subject anew and written the lectures over again, or have adopted far more extensively than he has done the views propounded in "a monograph only recently published," and which his assistant Dr Lauchlan Aitken could have expounded, modified, or corrected. This gentle-
man is well known to our readers as the contributor of several articles in our pages, and his works would be more highly appreciated were it more generally known that his views as published were far from being in accordance with those of his not particularly tolerant master.

Our author very early took great interest in ovariotomy, encouraged Clay, foresaw the future of this great operation, and defended it against unjust attacks then made on it from every side. His arguments were not always well laid, and sometimes his figures were quite unreliable, but he was energetic, skilful, and did good; making one of the early applications of statistics to the solution of practical questions. He operated frequently, and employed several of his young friends to operate for him. But he was not successful, and he declined, no doubt for reasons not far to find, yet surely insufficient in a man who got so much statistical aid from others, to give for publication the number of his cases and the mortality.

Dysmenorrhoea and sterility are two subjects whose treatment occupied Simpson very much, and no two diseases brought him more fame or patients. They are great subjects, and are worthy to occupy the attention of the greatest man. Simpson has merit in directing his powers to the solution of the difficult problems involved in the treatment of these diseases, and in some senses he cannot but have done much good. Yet we must add, that we have read his account of dysmenorrhoea with astonishment. To find him attributing the disease, for whose cure he applied a modified lithotome caché, to contraction of the external os uteri, creates for us a puzzle. There is no doubt it is an error, but this is not the place to discuss this point. There is no doubt that dilatation of it does not require a metrotome, and that it is not dangerous to life or to the health of the parts. Accordingly, our author writes that he believes he is entitled to say that there are few operations in surgery so perfectly simple in their performance, and so extremely satisfactory in their results, as division of the cervix uteri in cases of obstruction, dysmenorrhoea, and sterility!! Yet we are very far mistaken if it is not notorious that the operation has gone greatly out of fashion, that it is falling into comparative desuetude, and that it has been the cause of death and other disasters in very many cases. Simpson's own editor and assistant corroborates this. There is indeed room to question whether the operation has not caused much more disease and sterility than it has cured.

One of the chief diseases closely connected with the name of Simpson is retroversion of the womb or the minor displacements generally. At one time, it quite took the wind out of the sails of ulceration of the womb; it was the secret of the minor uterine ailments; its cure was to be effected by an intrauterine pessary. This theory of the minor uterine ailments is well known to be now in a very shaky condition. Scarcely any one adheres to it.
treatment by intrauterine pessary is well known to be an exploded heresy, and to have been long almost, if not altogether, disused by Simpson himself. Winckel, Martin, Beatty, and some others, are still a forlorn hope struggling for its partial defence; but the attempt is futile. In spite of all this, which is well known, the original paper on this subject, by Simpson, in the Dublin Medical Journal, is here virtually reprinted with scarcely a note or comment! and the book is intended for the guidance of practitioners! Nothing is said of the fatal and other injurious consequences of a plan of treatment the description of which remains much as originally proposed, although it was subsequently given up by the proposer himself.

We must stop somewhere in this line of criticism, though we have plenty of materials for continuing it, lest we weary our readers by its undue extension; yet it is natural for a reviewer passing over the work of a great author to seize its salient points, and especially those which offer prominent obstacles to complacently smooth reading. The student will find much interesting matter in this volume of a more commonplace kind than what has excited our remarks; much, indeed, in a tame old-fashioned style, which we scarcely recognise as the work of this impetuous author. This kind of work is all of recent date.

The three volumes of Simpson's works which we have reviewed constitute an admirable picture of the man, of his style of thinking, practising, and writing. They are the works of one who attracted as much notice and excited as much attention as any of his contemporaries. They ought to be in every medical library.

The West Riding Lunatic Asylum Medical Reports. Edited by J. Crichton Browne, M.D., F.R.S.E. London: 1871.

An Analysis of Post-mortem Appearances in 235 Insane Persons.
By James C. Howden, M.D. Edin., Medical Superintendent of the Montrose Lunatic Asylum.

The West Riding Reports are a collection of thirteen essays on subjects connected with insanity. They are, of course, of unequal moment and of unequal merit; but, taken all in all, contain a good deal of interesting matter, the result of keen observation and pains-taking research, discussed in a scientific spirit.

The first paper is on Cranial Injuries and Mental Diseases, by Dr J. Crichton Browne. It might fairly be conjectured that a knowledge of the physiology of the brain would be extremely useful in the prognosis of cerebral injuries, or that the careful observation of such injuries would tend to throw great light upon the physiology of the brain; but when we come to compare and
try to group these cases, we find the results so variable and contradictory, that there is no gainsaying Dr Browne's observation, that "the subject is still involved in doubt and confusion."

We regard it as a perfectly open question whether the cerebrum is an organ with a common function, like the kidneys, or whether different functions or different kinds of mental work ought to be assigned to its different morphological parts. Dr Howden, in his analysis of the post-mortem appearances in 235 insane persons, writes—"In referring to the summary, I shall not pretend to prove any connexion between the lesion and the mental symptoms during life." But, then, what is the use of men taking toil and trouble to slice up brains by hundreds if no connexion can be established between the symptoms during life and the appearances after death? Dr Howden, of course, knows this very well; but his experience has as yet afforded him no certain guide to help him in connecting the one series of observations with the other.

