PART SECOND.

REVIEWS.

Elements of the General and Minute Anatomy of Man and the Mammalia, &c. By Fr. Gerber, &c. &c.

(Continued from p. 287 of last Number.)

Passing over 52 pages of Dr Gerber's work, in which the author speaks of the different constitution of cells as found in the ovum and epidermic structures, according to the arrangement of Schwann, we resume our analysis at the 169th page, where he treats of cartilage.

"Cartilages are divided into permanent and ossific. The former, as their name implies, persist, as cartilages, to the time of old age; the latter, at a shorter or longer date, are converted into bone. Examined microscopically, they present three kinds of intimate structure: 1st, In one we observe cells, or cartilage corpuscles, as they are called, scattered through a hyaline or intercellular substance,—cellular cartilage; 2d, In another, the cells or cartilage corpuscles, instead of being dispersed through a vitreous matter, are scattered betwixt the meshes of a reticulated fibrous matter,—reticular cartilage; 3d, In a third, the texture is a mixture of the reticular and simply fascicular, the intersection of fibres being here very great, the fibres thus running more in the manner of those which make up the elastic tissues,—fibrous cartilage." P. 170.

The transformation of cartilage into bone is thus described:

"Whilst those parts of the cartilage that are remote from the point or points of ossification, are remarkable for a regular dissemination of cartilage cells through their substance, those that are close to it exhibit a clustering or agglomeration of these cells, separated by an apparently homogeneous intercellular substance. In these clusters, it is not difficult to distinguish cells of older and more recent formation, simple, and united or blended cells, and smaller and larger isolated nuclei. Where the ossification begins, these clusters are more closely crowded, and are ever more and more distinctly surrounded and enclosed by a delicate line. These lines, speaking of them in the plural, probably indicate primary or parent cells, those cells which arose in the fetus on the first formation of the cartilages, and within which the secondary cells, the prime means of growth in reference to the cartilages, have arisen." "Whilst the bone corpuscles appear in bone in progress of formation, the cartilage corpuscles disappear, and bone cells are produced in their stead, and thus fill the entire spaces. The ossification of the fetal cartilages proceeds entirely in the same manner. The cartilage corpuscles, ever more and more crowded together and compressed, cede the space they formerly occupied to the increasing osseous substance. This grows constantly
more and more opaque, bone corpuscles make their appearance, then vessels, &c., and the bone is achieved.”

“This process may be explained in the following manner. The secondary hyaline substance, an element included within the primary cells, and in cartilage not to be distinguished from the parietes of the parent cell, is constantly dissolved, and in the fluid state permeates or transudes the walls of the primary cells, now become invisible, or it coagulates on the inner aspects of these cells. Out of this cytoblastema, cytoblasts (the bone corpuscles) are formed by coagulation and organization of the now hyaline substance; and from them are produced the bone cells, which comport themselves in the same manner as the embryonic cartilage cells; in other words, they form a cellular mass, without any interposed matter or intercellular substance. Whilst the recently formed bone cells are growing, new cytoblasts arise between them and the shrunk parent cells, in the mass of cytoblastema, which is incessantly prepared by the transudation of fluid through the walls of the parent cells, or, it may be, which is laid up by coagulation upon their inner aspects. The cartilage corpuscles, as said, ever more closely pressed together, disappear; the nuclei of the bone cells acquire all the while calcereous salts, and become opaque; the bone cells themselves appropriate salts of the same kind; radiated points, nutrient vessels, &c. make their appearance, and the bone is fully formed.” Pp. 178—80.

Thus the bone corpuscles of Müller, according to Dr Gerber, are the nuclei of bone cells, and the canaliculi, or radiations from them, extend on every side to the confines of the cells. True bone is always formed in the manner described, in the ossification of embryonal cartilages, in the renovation and repair of broken bones by exudative inflammation, in the ossification of the cartilaginous epiphyses, in the more tardy ossification of the costal cartilages, and, finally, in that of the permanent cartilages in advanced age. On the other hand, such bony concretions as are sometimes formed in the arteries in the dura mater, ossified glandular cysts, &c., have never the structure of true bone.

The author now enters upon the description of the structure found in teeth. On this subject we need not enter, as English anatomists have pushed their researches regarding it much further than Dr Gerber seems to have any idea of. The valuable and laborious researches of Goodsir, Nasmyth, and Owen, must also be well known to our readers. The tissues are next spoken of; and, first, of the elastic, fibrous, and cellular tissues.

“By cellular substance, we understand the matter of which the fibres of the cellular tissues consist: by cellular tissue, the various compounds that result from the crossing or intertexture of these fibres. The structure which is commonly called cellular substance, is an extremely compound body, and, besides proper cellular fibres, contains blood and lymphatic vessels, serous fluids, blood and lymph, fat, nerves, &c. Cellular substance is a soft, moist, glutinous, elastic, white or grey coloured, and very transparent material. The peculiar delicacy of its elements gives it a certain resemblance to thick mucus. It forms cavities (arææ, areolæ) of various sizes, which are more or less completely filled with serum of fat. It seems to possess a certain degree of organic contractility, i.e. an inherent power, on the application of certain stimuli, of shrinking in bulk. It has, however, little ordinary sensibility. Delicate reticulations of blood-vessels and lymphatics extend in all directions and in great abundance betwixt its fibres and laminae; it is, therefore,
upon occasion apt to increase greatly in quantity, and is readily reproduced. The branches of nerves, which are visible in the cellular substance by the naked eye, do not belong to it, but merely pass through it towards other organs. In consequence of the delicacy of its elements, it possesses considerable powers of adhesion and capillary attraction, so that it is often seen to become rapidly and greatly distended with watery fluids from neighbouring parts. Its meshes and areoles are more or less connected through the entire body, so that air or watery fluid permeates it readily, the watery fluid, by reason of its specific gravity, falling down and infiltrating the most depending parts." Pp. 211, 212.

Mr Gulliver adds the following note:—

"After the cellular tissue has been completely destroyed, it is generally not reproduced. Witness the adhesion, as it is termed, of the scars of old deep ulcers to bones,—a circumstance which is often considered as a sufficient cause for the rejection of otherwise eligible recruits for the army; and every one is acquainted with the depressed cicatrices, where there is a want of subjacent cellular tissue, which follow various sores, particularly those in which there has been sloughing of the cellular substance. Independently of ulcers, the cellular substance of some parts, as of the legs, appears to be liable to atrophy, so that the limbs become hide-bound." P. 212.

In what has been said on the serous and synovial membranes, as well as on tendons and ligaments, we find nothing to detain us.

The author then enters upon the consideration of muscular structure. The muscles are divided into organic, animal, and mixed.

"Examined microscopically, the organic muscles are found in general to consist of delicate yellowish and coloured transparent fibres, with very faint boundaries, which, like the round fibres especially, though singly cylindrical, are flat or prismatic when united into bundles, the pressure of the several fibres giving them this figure. The fibres seldom run stretched out, and united into round bundles; they are far more commonly bent sinuously, or are even cramped and combined into flat cords. In a higher degree of rigidity, they are often irregular and shortly bent, by which they acquire the peculiar angular character which H. R. Ficinus has so faithfully represented in his figures. The fibres and bundles open and close under the mucous membranes in the manner of nervous plexuses, and for meshes in which mucous glands lie embedded, or they surround these like loops. The muscular bundles lying in the same plane from muscular membranes, which are disposed one over the other in two or three layers, the component bundles of each always crossing those of the other obliquely or at right angles, thus forming networks or gratings.

"In this manner appear for the most part the fibres of the muscular coat of the oesophagus, near the stomach; of the stomach itself, and the intestinal canal, with its immediately derived ducts, the hepatic and pancreatic ducts; of the urinary bladder and the ureters; of the vesicula seminales and vasa deferentia; of the trachea and bronchi; and of the middle coat of the veins and lymphatics." Pp. 233, 234.

The fasciculi of the voluntary muscles are characterised by transverse markings or striae, as also the muscular substance of the heart, and that of the oesophagus near its ventricular end. According to the author, "no voluntary muscle without trans-
verse streaks is known,”—a statement which is directly contradicted by Mr Gulliver, who says,—

"Many fibres of voluntary muscle are without these streaks. Such fibres appear to be composed simply of irregular granular matter inclosed in a sheath, (sarcolemma,) without the least appearance of primitive fibrils. In the pectoral muscle of the long-eared bat, (Plecotus auritus, Geoff.) examined immediately after death, almost all the fibres were of this character." P. 235.

It is now well known, that each muscular fibre, as seen by the naked eye, is made up of a greater or less number of fasciculi, (primitive fasciculi,) each of which is surrounded by a delicate sheath, (sarcolemma.) The fasciculi are again composed of numerous minute fibres, from $\frac{1}{40}$th to $\frac{1}{400}$th of a line in diameter, (primitive fibrillae.) These fibrillae are, according to the author, composed of granules which appear elliptical in the relaxed muscle, their longer diameter then corresponding with the long axis of the fibre; but during the action of the muscle, they become flattened pomegranate-wise on their contingent surfaces. The granular appearance he attributes to the short sinuous bendings of the fibrillae.

