

feared that his exertions, if uncontrolled, would soon exhaust him, or lead to serious determination to the brain, it was thought advisable to have a strait waistcoat in readiness, it being considered that he would suffer less from this mode of restraint than from manual force. Happily, however, this unpleasant measure, of last resort, was not needed, as he became tranquil after the first dose of the morphia, which amounted to nearly a grain and a half, for some of the grain dose having, in his struggling, been spilt, Mr. Fitzpatrick gave also the half-grain dose. He soon became drowsy, and at twelve o'clock fell asleep, and slept continuously and soundly till six in the morning, when he awoke quite calm and collected, perspiring copiously, but complaining of nothing besides weariness and exhaustion. He referred to the attack of the preceding evening, detailing very correctly the sayings and doings of the attendants and bystanders, who then thought him unconscious, and regardless of what was going on. Some brandy and soda water was now given, followed by another draught of morphia, which was succeeded by a sleep of three hours, when he again awoke, feeling very comfortable. After taking some breakfast, the morphia was repeated, with the usual effect of inducing sleep.

At our visit before noon, he was quite awake, with a natural expression, perfectly rational, with his pulse reduced to 76, and soft, and nothing complained of but weakness. Under these favourable circumstances, it was agreed that the opiate should be discontinued, and a draught of tincture of hops, sal volatile, and camphor julep, was substituted. It was necessary, however, to repeat the morphia that and the following night, as he was found, towards evening, to be getting rather talkative, fidgety, and excited. After this time, he passed good nights without the morphia, and from day to day the nervous symptoms subsided. His convalescence, however, was slow, and the return to full strength retarded by the great hepatic and intestinal derangement which was now found to exist. For a long time the evacuations consisted of black, pitchy matter, discharged daily in great abundance. This condition it was necessary to heal with some tenderness, for any amount of purgative invariably increased the nervousness. Under the use of small doses of mercury and chalk, with Dover's powder, and the calumba-bitter, with carbonate of soda or lime water, the secretions were improved, the appetite returned, and the patient eventually recovered perfectly.

The two cases just detailed illustrate the important fact, that much of the danger and much of the obscurity which attend a given disease, may be traced to the complications with which it is associated; and that a due regard to the nature of these complications would go far to explain the discrepancies which unhappily prevail among practitioners respecting the efficiency, value, and safety, of particular remedial agents, and general modes of treatment.

The last case, though not entirely uncomplicated, was so purely an example of delirium tremens, that no hesitation could be felt in having recourse to the ordinary remedy. The event justified this course, for the disease was subdued, and left the patient sound. In the first of the cases, the affection was an intruder, as it were, upon a group of diseases, which had long held possession of the whole frame, any one of which would, in no long time probably, destroy life. Still the facts of the case suggest the grave question, whether the treatment adopted caused or favoured the development of the ultimate serious head attack; whether, in short, considering that the specific poisonous effects of opium are of a lethargic character, and the resulting lesions, turgescence of vessels, increased vascularity, and sanguineous extravasation, the drug may not, when used as a remedy in the predisposed, lead to an apoplectic state? I am inclined to think, that in the instance before us such was not the case. The tranquillity which invariably succeeded its administration; the absence, subsequently, of stupor, or contraction of the pupils, and the cessation of tremor and other distressing symptoms, seem to show that the drug acted medicinally only. Dr. Sutton, to whom we are indebted for the introduction of the opium treatment in delirium tremens, and whose views, in other respects, have been so remarkably verified by the experience of subsequent observers, informs us, that he was in the habit of using the medicine with safety and success in certain cases of apoplexy and palsy, being decidedly of opinion that the association of paralysis with delirium tremens ought not to divert practitioners from the use of a medicine which is so eminently successful in the latter disease. It would even appear that this peculiar and dangerous affection of the nervous system has a paramount claim to attention in all cases, and that its specific treatment cannot be safely dispensed with, whatever

may be the nature of the disease on which it is engrafted. That the presence of traumatic inflammation is no bar to the administration of opium is abundantly attested by the experience of our surgeons, particularly of those serving in the metropolitan hospitals, who, on the supervision of delirium upon severe injuries and the great operations, find themselves often forced to lay aside their evacuates and the antiphlogistic regimen, and have recourse to stimulants and opium to save their patients.

Harley-street, Jan. 1847.

ON THE ACTION OF ATROPINE IN PAINFUL AFFECTIONS OF THE FACE, ETC.