Dr Browne sets himself to show how cranial injuries often are the cause of mental disorders; but, then, he has to admit the existence of a factor of very variable power in what we call hereditary or constitutional predisposition. This predisposition he holds is much greater in civilized than in barbarous peoples. Some savages in South America flatten the heads of their children in a monstrous manner by artificial pressure; but this, we are told, has no perceptible effect in diminishing the intellect of those who survive it. Then, again, Dr Browne cites authorities to prove that insanity is almost unknown among some tribes of the American Indians. Yet we may safely assume that cranial injuries are not uncommon among those savages; and thus, if the facts are correct, the greater predisposition of civilized man to become insane under injury to the nervous centres must be held to be proved. Dr Browne thinks that the greater size of the head of the male infant renders him more liable to injuries at birth than the female infant. "The diseases of the nervous system," observes Dr Farr, in the Registrar-General's Second Annual Report, "are twenty-three per cent. more fatal to males than to females, the chief difference arising from the diseases which affect children."

"Sir James Simpson," Dr Browne observes, "did not trace the pernicious effect of the greater compression of the male head at birth for more than a short period thereafter; but arguments might be adduced to prove that they may really extend over the whole of life, and either directly or through the instrumentality of convulsions, or some other intermediate disorder, eventuate in no immaterial propagation of mental aberration." Of course, Sir James Simpson would never have denied the possibility of such distant effects. As a general rule, we are disposed to think that if the child escapes in infancy, there is not sufficient proof that it is in much danger of suffering in later life.

We wish Dr Browne had attended more carefully to the relation
between the head of the child and the size of the pelvis of the mother. He quotes the well-known observations of Broca and others to prove that the cranial capacity of some nations has increased since the Middle Ages. It would, however, not be easy to prove that women nowadays run more danger in childbirth. The mean internal capacity of the skull in Europeans is 92·3 cubic inches, in Asiatics 87·1; yet childbirth is often very difficult with Asiatics, at least in India, and insanity is not unfrequent. Dr E. Weber has advanced facts to show that there is a relation between the shape of the head in different races and the shape of the pelvis; and it is not safe to assume, as Dr Browne appears to do, that while the size of the head has increased with civilisation, that of the pelvis has remained the same.

Dr Browne gives a number of interesting examples of the bad effects of slight concussions and oscillations of the brain in producing insanity in those already predisposed to it. His essay is well written, and if some of his conclusions appear open to question, this may be set down to the extreme intricacy of the subject.

There is a good paper on the Ophthalmoscope in Mental and Cerebral Diseases, by Mr Charles Aldridge, L.R.C.P. The following quotation will indicate the interesting nature of his observations (p. 89):—"Schroeder van der Kolk has shown that in all (?) epileptics there is a dilated condition of the capillary bloodvessels and secondary granular" (and fatty) "degeneration of the medulla oblongata. This dilatation he regards as the proximate result of the fits, and not as their cause. He also states that with these changes vascular dilatation of the brain, and particularly in the cortical substance, goes hand in hand. The small ganglionic cells become compressed by the dilated vessels, and perhaps also in consequence of the more albuminous nature of the intercellular fluid. Dulness and loss of memory are the results; or if, after a fit, an unusual current of arterial blood is supplied, we have following immediately upon the paroxysm, over-irritation, rage, and acute mania, which is present in so many epileptics. How well does all this chime in with the conditions found in the retinal circulation both during the paroxysmal and the interparoxysmal periods! The general condition of the retinal circulation, as has been shown, being one of passive hyperæmia, there is accompanying this, and in relation with it, a state of dementia; the dementia being most profound where the dilatation of the veins is the greatest. Again, the remark as to the mania or excitement following immediately after the fit in many cases being due 'to an unusual current of arterial blood' supplied to the brain, brought to my mind—for I had not seen Van der Kolk's book until my observations were completed—the case of M. H., No. 4, the details of which have been given above. I there noted that during the stage of excitement immediately following the epileptic fit there was increased vascularity of the disc, and that the arteries had become larger; and I remarked
upon the fact of this sudden restoration of the circulation being accompanied by excitement."

Dr Howden, in his pamphlet, observes—"Very marked injection of the membranes is often met with in patients maniacal, in the recent stages of general paresis, or after severe epileptic convulsions."

"Hæmorrhagic effusions in the pia mater are not unfrequent in epileptics; those I have met with are of old standing, and had assumed the character of rusty gelatinous deposits."

A noticeable feature about most of these reports is the hopeful view which is taken of therapeutic agents in insanity. In the observations on the physical action of nitrous oxide, it is thought that this gas "may be with advantage employed in certain forms of mental disease;" and Mr George Thompson, L.R.C.P. London, remarks that the Calabar bean has been extensively used in the treatment of general paralytic in the West Riding Asylum, and that in every case its employment has been attended with marked benefit. Mr E. Churchill Fox, in a short paper on the Use of Ergot in the Treatment of Mental Diseases, remarks:—"I have seen it secure rapid recovery from that singular and obscure condition, the status epilepticus." He is inclined to think that with bromide of potassium the fits are reduced in number and severity, but the paroxysms of mental disturbance are intensified and prolonged. The conclusion which Mr Thompson arrives at, in his article on the "Sphygmograph in Lunatic Asylum Practice," may perhaps be questioned by those who are prepared to thank him for his observations. "General paralysis of the insane," he writes, "is a disease which may be presumed to be owing to a considerable extent to persistent spasm of the vessels, which leads to change in their component elements, but more especially in the muscular substance. This persistent spasm, by reducing the amount of blood which can pass through the vessels to the parts to be nourished, prevents renewal of these parts, and, consequently, [causes?] wasting. The most rational treatment indicated is to relieve this spasm." Solus spasmus et simplex atonia—there is nothing like the sphygmograph for clearing up difficult points in pathology.

