operation in a like unhappy combination of circumstances; and the issue would seem to warrant operative interference at however late a stage the surgeon may be permitted to have recourse to it.

Montserrat.

Part Second.

REVIEWS.

Fruits and Farinacea the proper Food of Man, being an attempt to prove, from History, Anatomy, Physiology, and Chemistry, that the Original, Natural, and best Diet of Man, is derived from the Vegetable Kingdom. By John Smith. 2d Edit. London. 1849.

Report of the Second Annual Meeting of the Vegetarian Festival. Abridged from the Manchester Examiner and Times. July 12, 1849.

The object of Mr Smith in writing this book, is to prove by argument, as it is the aim of the Vegetarian Society to inculcate by example and speech-making, that ever since the period of the deluge, the whole human race have been living upon a plan contrary to nature; that their almost universal habit of making more or less of their food to consist of the flesh of animals, has been, and is, the cause of countless evils; and that the way to set matters right in this respect, is for all human beings to abjure animal flesh, and to nourish themselves exclusively with fruits and farinacea.

Although we do not find it distinctly stated by Mr Smith, it seems to be tacitly admitted to be consistent with vegetarian laws, and, as we gather from the bill of fare of their dinner, with the practice of the Vegetarian Society, to add to the fruits and farinacea what is commonly called dairy produce. At least, we presume that "savoury pies," "bread and parsley fritters," "blanc mange," "cheesecakes," and "custards," contain more or less of eggs, milk, and butter; and we cannot help entertaining a suspicion, that without these very wholesome additions to their dietetical resources, our vegetarian friends would find fruits and farinacea alone, by no means so rich and luxurious a diet as they would have us believe. We do not bring forward the case of these articles, as an accusation of inconsistency against them, because, so far as we can see, their system is directed only against the use of flesh, i.e., whatever is obtained by the destruction of animal life; but we must maintain, that, if they feel themselves compelled habitually to use these articles, it militates strongly against a physiological doc-
trine, to the support of which Mr Smith devotes nearly fifty pages of his book, "That vegetables contain all the elements and qualities necessary for the complete nutrition of man."

Assuredly one of the essential qualities in any system of nutrition must be, that it is one which can be persevered in without repulsiveness, as well as without injury, throughout a lifetime; and until we learn that vegetarians are able, without undue sacrifice of their comforts and tastes, to persevere in a diet, from which all animal matters whatever are rigidly excluded, we must hold that the above thesis has not been sustained, and that, noentes volentes, men will be compelled to make part of their diet, at least, consist of animal substances. If, then, it be admitted that certain animal matters, such as milk, are not only physiologically compatible with, but are essential to, supplying the requirements of human beings, surely others which resemble them in their nutrient qualities may be allowed on similar grounds. There can be no chemical or physiological objection to the protein compound fibrine as it exists in flesh, which may not be urged against the protein compound casein as it exists in milk, or the protein compound albumen as derived from eggs; and, therefore, physiology and chemistry failing them, the vegetarians must fall back, for their objections against flesh-eating, upon the aesthetical and moral objections. If, therefore, we found ourselves called upon to act as counsel for the defendant, in the case of Smith and others versus flesh, we should begin our pleading by a special objection to the list of witnesses for the plaintiffs, and move the court to rule that, until the above strictly vegetarian experiment have been fairly tried, chemistry and physiology were not admissible at all as evidence. We have, however, no intention of adopting this method of procedure. We give Mr Smith the benefit of the whole evidence which he can produce, and we unhesitatingly maintain that he has not succeeded in establishing his proposition, that all participation in animal flesh ought to be abandoned by those, who would wish to live both well and wisely. In fact, this grand fallacy runs through nearly the whole of Mr Smith's book, that he contends against the employment of flesh, to constitute part of the mixed diet in use among all well-regulated communities, upon arguments which are applicable only to the immoderate use of flesh along with vegetable matters, or to its use without vegetables at all. Thus, for instance, when he states upon the authority of comparative anatomy, that—