Dr Gerber seems to think that the transverse striae are at times dependant on the granules of the ultimate fibrillae being arranged in transverse rows, at others, on the presence of a wrinkled sheath, and sometimes on fibres being wound spirally round the primitive fasciculus; at least such is what we gather from the following paragraph:—

"If the granules of the associated primary fibres present themselves arranged in transverse rows, and the common transverse connecting lines become forked in consequence, so that the spheroidal granules project in higher relief along the contracted primary fasciculi; the fasciculi then appear more or less regularly striated transversely. These relations appear not to obtain beyond the surface of the primary fasciculi; at all events, the appearances in the deeper bundles are so far modified, that the cross streaking seems frequently to depend on the presence of a wrinkled fascicular sheath; for when the more superficial fibres chance to be removed, and the deeper ones exposed, these appear cylindrical, and the bundle at the part is longitudinally streaked. At the extremity of a torn fasciculus, too, the peripheral fibres often appear so distinctly marked off from the internal and more pulpy substance, that the existence of a more compact transversely streaked sheath can scarcely be called in question. For the accuracy of this view of the matter, the observation of fibres wound spirally about the primary muscular fasciculi of the dog is a farther and strong assurance; so also is the observation of Professor Valentin, to which fig. 80 bears reference. The suspected sheath, I believe to consist decidedly of granular fibres, which may, however, by possibility be separable in two directions, viz. transversely and longitudinally, according as the union of the neutral connecting medium of the granules in the peripheral layer is more intimate in the long or in the transverse axis of the fasciculi. Every trace of granules disappears from the animal muscles, even after boiling, under the action of the oil of turpentine, continued for a day or two." Pp. 241, 242.

Thus then the author seems to consider the transverse mark-
ings to be sometimes dependant on one kind of structure and
sometimes on another,—a conclusion to which we are decidedly
opposed. The transverse striæ are common to voluntary muscles
in all the vertebrate and higher tribes of the non-vertebrate
animals; and a phenomenon so uniform in its appearance, must,
we conceive, be always dependant on the same structural arrange-
ment. The author, however original he may be in ascribing it to
different causes in various animals, or different parts of the same
animal, is certainly not so, in referring the striæ to the granular
fibrillæ, the sheath, or the spiral fibre wound round the fasciculus.

On examining into the observations made upon this subject,
we find that the explanation of the transverse striæ has exer-
cised the ingenuity of microscopic observers ever since the days
of Leuwenhoeck, who ascribed them to circular contractions in the
primitive fasciculus. By Prochaska they were attributed to the
impressions made by the vessels, nerves, &c., on the surface of the
muscles. Fontana speaks of them as ridges or diaphragms caused
by the contractions of the fibrillæ, and their then assuming a globu-
lar form. According to Treviranus' first opinion, they were ridges
formed by the shortening of the cylinders; latterly he ascribed
them to folds of the membrane investing the fasciculus. Prevost
and Dumas also attributed them to the presence of this sheath.
Turpin thought that the bundle of primitive fibrillæ, constituting
the fasciculus, was surrounded by an aponeurotic gut, which was
firmly puckered or folded across; hence the appearance of striæ.
Mandl was the first to describe a filament of cellular tissue wound
round the fasciculus in a spiral manner, causing alternate white
and dark transverse lines. Mirbel attributed the striæ to granu-
lations on the membrane of the fibre—Wagner, to furrows on
the surface of the fasciculus. According to Schwann, the primi-
tive fibrillæ have a varicose structure, to the opposition of which
the transverse striæ are to be attributed. Prevost describes
them as rings surrounding the fasciculus. Skey considers them
to be the woof to the warp of the longitudinal filaments, to
which they are intimately united. Bowman is of opinion that
the fibrillæ are composed of globules, and that the adaptation of
these side to side, constitutes the transverse striæ. From the re-
searches of this observer also, it would appear that the striæ are
the edges or focal sections of plates or discs, arranged vertically
in the course of the fasciculi, each of which is made up of a single
segment from every fibrilla. He notices also, as does Gerber,
that the fasciculi crack across in the direction of the transverse
striæ, or split up in the direction of the fibrillæ, according to the
amount of cohesion endways or sideways, existing between the
segments.

The description of Bowman entirely corresponds with our own
observations. Indeed, we are at a loss to conceive how obser-
vers could ever have attributed the striae to the sheath, inasmuch as they are well marked on isolated fibrillae. Any one may, in a moment, convince himself of this fact by crushing, between glasses, and examining microscopically, a small portion of muscular fibre from a boiled haddock, in which animal we have always found it very easy thus to demonstrate the ultimate fibrillae. The discs also described by Bowman admit of ready demonstration, after keeping some portions of human muscle in spirits for a few days, when they may readily be shown isolated. We are somewhat surprised indeed that there was not added to the work some condensed account of Bowman’s researches, in whose opinions regarding the transverse striae, so much at variance with those of our author, we fully concur.

Dr Gerber describes and figures the ultimate fibrillae in the masseter muscle of the horse, as being coiled up like intestines within two distinct sheaths. We should like to have had farther details upon this point, as such an appearance is quite different from what has hitherto been seen by any other anatomist. He also speaks of crescentic or half-moon-shaped bodies, from of a line in diameter, with rounded heads, centrally raised, and blunt-pointed tails. Can he mean, by these bodies, the corpuscles, described and figured by Bowman as cells arrested in their development? In our opinion, the author ought to have been more explicit on these novel points. It is singular, also, that he has not figured the curious bodies latterly alluded to.

Dr Gerber now speaks of the tubular or hollow filamentous tissues, to which division belong nervous, vascular, and glandular structures.

We must here remind our readers, that the researches of Ehrenberg, confirmed by those of Treviranus, Remak, Burdach, Valentin, and others, have demonstrated that the grey matter of the nervous substance is essentially granular, and that the medullary matter is tubular. It is also admitted that the white matter of the nervous centres, and nerves of pure sensation, are composed of varicose tubes, and that of the motor and spinal nerves, of medullary or cylindrical tubes. Both these kinds of tubes contain a fluid, the physiological action and use of which is unknown. Now, that portion of Dr Gerber’s work dedicated to the description of nervous structure, contains much original matter; and, should the statements brought forward be confirmed, will have added much to our knowledge of this subject. They are of such a nature, however, as must excite great caution amongst anatomists. Speaking, for instance, of the elementary tubes of the nerves, he says,

“More particularly examined with the assistance of high powers and artificial light, a more delicate investing membrane is discovered, within the outer thicker and sharply defined one of each particular fibre. This fine membrane appears to be composed of, or, at all events, to be covered by a ciliary
epithelium, the ciliæ of which lie very obliquely, and apparently in spiral lines upon its inner aspect.” Pp. 256-7.

In a note, it is stated,—

“If this structure be confirmed by the observations of others, the nerves would come to be ranked among the true vessels. The peculiar structure in question was first noticed by Professor Valentin, in a course of observations upon the nerves of living animals, which we had undertaken in common about a year ago. The object of the movement of the nervous fluid in the interior of the tubes, supposing it to be continued backwards from the ultimate loops, would be precisely that which is accomplished by the heart in regard to the blood—a constant, though, perhaps, slow change of the contents of the nerves from the centre towards the periphery, and from the periphery towards the centre.” P. 257.

Since reading the above paragraphs, we have sought for these ciliæ in various animals immediately after the extinction of life, and taken every precaution to find them, but in vain. We have thus examined the nervous tubes, under magnifying powers varying from 250 to 4000 diameters—not that we consider any enlargement after 800 diameters of much use—but simply that we might not neglect any means in our power to detect them. We cannot, therefore, but regret that the author has not entered into more details upon this interesting point, or indicated the proper means to be followed in order to insure success in the examination.

The peculiar form of the varicose tubes has by some been ascribed to the pressure employed in demonstrating them. No doubt, pressure increases the size and number of these varicosities, but the disposition which these tubes have to assume this form may be considered highly characteristic. The author seems to attribute it to an irregular contraction of the nervous tubes soon after death, “probably in consequence of the unequal density of the contained fluid after its coagulation.” No arguments or reasons, however, are brought forward in support of this supposition. At the same time he looks upon this moniliform state of the “fibres” as the natural condition, at their origin in the brain and spinal cord, a circumstance which, if true, cannot but be considered as inconsistent with the opinion previously expressed. He has figured these tubes (fig. 89) as giving off branches in a dichotomous manner, with the granules of the brain attached to them, like berries to a plant;—the whole presenting an appearance such as we have never seen in the numerous demonstrations of the brain we have made, and which, if drawn from nature, cannot but be regarded as somewhat singular.

The question, whether any different structure exists between the motor and sensitive nerves, is thus disposed of:—

“The most careful examination discovers no difference in the structure and appearance of the bundles and their fibres, whether they be connected with sensation or motion.” P. 259.
Regarding the central terminations of these tubes, it is well known that Valentin describes them as forming loops at the line of contact of the white and grey substances. We have in vain looked for these loop-like terminations in the nervous centres, and, moreover, have never met with any anatomists who have been more fortunate than ourselves. Burdach thought that they were produced by the pressure of the scissors which Valentin employed in slicing the brain. At all events, it is singular that Gerber, the companion and demonstrator of Valentin, however positive he may be on other points, here speaks with some caution. He says, "in all probability, two fibres of the roots of the nerves form a loop in the brain and spinal marrow, as they do in the periphery" (p. 258); hence we may conclude that great obscurity still hangs over the central terminations of these tubes.

In their course, the ultimate tubes or fibres of various nerves blend in various ways with one another, constituting different kinds of nervous mixtures, such as could not have been conveniently formed at the commencement of the trunk.