In an article on "Phthisis and Insanity," Dr Clouston's views, that tuberculosis is a frequent cause of insanity, are ably criticised, and the statistics on which he relied opposed by other statistics. Certainly it would be difficult to convince one who has observed a large number of cases of phthisis, as they occur in ordinary practice, that this disease has any special connexion with insanity. A man labouring under consumption has some reason to feel unquiet in mind, and it often brings with it poverty and many other evils known to be exciting causes of insanity; yet, take it all in all, we suspect that phthisical patients are not more apt to become insane than might be expected from their proportion to the general population, and that the depressing influences of melancholy, exhaustion, or confinement are the real causes of the frequency of consumption in asylums.
On the Treatment of Fractures of the Limbs. By Sampson Gamgee, Fellow of the Royal Society of Edinburgh; Surgeon to the Queen's Hospital, Birmingham. London: J. and A. Churchill: 1871.

This is, in many respects, a remarkable book. The author's name guarantees its cleverness, and assures us that we shall find in it strong individuality and originality of thought, expressed in bold but distinct language, unfettered by any trammels of reverence for tradition or false modesty.

The arrangement of the work, as the author himself points out, is fragmentary and irregular; there are many repetitions and redundancies, and the facts and deductions from them are not well systematized. We think the work would have been more effective had it been arranged as a treatise, and very much abridged.

The aim of the work is to introduce a very great and radical change in the principles guiding treatment of fractures of the limbs, which he believes, in this country, are usually treated on a wrong principle. Mr Gamgee enunciates three chief rules:—1. That the fracture should be instantly reduced; 2. That the limb should be subjected to instant and thorough circular compression; 3. That the fracture should be thoroughly, not partially, immobilized.

As to the first of these principles, he will find no surgical authority of any eminence to dispute the point. As to the third, we are all theoretically at one; but we quite agree with Mr Gamgee that, practically, many surgeons are most unsuccessful in obtaining—most are most careless in attempting—really to immobilize a fracture. We most thoroughly agree in every word he says as to the unsatisfactory nature of the retentive power of a Macintyre splint, and of most of the usual bandages and splints for broken humerus or broken femur. All his observations on these two heads are most admirable and sensible. In Syme's words—"the great requisites for treating fractures successfully are coaptation and immobility."

As we find that, as to two-thirds of his principles of treatment, Mr Gamgee is really at one with the great bulk of surgical authority, we with some difficulty understand the numerous allusions in various parts of the work to the radical differences between his method and that of his teachers and contemporaries, as these differences are really reduced to very narrow compass. He tells us—"I have been in the habit of treating the subject somewhat dogmatically, strong in the correctness of the principles to which I adhere, as opposed to those held by the vast majority of surgeons."—P. 5. "The numerous splints and contrivances recommended for different fractures of the upper and lower extremity are evidence of an imperfect understanding of the subject, and must eventually give way before a philosophical appreciation of its fundamental principles, and a correct estimate of their practical application."—P. 70. "The
yoke of authority in science is so enslaving and so fatally inimical to progress, that on no condition could I myself support it, or counsel its being borne, however eminent might be the intellect which sought to impose it.”—P. 144. Surely such a principle ought to prevent Mr Gamgee from promulgating, on his own authority, his new doctrines. Surely, if carried out to the full, we ought all to begin de novo, as if nothing had been written, nothing sitted by long labour and experience—as if, indeed, no science of medicine existed at all. Again, is there not a naïveté almost approaching innocence in this remark?—“It is curious how some of the best authorities, old and recent, agree in condemning what is right.”—Footnote to p. 146. A most enviable frame this, that is so certain as to what is right in spite of the best authorities.

Let us now see what this second proposition really means that is so much against the best authorities. It is the principle of circular compression, which, at page 83, is described as “The gentle and perfectly uniform, the evenly distributed, and in no way constricting action, which I understand by compression as applied in the treatment of fractures, is a therapeutic agent than which I know none more demonstrably beneficial.” We agree with every word of this. It is an admirable description of what a well-applied bandage and splint ought to be. Mr Gamgee differs from many surgeons in the boldness with which he applies them at once, while most surgeons wait for a day or two, or even more. We believe that, in principle, he is right, and those who wait are wrong; but in practice, and in the hands of the great majority of ordinary surgeons, we believe that Mr Gamgee’s plan would be dangerous, and the ordinary one more safe. With skill and care—with the practice which none but hospital surgeons can acquire—and, still more important, with a house-surgeon constantly at hand—we believe Mr Gamgee’s plan of immediate compression to be simple, pleasant, safe, and effectual; but, in the hands of the great majority of surgeons, and still more, in cases treated at their houses, and seen perhaps once a day or once in two days, Mr Gamgee’s plan has risks both to limb and life.

He supports his arguments by the recital of some most excellent cases, the headings of one or two of which we quote to show their nature. “Case 12. Fracture of both bones of the leg, with eversion, great swelling, and blebs. Application of starched apparatus. Rapid subsidence of swelling. Patient able to lift leg the fourth day; out of bed the ninth. Uninterrupted recovery.