By W. PHILPOT BROOKES, M.D., M.R.C.S.E.,

SURGEON TO THE CHELTENHAM GENERAL HOSPITAL AND DISPENSARY.

A FEW weeks back, I was called in to a lady in this town, suffering from a severe cold, accompanied with a most intense and painful affection of the right side of the face, forehead, and around the orbit of the eye. The pain continued after all the symptoms of the cold had left her, and I could not allay it with warm fomentations or other common remedies. I at last tried the application of an ointment, composed of atropine, five grains, lard, three drachms, with one drop of oil of rose; a piece the size of a pea to be applied three times a day. The pain was allayed after the second application by day, but at night returned with as much violence as before. The remedy was continued, and after two days, all pain ceased, and has not since returned. The effect of it was so marked, that I am inclined to think it will prove a most useful remedy in painful neuralgic affections.

I must also mention the marked effect it had on the pupils of the eye, in this case, after the second application of it: they were dilated to a great extent, (much more than I ever saw from any other preparation of belladonna,) and continued so for two or three days after it was discontinued.

I have since tried it in the case of a man on whom I operated for cataract in both eyes. The one in the right eye was not perfectly depressed, and rose again; (he had also lippitudo of the eyelids from a burn.) Belladonna had but little effect on this eye, (although it perfectly dilated the other,) but the ointment of atropine, three grains to two drachms, dilated it effectually. In a case of glaucoma I have now under treatment, belladonna will not increase the size of the pupil in either eye to any great extent, but the ointment does so satisfactorily.

I certainly think this active principle is well worthy of a trial; and it will be found a much cleaner, more certain and elegant form than any other preparation of belladonna.

Cheltenham, Dec. 1846.

THE OPERATION OF PARACENTESIS THORACIS.

By J. CARSON, M.B., Physician to the Fever Hospital and Workhouse, Liverpool.

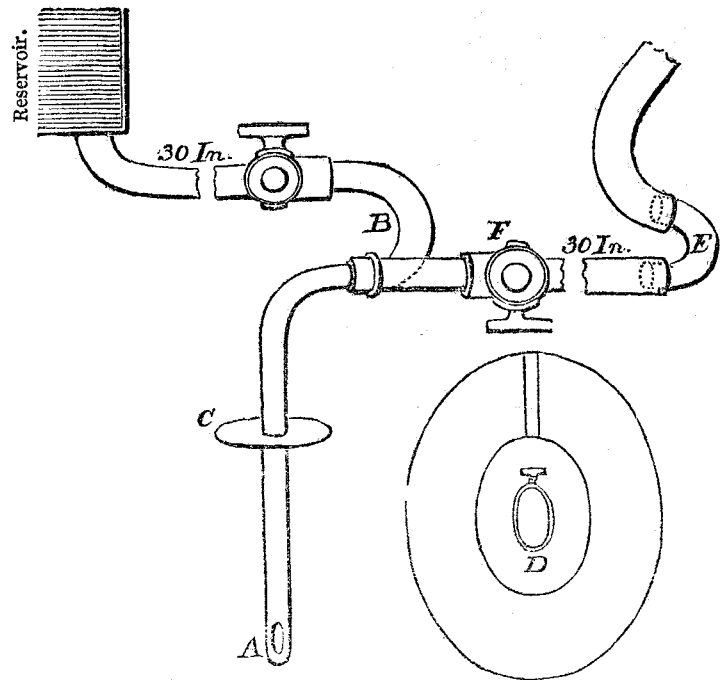
PERMIT me to draw the attention of the medical profession to a plan of performing the operation of paracentesis thoracis, published in the eighteenth volume of your journal, p. 456, in the year 1830, by my father, the late Dr. Carson. I am induced to revive the idea by the highly favourable opinion of the plan expressed in a letter addressed to me on the subject by that eminent physician, the late Dr. Macdonnell, of Belfast, and by the notion, that with some slight modifications, the instrument may, with advantage, be made use of in the treatment of other surgical diseases. A case which recently fell under my own observation, in which the operation of paracentesis was performed in the usual manner, affords a good example of the nature of the obstacles to the successful performance of this operation, which it is one of the principal objects of this plan to obviate.