"The nerves which proceed from different parts of the central system, and unite in this way in their peripheral expansions, generally combine in retes or networks, giving and receiving alternately bundles and isolated fibres from neighbouring branches; these bundles and fibres, however, merely joining with each other, and proceeding side by side, never anastomosing and blending into single trunks, as vessels do when they meet. The primary or ultimate fibres of nerves, in fact, only form loops or circles; they never end; the mutual interchange of bundles and single fibres is often extremely complicated, but no one is ever lost; it either returns upon itself, or joins some neighbouring fibre or fasciculus, and so begins its backward course to the central system whence it had proceeded. The reticular unions of the nerves are universally designated plexuses, which are of different kinds:—1st, Plexuses of the roots; 2d, Plexuses of the trunks; 3d, Plexuses of the branches; 4th, Ganglionic plexuses; and 5th, Terminal or peripheral plexuses." P. 260.

With respect to the peripheral terminations, the author observes:—

"From the ultimate peripheral plexuses of the nerves, individual primary fibres at length take their departure, and form terminal loops; or otherwise, the finest fasciculi and cords resolve themselves into primary fibres which form the terminal loopings, these being always constituted by two primary fibres from the same or from different fasciculi. Such final loopings present themselves wherever peripheral nervous influence or impressibility is manifested; for the nervous workings in the various organs depend, not upon the trunks, branches, ramuscles, or even the most delicate fasciculi, but upon these final loopings of the nerves, which are, therefore, the necessary media by which the motory nerves elicit motion, and the sensory nerves convey sensation. The pain or impression produced in the point of the trunk of a nerve which is irritated, and which usually accords in kind with that which belongs to the peripheral expansion, depends, as my discovery has shown, upon the presence of terminal loopings in the fasciculi themselves; nervi nervorum, in short, which stand in the same relation to the nerves as the vasa vasorum do to the larger blood-vessels." P. 261, 262.
The discovery of nervi nervorum is interesting, and the explanation of phenomena founded upon it ingenious.

In the peripheral structures of the body the nervous tubes form loops, generally of considerable size.

"From a terminal fasciculus, which generally runs parallel with the muscular fasciculi, primary fibres proceed, and forming wide arches across the line of the muscular fasciculi, associate with another nearer or more distant nervous bundle, and begin their backward course." P. 262.

The different loops so occasioned assume different forms, which are thus described:

"The final loops of the nerves of sensation, those of touch in especial, are less open than the final loops of the voluntary muscles. In those that surround and that penetrate the bulbs of the hairs, the loops seem even to be completely, or all but completely closed; those of the pulps of the teeth are also, according to Valentin, but very slightly open, and, like the loops in other situations, are formed now from primary fibres proceeding from and returning to the same bundle, now proceeding from one, and returning to different and more distant bundles. The final loopings in the less sensitive portions of the skin comport themselves like the associated capillary inclusions of the blood-vessels, which have long been familiarly known; and where the final loops resolve themselves into many subordinate or smaller ones by doublings and convolutions, for the purpose of forming a multiplier for the peripheral nervo-electric function, as they do in the tactile papillae, the peripheral distribution of the capillaries will be found to be of the same description. The highly sensitive tactile papillæ seem often to consist of a single greatly-convoluted primary nervous fibre. Fusiform multipliers of the same kind are occasionally found in the course of straight primary fibres. Several shortly convoluted terminal loops, disposed like the segment of a sphere, sometimes form the rosette-like nervous or tactile papille, which are exhibited in figure 101. Between such tactile rosettes, or capitate nervous papillæ, we sometimes observe simple loops included; for example, in the finger of man." Pp. 262—264.

The above description is well illustrated by plates 17, 18, 19, and 20.

The author is of opinion that the cellular fibres which surround the oftentimes scantily-distributed primary nervous fibres; may serve as subordinate means for conducting the nervous influence, which he considers likely to be the case from the constancy of their presence. For our own part, we see no necessity for this assumption, and consider that the neurilema of nerve, as well as the sarcolemma of the primitive fasciculus, may be present without participating directly in the functions of the structures they enclose.

In the grey matter of the brain and spinal marrow, besides blood-vessels, albuminous granules, grey organic or naked fibres, and nascent roots of nerves, which form the largest portion of the mass, are found bodies which have been termed ganglionic cells or ganglionic globules. They resemble, in appearance, the unimpregnated ova of the ovaries. They are found in the grey matter of the brain and spinal cord; in the trunks of nerves
where the roots of sensitive and motor nerves come in contact; in the course of the grey organic nervous trunks; the sympathetic, and in the peculiar ganglia of the trunk and branches of the sympathetic nerve.

The following paragraph affords a good example of the method in which Dr Gerber summarily settles some of the most interesting and difficult physiological questions. Whether such positive assertions, unsupported by observations or experiments, will satisfy our readers, we know not; we must confess that to us they are far from convincing.

"Peripheral impressions are transmitted to the nearest ganglion with which the part of the periphery impressed is connected, and are received with or without consciousness, according as the receiving grey or ganglionic substance is contained in the brain or spinal marrow, or in one or other of the disseminated ganglia. From thence follows, in a centrifugal direction, the nervous reaction which is proclaimed or manifested by motion, reflex motion, or by phenomena or actions of other kinds, either in the impressed periphery itself, or in its neighbourhood, or in some more distant parts only connected with that peculiarly impressed, in virtue of the general association which makes one whole of the nervous system." Pp. 266, 267.

The author now enters upon a consideration of the vessels, and first, of the lymphatic and absorbent vessels. The following passages, describing the structure and relations of the mesenteric glands, present that mixture of fact and hypothesis which we regard as, in some measure, characteristic of the German school:

"The glands which we observe in such numbers at the root of the mesentery, and which are therefore called mesenteric glands, are, like the conglominate or lymphatic glands, in general convoluted or plexiform masses of lacteals, assuming the appearance of solid fleshy organs. The mesenteric glands are interposed between the peripheral and central orders of abdominal absorbent vessels. By means of the glands in question, the chyle, probably with a view to its assimilation, is brought under the peculiar influence of the organic nerves, at the same time that it is in intimate contact with a large amount of living organic surface. The branches and subordinate divisions of the lacteals anastomose freely in these glands, and the finer twigs finally form a pretty uniform, close, and fine-meshed rete, which again gathering itself into minuter, and then into larger branches, these unite and produce efferent vessels, which carry the fluid onwards in its course. The mesenteric glands are well supplied both with blood-vessels and nerves, which pierce them at every point, and surround the various subdivisions of the lymphatics. The various constituents of the mesenteric glands, as now enumerated, are connected by means of cellular substance."

"The mesenteric glands, therefore, unite a portion of the periphery of the vascular and of the nervous system with their own proper substance, with the reticular mass of lacteal vessels of which they principally consist; and, as the efferent vessels proceed, after the formation of the glands, in the same onward direction as the afferent vessels, they may be held as standing in the same relation to the blood glands generally, as the fusiform nervous papilla stands to the more ordinary form; and as the primary bundles of the nervous ganglia open up, and resolve themselves into their primary fibres, which, after surrounding the ganglionic cells, again unite, and form an onward trunk, so the lymphatic glands, which have an analogous structure, are often, and not inappropriately, spoken of as lymphatic ganglia; and,
by an extension of the same views, the spleen, thymus, thyroid, and suprarenal bodies, are sometimes mentioned under the name of blood ganglia.” Pp. 274—276.

The following is important in a pathological point of view:—

"As the very finest lymphatics are still considerably larger than the system of intermediate peripheral blood-vessels in the passage of arteries into veins, wounds, abscesses, &c., may give occasion to the entrance into the general circulation of pus and other corpuscles of larger sizes than the blood discs. These flow readily enough on to the heart; but, forced into the lungs, they are apt to stick fast in the capillaries of these delicate organs, impeding the circulation through them, and, after the lapse of a few hours, giving rise to exudations and to the formation of cytoblast tubercles.

"Indubitably, also, stases of the lymph and chyle occur in the glands connected with the lacteal and lymphatic vessels, either in consequence of the coagulation of their contents, or of inflammation of the vessels themselves. The effects of such stoppages are not only frequently obvious among the larger glands in depostions of albuminous matter, the solvent of which, the serum, has been removed by absorption, but also in the innumerable peripheral false glands. In the scrofulous diathesis, it is well known to what extent the central as well as the peripheral lymphatic glands will enlarge; and when examined microscopically, their pathological contents, besides imperfect exudation corpuscles, present albuminous granules and amorphous coagula in quantities so much the larger, as the glands examined belong more completely to the periphery, and as the formation of fibrine seems to have been rendered difficult by the discrasy of the fluids or general cachectic condition of the individual.” Pp. 283, 284.

Considering what has been done in Germany with reference to the capillary or, more properly speaking, intermediary vessels, we cannot but think the description of Dr Gerber somewhat meagre. The following comprises what he has to say with regard to their mode of distribution:—

"The peripheral portion of the sanguiferous system presents itself under a variety of appearances, as a glance at the figures will render obvious. In general, it bears a close resemblance to the peripheral expansion of the nerves of the corresponding part of the body, inasmuch as the terminal plexuses of the nerves form a more or less continuous and closed rete, the meshes of which inclose similar meshes of the capillary arteries. The terminal loops of the nerves are also accompanied by very similar terminal loops of the arteries or intermediate capillary vessels. Even the particular forms of peripheral nervous distribution have their analogies in the peripheral vascular system.” Pp. 292.

We should like to have seen a good exposition of the views and researches of Berres, Krause, and Hyrtl on this point, which we do not think are much known or sufficiently appreciated in this country.

While speaking of erectile vessels, the following observations deserve notice.