Case 14. Compound and comminuted fracture of the leg from direct violence; great shortening—violent spasm. Immediate application of starched apparatus; able to get up on the eleventh day. Perfect union without any shortening, and very slight thickening.”

A well-written, but rather exaggerated, contrast is drawn in the sixth lecture between a case treated by leeches, fomentations, and utterly inefficient splints, and another treated on his own plan, much
to the advantage of the latter. It also includes a very interesting note on Vanzetti's treatment of inflammation by digital compression.

The remainder of the book contains practical directions as to the making and applying his splints, which any who intends to practise his method would do well to peruse.

A most ingenious pillow, devised by the author, on the basis of an idea of Mr John Grantham's, is thus described:—"My fracture pillow is of indiarubber, and is divided longitudinally into three compartments, of which the middle one is filled with water, and the two lateral ones with fine sand. A strip of thin board, with straps under the water compartment, admits of the lateral sand partitions being brought close round the limb, and the whole being suspended to an ordinary swing cradle. The elasticity of the central water compartment is a source of great comfort, and the long lateral sand-bags give very efficient support."

In conclusion, we would advise all surgeons to read this suggestive and interesting work, though we believe that the enthusiasm of a partisan may have to be toned down by the results of experience.

On the Treatment of Hyperpyrexia, as illustrated in Acute Articular Rheumatism, by means of the External Application of Cold. By Wilson Fox, M.D., F.R.C.P., etc. London and New York: McMillan and Co.: 1871. Pp. 78.

It seems to be an unfailing consequence of all advances in physical diagnosis, that they lead primarily to an over-valuation, from a therapeutic point of view, of the means by which they are attained; so it was with the stethoscope in times not long past, and so it seems now likely to be with the thermometer. The revelations of the stethoscope led to the assumption that the essential part of the disease consisted in the physical alteration of the parts affected; and the therapeutic abuse to which this gave rise, was the extension of a treatment adapted for certain well-known symptoms to all cases presenting the physical signs usually associated with such symptoms, quite apart from the actual presence of these symptoms. The result of this, as might easily have been foreseen, was a questioning of the utility of the therapeutic means employed, and an ultimate discarding of them—perhaps too completely. The revived use of the thermometer in disease seems likely to lead to somewhat similar results—an over-valuation of mere temperature as a therapeutic indication, and possibly an ultimate discarding of useful remedies, because pushed to extremes by over-zealous advocates.

We freely acknowledge the great advantage to diagnosis, prognosis, and therapeutics which result from a judicious use of the thermometer, and a rational estimate of its indications as one of a
series of valuable symptoms, but we demur to the tendency of the present age to regard temperature *per se* as the most important object of treatment, to the means most commonly advocated for this treatment, and to the reasoning adduced in support of the success of this treatment.