Martin M—, an Irish labourer, of intemperate habits, was admitted into the workhouse hospital on the 27th November. He had been discharged six weeks previously after a severe attack of dysentery. He had, at that time, slight bronchitis, which continued, in a chronic form, until six days previous to his readmission, when he was seized with shortness of breath, and says he noticed swelling of the right side of the chest. For this he had been bled twice.

On admission, the right side of the chest was found to be

an inch and a half larger than the left, and the vocal vibrations were not felt. There was general dullness on percussion. On the left side respiration was exaggerated, and attended with general mucous rhonchus. There was no respiratory murmur on the right side, but at the root of the lung there were heard bronchophony and bronchial respiration. The expression of countenance was anxious, and there was considerable dyspnoea. Calomel and hyoseyamus were ordered, with the view of affecting the mouth, and a large blister was applied to the right side. This course was proceeded with for two days, when, the debility increasing, some wine, with stimulant expectorants, were administered.

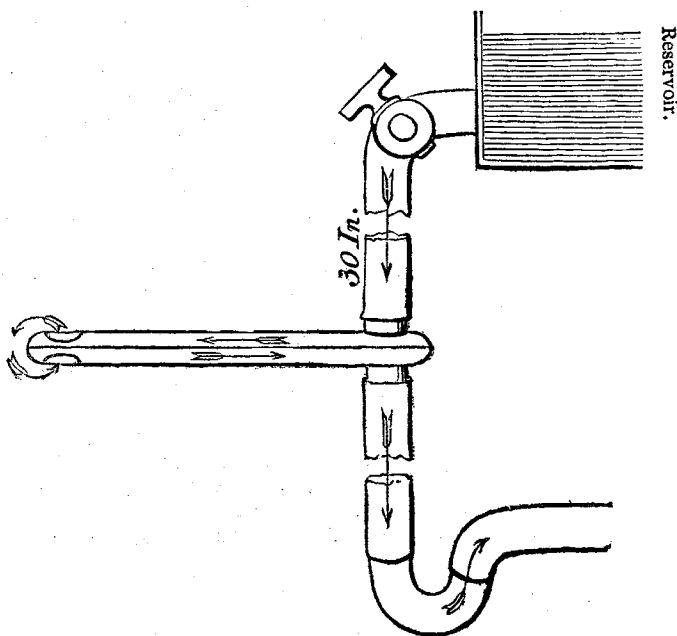
On the evening of the third day, I was sent for to see him, and found the dyspnoea much increased, approaching to suffocation, as the effusion had obviously increased in quantity, and by its pressure prevented the perfect expansion of the left side of the chest. I requested my colleague, Mr. Burgess, to perform the operation of paracentesis thoracis. The puncture was made beneath the fifth rib with a trochar, the canula of which was about the size of an ordinary goose-quill. Through this was evacuated, in a free stream, about twenty-two ounces of clear serum, without any appearance of shreds or particles of lymph. The stream was affected by the respiratory movements in jerks, which were more marked as the fluid ran out, and at the last, care had to be taken at each respiration, by placing the finger over the orifice of the canula, to prevent the entrance of the air into the cavity, although there was obviously remaining a considerable quantity of blood in the cavity, and although the passage was not impeded by any particle of lymph, or by the apposition of the diaphragm or lung to the internal orifice, yet by no change of position could any more fluid be drawn off than the quantity mentioned. The outflow of the fluid was stopped, as is the current from a barrel of beer, before the vent-peg is withdrawn. The canula being withdrawn, and the wound dressed with a pledget of wet lint, secured by adhesive plaster, the patient was allowed to lie down. His dyspnoea was obviously much relieved, and he expressed himself as easier, though exhausted. Some wine was given to him, and the following day, at noon, he informed me that he had spent a better night than for some time previously, that he had breathed much easier, that his cough had been less troublesome; but that he felt the difficulty of breathing and uneasiness coming on again, and begged me to repeat the operation. He was, however, in a state of very great debility, and, in spite of the favourable effect of the first evacuation, my opinion as to the ultimate event was not improved. I directed the wine and stimulant expectorants to be continued, and at eight o'clock in the evening, about sixteen ounces more of fluid were withdrawn from the chest, of the same character. The same circumstances presented themselves as in the former operation, and prevented a more complete withdrawal of the fluid. This operation was not followed by the same beneficial results as the preceding, and the patient sunk exhausted early the next morning. On a post-mortem examination, the right cavity of the chest was found to contain several pints of serum, free from lymph or purulent admixture; the lung was compressed into a small size, of a blue colour, quite free from air, not crepitant, and sinking in water. There were no adhesions, nor the slightest appearance of false membrane upon its serous surface, which, as well as that of the costal pleura, was in a perfectly natural condition. The left lung was congested, especially at the base, and there was marked sanguineous congestion of the lining membrane of its bronchi, which were filled with the yellow mucous expectoration which he had thrown up during life. The liver was contracted in bulk, and presented an admirable specimen of the hob-nail degeneration. I regret that, from hurry and inadvertence, I did not examine the kidneys, as from the intemperate habits of the man, and the puffy, anæmatous character of his countenance, I think it is probable that he laboured, in addition to his other disease, under Bright's affection of the kidneys. Although from the first, in this case, the prognosis was necessarily unfavourable, yet from the relief obtained from the first tapping, I am inclined to think that had the operation been earlier performed, and had we been able more effectually to evacuate the effusion into the right cavity, a better chance might have been afforded of recovery of the patient from the present urgent symptoms at least. The condition in which the liver was found puts out of the question altogether the idea of ultimate recovery. I believe the plan suggested by my father would have obviated the difficulties met with in the operations just detailed, and I have since had constructed (by Messrs. Reay and Robinson, of this town) an apparatus on the principle he laid down.