"The spleen, like the male organ, is penetrated in all directions by a reticular fibrous tissue in connection with its general outer investing tunic. The spleen is beyond all question an organ susceptible of various degrees of injection with blood, and, therefore, of distension; but it is not an erectile organ in the same sense as the penis or clitoris; this, however, happens ra-
ether from the manner of its attachment than from any difference of structure. Were the spleen implanted upon a bone, it would upon occasion, and without any impediment to the return of its blood, become erected instead of being simply distended." P. 300.

Erection of the penis is thus explained:

"It would seem that neither the more rapid action of the arteriae helicine, nor the repletion of the venous rete of the corpora cavernosa in consequence of this, nor the action of the ischio-cavernosi muscles, nor yet the compression of the dorsal vein against the symphisis pubis, are competent to produce erection of the penis, although each and all of these acts contribute, and are indeed essential to the effect; but that it is principally and more immediately dependent upon the agency of those reddish fibres and fasciculi, which I regard as contractile tissue, which enter into the structure of the organ. I have already had occasion, oftener than once, to mention this tissue as presenting itself in the composition of the scrotum, where it is known under the name of the dartos, of the nipple, of the skin in general, and of the iris; and which appears everywhere to stand in a peculiar and especial relationship to the nervous system." Pp. 301, 302.

The intermediary vessels must always excite, in an especial manner, the interest of the pathologist. It is they which are directly concerned in the production of those morbid effusions and depositions which constitute organic disease. Of inflammation we have previously spoken. Another subject of equal importance, however, here claims our attention, namely, the deposition of tubercle.

Tubercles, according to the author, are conveniently divided into albuminous tubercles, fibrinous tubercles, and tubercles of a mixed nature.

"Albuminous or unorganized tubercles can only be produced from exudations abounding in albumen, poor in fibrine. They consist almost entirely of granules from the 1/100th to the 1/200th of a Paris line in diameter; but with the granular matter, nucleoli, nuclei, or cells, are mingled in quantity bearing relation to the amount of fibrine which the exuded fluid contained." P. 305.

If nucleoli, nuclei, or cells are found in this albuminous tubercle, we are not warranted in considering it unorganised. It may be non-vascular, but organization exists long before vascularity. A nucleated cell, for instance, must be regarded as a highly organized structure.

"What may be called false albuminous tubercles also arise occasionally within the substance of the secreting glands, in the granular degeneration of the kidneys for example. In the earlier stages of the disease, indeed, the albumen is deposited in the tortuous uriniferous canals of the cortical substance; in the fully formed disease, however, it is met with among and between the tissues also. The albuminous, or granular tubercle, is with great propriety often spoken of as the scrofulous tubercle, the disease being especially developed among scrofulous individuals." Pp. 305, 306.

We are of opinion that Dr Gerber has confounded together two different kinds of tubercle. That there is a granular and, strictly speaking, unorganised tubercle, we have often convinced.
ourselves, as must every pathologist who has examined this morbid product microscopically. No doubt numerous specimens of tubercle are to be found in which the change from the granular to the cellular variety is very gradual, and that more granules or cells may be present, according as the portion examined belongs to one form or the other. At the same time, it is necessary to make a distinction, and we hold that, whenever a distinct cell is observed in tubercular matter, we ought to class it amongst the cell tubercle.

The fibrinous tubercle is described as consisting of the plastic exudations from the blood-vessels into the different softer textures, which take place in consequence of impediments to the flow of blood through the capillaries. Taking degree of organisation as the basis of a division, the author distinguishes as varieties, the hyaline tubercle, the cytoblast tubercle, cell tubercle, the cellulo-fibrous tubercle, and the filamentous, cicatricular, or organised tubercle.

It must be evident that if these exudations are produced in the manner described, the same cause, namely, stasis in the vessels, must occasion inflammatory and tubercular depositions. Yet nothing can be more distinct than these two morbid products when examined microscopically. Hence, then, we consider the mechanical explanation of these productions erroneous, and herein find another powerful argument in proof of the opinion that this difference must in a great measure be attributed to some previous change occurring in the blood. What the author calls mixed tubercle we do not know, for although it constitutes his third division of tubercular depositions, he does not again allude to it.

Mr Gulliver's description of tubercle is much more consistent with our own observations, and we consider it to be a good one.

"It most frequently happens that tubercle exhibits no regular structure, so that the nicest examination can detect nothing more than a granular matter, minute spherules, and shapeless flakes or fragments; this is especially the case in caseous tubercle, whether occurring in the lungs or elsewhere. Sometimes the fragments, though still shapeless, are more distinct, yet unlike cells or their nuclei; and many of the minute spherules may be attached to some of the fragments. In smaller tubercles corpuscles are often seen, having much the character of cells or their nuclei; the envelopes are either absent or indistinctly blended with a minutely granular base. It is only in minute and recently formed tubercles that perfect cells exist; cytoblasts and cells, however, may sometimes be found at the periphery of crude tuberculous matter; and corpuscles, which are probably effete cytoblasts, are often present in softened tubercle. In tubercles of the most recent formation I have occasionally seen vesicles, and an aggregation of these sometimes forms a pretty large tubercle. These vesicles appear to me to be much more common in the lower animals, particularly in the quadruped, than in man; and they are most easily examined in transparent parts, as the omentum." Ap. p. 85.

We have frequently detected the vesicles here referred to in
the sputa of phthisical patients. They are very often perfectly round, although sometimes oval, as Mr Gulliver has figured them. Some of them contain smaller ones in their interior, which may be often seen floating loose in the field of the microscope on rupturing the parent cell. We also, with Mr Gulliver, have been much struck with their resemblance to hydatids, and if they should hereafter really be found to be animal in their nature, the opinions of Baron and Carmichael will have received positive confirmation.

We cannot conclude our analysis of this work, without attending to the valuable observations of Mr Gulliver on the blood-corpuscles of mammiferous animals and of birds. He has carefully examined, during a period of five years, and with the animals of the Zoological Gardens in London at his disposal, the blood corpuscles of 176 different mammiferous animals, and 204 different species of birds. From the facts he has brought forward, there can be no doubt that the globules vary in size even in the same species. This variation may be very great in disease, and is often sufficiently perplexing in health. But in the latter state, the change of size seems to be confined within certain limits. The corpuscles of the horse, for example, at different times are remarkably variable in magnitude, but never so much so as to render it difficult to distinguish them from those which are clearly intermediate in size, as of the rabbit and sheep.

He observes,

"It is not uncommon to see the majority of the discs of two remarkably distinct sizes, one about half or two-thirds the magnitude of the other. The larger appear to be the regular corpuscles, and readily run into the characteristic piles, which the smaller seldom do. The two sizes in question seems to be most frequent in blood obtained from dead animals. The smaller variety scarcely ever presents either the swollen edges or the cup-shaped appearance." Ap. p. 3.

Are not the smaller discs, here alluded to, the separated nuclei? He alludes to the account of Hewson, that the blood globules of young animals are generally larger than those found in the adult. He thinks, however, that this is only the case in embryos at an early period of existence, and he has sometimes found them smaller in the foetus than in the mother. The corpuscles of the elephant are the largest yet discovered, as was first pointed out by Mandl; next in size are those of the capybara, a rodent animal. The corpuscles of the napu musk deer are the smallest, and not those of the goat, as was previously supposed. They are slightly thicker in animals than in man. In the camelidae the discs are oval, a fact first discovered by Mandl in the dromedary and paco, and subsequently by Gulliver in the vicugna and lama.

The tables of measurements of the blood discs of mammalia and birds, occupying 53 pages of the work, present a strong
proof of Mr Gulliver's industry, and offer a mass of facts that cannot but be highly appreciated by all microscopic observers. We consider it, however, a matter of great importance to know positively, whether all the measurements were taken while the blood was perfectly fresh, and whether some of the smaller discs be or be not separated nuclei. We would take the liberty of requesting Mr Gulliver to make these points known to the scientific public, as a knowledge of these would render his labours more exact and more extensively useful.

The work is accompanied by an atlas of 34 plates, containing 294 figures. These, although not executed in a high style of art, are for the most part sufficiently characteristic. Their chief fault is harshness of outline, one, indeed, that has been noticed by the author, but which we are far from thinking so difficult to avoid as he supposes. A reference to the plates in Müller's Physiology, and those which illustrate the papers of Bowman and Barry in the Philosophical Transactions, will evince that British artists have succeeded in successfully transferring to paper much of the natural delicacy of structure as seen under the microscope. The plates illustrative of Mr Gulliver's papers appear to us more carefully got up, and to be more true to nature, than those which refer to the body of the text.

The length to which our analysis of this work has extended, will sufficiently evince our opinion of its merits. Although far from perfect,—although it contains statements and opinions to which we cannot subscribe,—notwithstanding many of the facts it announces are far from having been confirmed, we regard it as a valuable contribution to anatomy and pathology. It presents us with a systematic consideration of the structure of healthy and morbid tissues, written by an individual who has himself minutely examined the textures he describes. It is not a work compiled from the labours of others, but bears the indelible traces of original research and profound thought, and as such must demand the attention of all those interested in the progress of anatomy, physiology, and pathology.

We would fain hope that the appearance of this work will awaken the attention of the profession to the importance of microscopic investigations. The time has now passed when the pathologist can feel satisfied with the vague assertion, that inflammation exists in such or such a texture,—that induration is present here and softening there,—that one organ is infiltrated with pus, another condensed by depositions of lymph, or a third studded with tubercular matter. Knowing the impossibility of detecting in all cases the presence of inflammation, lymph, tubercle, &c., by the eye alone, the scientific pathologist now requires more positive information, and demands an investigation into the organised or unorganised particles of which every morbid pro-
duct is composed. By following such a system of investigation, it will be found that we shall be enabled to clear up many of the inconsistencies that at present obscure our comprehension of diseased processes; that we make a vast stride forwards in our pursuit of knowledge; and that, whilst we leave behind us much that is vague and uncertain, we at the same time enter upon a new field, as yet scarcely investigated by the scientific inquirer, and one which holds out the richest prospects of return to those who may diligently cultivate its soil.