There is no doubt a degree of temperature necessarily fatal under all circumstances, just as there is an amount of consolidation of the lung also fatal under all circumstances; but just as we have lower degrees of consolidation proving fatal under certain circumstances, so we have also lower degrees of temperature which prove fatal—or at least accompany mortality—in certain conditions. But there is no reason to conclude that in these lower degrees it is the temperature *per se* which is fatal; it is only one of many phenomena which constitute the disease; it is really only a symptom—nay, only a consequence—of those phenomena which produce the fatal ending, and which may go on increasing, as in cholera, after that result has been attained. It is obvious that death, occurring with a temperature short of one absolutely and necessarily fatal, is not the result of the temperature *per se*, but depends upon other concomitant circumstances, the influences and effects of which must be duly considered in regard to the treatment to be employed with most hope of success; and it is also obvious that, under these circumstances, any treatment solely directed towards a reduction of temperature proceeds upon limited and imperfect views of the nature of disease, and if successful is only so because it has not interfered injuriously with the natural tendency to recovery. Now, if Dr Fox has found (p. 27) that the temperature of 106°, and Dr Murchison that of 106°·5, is uniformly fatal in rheumatism, while other observers have seen recoveries in other diseases, such as sunstroke, relapsing or scarlet fevers, subsequent to a much higher corporeal temperature—say 108° or 109°—it is self-evident that temperature in itself has not been the cause of death in these cases of rheumatism, and that treatment directed solely to the diminution of temperature has not proved successful because of its paramount utility, but only because, while possibly aiding in recovery, it certainly opposed no obstacle to that desired end. But even supposing the reduction of the corporeal temperature to be a paramount therapeutic necessity, it is surely a most unscientific procedure to treat the living body as we would a hot poker, and simply plunge it into cold water. We state only well-known physiological facts when we say that exposure to a minor degree of cold induces reaction and increased heat in the living body, while exposure to a severer degree of cold, if not followed by reaction, is uniformly fatal; and if this be the case in health, how much more likely is it to be the case in disease, when general vitality, as comprised in nervous reaction, is not inconsiderably lowered.” Indeed, even in sunstroke, which is accepted by Dr Fox as the analogue of hyperpyrexia from internal causes, it is not so much the direct cooling of the douche that is of importance, as
the reaction consequent on its use; for if the skin refuse to act after
the douche, the high temperature is maintained and the patient's
danger persists. But if the skin acts or perspires, it certainly does
not do so simply because it is cooled down, but because, by inter-
mittent douching with cold water, nervous reaction is roused, and
surface circulation and secretion restored. In *asphyxia neonatorum*,
which is somewhat analogous, pathologically, inasmuch as it con-
sists in pulmonary congestion with nervous depression, but where
there is no excess of heat internally or externally present, it is
necessary to supply this artificially to insure the complete success
of the cold douche in rousing the little patient to vitality; a very
inconsiderable prolongation of cold would be sufficient for ever to
prevent this revival, but by the alternate application of hot and
cold water, nervous reaction is exhibited in increased cutaneous
circulation, proved by the turgor and redness of the skin, and
firmness of the limbs, which are felt and seen long before vitality is
sufficiently restored to be evinced by a cry. Numbers of new-born
children have died from sudden or prolonged immersion in cold
water; and Mauriceau, in his treatise on Obstetrics, says that
infants have even died merely from baptism in cold water; yet
cold, properly applied so as to insure reaction, has proved one of
the most powerful means of rescuing even infants from a dangerous
state of asphyxia. During the Russian campaign, soldiers were
known to expire suddenly, as if thunderstruck, from excessive cold;
and at Smolensko more than thirty grenadiers thus fell dead when
attempting to set themselves in a line on a height beyond the
Borysthenes (Beaupré). Cold, in producing stupefaction, acts, as
Prosper Alpinus says, exactly as narcotics do; that is, it produces
internal congestions, chiefly of the brain and lungs, with coma and
asphyxia. Of course, in the treatment of disease, no one would
think of pushing cold to such extremes; but when the nervous re-
actions are weakened by the exhaustion of disease and defective
nutrition, as well as numbed by poisonous blood, a very much
lower degree of cold applied externally will suffice to produce dan-
gerous symptoms, and to attempt to treat the living body as we
would a hot poker, seems to court their advent. A prolonged cold
bath will effectually kill a healthy man—how much more speedily
may it not kill a sick man? All physiology teaches us that the
mere abstraction of heat is not only insufficient to restore a sick
man to health, but rather the reverse, unless the cold be so applied
as to insure vital reaction and an improvement in all the secretions.
This, however, seems hardly to be the tendency of modern teach-
ing, which, so far as we can understand it, seems to be that it is
only necessary to cool the body down to a certain degree, and
avoid if possible any reaction. It can only be by chance that any
such procedure is ever successful. It is many a day since Lancisi
wrote—"Nullum optimum ad curandos ab epidemicis febribus in-
ventum esse remedium ipso nivis usu;" since then many authors
have treated of this subject. Much may be learned from the works of Currie, Robert Jackson, Beaupré, etc., and it would be well perhaps to reconsider the whole subject from an eclectic point of view. The tendency of Dr Fox's reasoning is to show the paramount importance of the abstraction of heat from hyperpyrexial patients, but the tendency of his carefully-observed facts seems rather to confirm the importance of a judicious employment of cold in the rousing of the fast-failing nervous reactions; the return of free perspiration, and the reappearance of the rheumatic joint affections being, in his opinion, the most favourable symptoms—symptoms which, we need hardly say, cannot be regarded as simply the result of chilling the surface of the body, or lowering the general corporeal temperature. Our remarks, therefore, have been more directed against an over-zealous pushing of Dr Fox's views rather than against his own cases, which form a most remarkable contribution to the science of therapeutics, and are well worthy of careful study. What effect, if any, the large doses of quinine and the very considerable doses of brandy (20 to 30 oz. in twenty-four hours) administered had upon the course of the disease, and how this powerful stimulation acted, is also deserving of the most careful consideration in any attempt to estimate at its true worth the treatment employed, and to assign to each of its elements its proper value.

Psychic Force and Modern Spiritualism: A Reply to the Quarterly Review and other Critics. By William Crookes, F.R.S., etc. London: Longmans, Green, and Co.: 1872. Pp. 24.

In this pamphlet Mr Crookes attempts to combat his reviewers. It is not worth while to follow his arguments. In his preface he states that he wishes for the present "to be considered in the position of an electrician at Valentia, examining, by means of appropriate testing instruments, certain electrical currents and pulsations passing through the electric cable, independently of their causation, and ignoring whether these phenomena are produced by imperfections in the testing instruments themselves—whether by earth currents or by faults in the insulation—or whether they are produced by an intelligent operator at the other end of the line."

At p. 474 of the Quarterly Journal of Science for October 1871, Mr Crookes says:—"Before fitting up special apparatus for these experiments, I had seen, on five separate occasions, objects varying in weight from 25 to 100 lbs., temporarily influenced in such a manner, that I and others present could with difficulty lift them from the floor. Wishing to ascertain whether this was a physical fact, or merely due to a variation in the power of our own strength under the influence of imagination, I tested with a weighing
machine the phenomenon on two subsequent occasions when I had an opportunity of meeting Mr Home at the house of a friend. On the first occasion, the increase of weight was from 8 lbs. normally, to 36 lbs., 48 lbs., and 46 lbs. in three successive experiments tried under strict scrutiny. On the second occasion, tried a fortnight after, in the presence of other observers, I found the increase of weight to be from 8 lbs. to 23 lbs., 43 lbs. and 27 lbs in three successive trials, varying the conditions.” We venture to think that these experiments thus detailed will leave no doubt in the mind of any party of average intelligence as to the existence of an “intelligent operator at the end of the line,” though they may create great wonderment why so intelligent an operator should content himself with mystifying Mr Crookes, Lord Lindsay, etc., and not rather at once realize a fortune by a judicious employment of his peculiar art in the ordinary course of trade. We concede to Mr Crookes all honesty of purpose; but we need hardly say, that we deem him deficient in the only psychic force whose existence we acknowledge—the force of intellect.