"All the human organs connected with alimentation, are evidently very different from those in carnivorous animals; and, although in some respects they differ also from the organs of herbivorous animals, they are evidently much more closely allied to those of the latter class than those of the former;"

he merely proves, what every one will most readily admit, that man, on the one hand, was not intended by his Creator to eat grass like an ox; nor, on the other, to tear and devour raw flesh like a tiger.
Whatever we may think of vegetarianism as a system, or of the arguments in its favour as stated by Mr Smith, we must concede to him considerable merit as an author. We shall not assign to him as characteristics, merely the energy of an enthusiast, and the proverbial zeal of a proselyte, but we will say for him that he is distinguished by an intense honesty of purpose, which creates in us the impression, on reading his book, that he (however erroneously) feels and believes all that he writes and contends for; that he is a man of amiable sentiments and charitable feelings, and one who has a proper, but not overstrained regard, for whatever he conceives to be taught him by the word of God. His book, as regards composition, is readable and entertaining, and displays an amount of industry in study which reflects the highest credit on him. Though not a medical man, he has perused, obviously with care and intelligence, whatever chemistry, physiology, and pathology have made known, as bearing at all on his darling subject; and, although we think his conclusions in most instances very unwarranted, and his reasoning often very loose, these faults seem to arise, not from careless reading, still less from a desire purposely to distort the truth, but rather from a want of that fundamental acquaintance with medical science, which so commonly tends to error on the part of those who write popular books in which medical subjects are discussed.

We regret that our space does not permit us to make a detailed analysis of Mr Smith's book; we must content ourselves with pointing out the general line of discussion which he follows, and selecting a few of his arguments, to justify the general statement which we have made above, that though we have read his book, we have not been converted to vegetarianism by it.

Mr Smith begins his argument with an inquiry into the original food of man, and of course maintains that our first parents in Eden subsisted solely on vegetable matter, inasmuch as the only articles mentioned in the Mosaic account as being given to them for meat, were "every herb bearing seed," and "every tree in which is the fruit of a tree bearing seed." It is true that they already had dominion over the animal as well as vegetable creatures, but there is no mention of the former being to be consumed by them as food, and therefore we have nothing to oppose to Mr Smith's doctrine in regard to this matter. But whilst we admit it as a historical fact, we are by no means prepared to allow, that the manner of life of our first parents living in paradise, "in native worth and honour clad," was recorded in the Scriptures for the purpose of being adopted as a physiological rule, for the guidance of their fallen posterity in after ages.

The chairman of the Vegetarian Festival, however, the record of whose speech stands at the head of this article, has no doubts on this point. He says—

"The free will of man might make it necessary that the flesh of animals
should be allowed, but if they would live as near the order of creation as might be, they must adhere to the teaching which was first established by the Creator himself, since the facts of scientific research, confirmed by experience, showed, that what was good at creation, was most reasonable in 1849."

Now, if this doctrine be tenable as regards habits of feeding, it appears to us to be equally so as regards habits of clothing; and if it be reasonable in 1849 to refrain from animal food, because we have reason, on negative evidence, to believe that Adam and Eve did so in paradise, a fortiori we ought to follow their example in dress, seeing that we are positively informed that they both went naked from the creation to the fall. To follow out, therefore, the vegetarian chairman's rule of "living as near the order of creation as may be," we apprehend that we ought not merely to abjure our chops and beefsteaks, but put our breeches in the fire, annihilate our "Nicoll's registered Paletot," and sally forth, stethoscope in hand, to see our patient's to-morrow, in puris naturalibus (that being "as near the order of creation as may be"), instead of being attired in "customary suits of solemn black." We do not stop to inquire, whether, in the present easterly wind (horresco referentes), this would be a good sanitary measure, or whether, in our present fallen mundane condition, it would be regarded as being exactly decent; but this we maintain, that it is every whit as reasonable to imitate Adam and Eve in the matter of dress as in that of food, if we are to be guided by the rule of living, as regards the corporeal man, "as near the order of creation as may be."

Mr Smith, in pursuing his history of the original food of man, reminds his readers that it was not till immediately after the flood, that animal food was permitted as an article of diet; that then the average period of human life, which previously had been about 900 years, was reduced to 400, and when Jacob lived it had gradually declined to 150 years. He then says—

"This abbreviated period of human existence, may not have been the effect solely of animal diet, but it doubtless had a considerable influence."

We have no objection to an inquiry, within proper limits, into the secondary causes by which God executes his decrees, either as regards the duration of human life, or any other subject of his providence. But in the total absence of any declaration on the subject, to assign this as in any respect a cause for the abbreviation of human existence, is, to our thinking, not merely an unphilosophical but a most unwarrantable bare-faced assumption. Before being doubtless on this point, Mr Smith ought to have been able to bring out, with some degree of probability at least, not merely that the shortening of man's days up to Jacob's time, bore some relation to the extent to which the use of animal food then prevailed, but he ought to have ascertained that when human life was further limited to three score years and ten, the use of animal food had increased in a corresponding ratio. We need hardly say that facts on these points are entirely wanting.
Mr Smith next enters upon the consideration of the natural food of man, and draws comparisons, from comparative and human anatomy, in favour of vegetarianism, which, as we have before observed, are not warranted by his premises, his arguments being good enough against the undue use of flesh, or the eating of it raw, but certainly not against its moderate use, when cooked, along with vegetable matters. According to him, likewise, our senses of sight, smell, and taste, all naturally prompt us to eschew animal food, and adopt vegetables. Here is a kind of résumé which he gives us on this point—