On the Treatment of Stone in the Bladder by Medical and Mechanical Means. By R. Willis, M.D., of the Royal College of Physicians, Physician to the Royal Infirmary for Children, &c. 8vo. pp. 183. London, 1842.

When the title of this work, and the name of its author, first caught our eye, we felt somewhat anxious to ascertain the cause of a separate publication, on a subject with which Dr Willis's name was already well known to the profession. We imagined that since the appearance of his own excellent work on Urinary Diseases, and their treatment, in 1838, and the more recent edition of the standard work by Prout, nothing connected with the treatment of stone in the bladder had occurred in the interval, of sufficient importance to call forth a new book, on a subject apparently exhausted, and already so familiar to all.

A glance, however, at the volume itself, soon showed us, that it was in most respects of a very different character from that already published by the author; that it related solely to the treatment of stone when in the urinary bladder; and that it contained matter of grave importance for the consideration of practitioners. We moreover speedily observed, that the greater portion was a reprint of an article in the British and Foreign Medical Review for October 1841, which had already attracted our attention, and in which it appeared to us, that the author had, after a searching and unprejudiced inquiry into the history of lithotrity, given an account of the subject, exhibiting that method of treating stone in a very different light from that in which it had previously been seen by the public at large, as well as by the greater portion of the profession, who had all along exhibited a blind faith in the correctness of every statement regarding this most important subject, whether made by lithotritists, or by those who had casually enjoyed opportunities of witnessing their proceedings.

The present publication has been divided into five chapters, in each of which a different method of practice is treated of. Destroying the stone by solvents, removing it entire by the urethra,
piecemeal through the same passage,—by lithotrity, by lithotomy, and by dilatation of the membranous and prostatic portions of the urethra and neck of the bladder,—lithoctasy, cystoctasy, so named by the author.

The subject of "removal of stone by solution," has naturally attracted a considerable share of Dr Willis's attention, being in some respects more in accordance with his own particular walk in the profession; and he seems to us to prove incontrovertibly, that there is still a broad and interesting path open for investigation to the practical surgeons of the present day, who, as the Doctor justly observes in his preface, have a kind of monopoly of all cases of stone in the bladder, and are consequently the parties having the greatest opportunities of putting the efficacy of any particular mode of practice to the only satisfactory test, viz., that of demonstration on the living subject. The author has referred to the apparent apathy of the profession and of the public, regarding any mode of treating stone, excepting that by cutting, for, although from time to time various means have been proposed, with the view of relieving the unfortunate sufferer, without the necessity of resorting to the knife, it must be admitted, that whether from the inefficacy of the means proposed, or a reluctance to give up such a brilliant proceeding as that of lithotomy, few surgeons of any notoriety have ever given more than a passing thought or a sneer to the subject. "For a long time," says our author, "every means save those of a mechanical nature, appeared to have been lost sight of in the treatment of stone. Surgeons and the public seemed to have persuaded themselves that lithotomy, the operation always resorted to, was neither very formidable nor very dangerous in itself; whoever had a stone in his bladder, was cut as a matter of course, and without an effort to procure relief in any other way. Surgery in regard to stone, it might be maintained, has continued in arrear of surgery in reference to almost all the other ills that flesh is heir to; the glory here has not been to avoid any operation, but to have it to do." We greatly fear that there is too much truth in what is here stated, but with whom the blame lies, it may perhaps be difficult to say. The surgeon, however, who will first give this field of therapeutics a full and impartial trial, will, whatever be the result, honourably perpetuate his name.

In modern times no lithotriptic remedy has acquired so much celebrity as that of Mrs Joanna Stephens, for whose prescription Imperial Parliament sought with almost as much anxiety as has lately been evinced for our present premier's. Five thousand pounds were voted for her secret, but, unfortunately, no sooner was it known that calcined egg shells and soap formed the principal ingredient in that once famous nostrum, than all faith in its virtues ceased. We sincerely trust that the prescrip-
tion at present in the state of elaboration, will form a greater boon to suffering humanity. In our own day the waters of Vichy seem deservedly in high repute; and our author has collected the accounts of different cases evincing their potent influence; they appear to owe their efficacy chiefly to the bicarbonate of potash, which they hold in solution. In 1826, their power in rendering the urine alkaline, was made known by M. Darcet, who at the same time pointedly adverted to the advantages that might be derived from their internal use, in calculous disorders. It is to Dr Charles Petit, however, that we are most indebted for illustrations of the effects of the Vichy water, and, until we learn to the contrary, we shall continue to hold them in much veneration. We must desire our readers to look over these narratives for themselves; but, in justice to the subject, we cannot omit quoting one of them, and almost at random we quote case, No. 9.

"Case 9. D. B. Jacob, aged 53, entered the Hôpital Beaujon on the 28th of August 1838, labouring under stone in the bladder, which had tortured him for several years. A fortnight after his admission lithtrity was attempted; the stone was seized several times, and splinters were detached, for many pieces were voided on the day of the operation; but acute inflammation of the bladder having supervened, no second operation could be attempted. Even on the 20th of October the patient was extremely ill, complaining of great weakness, of inappetence, and of a fixed and severe pain in the hypogastric region. The excretion of the urine was frequent and painful, and the fluid deposited a great quantity of purulent and glairy matter.

"On the 20th of October the patient was put upon the use of the Vichy water in the dose of from a bottle and a half to two bottles a-day; almost immediately the urine became less turbid, the appetite and the strength revived, and, by the 5th of December, he felt himself so much better, that he insisted on quitting the hospital.

"On the 8th of March the patient returned to the Hôpital Beaujon with a renewal of his calculous symptoms. On the bladder being searched a stone was readily struck. He was ordered the Vichy water for the second time; but none could be had before the 18th. The course once begun, however, was continued to the 18th of June, the date at which Jacob was visited by the members of a committee of the Royal Academy of Medicine, for the purpose of being examined previously to his being sent to Vichy at the public expense. The stone in the bladder was caught nine different times by means of a lithometer; the greatest diameter ascertained was seventeen lines and a half, the smallest seven lines (rather more than one inch English in length, by something more than half an inch English in breadth or thickness.)

"Placed under the care of Dr Petit at Vichy in the season of 1839, the patient proved refractory, and only followed the treatment prescribed for him very irregularly. Nevertheless, when he was examined on the 30th of September by the committee, the longest ascertained diameter of the stone was but fourteen lines; the smallest eight lines and a half.

"Previously to being remanded in the season of 1840, the stone in this patient's bladder was repeatedly seized by a committee consisting of Messrs Civiale, Blandin, and Bérard, and ascertained to be of the diameters of 13, 14, and 15 lines. The treatment by the Vichy waters, begun on the 23d of June 1840, was pursued with great regularity to the middle of September, the patient taking from twelve to twenty-five glasses of the water and a bath daily, and in addition having a stream of the water sent through his
bladder by means of a double-current catheter, once, twice, and even thrice a-day for some considerable time. In the beginning of August, the patient began to pass fragments of his stone, and at the same time he obtained complete relief from his sufferings. On the 18th of September, he was sent back to Paris, where, having been sounded on two different occasions by the several members of the committee, it was formally declared that there was no longer any stone in the bladder.

"When this patient was first placed under the care of Dr Petit on the 20th of October, he was so alarmingly ill that his life appeared in danger to Messrs Marjolin and Langier, surgeons of the Hôpital Beaujon; M. Marjolin, in particular, recommended Dr Petit to find a patient in circumstances more favourable for a decisive experiment. Had there been any choice of patients, Dr Petit would have followed this friendly advice; as it was, he had to content himself with the man whose life had been brought into jeopardy by the course of his disease, and the ill-advised attempt that had been made to perform lithotritry. These adverse circumstances, however, only make the triumph that was finally achieved the more signal." Pp. 29-31.

It would appear that the solution of bicarbonate of potash, as produced by nature in these waters, is more efficacious than any artificial substitute, and hence it is that patients should resort to the fountain-head, instead of partaking either of the fluid as imported, or of any solution made in imitation. There is another reason pointed out by our author, which ought not to be lost sight of, that, as the benefit derived from the libation seems to be in proportion to the quantity, the diluent effects as well as the dissolvent, (if there be any difference in these two terms in such cases,) are greatly increased by the amount of fluids taken into the stomach. Our author, therefore, recommends a course of mineral water at the spring, as having "the vast advantage of securing the individual whilst pursuing it against countervailing influence of every kind; to say nothing of the circumstance, that the water, costing little or nothing at the fountain-head, is always taken in quantities that would be felt as ruinously expensive if used at a distance." Were it not for the acknowledged respectability of Dr Petit, and the well-known character of Dr Willis himself, we might have our suspicions about the above recommendation of a watering-place; but if there is virtue at all in these waters, (and we ourselves have no doubt on the subject, though we feel assured they will not answer in all cases,) we can well believe the statement quoted, as we have full proof in our island, in which mineral waters abound, that no importation or exportation, or artificial substitutes, are ever equal to the waters direct from nature's laboratory.