Scheme for obtaining a better Knowledge of the Endemic Skin Diseases of India. Prepared by Tilbury Fox, M.D. Lond., F.R.C.P., etc.; and T. Farquhar, M.D., Surgeon-Major, H.M.S. Bengal Medical Service (retired). 1872.

The object of this document is to collect information relative to certain diseases specially prevalent in, or peculiar to, India. The document, which goes to India by next mail for general distribution, has been prepared by Dr Tilbury Fox, with the assistance of Dr Farquhar; the former is the physician to the department for diseases of the skin at University College, London; the latter lately held an important medical appointment in India.

The first section of the paper gives particulars as to the means by which it is suggested the desired information should be obtained, and the diseases to which reference is made.

Two main objects are proposed:—

1. To obtain and then to circulate a better knowledge of the more important endemic skin diseases of India, or such as principally attack the skin; and thereby

2. To bring about an agreement, which is far from existing at present, between the profession in India and in England as to the nomenclature, the typical character, the varieties, and the probable or demonstrated causes of the diseases in question.

At present very considerable confusion arises from the want of an agreement as to the use to which particular terms should be put—elephantiasis, for example—the same disorder being spoken of by different persons under different names, and vice versa. The
prevention of such confusion would itself be a great gain to science, as it would enable observers to conduct investigations relative to diseases with infinitely less labour, and unquestionably with greater certainty of obtaining reliable data.

One special though indirect result of the inquiry would be, that those who were training in this country for medical service in India would be enabled to acquire with no little readiness a satisfactory knowledge of a certain class of diseases of the commonest occurrence in India, the components of which class they would have at once to treat on their arrival in that country, and of which no great amount of clinical experience can be obtained in England. Further, the inquiry would be of great use to the English, and indeed the Continental practitioner, in furnishing him with valuable guides for the more speedy recognition and the better treatment of the numerous cases of peculiar skin diseases that are imported into this country and elsewhere from India, a locality that abounds in material for scientific investigation as regards the influence of climate, etc., upon cutaneous disease.

It is also believed that the inquiry may tend to show not only what are the special diseases due directly to special climatic influences, but also the character and extent of variations in the same disease induced by differences of climate: for the same disease does undoubtedly vary in its characters in different countries.

The mode in which it is believed the objects above stated may be easily secured is by seeking the assistance of the scattered and able medical officers of India in the collection of facts, and by utilizing and unifying the experience and opinions of these gentlemen.

The details of the scheme are so arranged as to remove, it is believed, some of the special difficulties under which the Indian medical officer labours in the prosecution of careful investigations into the nature and causes of disease in India; though in the face of very special obstacles it is surprising how good is the work done by Indian observers from time to time. Reference is specially made to the few opportunities afforded Indian medical officers of consulting libraries, of obtaining access to the multifarious periodical publications in which are recorded the most recent researches of European pathologists, the difficulty of carrying about with them the necessary apparatus for minute and experimental inquiry, and the like.

The difficulties referred to will be in great measure lessened by giving a résumé of the latest researches and the opinions of European dermatologists relative to the various diseases to which it is thought desirable to direct attention, and by indicating the points of doubt which require to be cleared up, and the line of investigation which should be pursued in the future, for the further elucidation of the nature and causes of particular diseases.

By indicating the several points upon which information is
specially needed, not only will the main objects of the inquiry be promoted, but the time and labour of Indian medical officers will be greatly economized.

The endemic skin diseases of India deserve to be studied for themselves for several reasons. They are a great source of distress to sufferers from their attacks. They are the indications in many cases of morbid action, affecting the body profoundly and arising from preventible causes, and when closely studied they afford valuable information as to the causation of disease. They crucially test the action of remedies, whilst a more correct and a wider knowledge of their causation will greatly tend to increase the confidence of man, be he European or native, in the power of hygiene and medicine to relieve suffering. It is not at all unlikely, moreover, from concentration of attention to this, till recently, but slightly cultivated branch of medical science, important facts may be gained as to the nature and action of remedies which are successfully employed by European and native medical men in India, but with which we are unacquainted in this country. Some of these remedies are frequently mixed up with inert or dangerously active substances which compose the lengthy prescriptions of the hakeem, in such a way as to render it a matter of difficulty to determine what the active powers of these medicines individually are. Already something has been done to define them by medical officers in India, who would be glad to see them attract the attention of the therapeutic student in Europe. Many good drugs have still to be recognised, and they are considered too valuable to be disclosed by the native practitioner. With the progress of education, however, it is to be hoped the prejudices that lead to secrecy will be overcome, and the real or pretended powers of many undisclosed remedies be fully understood.

The scheme is to give, under the head of particular diseases, a brief statement of the views of the leading European dermatologists as to the nature of these diseases, to indicate doubtful points and the chief questions to be now determined in regard to them, and to ask for answers to precise questions.