"It seems reasonable to conclude, that the following relations between fruits and man, are the results of special design. The general height at which fruits grow, is adapted to his stature and upright position; their elevation and shape to his hands as organs of pretension; their consistency to his teeth; the liveliness and beauty of their colours to his organ of vision; their agreeable fragrance to his organ of smell; and their grateful and delicious flavour to his taste."

This is as good an example as we ever met with, of the process commonly called taking a broad margin. The general height and elevation of fruits specially adapted to our stature! They vary from a strawberry, which a tottering infant may pick off the ground, to the banana, which is only to be acquired by an active nigger shinning up a tree forty feet high. Their consistency adapted to our teeth! He who eats a strawberry may do so without teeth at all; he who tries his grinders on a cocoa nut will soon find that he had better attack it first with a saw or a hammer. Their fragrance attractive to our smell! A melon is certainly overwhelmingly fragrant, but the more important fruits of the cereals are not fragrant at all. Mr Smith feels the force of this, and says—

"Many roots, nuts, corn, rice, and esculent vegetables, seem to be equally proper for man as articles of diet; and it may be urged that the organ of smell fails to direct him in the choice of them. But it must not be forgotten that the instincts of man are no longer in their original state; and the sense of smell may now have lost much of its primitive sensibility and discrimination."

Whether our noses are less or more sensitive than those of the patriarchs, we cannot pretend to say; but if the instincts of man are now no longer in their original state, what becomes of all those arguments derived from the original state and early history of our race? They are truly full of interest in a theoretical point of view, and as showing how much man has fallen away from his primitive condition; but, unless Mr Smith means to maintain that vegetarian diet may succeed in restoring man to the model of his antediluvian ancestors, and people the earth with a race of Methuselahs, they cannot serve as practical rules for his guidance in his present state of being.

Into the argument from the moral and sensitive feelings of man, it is not our province to enter. We pass on to the remaining and
largest portion of Mr Smith's book, which is devoted to proving that vegetable food is not only the original and natural, but the best food of man. As this, however, is discussed in not less than seventeen chapters, we must be content with offering a single comment on one or two of his statements.

To his very first chapter, where he maintains that vegetables contain all the elements and qualities necessary for the complete nutrition of man, we have already objected that this has never fairly been tried. Even if the stories of anchorites and holy hermits living on parched peas and water be true—even if it be a scandalum magnatum that these worthies now and then their "flesh did mortify With a dainty slice of a warden pie"—

we take the liberty of doubting the goodness of their physique as men, whatever we may think of their morale as devotees, and therefore do not admit that their nutrition was complete. Furthermore, even if, on such a diet, they were all as vigorous as Friar Tuck (who, however, understood the philosophy of alimentation better than to adopt it), we should hold such examples worthless; because, to have a perfect diet, we must have one which is at once good and agreeable, and not one which infers the severe ordeal of a lifetime's penance. Mr Smith, we must mention, does not cite these instances of vegetarianism; but the experience of communities and individuals, quoted by him, do not in the least establish his proposition; because we have good reason to believe that, in these instances, milk was used; which must be rigidly excluded, before we can test the capabilities of a purely vegetable diet to subserve the purposes of nutrition. Mr Smith may rest assured, that our railways could never have been completed had the "navies" enjoyed no better diet than potatoes and water. We hold, then, that the experiment of an absolute vegetable diet has never been fairly tried on ordinary men.

When Mr Smith gets upon chemico-physiological ground, he takes some bold strides in directions where we cannot follow him. Thus, in order to make out a good case for the nutritive qualities of vegetables, he adopts the hypothesis that starch may be converted into the protein tissues of our bodies, and he effects it in this way:—

1st, he assumes that nitrogen is absorbed to a greater extent than it is evolved in the process of respiration (a proposition by no means established); 2d, that when absorbed it can form combinations with other elements, chiefly, it would appear, by its combining with nascent hydrogen in the capillaries and intestinal canal! and then he makes protein from starch in the following manner:—

"If we admit that the nitrogen of the atmosphere combines, in the digestive process, with the elements of food—of which there can be little doubt—
then four equivalents of starch \([\text{C}^{12}, \text{H}^{10}, \text{O}^{10}]\) may be converted into one equivalent of protein, \(\text{C}^{48}, \text{H}^{36}, \text{N}^{6}, \text{O}^{14}\), and four of water, with a separation of oxygen.”