In this section of his work, Dr Willis has not confined himself to the consideration of those remedies which act through the constitution, as it were, by conducing to those changes in the urine which give relief, or completely dissolve the stone, whether these be of an alkaline or acid character; but he has devoted particular notice to the advantages which may be derived from similar agents, when applied directly to the concretion, by
injections thrown into the urinary bladder. Though Grinthuisin, Amussat, the Arnotts, and others, had not lost sight of the double catheter, it appears that this invention of Dr Stephen Hales (the practicability of using it having been fully proved by him by experiments on dogs) has been in a manner overlooked by most of our moderns in much practice in stone cases. Sir Benjamin Brodie is one of the exceptions, however; and the success which attended his use of a solution of nitric acid, in so far diminishing the size of two concretions of the mixed phosphates as to permit their escape by the urethra, has often given us cause to wonder why the practice had not been more frequently repeated. To us it appears the more strange, that the method of injecting solvents into the bladder should have had so little notice bestowed upon it, when we are reminded by the case referred to by our author, that this mode of practice was successfully resorted to in the Infirmary of Edinburgh, nearly a century ago, by Dr Rutherford. The menstruum used in this instance was lime water; and considering the success of the treatment, as detailed by Mr Butter, as well as the large number of cases of stone which have, since that date, been treated in the same institution, though chiefly by means of lithotomy, we cannot but again express our amazement that further trials were not given to a plan of treatment, which, if not invariably successful, might, with the most ordinary care, be at all events invariably safe.

Convinced as we are that chemistry will yet do even more than she has hitherto accomplished for the science and art of medicine, we confidently anticipate that something great will yet be done for the class of diseases at present under our notice. Many active minds are now at work on the subject. Prout and Bright still pursue their labours; and whether the stone is to be destroyed by a series of manoeuvres carried on within the patient's own system, by a direct attack from the urethra, or by a coup de main from both quarters, still remains to be proved by further experience. That cures can be accomplished by one or other of these means, we entertain no doubt; but knowing well the scepticism which prevails on the subject, we must give praise to our author for not hurriedly passing over this part of his topic, but making out a clear case demanding further inquiry, and sufficient, in our mind, to induce the present race of Jaquez's, Côme's, and Cheselden's, to pause in their sanguinary career, and ponder well whether milder means, pursued for months, may not, after all, add more to the brilliancy of modern surgery, than the "minute time" of the most dexterous performance with the knife.

We must refer our readers to the chapter on the removal of entire calculi through the urethra. It seems quite apparent that unless in the female subject, this mode of procedure can never be of much, if, indeed, of any certain benefit. By means of the an-
cient Egyptian method of insufflation described by Prosper Alpinus, by ivory or metallic instruments, or by the sponge tent, the female urethra may be dilated to an amazing extent. In the Museum of the Royal College of Surgeons of Edinburgh, is one of the largest calculi which have been extracted from the female bladder by means of dilatation.

Dr Ramsay of Dundee, the operator, has given an account of the case in the second volume of the Medico-Chirurgical Transactions of Edinburgh, from which we make the following brief extract:

"About two weeks were spent with very partial success, in dilating the urethra, by means of sponge tents, which were generally expelled in a few hours after their introduction; but they sometimes remained during the course of a night, though not without much inconvenience and distress. The common polypus forceps, and afterwards a better instrument, (commonly used for extracting balls), which Dr Thomson was so obliging as to send me, were tried with better effect. But in Weiss's dilator I found an instrument well suited to its purpose, and succeeded in expanding the urethra more and more every day, from twenty to thirty minutes at a time, until, in the course of ten days, I could easily introduce the fore-finger within the urethra, and examined with sufficient accuracy the figure, size, and surface of the calculus.

"Prior to extraction, I introduced the dilator, in the presence of Mr Crichton and Dr Nimmo, and turned the screw very gradually, until, in the course of about thirty minutes, I had opened the blades sufficiently to admit the flat side of the forceps within them. The dilator was then withdrawn. I fortunately seized the stone in the best manner for its extraction, which was accomplished with some difficulty, and a good deal of pain to the patient. The chief resistance was found at the orifice of the urethra, and was at first so great as to render it doubtful how far the bistoury might not have been advisable; but as this might have defeated the great object in view, I persisted a little longer, and effected the extraction without its aid.

"From the size of the stone, (which we found to be 5½ inches in its long circumference, and 3½ inches in the short, and weighing 7½ drachms), I apprehend it would have been impossible to have extracted it in this way, without rupturing the urethra, had not the dilatation been effected in a very gradual but decided manner for some weeks previous to the operation.

"A good deal of general irritation prevailed in the system for a few days after the removal of the stone, with pain and swelling of the parts; but these symptoms soon gave way; not, however, without considerable mucous and puriform discharge from the vagina, with some membranous sloughs, which seemed to give considerable uneasiness, by obstructing the free passage of urine.

"The bladder and urethra very quickly resumed their natural functions. The power of retention, which, from the first, was never lost, has ever since remained perfect; and all the distressing symptoms depending upon stone in the bladder entirely left her after the operation."

Without stopping to inquire into the probable results of excessive dilatation, we shall hasten to give our opinion, that the value of the practice of removing small calculi through the male urethra, has been much overrated. Such cases as the celebrated one, in which Sir Astley Cooper removed eighty-four small calculi through the urethra, by means of an instrument, are truly of rare occurrence. We should deem such an instrument a most
dangerous weapon in less expert hands, and, taking into consider-
ation the variety of circumstances, which, in almost every exam-
ple of urinary disease, must render such means totally inapplica-
ble, we never could concur in opinion with those who have sup-
posed the urethra forceps of much general utility. The rare
event of a small stone following the point of a bougie, as it is
withdrawn from the bladder, or the escape of a few particles of
gravel through the eye of a catheter, are facts which every surgeon
ought to be aware of, but for every such example of either one
or other of these events, it would not be difficult to adduce many,
to prove that stones have in general not evinced such a ready in-
clination to quit their quarters. No one can appreciate more
highly than we do, the great authority on which such means of
relief have been introduced,—Dr Hale's, John Hunter's, Sir Will-



William Blizard's names ought to, and do go far in any such topics;
but we cannot agree with Sir Benjamin Brodie, in believing that
what was done by Cooper is among the greatest achievements of
modern surgery; and we feel more inclined to entertain the views
of Deschamps and Professor Syme, both of whom, in the words of
Dr Willis, think it "highly injurious", (Syme,) "imprudent and
murderous" (Deschamps.) We must apologise in some measure for
acquiescing in the expression referred to the latter. We only
quote, but this much we must say, that during an experience of
nearly twenty years, we cannot affirm that we have seen great
results from the "urethra forceps," and it seems apparent to us,
that had they been of the utility which some have claimed for
them, the more modern operation of lithotrity would have been
without some of its most objectionable features, viz. the difficulty
of getting the small fragments away from the bladder, after the
mass of the stone has been comminuted.

The chapter on lithotrity is evidently our author's chef d'oeuvre, and it seems to us, that this operation has given him
the apology, as it were, for going over ground already well known
to those who have paid much attention to the history of the
treatment of stone in the bladder.

Though it is more than thirty years since Gruithuisen sugges-
ted the practicability of removing stone in the bladder by me-
chanical means,—though the celebrated case of Colonel Martin
had been made public in 1799, by the late Sir John Sinclair,—
though Elderton, in 1819, gave the first description of what may
be called a lithotritic instrument,—though there has been neither
lack of talent, nor (we regret to say) time to bestow in research,
in the body of the profession, it is evident, that up to the present
date, the whole subject of lithotrity, since its first performance
in 1824 by Civiale, has been, in a manner, totally neglected in
England. Whilst the press in France has teemed with memoir
after memoir,—whilst there, too, the professed lithotritist and the
already accredited surgeon, have each laboured in this interesting and novel field of art and science, our own countrymen have stood listlessly, and, we may almost say, stupidly looking on, even up to the present day. It is true, that the operation has been frequently done by British surgeons, but it is equally true, that they have merely followed in the wake of their continental neighbours. We have had no Amussats, Leroy, or Civiales in our country; no commissions issued by our corporate bodies, to report on the progress of the art; none of our Percys, Boyers, or Larreys have given more than the daily routine thought to this most engrossing topic. Every assertion made by the professed lithotritist has been silently taken for granted; the observations of such practical men as Souberbielle and Velpeau have been passed over in silence; lithotrity has been practised here on such authorities as the printed works of Leroy, Civiale, Bancal, &c., the exhibitions of Heurteloup and Costello, or the more vague reports of the medical student. We have had no Hunters, Hornes, Abernethys, and Coopers, to pioneer us on in lithotrity, as in the subject of aneurism, and now, almost at the eleventh hour, and when lithotrity has in a manner established a reputation as a grand substitute for the well-known and dangerous proceeding of lithotomy, an English physician steps forward, throws down the gauntlet to the professed lithotritists, and actually (and with their own weapon, too) exposes and breaks up a system of deception and equivocation, to which the history of the mysteries of Stephens can scarcely afford a parallel.

We profess ourselves, on the present occasion, to advocate neither one side of the question nor another, but we cannot help expressing the powerful effects which Dr Willis's observations have had on our mind. In giving acquiescence to the general deductions drawn by our author, we shall leave it to himself to settle with Messrs Civiale and Company as best he may. We have our own ideas, that the Doctor will be found by the lithotritists, what in England is called "a tough customer," but under any circumstances we have only one duty, in honesty, to perform, and, with the evidence before us, it is impossible that we can draw any other conclusion, than that the success of lithotrity has been much overrated, and that consequently the proceeding itself has been greatly abused, both by its own advocates and those who have heedlessly followed in their course.