The following heads indicate the kind and extent of information sought:—

a. The accurate observation of cases, especially with reference to the exact mode of origin of disease.

b. The microscopic characters of morbid products.

c. Precise information, inasmuch as climatic influences have much to do with the genesis of disease in India, touching the name and character of particular localities in which particular diseases prevail, with exact statements as to the nature and alliances of those diseases. [Situated as medical men are in India, in such varying climates and among such different races acted upon by such a variety of circumstances as to food and clothing, etc., they might gather valuable information about the geography of diseases.
of the skin. It is notorious that sharp lines seem to cut some diseases off from different parts of the country. This is the case as regards severe forms of itch, leprosy, Madura foot, Delhi sores, etc.]
d. The nature and peculiarities of the food and water supply of the affected population.
e. The tribes or castes in which particular diseases occur, and the habits of these tribes, ex. migratory or otherwise. [The influence of certain religious practices, such as that of covering the body with ashes and other earths, as followed by fakeers and religious medicants, deserves notice.]
f. The occupation of the attacked.
g. The dress of the attacked, especially with regard to the exposed or unexposed nature of the seats of disease. [It is an interesting question, for example, to determine whether the diseases of the scalp are more frequently developed in races that cut short their hair and shave their head, or among those who keep their hair long. Many of the Hindoos shave all or the most of their scalp, head, arm-pits, etc. The Mahommedans, as the rule, cut the hair close. The whole Sikh race never allows a razor or scissors to touch a hair. Many millions of Indians go about bareheaded; others wear only skull-caps; others wear great masses of cloth like tablecloths on their heads. The influence of these practices on disease it is important to know.]
h. The observed differences between disease as seen in the darker races of the East and in Europeans, and the differences in the same disease as observed in India and in Europe.
i. The connexion between season and disease. [Where the variety of climate is so great as in India, the observation of this point might lead to important results.]
The following are the diseases to which it is thought desirable that attention should be directed:—
1. Morphea. 2. Scleroderma. 3. Framboesia. 4. Delhi sore. 5. Keloid. 6. Fibroma. 7. The Elephant leg, or Elephantiasis Arabum, or tropical big leg. 8. The Fungus foot of India, or Madura foot. 9. The true Leprosy, or Elephantiasis Graecorum. 10. Leucoderma. 11. Pityriasis vesicolar in unusual forms. 12. Burmese ringworm. 13. Malabar itch. 14. Lichen tropicus, or prickly heat.

N.B.—The information which is particularly asked for, is essentially such as is precise. Mere general statements or replies to questions of a wide and general character are specially undesired. Special forms for note-taking and for tabulating cases are given with the accounts of certain of the diseases. When sufficient data have been collected, a special summary report will be made on the whole subject. Each observer will

Vol. XVII.—No. IX.

5 P
have full credit given him for any contribution he may furnish towards the report.

Morbid specimens properly preserved, if sent home, would be thoroughly investigated and the results embodied in any report made. Representations of diseases in the way of photographs, sketches, and coloured drawings, will be very useful.

_The Skin-Milk Treatment of Diabetes and Bright's Disease; with Clinical Observations on the Symptoms and Pathology of these Affections._ By Arthur Scott Donkin, M.D. Edin., M.D. Durh., Lecturer in Medical Jurisprudence in the University of Durham, etc. London: Longmans, Green, and Co.: 1871.

There are no questions of more importance to the practitioner than those connected with the treatment of diabetes and of Bright's disease, both of them most interesting, important, and unfortunately too common diseases. In estimating the value of the present work from a practical point of view, we must first separate Dr Donkin's pathological from his practical researches, and next endeavour to ascertain the value of the latter. In the pathology of diabetes, Dr Donkin has not advanced beyond Pavy, and he ignores the researches of Flint, Lusk, and others, which have tolerably conclusively proved the glycogenic function of the liver to be a fact. Here, therefore, he is behind the age; not only has he not added anything to our knowledge of the causation of diabetes, but he is even ignorant of our present standpoint in regard to its connexion with the liver. The same may be said in regard to the mode in which the glycogenic function of the liver is affected by the nervous system; here he has not got beyond Dickenson, whose views he controverts, while he is wholly ignorant of the researches of Cyon and Aladoff, on the influence of the vagus in the production of sugar. No mention is even made of Cantani's views as to the elimination of sugar being the result of diminished combustion, though that is intimately connected with any rational theory of his own method of treatment. The essence of the pathology of diabetes seems, in the present day, to resolve itself into two questions: 1st, How far the appearance of sugar in the urine is the result of imperfect combustion or combustibility of the glycogen formed in the liver? 2d, How far this glycosuria is the result of simple increase of the glycogenic function—an increase which may be brought about in two ways, viz., by alterations in nervous action produced reflexly by derangements of other organs, or by organic disease of certain parts of the nervous system itself. All these causes we believe to be in action in different cases, and essentially to influence their curability; but Dr Donkin has not only not
endeavoured to ascertain their relative importance, but he seems entirely ignorant of their existence.

Dr Donkin seems also to take for granted that our present tests for sugar in the urine are all that could be wished, yet any considerable clinical experience would have taught him that this is far from being the case, that density varies even more with diet than with amount of sugar, and that all the tests—except perhaps those by polarization or fermentation, of which we ourselves have no accurate experience—are so fraught with fallacies as to be utterly unreliable in the ascertaining the presence of small, yet clinically important, quantities of sugar.

In estimating the value of Dr Donkin’s work, we must therefore restrict ourselves entirely to his cases; and here we are met by the remarkable facts that, during a period of more than two years, Dr Donkin has only met with seven cases of diabetes whose results he cares to record, in not one of which has the patient been restored to such a state of health as to be able to return to ordinary mixed diet containing starch, while three of these cases proved fatal, and a fourth gave up the treatment, the result being unknown. Of these seven cases, therefore, collected—and we must surely suppose selected, for diabetes is no uncommon disease—only three have attained a modicum of health consistent with a diet from which starch is still rigorously excluded. This we conceive is no result to boast of, and the attainment of which does not demand any such exuberant flourish of trumpets as is represented by the publication of a work of upwards of 300 pages.