This is an erroneous kind of reasoning, by no means peculiar to Mr Smith. A great deal of this sort of philosophy runs through the writings of the chemical physiologists and pathologists of the present day. We have no objection to these chemical equations, as affording a probable solution of a fact already established by experiment; but we have no experimental proof whatever to show that fresh blood or muscular tissue can be formed in an animal fed upon starch. When this has been done, we may, perhaps, adopt this hypothesis, as an ingenious way of explaining it; but, in the total absence of experimental fact, we can regard it as nothing more than an exercise in arithmetic.

Mr Smith having, in a chapter on “Vegetable Food consistent with Physical Strength and Activity,” treated us with some very astounding narrations of personal vigour, and having, under “Climate and Temperature,” made a concession, for which the Equinox must be grateful to him, that—

“If farinaceous articles of diet cannot be procured in high latitudes, or if the persons residing there have not been accustomed to that kind of food, then, undoubtedly, animal oils and fat are the only substances that can be substituted for them,”—

and, having shown forth the manifold advantages accruing to mankind from vegetable food; next holds out to us, in terrorem, the evil effects of animal diet in causing certain diseases. He not being a medical man, this matter rests solely on quoted authorities, and the way in which he selects examples which he supposes favourable to his views, and disposes of those which do not agree well with his ideas, will be amply illustrated by two examples:—

“Abernethy says—‘Animal substances are changed into a putrid, abominable, and acrid stimulus,’ which was verified by Sir Edward Berry, who prevailed upon a man to live on partridges, without vegetables; but after eight days’ trial he was obliged to desist, in consequence of strong symptoms then appearing of an incipient putrefaction.’

This will be admitted, on all hands, to be a very fair proof, if proof were needed, that man is not a hawk, and should not attempt to feed in the same way as that feathered biped. That it proves anything more, we entirely deny.

“In that usually fatal disease, diabetes mellitus, a diet, consisting almost exclusively of animal food, is considered by most practitioners absolutely necessary; and all vegetable productions containing starch or sugar are most scrupulously forbidden. But if fruit and farinacea are the natural and best food of man, they must be equally proper, when judiciously selected, in health and in diseases of every description. Medicines which are unnatural to man in a state of health, are, doubtless, of great use in disease; but food is surely intended to supply nutriment to the system, and to support respiration; and these purposes will always be best effected by such a diet as bears the closest
relation to the structure of the digestive organs. The general use of animal food in diabetes must, therefore, be attributed to an imperfect knowledge of the cause and nature of the complaint."

We believe that no one will deny our very imperfect knowledge of the cause and nature of diabetes, and that we have need of all the enlightenments which sound theory and experience can throw upon the subject. Science and theory have not done very much for us, and experience not much more; but certainly the latter has shown us that, if anything do good, it is an azotised diet, under which, in a considerable proportion of cases, we, like Dr Watson, dare not say the disease is cured, but the exhaustion of the vital powers is delayed or arrested. It is a little too much that Mr Smith, after quoting a detached sentence or two from Prout, and Bernard's experiments on the existence of minute quantities of sugar in the liver, should undertake to teach us how to treat diabetes, in the following sentences:

"As in dyspepsia, a well regulated diet of animal food, and a sparing supply of vegetable matter, will be much more beneficial to the diabetic patient than the indiscriminate use of all kinds of food of which he may be tempted to partake; but, on a mild farinaceous diet, exclusive of all animal food, there would be a much better prospect of recovery, although the saccharine state of the urine might for a while be increased by it. The presence of sugar in the urine, is only a symptom, not a cause, of the disease; and as an azotised diet may diminish the amount of sugar without curing the complaint, so a purely farinaceous diet may, by its mild and nutritious qualities, restore both organ and function to health, though the symptoms should at first appear to be aggravated."

In a subsequent chapter, on "Vegetable Diet favourable to Longevity," we have a statement made of so curious a nature that we cannot resist extracting the passage. The Mr Graham here named, is an American, author of a series of lectures on vegetable diet, from which Mr Smith quotes largely:

"It is said by Mr Graham to be 'well known, also, that human blood, formed from animal food, will putrify, when taken from the living vessels, in a much shorter time, than that formed from pure vegetable aliment; and that there is always—other things being equal—a much greater febrile and putrescent tendency in the living bodies of those who subsist mostly on animal food than in those who subsist mostly on pure vegetable aliment. Hence, if two healthy robust men of the same age—the one subsisting principally on flesh meat, and the other exclusively on a diet of vegetable food and water—be suddenly shot down and killed, in warm weather, and both bodies be laid out in the ordinary manner, and left to the action of the elements and affinitires of the inorganic kingdom, the body of the vegetable eater will remain two or three times as long as the body of the flesh eater will, without becoming intolerably offensive from the process of putrefaction."