Plan of instrument arranged for tapping or injecting.

A B is a flat canula, about three inches and a half long, closed at *A*, with eye-holes at the side sufficiently large to permit the passage of small flakes of lymph. At *B* the canula is bent down, and there is a ledge round its open extremity by which a tube of Vulcanized Indian rubber can be attached. *C* is a flange, of which *D* is a plan, which slides upon the canula, and is fixed at any necessary point of it by a small screw. The Indian rubber tube attached to the open end of the canula should be about thirty inches in length, and to its lower extremity is fixed a bone or wooden continuation of the tube, of the form represented at *E*. A piece of cat's intestine, three inches long, attached to the extremity, *E*, completes the instrument. There should be a small stopcock, *F*, on the tube near *B*. The operation is performed as follows:—An incision about an inch in length being made at the point selected parallel and approached to the upper edge of the rib, and continued down to the pleura; a perforation is to be made in this membrane sufficiently large to admit the closed extremity of the canula, which, with the tube attached, is to be prepared in the following manner:—A circular piece of adhesive plaster is to be fixed to the inner side of the sliding flange, so that its adhesive surface be towards the wall of the chest. The tube and canula are to be filled with blood-warm water; the small piece of cat's intestine being previously well soaked in warm water to make it flexible. The tube is to be inserted into the aperture in the pleura, to the requisite depth, the lips of the wound being pressed with the fingers against the flattened sides of the tube, to prevent the escape of fluid between the tube and the walls of the incision. The sliding flange, with the sticking plaster, is to be pressed upon the chest, secured by its screw, and more firmly attached to the chest by straps of sticking plaster passed over its outer side. The cock, *D*, being then opened, the fluid gushing out of the chest will descend through the tube, without the possibility of any air entering into the cavity. When, however, the evacuating force of the distended chest has ceased to discharge the fluid, it is still drawn out by a force equal to the pressure of a column of water, of thirty inches, or of any height to which it may be thought necessary to increase the pendent tube. The use of the cat's intestine is, to act as a valve, which freely permits the exit of the fluid, but will prevent any regurgitation which might be caused by a deep inspiration. In the instrument proposed by my father, the tube was recommended to be of glass or metal, to prevent the collapse which the pressure of the atmosphere would produce in a flexible tube, the amount of which would of course be in proportion to the length of the tube. It is conceived, however, that the elasticity of the tubes of Vulcanized Indian rubber is sufficient to obviate this difficulty. The regurgitation and entry of air was to be prevented by keeping the lower extremity of the tube in water. The flap-valve, made by the flexible cat's intestine, suggested by M. Reybaud, in 1841, in an instrument for paracentesis thoracis, which I will now describe shortly, is a decided improvement on my father's plan. M. Reybaud's instrument is precisely the one described, with