Our limits will not permit us to follow the close reasonings of our author on this part of his subject, but we feel that we owe it as a duty to the profession, to recommend a careful perusal of this chapter. We do so, in the conviction that the results of lithotrity have not, as yet, been fairly put before our readers in large aggregate numbers, and also that this part of the work is of such a nature that the subject cannot be allowed to rest here.
We must permit Dr Willis to speak for himself, however; and with regard to M. Civiale, to whom he naturally refers as the highest authority as a lithotrítist on this question, he states:—

"In his Traité de l’Affection Calculeuse, p. 613, he speaks of the number of persons affected with calculus who had sought his assistance up to the year 1836. They amounted to 506. Of these 199 were either unfit subjects for lithotrity, or were otherwise prevented from submitting to the operation. Supposing the whole of the 199 to have been really unfit, this would be in the proportion of one in two and a-half very nearly, to whom lithotrity held out no chance of relief, or who, were they subjected to the operation, would almost certainly lose their lives. We have, therefore, 307 subjects favourable for the operation. Of these, says M. Civiale, (op. cit. p. 630), 296 were completely cured; 7 died; 3 were only partially relieved; and in 1 the issue is not known.

"But when we turn to such public documents as we possess, we are amazed to find how little the conclusions as there stated accord with the numbers of M. Civiale. In a report presented by Barons Larrey and Boyer to the Royal Academy of Sciences in the month of April 1837, upon a comte-rendu or statement in regard to the patients affected with calculus confided to the care of M. Civiale at the Hôpital Necker, we find it stated with something like a censure, that M. Civiale should have confined himself to the mention of five cases, in which he had recourse to lithotrity with a success more or less decided; but passed by in silence, the patients who underwent the operation of lithotomy, (after having been vainly essayed by lithotrity), so that, say the reporters, ‘we should have remained in complete ignorance of the fate of these individuals had we not seen the movement of the Hospital, which M. the Controller was obliging enough to lay before us. We find,’ continue the reporters, ‘that 24 patients (not 16, as stated in M. Civiale’s comte-rendu) had undergone the operation of lithotrity or lithotomy. Of these 24 patients, of whom six were cut [after lithotrity had been essayed in vain,] eleven died more or less immediately after the operation. Eleven deaths in twenty-four cases immediately after the operation! Verily there is little to boast of here, and we can already afford M. Civiale’s seven deaths in 307 cases!’ and yet find four at our disposal to carry to the next account.

"Let us go on to public document the second. This is a report to the Royal Academy of Sciences, presented by Messrs Boyer, Double, and Larrey, upon the operations performed at the Hôpital Necker during the years 1831 and 1832. ‘Fifty-three patients affected with calculus were received at the hospital. Of this number twenty-seven treated by lithotrity were discharged completely cured; sixteen having had various attempts at lithotrity made upon them, the operation was definitively found impossible, or useless, or it proved fatal. Of these sixteen, ten died and six remained unrelieved. Eight other patients were subjected to attempts at lithotrity and then to lithotomy; of these five died and three recovered.’ If we analyze this statement, we find fifty-three receptions and twenty-seven recoveries; which is as nearly as possible one recovery in two cases by means of lithotrity; but as three recovered by means of lithotomy after the failure of lithotrity, we have three to add to the list of cures, which therefore amount to thirty in all. On the other side, we find ten deaths as immediate consequences of lithotrity, and five more after lithotrity and lithotomy combined, that is, fifteen deaths in all. Eight cases remain unrelieved, which must terminate fatally within a brief period, all the more quickly, for the sounding, &c. which the subjects of them doubtless underwent at the Hôpital Necker. Fifteen dead, eight unrelieved and expecting death, make twenty-three cases of non-success to twenty-seven of success by lithotrity. Surely neither is there ought to be proud of here; the cases in which no relief could be afforded were actually within four of being as numerous as those in which
lithotrity was found of avail! and this is an operation that boasts of all but invariable success! of 'three hundred and odd cures to seven untoward results!'

"Turning from public documents, let us see what some of M. Civiale's contemporaries have made out by an analysis of his cases, somewhat more rigorous than his own. M. Velveau, than whom there is no more honourable man or trustworthy writer in France, has given an analysis of five series of M. Civiale's cases, and I here append his table:

| Series | Number of Cases | Cured | Dead | Unrelieved, the stone remaining | Otherwise, Success in | Failure in |
|-------|----------------|------|------|---------------------------------|----------------------|-----------|
| 1st.  | 83             | 41   | 39   | 3                               | 41                   | 42        |
| 2d.   | 24             | 13   | 11   | 0                               | 13                   | 11        |
| 3d.   | 53             | 30   | 15   | 8                               | 30                   | 23        |
| 4th.  | 30             | 18   | 8    | 4                               | 18                   | 12        |
| 5th.  | 16             | 6    | 7    | 3                               | 6                    | 10        |

That is to say, of 206 patients operated on, 108 (a very little more than one in two) recover immediately; 80, or nearly one in two and a-half die; and 18 retain the stone, and will be lost. One hundred and eight cases cured, to ninety-eight, in which death is immediately induced or may not be averted within a brief interval of time. This is a very different tale from the one told by M. Civiale himself; the reader may adopt the conclusions of whichever of the two statements he pleases. There can be no question as to the one than conveys the truth.

"But M. Civiale can readily be convicted of misstatement out of his own mouth, or by the act of his own pen; indeed, it is from data furnished inadvertently by himself that M. Velveau has arrived at the conclusions as just given; and whoever will be at the trouble to turn to the work, entitled 'Parallele des diverses moyens de traiter les calculeux,' will straightway find himself picking his steps over a sort of battle-field. In ten pages of the preface he will meet with a registry of eight deaths, and between the 12th and 65th page of the body of the book, no fewer than fourteen failures make their appearance,—twice in a single page, there are records of two disasters.

"I am charitable enough to seek for some means of accounting for the singular discrepancy between the reports of M. Civiale and the statements of contemporaries. Men do not usually come openly before the world with an unmitigated falsehood in their mouths; they can generally explain their statements. In regard to lithotrity the explanation is this: All the cases in which the process is essayed, but unsuccessfully, in which one or two, or even three attempts have been made to seize or perforate the stone, and even in which it has been seized and perforated once or twice, but in which it is found impossible to proceed in consequence of pain, of inflammation excited in the bladder, &c. &c.—all such cases are thrown out of the account; in them lithotrity was not performed,—those that die under these circumstances die before the operation, or from some other cause than the operation.—Where men are deeply interested, where they think their reputation is at stake, they deceive themselves, and then they try to deceive others. So respectable a surgeon as Mr Martineau, who prided himself on his success as a lithotomist, thought that he did not select his patients. 'In this number, (eighty-four cases in which he had performed lithotomy) he states that 'no selection of patients was made, as I never rejected any one who was brought for operation.' (Med. Chir. Trans. vol. xi. p. 409.) "But what is the language of his contemporaries? Let us consult Mr Crosse: 'During the many years
that I witnessed Mr Martineau’s public practice, he carefully selected his patients;” (On Urinary Calculus, p. 155.) Unless he had a great chance of success, therefore, he never ran the risk of failure. In charity we must presume that it is the same with M. Civiale, although candour also compels us to admit that, however emphatically warned, he will not consent to be set right.

“The following case, which is given by M. Civiale (Parallels, &c. p. 337) to illustrate the ill effects of *simply sounding the bladder* will serve me sufficiently well to corroborate the above views and statements:—

“M. Bizouard, aged 65, though he had long suffered from symptoms of stone, had such a dread of everything in the shape of a surgical operation, that it was only when fairly vanquished by his sufferings that he would consent to have his bladder searched. This being done at length by M. Hervez de Chegoin, a stone was struck. M. Civiale being called in a few days afterwards, ascertained, without any difficulty, that the bladder contained a foreign body, of about the size of a large walnut. The urethra was sensibly contracted below the pubic arch, and the prostate was larger than natural. Some preliminary treatment, the necessity for which was indicated by the character of the patient and the irritability of the parts, was then had recourse to. ‘An exploration of the bladder, made by means of the instruments of lithotrity,’ says M. Civiale, ‘convinced me subsequently that it would be an extremely difficult matter to crush the stone, in consequence of its size, the enlargement of the prostate, the small capacity of the bladder, and the general irritability of the subject. *This perquisition was long and painful. It was followed by a little fever, difficulty in making water, &c.—These accidents never subsided.* Lithotomy was proposed to the patient as his only resource. A consultation was therefore called, and lithotomy was determined on. The day was fixed, the patient had the perineum shaved, he was laid on his bed, and the sound was passed as the first step in the operation, but no stone could be discovered. It was searched for in vain by more than one experienced hand; the patient had to be put to bed again. Inflammation of the bladder now set in, and the patient died four days afterwards. ‘Nous trouvons là,’ continues M. Civiale (l. c. p. 378), ‘une preuve éclatante que les explorations pratiquées au moyen du cathéter ordinaire peuvent être plus dangereuses que celles pour lesquelles on emploie les instruments de lithotritie.’

“Now, I hold this case to be extremely valuable; it gives us a key to M. Civiale’s mode of interpreting his practice. This particular case would not be set down by him in connexion with lithotrity at all—there was ‘a mere perquisition of the bladder.’ But when we look with unprejudiced eyes to the whole circumstances, we see that there was a very determined effort to seize and crush the stone in the first instance; this was found impossible; but the futile attempt was followed by local inflammation and general symptomatic fever, ‘which never subsided,’ and the patient fell into such a state, that he must be relieved of his stone or die; the searching of the bladder by the simple sound, preparatory to the performance of lithotomy, rekindled, or rather aggravated the inflammation which had been originally lighted up by the vain attempt to perform lithotrity, and the patient was the victim.