We are not informed where this curious observation was made; for made it surely must have been, from the precise manner in which it is stated. We can only hazard a conjecture, that Mr Sylvester Graham may, at one time or other, have been resident at the court of the King of the Cannibal Islands, and have had the entrée to the
royal game larder, where he could observe the progress of the various samples of human g&ibre, as they gradually attained the haut-gout requisite for his Anthropophagous Majesty’s fully enjoying a good dinner. It is added, that “Majendie fully confirms this statement;” but, as the particular passage is not quoted, we cannot append to the above experiment the ipsissima verba of the French physiologist’s corroboration.

We would gladly set before our readers some more specimens of Mr Smith’s statements in favour of vegetable diet; but we have, we believe, quoted enough to justify our avowal, that he has not succeeded in seducing us into vegetarianism.

The reasons assigned for a system of action, however, may be but indifferent, and yet the system may be good in itself, if it only be unquestionably established to produce good effects in the end. The proof of the system lies in its results; we, therefore, quote, in conclusion, one passage more from Mr Smith, both because it shows what vegetable diet can do, and because it brings us back once more to the Vegetarian Society, of which we had almost lost sight, in our devotion to Mr Smith. In reporting the first meeting of the Vegetarian Society, he gives us, among other instances of the good achieved by it, the following:—

“The wife of another gentleman at the meeting had abstained from flesh, and all intoxicating drinks, for thirty years; had given birth to fifteen children during that time; fourteen out of the fifteen she had suckled; and yet remained hale, young, and happy-looking.”

Now, to us, this result of vegetarianism wears rather a formidable aspect. We speak as paterfamilias, blessed with a fair allowance of children, who, on a moderate mixed diet of vegetable and animal matters (certainly never eight days’ partridges), have enjoyed an amount of vigour, and exemption from disease, for which we ought to be, and hope we are, grateful. We have, therefore, no direct inducement, from our own experience, to fly from the use of animal food; and if the above be due to vegetarianism, we have no temptation to rush into it, for we tremble to think that the result of its adoption by the wife of our bosom might be, in the course of a few years, an increment of our “little responsibilities” to the tune of a round dozen or so. We have often heard of fish and mollusca as being rather anti-Malthusian in their effects, but they must yield the palm to vegetables.

Mr Smith’s arguments, therefore, have not converted us to vegetarianism. We willingly concede to him, that a great many persons in this country, and a still greater number of Europeans in tropical climates, eat a great deal too much flesh; and had he contended himself with showing the abuses of animal food, instead of arguing for its total exclusion from our tables, he might, with a little less zeal and a little more judgment, have done some real good by his book. We fear that its extreme doctrines will effectually prevent it from bringing about any such result.
Dissertation on Scientific Nomenclature, Medical and General; exhibiting the Defects, Anomalies, Errors, and Discrepancies of its present condition, with Suggestions for its Improvement. By R. G. Mayne, M.D., Surgeon to the Leeds Lock Hospital. 8vo. pp. 82. London, 1849.

We confess we took up this book without much hope of finding in it matter for approbation. We have been agreeably disappointed. We have read it with attention, and now unhesitatingly recommend it to our readers for a careful perusal. It was our strong conviction of the chaotic state of medical nomenclature, which led us to despair of Dr Mayne's efforts for its improvement becoming anything else but a waste of his time, learning, and talents. The end he has in view is most desirable, and though we think the utmost exertions of one individual can go but a short way towards its accomplishment, yet we feel satisfied that our author has begun his undertaking in the way best calculated to attract other labourers into the same field. We therefore fondly anticipate that the correction of medical nomenclature may, ere long, become a prominent topic of interest, and a subject of cultivation, with the medical profession at large.