the exception of the long tube. The sliding flange for fixing the instrument to the walls of the chest, and the flap-valve, are both suggested by him, and are decided improvements. The canula is perfectly straight, and is so made at its open extremity, that an instrument for injecting the cavity of the chest may be connected with it. In the event of the necessity of such injection, it may easily be effected by the instrument which I have described. If the tube, filled with tepid water, as in the commencement of the operation, be inserted in the bottom of a reservoir, containing the fluid to be injected, raised to a sufficient height above the level of the orifice in the chest, an injection will thus be thrown into the chest, with any degree of force required. An account of M. Reybaud's instrument, and of the successful use of it in several cases, is given in the New "Dictionnaire de Médecine," article "Poitrine," tom. xxv. I conceive this plan of operation may be applied with great advantage in the evacuation of lumbar abscesses and other large collections of pus anywhere in the body, in which openings made in the ordinary way are followed by such alarming constitutional symptoms, from the putrefaction of the pus caused by the admission of air. A very simple modification of the instrument which I have described, would enable us to inject and evacuate the cavity simultaneously, and thus to establish through the interior a constant current of tepid water, or whatever fluid it might be thought proper to throw in. If the canula were divided into two cavities by a septum, as in the catheter by which fluid is injected into the bladder, and if the upper division were in communication with a flexible tube descending from a vessel containing the fluid to be injected into the cavity and the lower, with a similar depending tube, as in the annexed engraving, a continued



current of the fluid, in the required quantity, and with the necessary degree of force, might be made to pass through and wash the walls of the cavity. Stop-cocks on each of the tubes would with ease regulate the quantity, and the requisite degree of force would be determined by the length of the tubes, and would, for either, be within the limits of the pressure of a column of water thirty inches in height.

In the forty-third volume of the *Philosophical Transactions*, in the twelfth page, there is the history of the cure of a dropsy by the use of stimulating injections into what was supposed to be the cavity of the peritonæum, by Dr. Warwick, of Truro. The disease, supposed to be an ascites, was evidently an ovarian cyst. Equal parts of claret and Bristol water, in large quantities, were injected twice into the cavity, and in less than a month the woman was cured of a disease under which she had laboured for several years. The case at the time seems to have excited considerable attention, and, in consequence of it, the celebrated Stephen Hales suggested, in the same volume of the work, a method of throwing fluids into the peritonæal cavity, in principle similar to that now proposed. Instead, however, of a canula with a double chamber, he advises the insertion of canulæ into two punctures in different parts of the abdomen—the one for injecting, and the other for drawing off, the fluid. Dr. Warwick repeating his experiment with this apparatus, in real cases of ascites, was induced, by more than one fatal result, to retract, in another volume, his opinion of the propriety of this remedy. In the present day, when