The therapeutic employment of milk in disease generally is a matter dating from the days of Hippocrates, and which has been comparatively recently elucidated in our own pages, in an admirable paper by Dr Karell of St Petersburg (August 1866). While its special application to the treatment of diabetes has been recommended by Dr Headland since 1859, and insisted on by Dr Smart in 1863, the only absolute novelty in Dr Donkin’s views consists in his absolute exclusion of fatty matters from the diet—in the rigid employment of skim-milk. Fatty matters have not hitherto been considered as deleterious in the treatment of diabetes, but, on the contrary, have generally been recommended—our own experience, so far, leads us to confirm Dr Donkin’s views, and to regard fatty matters and eggs as both likely to be injurious; but this, as yet, is a mere plausible conjecture, and is not founded on any certain basis.

Even the very meagre statistics of Dr Donkin leave us in no doubt as to the value of the skim-milk treatment of diabetes; they show us clearly enough that it is not sufficient for a perfect cure, though the diet, from its nature, cheapness, and ready procurability, is in many cases an admirable means of keeping the disease in abeyance. On the other hand, it is a diet difficult to inaugurate in such ticklish customers as diabetic patients, and, from its insipidity,
mawkishness, and want of solidity, it is difficult to continue for any length of time. It is certainly not a cure, as Dr Donkin's cases themselves prove; for poor people of firm minds who value life for its own sake, it affords the only diet they are likely to be able to obtain by which life and a certain amount of usefulness may be maintained; but for those who are better off, there is no need to lead them through the risks of an apprenticeship to a milk diet, afterwards to be combined with animal food and green vegetables, when the same benefits can be obtained without the risks by commencing at once with this diet mixed as to quality and only restricted as to starch, which very few diabetics indeed can ever be brought to ingest with impunity.

In regard to the treatment of Bright's disease by skim-milk, Dr Donkin is inclined to restrict its utility solely to inflammatory cases; but these are precisely the cases in which similar symptoms are so often associated with very different pathological lesions. A simple catarrh of the tubules, even of some standing, is a very different pathological disease from a large white kidney; yet the symptoms may be identical; the one may be recovered from, the other never is,—the mere administration of milk is a matter of slight moment in the history of the case. A restricted diet is no doubt useful in warding off uræmic complications in all cases—it can only promote a cure when the disease is purely catarrhal or restricted to a limited portion of the kidney; but a restricted diet may be accomplished by other means more pleasing and more safe to the patient than by confinement to skim-milk. While not denying, therefore, the usefulness of skim-milk in some such cases, we may safely enough object to the dogmatism which would propound it as in itself curative on the strength of only five cases, one of which was fatal.

Recollections of Past Life. By Sir Henry Holland, Bart., M.D., F.R.S., D.C.L., etc. London: Longmans, Green, and Co. 1872. Pp. 346.

Sir Henry Holland has been a man singularly fortunate in his life. Blessed with a remarkable equanimity and moderation of mind, his early introduction into that elevated society amid which he has ever since continued to move, rapidly enabled him to attain the extreme of his worldly ambition as to wealth, while his extensive and well-grounded erudition enabled him to profit in a desultory sort of way by the conversation of the various remarkable and distinguished personages into whose company he was temporarily thrown, as well as by the remarkable cosmical phenomena with which he was brought into contact in the course of his annual travels. The result of this is this extremely in-
interesting volume, embodying the recollections of a life now in its eighty-fourth year, and spent amid the most remarkable personages, scenes, and events of that period which comprises certainly the most extraordinary historical phantasmagoria ever exhibited to mankind, in the rise and fall of Napoleon I., and all involved in that as well as in the lamentable episode of the early history of Queen Caroline as Princess of Wales. Apart from the scientific history of this period, which in itself is of no mean value, its social history involves the consideration of problems which can never fail to interest mankind, and, as revealed by one so much behind the scenes as a fashionable, trusted, and, clear-headed physician, must possess an importance which cannot fail to ensure a widespread and intelligent auditory. We shall not spoil the interest of this work by imperfect quotations; it must be read to be enjoyed, or even understood; and we cordially recommend it to our readers as alike interesting and enjoyable.

A Treatise on Haemophilia, sometimes called the Hereditary Haemorrhagic Diathesis. By J. Wickham Legg, M.D., etc. London: H. K. Lewis: 1872. Pp. 158.

Dr Wickham Legg is already favourably known as an accurate and trustworthy observer of medical facts, and this little work will certainly add to his professional reputation. The disease he treats of is one concerning which not much is known reliably, and we cannot indeed say that Dr Legg has added much to our knowledge of the subject; but in presenting us with an analysis and epitome of 163 authors who have written on the subject, he has deserved well of the profession, and has laid a foundation upon which himself or others may hereafter securely build. As yet, however, we have obtained no clear idea of what this disease is, nor whether it is essentially a disease or only a symptom, and the proper treatment is, as we may conceive, still a desideratum. Apart, however, from all practical value which it does not possess, this little work has a literary value which can scarcely be over-estimated, and Dr Legg deserves all credit for the labour he has bestowed on the production of a monograph which hereafter will constitute the chief repertorium of facts connected with this disease, which is not more interesting than obscure, and whose very name is a riddle—for what is the signification of haemophilia? And whence is its latter half derived? Φυλία is said to be the derivation, but it seems so senseless and absurd, that we hope etymologists may succeed in discovering some more intelligible origin for it.