The book before us is an octavo of about eighty pages, exemplifying some of the principles on which Dr Mayne proposes to publish an Expository Lexicon of the terms in medical and general science. It consists of two parts: the first relates to terms of science having certain terminations,—as ides, ideus, adæ, idæ, ida, odes, &c.; the other treats of some examples of terms belonging to medical nomenclature, among which are anasarca, gastrocnemius, psalloides, emprosthotonos, opisthotonos, pleurosthotonos, platysma-myoides, ephemera, narcotics, antispasmodics, tonics, fibrine, stearine, salicine, &c. This little work, then, is a mere specimen of what the author intends to do in his larger work—and this specimen we uphold, as giving ample promise of Dr Mayne's fitness to make a valuable addition to the literature of medical nomenclature. We urge him to proceed with his task.

We should willingly have entered to some extent into many of the topics suggested in the pages of our author, but time presses, and doubts arise how far a lengthened discussion of such subjects would be suitable for this publication. Our remarks will hardly extend beyond his first section, namely, on terms ending in ides—but even these, we trust, will serve to give our readers some idea of the magnitude and importance of the subject, and of the manner in which he proposes to perform the task which he has taken upon himself.

Referring, then, to Latin terms in ides, as clinoides, deltoïdes, and hyaloides, our author shows satisfactorily that these are in general inaccurately pronounced, as if the oi were a diphthong, and
that the only proper pronunciation is as if the word were written clino-ides or clinoides, &c. All such words in their Greek form are unquestionably written as σφαιρειδής, like a sphere, are declined as adjectives, and signify, not having the exact figure of the object from the name of which they are derived, but making an approach to its figure. We entirely agree with our author that adjectives of this form are not synonymous, as some think, with those derived from the same root, ending in us or eus, as sphaericus, or deltoides. The latter can have no other signification than “of or belonging to a deltoid body,” and in anatomy is only applied correctly to things standing in such relation to the deltoid muscle as its vessels, nerves, &c. Such adjectives, however, as sphaericus, conicus, &c., have a less definite signification—since these not only denote “of or belonging to a sphere, a cone,” &c., but also “having the figure of a sphere, a cone,” &c. These differ from the corresponding words in oides, inasmuch as they express, not an approach to the figure referred to, but its actual type. Our author inclines to think the termossa sesamoides, an example of the substitution of an adjective in eus for the neuter plural of sesamoides. There we must dissent from him; and it is enough in defence of our opinion to say, that his assumption is unnecessary. We know how many dictionaries he can quote in support of such a statement; but on a point like this, the real question is not what grammars and dictionaries teach, but what is the actual usage of the original authorities in medical science who employed such terms. If Galen, as our author says, makes use of the term ὀστεία σεσαμοειδεα, there is an end of all difficulty—the term “ossa sesamoides,” of modern times, beyond doubt, represents Galen’s expression, whatever view dictionary-makers may have since taken of it, and his adjective can be nothing else but the neuter plural of σεσαμοειδής. The early Latin authors, in adopting technical terms from the Greek, used the actual Greek words, written in the Greek character. By degrees these terms were changed into a Latin form, and were written in the Roman character, the inflexion of the words, however, being kept as near as possible to the Greek model. Thus, Celsus, speaking of the eye, says, “quod a vitri similitudine ἵαλοειδες Ἑξαί οὐκ ὑποί” where the Greek term is plainly declined so as to agree with the accusative case neuter of the relative quod. And in the sentence immediately preceding, the same author, referring apparently to what anatomists now term the hyaloid membrane, says, “quam Herophilus ἄφαντανοείδης νομι- navit;” where the Greek word is also inflected in the manner proper to the language. At a later period, for example in the fourth century, Servius writes, “cupressus et ipsa conoides est; nam a rotundat in acumen levatur,” on which a commentator remarks, “sed melius Εξαί scribatur.” We attach no value to the rule quoted by our author from the Eton Grammar, that Latin adjectives in ἐς, or ἐς have seldom or never the neuter in the singular number, and very rarely in the plural. However true this may be
of the words employed by classic authors in general, it is wholly inapplicable to terms of science, and we would recommend our author, in his future researches, to trust as little as possible to grammars and dictionaries, the authors of which are seldom sufficiently conversant with the ancient works on technical subjects. Let him have recourse to the best classical authorities as exhibiting the actual use of scientific terms in their original purity. It must be confessed, however, that to throw the expression, "ossa sesamoidea" back on a classical form, is but a small point gained when so many similar words in anatomy are left in an almost hopeless state of inaccuracy.