the pathology of ovarian disease is better understood, and when the formidable operation of ovariectomy is so much in vogue, I think it is a question whether Dr. Warwick's mode of treatment may not, in a great proportion of cases, be with advantage resorted to. In a communication made by me to the *Provincial Medical and Surgical Journal*, I recommended a recurrence to the operation of Houston and Ledran. And although some of the most eminent surgeons both in this country and in France have condemned it, I hope to be able shortly to show that they have done so on insufficient grounds, and that this plan of treatment has been more eminently successful than any other in the treatment of the disease. Since I made this communication, and in consequence of it, I was called in to assist at an operation performed on these principles by Mr. Bainbrigge, of Liverpool, to whom I suggested the operation and its details, and to whom, from evidences which I had at that time collected, I was enabled to show the extreme probability of a safe and favourable result. The history of this case has been published by Mr. Bainbrigge, in the *Provincial Medical and Surgical Journal*, in terms that oblige me to recal to that gentleman's recollection, that his original intention, previous to consulting me upon the subject, was to remove a portion of the cyst, to return the remainder into the peritonæal cavity, to close the opening in the walls of the abdomen, and to leave the matter to Nature and the absorbent process. In this case, the presence of the adhesion of the ovarian cyst to the abdominal parietes at the place of incision, rendered unnecessary the steps by which it was intended to secure this condition. The sac was perforated and the communication with the outward air preserved by means of a tent. Inflammation and suppuration of the lining membrane of the sac was soon established, without any pain, or symptom of constitutional irritation in the patient. The cavity of the sac, which held, at the period of the operation, three gallons, gradually contracted, until, in the course of two months, the whole amount of discharge was reduced to about an ounce in the twenty-four hours. At present, I understand there remains a fistulous opening from which about half an ounce of pus is discharged per diem. Stimulating injections of strong solutions of alum, of iodine, in the proportion of one ounce of the tincture to seven of water, were at an early period thrown into the sac, without being the source of the slightest pain, or constitutional irritation to the patient. The principal inconveniences were the exhaustion occasioned by the profuse muco-purulent discharge, and the anorexia, from its excessive fœtor. I am now of opinion, though then cordially agreeing with Mr. Bainbrigge in that line of treatment, that it was not desirable to keep up, by stimulating injections, the irritation of the lining membrane of the cyst. On the contrary, I believe that if it were possible to have prevented inflammation altogether, the sac would have contracted at least as soon as it did, and the profuseness and fœtor of the discharge might have been altogether prevented. I hold this opinion, from confidence in the correctness of the great principle first introduced into modern surgery by the late eminent professor of Dublin, Dr. Macartney, that inflammation, in whatsoever degree it exists, is so far prejudicial to the reparatory processes by which a return to health in diseased structures is effected. A constant supply of tepid fluid water, washing out every part of the sac by means of the apparatus that I have suggested, would, by the removal of every irritating substance, and the constant application of the best emollient we possess, have most effectually served this purpose. The exhaustion from a profuse and protracted purulent discharge, and its unpleasant fœtor, with its consequences, would have been prevented. The question of the solution of calculi in the bladder, either by an alkaline or acid condition of the urine, induced by the use of internal remedies, or by the direct action of a solvent thrown into the bladder, has at all times occupied the attention of surgeons. Having written thus far, I referred to Dr. Willis's able work on "Urinary Diseases," and I cannot help joining in his expression of astonishment, that the removal of stone by the injection of solvents has not more frequently been tried, after the successful issue of the case thus treated by Dr. Butter. I find, moreover, from the same work, that Dr. Hales had invented an apparatus, similar in principle to the one I have suggested; and, by experiments on dogs, had shown that alkaline solutions of considerable strength could be borne in the bladder, without uneasiness, for some time. The instrument was improved upon by Gruithusen, and employed by Messrs. Majendie and Amussat, with decided benefit, in the case of an English gentleman in Paris. Dr. Willis quotes two cases in which this practice was followed by a complete cure; the one related by Sir B. Brodie in the *Medical*

Gazette for June, 1831, and the other a case of complete solution of a calculus, effected at Malaga, by Dr. Rodriguez, in the space of forty days, by an injection in which lemon juice was the solvent. By the instrument suggested, weak alkaline, or acid solutions, according to the diathesis under which the patient may at the time labour, in considerable quantities, may be passed in a continued or interrupted stream through the bladder, and alternated with currents of warm water, as an emollient, in the event of irritation resulting from the presence of the solvent. The experiments of Littre, Gruithusen, and Billaret, on the solubility of calculi in ordinary water, encourages the hope that, with patience, a continued stream of tepid distilled water would effect this desirable object. At least, such a mode of treatment is the best remedy for the chronic inflammation and pain caused by the presence of these foreign bodies in the bladder. If it were deemed necessary to keep the double-chambered catheter permanently in the bladder, the inconvenience of the incrustation by salts of the part of the instrument in the bladder, would be obviated by the dilution of the contents of that viscus, and the irritation of the neck of the bladder would be alleviated by the emollient influence of the water.

Liverpool, December, 1846.

ON THE NOMENCLATURE OF THE SCIENCE OF MEDICINE.

By R. G. MAYNE, M.D., Leeds.

No. III.

COMPOUND TERMS ENDING IN ODES.—NAMES, OR, RATHER, TERMS APPLIED TO THE LINNÆAN CLASSES AND ORDERS OF PLANTS.

THE quality and character of compound terms ending in *odes*, as *Icterodes*, *Hæmatodes*, &c., the number of which is comparatively small, I consider to be precisely similar to those of the terms in *oides*, noticed in No. 1 of these inquiries and suggestions. There is this difference, however, in their formation, that the Greek diphthong *ei*, of the root *εἶδος*, is changed into long *ō*, instead of long *ī* of the latter. The remarks, therefore, formerly offered are, with this exception, equally applicable to them.

The compound terms which distinguish the classes and orders of plants in the Linnæan, or Sexual System of Botany, have been hitherto characterized, where characterized at all, as nouns feminine of the first declension. In advancing an opinion that this character has been erroneously assigned to them, I fear, from the familiar intimacy with which they, of all other scientific terms likely to come under the present discussion, have long been entertained, the announcement will be received with doubt. Nevertheless, such a feeling must not be permitted to deter me from endeavouring to establish its correctness, as an important part of the purpose for which my inquiries were instituted.