“The nature of what the lithotritists entitle preliminary examinations—or, to adopt their own French word, *perquisitions*—which all the world save themselves regard as very determined efforts to drill or crush the stone, has been spiritedly sketched by M. Velpeau. ‘Let us see,’ says M. Velpeau, ‘what these preliminaries are in fact. They introduce the litholabe, the lithontriptor or the percutor into the bladder, where its point is moved about to ascertain the existence and the position of the stone. They then open the instrument, its branches are separated to seize or embrace the foreign body, and to appreciate its size and its form. They then endeavour to perforate, to crush, or to fracture the stone by acting on the other and outer end of the
instrument, which is thick and straight within the urethra. All this is done once, twice, or three times at intervals of a few days. And now, doubtless; some one will ask, in what the operation properly so called differs from these preliminaries? *Ma foi, je n'en sais rien!*

"Let us go further, and ask M. Civiale himself, in a couple of instances or so, what he understands by patients who had died 'without having had any operation performed on them?' Let us take the case of Lecomte, for instance, and see what is stated in certain private records of the Hôpital Necker, which M. Velpeau seems to have got a sight of, for he puts what follows within inverted commas, and adds in a note that he hopes that M. Civiale will not dispute the accuracy of the statements, as he will have no difficulty in divining the source whence they were derived.

'This patient (Lecomte) had suffered from stone for two years, and had an ample urethra: the intention was to have begun the operation on the 5th of June 1830. The instrument was passed into the bladder previously injected. Acute pain was experienced in the region of the prostate. Scarcely had the instrument been expanded when the pain became intolerable. The instrument had to be withdrawn before the stone could be attacked. From this time there were incessant calls to micturate, accompanied with tenesmus and excessive pain during the emission of the urine. *Death on the fifth day afterwards.*

"There is room for farther selection. Here is another where matters were as bad, though the issue was complicated by lithotomy, which became necessary to give the man a chance for his life, which, unfortunately, he lost nevertheless. 'Godailler, aged 57, had suffered from stone for three years. The instrument was introduced the 17th April 1830, and the stone charged with the greatest ease; the drill was worked. After this operation the calls to urine became extremely frequent; in the evening the patient had a rigor, and then he was attacked with fever. A second sitting took place on the 24th; the febrile paroxysm returned with renewed violence; the urine, loaded with mucus, acquired a sanguineolent tint. The state of the patient getting worse and worse, he was cut (and died).' Now, the death of neither of these unhappy men is ascribed to lithotripsy. Lecomte died without having had any operation performed; Godailler died of lithotomy—according to M. Civiale!!

It would appear from our author, that only two British surgeons, among the many who must have practised this operation, have given any thing like a narrative of their experience or practice. Mr Fergusson and Mr Key, in his estimation, and likewise in our own, are entitled to much praise for the candour of their statements; but it appears to us, that, much as we esteem their authorities on any subject relating to stone in the bladder, there is still much wanting on the part of our brethren to give a strictly professional tone to the whole question. With all due respect to Heurteloup and Costello, we deem it no disparagement to them that, in addition to the two surgical authorities among our countrymen, quoted by Dr Willis, we should ask the opinion of such as Crampton, Brodie, White, Guthrie, Liston, and a host of others, whom we know to have practised this operation, and who are among the acknowledged heads of surgical science and art in Britain.

The distinguished Civiale is certainly handled in a somewhat rough manner;—drilled and triturated like an offending stone,—
and has had the brise-coque of Dr Willis applied to him with might and main; a proceeding which could not be avoided, were justice to be done to this most important professional question. Like many physicians, Dr Willis leans to "mild measures." He gives a favourable view of the little that has been done by modern scientific men towards dissolving the stone; he speaks in terms of commendation of the method of removing small calculi by the urethra, with the aid of forceps, dilatation, &c.; and on the assumption that lithotrity is a milder method than lithotomy, he gives what he considers a fair estimate of the effects of the former operation,—effects which he proves to be actually more disastrous than those of lithotomy. He is neither lithotomist nor lithotritist himself, and unless it be to do justice to the public and to a great professional question, overlooked previously by the great body of his contemporaries in Britain, it is difficult to perceive any reason for the vast trouble which he must have given himself in collecting the data on which his opinions are founded.

The chapter on lithotomy presents us with nothing sufficiently new for our present consideration.

By the novel terms of lithectasy, cystectasy, our author refers to a modification of the ancient Marian operation for stone, with the apparatus major, which seems to have been first proposed by Mr John Douglas in 1727, when he expressed his opinion that "artificial fistulas in males, and in the urethra in females, may be dilated so as to extract any stone without cutting the body of the bladder or lacerating any of the parts." Instead, however, of dilating or tearing open the parts about the neck of the bladder, as was done by Marianus, it was proposed to enlarge the opening in a very gradual manner; and the plan was actually carried into execution in 1819, by Sir Astley Cooper, at the suggestion of Drs Neil and James Arnott. The subject of this operation was—

"A gentleman of middle age, who had some nine months previously undergone the usual operation for stone, in the course of which the rectum had been wounded, so that a fistulous opening remained between the bladder and the bowel, feces passing with the urine and urine escaping with the feces. In this state, and still suffering much from pain and irritability of bladder, the patient placed himself under the joint care of Drs Neil and James Arnott, and Sir Astley Cooper. With a view to cure the fistulous communication between the urethra and rectum, Sir Astley Cooper made an opening into the urethra from the perineum, by which he passed a female catheter into the bladder, and immediately struck a stone. As it was likely to be small, Sir Astley did not object to the proposal now made by the Drs Arnott, to try the effect of the new dilator in opening the passage for its removal. This instrument was accordingly used, and in the course of thirty hours the passage in the perineum, the membranous portion of the urethra, and the neck of the bladder, were opened up till they were about two inches and a quarter in circumference, or three quarters of an inch in diameter. The lithotomy forceps was then introduced by Sir Astley Cooper into the..."
bladder, and the stone immediately felt, seized, and extracted. It was as large as a middling-sized walnut.

"This operation was eminently successful. During the process of dilatation the patient had an uneazy feeling of distension, but nothing approaching to pain. In four days all the irritation of the bladder had subsided; the patient was able to retain his urine, and left his bed-chamber. On the ninth day the wound in the perineum was whole, and he began to take exercise abroad." Pp. 71, 72.

A somewhat similar operation was performed by M. Guérin, surgeon to the General Hospital of Bourdeaux, who, however, instead of limiting the incision to the raphé of the perineum, between the bulb and the sphincter ani, divided the latter part with the rectum, at the time of making the wound in the perineum. From the evidence afforded by these two cases, Dr Willis draws the conclusion, that the free wound in the prostate, or the sudden process of dilating or tearing the neck of the bladder, is now no longer necessary. It appears to him, "that with this operation at command, stone in the bladder has already lost a great proportion of its terrors. There is hardly a case to which it is not applicable, and its application is without danger, immediate or prospective. The brief interval of from four-and-twenty to eight-and-forty hours, is all that is necessary to begin and end the operation; and this with no doubtful prospect,—it cannot end otherwise than well. Even if we found a stone of five or six ounces in weight, we should feel neither embarrassment nor alarm. We never should think of extracting such a mass entire, indeed; but we have an aperture of two, or, if we choose, of three inches in diameter, and a canal of little or no greater length, through which we can introduce an Earle's, or any other commodious instrument for breaking up large stones in the bladder, and reduce it to fragments in a twinkling." We sincerely wish, for the sake of our suffering fellow-creatures, that we could conscientiously acquiesce in the Doctor's opinion; but alas! it appears to us that he has drawn his conclusions from premises far too limited in extent. From the time of Douglas (1727), up to 1819, no one seems to have attempted the operation, and this for a period when England possessed its highest authorities in cases of stone, from Cheselden to Sir Astley Cooper himself; and what is also equally remarkable, it does not appear that the last-named distinguished surgeon, even with his almost unlimited practice, had ever again resorted to a similar proceeding. Far be it from us to decry what may reasonably be thought likely to diminish the risk to life, in any attempt to rid a patient of stone; nevertheless, we cannot but say that mere arguments alone will not convince us on this most important point; and with a full reliance on the authenticity of the two cases we have quoted, as the only two on which our author has founded his apparently correct conclusion, we must yet ask for further reasons, and that, too,
from the surgeons of the present day, that this mode is to be preferred to the lateral operation as now done in this country, whether with gorget, bistoury, or scalpel.

We close this volume in a very different spirit from that in which we opened it. The physician has proved that he possesses an ample knowledge of a great surgical question: but he has left the subject of lithotrity in such an equivocal position, that it seems equally incumbent on lithotritists as on surgeons, to come to some issue. We can no longer listen with implicit confidence in the statements of a Civiale, (as our author has now exhibited them,) nor is it in accordance with the dignity of our own surgeons to allow the vulgar saying still to pass current, that Sir So-and-so, Mr This, or Mr That, says that "lithotrity is a most excellent operation; that it is not understood by the generality of our surgeons, but that in their own individual practice it invariably proves efficient?" We beseech our surgical brethren, who have had experience, to speak out. Meantime we heartily thank Dr Willis for his talented treatise, of which we take leave, by earnestly recommending to the careful consideration of our readers,