The radical error undoubtedly lay, as our author has pointed out, in regarding the word in *ides* as a substantive, from which an adjective in *eus* might be properly formed without any change of meaning, when joined with a new substantive. For those who wrote in Latin without understanding the Greek inflections, this was a natural, if not an unavoidable, error. And hence, doubtless, has arisen the present form of so many of our technical terms, particularly in anatomy. The difficulty of restoring anatomical nomenclature to accuracy, even under words ending in *ides* alone, looks very formidable when we run over such terms as—tunica choroides, membrana hyaloidea, tunica arachnoidea, processus odontoides, cartilago thyroidea, cartilago cricoidea, flexura sigmoidea, processus mastoideus, processus pterygoideus, processus coracoideus, processus styloideus, cavum cotyloideum, cavum glenoideum, valvulae sigmoideae, cartilagines arytenoideae. When, however, the difficulty, as it exists in reference to words of this form, is fairly examined, it is found to be hardly so great as it seems in the distance. In the first place, as Latin is no longer in much use for description in anatomy, no more is required than the uninflected form of the correct expression, such as tunica choroides, membrana arachnoidea, processus odontoides, membrana hyaloidea, &c.; secondly, the adjective in *eus* is quite proper where the constituent elements or appendages of the parts referred to are concerned—for example, arteria sigmoida, arteria mastoidea, musculus pterygoideus, ligamentum styloideum; thirdly, there are a few models of this form of expression which have never ceased to be in universal use, witness os hyoides, os ethmoides, os sphenoides; lastly, the plural number, which in this form of word alone creates an insurmountable difficulty, is required in a few cases only. When the word is neuter, the neuter plural of the correct form of the adjective coincides with that of the incorrect form, as we have seen in the case of *sesamoidea*. Of the rest, cartilagines arytenoideae, and valvulae sigmoideae must be left in an inaccurate form, unless anatomists will consent to say cartilagines arytenoidees, and valvulae sigmoidees.

As the adjectives in *eus* are undeniably of a Latin form, the com-
mon pronunciation at present of such terms as deltoideus, mastoideus, cannot be defended on any ground whatever. And thus it may be found as easy to change flexura sigmoidēa into flexura sigmoidēs, as to train the organs to pronounce flexura sigmoidēa.

Fortunately the English form of the terms under discussion, now so generally adopted, creates no difficulty—choroid, hyaloid, atrytenoid, sigmoid, serve for all cases, genders, and numbers. And while we agree entirely with our author, that in the Latin form all such words are not correctly pronounced, otherwise than by a distinct enunciation of the two vowels o+i, hyaloïdes, &c., we cannot suppose that he wishes us to imitate the same rule in English. We allow that the conversion of the o+i of these words into a diaphong in English is a corruption; but it is already an unalterable rule of the English language—in it oïd is a monosyllable, signifying likeness, as in the words rhomboid, conchoid, cycloid, epicycloid, cora-loid, paraboloid, prismoid, ginglimoid, conoid, spheroid. Were we called on, in modern languages, to renounce whatever can be proved to be a corruption, there would be an end even to that short-lived stability which words are allowed to attain.

We agree with our author entirely in what he says as to the proper etymology and signification of such words as phlegmonodes, erysipelatodes, impetiginodes, icterodes, though we cannot allow that the members of the medical profession in general confound these with terms in oïdes, like the lexicographers whom he quotes.

Before we part with our author, we wish to show our good will by tendering to him one or two pieces of advice; and, had circumstances permitted us to go over the whole book with the same minuteness with which we have treated of the earlier part of it, perhaps we might have had more advice to offer. In the first place, we advise him to leave alone the names of the classes and orders of the Linnaean system. As a reformer of purely medical nomenclature, "satagit rerum suarum." Besides, the Linnaean system is rather a matter of history than of present use in botany. We recommend to him, also, to leave the spiritus asper (familiarly known as the letter k), in connection with the letter r, in the place where lexicographers have placed it. We assure him that any labour he may choose to bestow, will be thrown away in the attempt to persuade the whole of Europe to change the present mode of spelling such words as rheumatic into hreumatic, rhetoric into hretoric, rhapsody into hrapsody, catarrh into catarrhr, hemorrhage into hemmorraghe. If he adopts such a mode of spelling in his lexicon, he will surely gain for himself the character of a wild speculatist. We beg of him to eschew it.

We will make but one remark on the first section of the second part of the book, entitled "Terms Peculiar to the Nomenclature of Medical Science." The first term spoken of is anasarca. We en-
tertain no doubt that anasarca or hyposarca was originally two
words, ανα σαρκα, υπο σαρκα, and the proof is, that the corresponding
Latin word, manifestly a translation, is "inter cutem," which after-
wards became a declinable word from an assumed nominative
"intercus."

Pathological and Practical Observations on Strictures, and some other
Diseases of the Urinary Organs. By Francis Rynd, Esq.,
A.M., M.R.I.A., Fellow and Member of Council of the Royal
College of Surgeons in Ireland, &c. &c. London, 1849. Octavo,
p. 195.

This work bears the impress of being the result of very consid-
erable experience in the important class of diseases of which it treats,
and though there does not appear to us to be much that is new in
the present treatise, yet the author's descriptions of disease, illus-
trated as they generally are by appropriate cases, are so clear and
truthful, that we think the treatise is one which will prove accept-
able both to the student and the practitioner. The former will
find in it ample and perspicuous details of the pathology and treat-
ment of some of the most important diseases of the urinary organs,
and a safe and judicious guide to practice; whilst the practical
surgeon will recognise the faithfulness of the descriptions of dis-
ease contained in it, and may draw some useful hints from it as
to the treatment of special cases. The chapters on Rupture of the
Bladder, Retention, Extravasation of Urine, Urinary Abscess,
and Fistula in Perineo, are well worthy of perusal. We can scarcely
suppose, however, that the author means to claim, in reference to
the treatment of fistula in perineo, the degree of originality which
his words would imply; for, at page 144, after stating his opinion
that all operations for fistula are futile, unless attention has been
previously paid to the state of the urethra, he goes on to say:—

"The difference, then, between the principle I advocate and that I have
more generally seen followed is, that I operate on the urethra in the first in-
stance, in order that the impression made on it may guide and direct my pro-
ceedings with respect to the fistula; whereas others divide the latter in the
first instance, and take the chance afterwards for dealing with the urethra
concurrently with the healing of the wound."

We think that the principle insisted on by Mr Rynd—that of
primarily and principally directing attention to the state of the
urethra, before having recourse to any other operative procedure
for the cure of fistula in perineo—is one which has been long and
generally recognised by the profession, being advocated by most
modern writers on surgery, both British and foreign. Though, of
course, our author must be better acquainted than we are with the
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practice of Dublin surgeons, we may observe that the same principle was distinctly pointed out by the late Mr Colles of that city. In the lectures of that eminent surgeon, lately published, after stating his opinion, that the passage of the urine over the fistulous track is not the principal obstacle to the healing, he says (Lecture XXIX., p. 53, vol. ii.):—"The fact is, it is the morbid condition of the urethra that keeps it open." So that, whilst the principle is one that cannot be too often or too strongly inculcated, it is at the same time one which we had thought was universally followed in practice.

As a whole, we have no hesitation in heartily recommending Mr Rynd's treatise to our readers, as containing much valuable and instructive matter.

**Part Third.**

**MEDICAL NEWS.**

**DISCOVERY OF A PECULIAR CORPUSCLE IN THE VOMIT, DEJECTIONS, AND SWEAT OF CHOLERA PATIENTS; AND IN THE ATMOSPHERE OF INFECTED LOCALITIES.**

In the beginning of July last, Mr Brittan of Bristol, and Mr Swayne of Clifton, were deputed by a committee of the Bristol Medico-Chirurgical Society to examine microscopically the evacuations from two cholera patients, and to make reports and drawings of the appearances met with. The drawings were presented at next meeting of committee, and attracted attention from their representing bodies which each observer considered characteristic of the evacuations in cholera. In the "Medical Gazette" for September 1849, Mr Brittan has given the following account of his observations, which he has illustrated by drawings and a series of tables. "I examined, in conjunction with Mr Swayne, two specimens of rice-water dejections, and on comparing our drawings we were struck with the peculiar appearance of certain bodies depicted in each. On further prosecuting this investigation, I found these bodies to be constantly present in the rice-water evacuations of the cholera patients, and offering the same characteristic appearance that distinguished them from anything I had before observed. In order to ascertain if they bore in their size or quantity any relation to the severity of the symptoms, I endeavoured to obtain specimens passed by the same patient at different periods, as well as to complete the observation by an account of his condition at the time. The result was, I became convinced that a certain relation does exist between the size and number of these bodies, and the time elapsed after the seizure, taken in connection with the severity of the symptoms. That is to say, they are small and clearly defined in the matter vomited; they become larger and more compound in the dejections; and as the disease progresses favourably, where I have had the opportunity of examining, they vanish as the symptoms disappear, and the motions regain their natural appearance. I have also found that in very rapidly fatal cases these bodies are sometimes to be met with only in very small quantity, or are altogether absent, though this observation must be qualified by the remark that it is not always possible, or at all events has not been with me, to obtain portions of every motion passed, and that these bodies might have been present in those not examined. On every opportunity that I have had of examining the intestines of those who died from cholera, these bodies have been