These terms have no classical existence, but appear to have been invented and adopted for the systems of botany, to illustrate their peculiar arrangements. Those of them which are employed to distinguish the Linnæan classes, from one to fifteen inclusive, were meant to indicate the number of stamens, or male organs, existing distinct from the pistil, and from each other, in hermaphrodite flowers. The first class was accordingly named *Monandria*, from *μόνος*, one; *ανήρ*, a man, as the symbol of the male organ of plants; the second, *Diandria*, from *δῖς*, twice, or double; and *ανήρ*: the third, *Triandria*, from *τρῖς*, three; and so on with the remaining twelve, varying the first, or the first two syllables correspondently with the number of stamens. In the same manner, the terms used for the orders, from one to thirteen inclusive, were meant to denote the number of pistils or female organs in hermaphrodite flowers. The class *Triandria* has three orders—*Monogynia*, from *μόνος*, one; *γυνή*, a woman; the symbolic term for the pistil, or female organ: *Digynia*, and *Trigynia*, according to the number of pistils in the flower. In short, these stand exactly in the same position as the terms applied to the classes in all points, only, have reference to the number of pistils as they to that of stamens. It is unnecessary to specify the titles given to the succeeding classes, or orders, which differ from those quoted only in composition, and not in the point under consideration, as the arguments to be stated in respect to the latter, if held good, will equally affect the whole. One and all of them, I contend, from their meaning, and application, and from analogy, are to be regarded, not as nouns at all, but as adjective terms; it being impossible to attach to them a distinct signification

otherwise. I will now, with all deference, try to justify this opinion on the grounds stated.

1. By the meaning of these terms. Let us take, for example, that applied to the letters last quoted, *Triandria*, from *τρῖς*, three; *ανήρ*, a man, or a husband—that is, a stamen, or male organ. It is difficult to conceive how such a compound term could, with due regard to correctness, have been considered a noun f.; for it is not the name of a thing, but a term plainly expressing the quality of some word that is the name of a thing—of a noun. We might, indeed, by some straining, appear to give a substantive expression to it in this way—a triple stamen, or tre-stamen, as we say tre-foil. But such a word would indicate a single, particular object—like the tre-foil, which, although it presents the semblance of three leaves, and hence its name, they proceed from *one* stem—and not the class of objects intended. For it is to be marked, that *Triandria* (or any other of the Linnæan titles referred to) does not bear special reference to three stamens united in some way or other so as to form a triplet, or threefold object; but is applied to a set of plants, the flowers of which are provided with three stamens, not connected with each other by a single part of the flower, but distinct from the pistil and from each other. The strictly correct and only legitimate translation of this term, therefore, must be adjective—viz., having, consisting of, or provided with, three stamens, or more tersely, perhaps, *three-stamened*; and so of all the rest.

2. By their application. All the titles of the classes and orders are applied to *genera*, or kinds of plants, which agree in the particular arrangement of stamens and pistils, signified by such titles respectively. Thus, as I conceive, were they originally employed, for thus, alone, can they be regarded as correct and proper terms. *Triandria*, as the nom: pl: n. of an adjective of the second and third declensions, “agrees, in gender, number, and case,” with *genera*, the nom: pl: of *genus*, n.; a kind, or species; and they, together, aptly express the *three-stamened kinds* of plants; while *Monogynia*, *Digynia*, or *Trigynia*, also agreeing with *genera*, as fitly notify the same kinds, as *having*, or *provided with one pistil*, or more, as the case may be; in other phrase, as one, two, or *three-pistilled*. Added to this, it is common for botanists, in their writings, lectures, or in conversation, to make use of English adjective terms which are synonymes directly formed from the Latin ones, as *Monandrious*, or *Monandrous*, (the presence or absence of the letter *i* being immaterial as to most of them,) *Dian-drous*, or *Triandrous* plants; also *Monogynious*, *Digynious*, *Trigynious*, &c. To render these into Latin again, as would be very necessary if the character of nouns feminine of the first declension, hitherto given to the titles of the classes and orders, be maintained, we should have to create new adjectives in addition to, but quite the same in one gender, at least, as these reputed nouns. This were a clumsy expedient, at the best, and one altogether superfluous, the terms in their proper and adjective quality already answering that, as well as their original purpose.

3. By analogy. The terms applied to the classes, divisions sections, orders, and families, in natural history, are compound adjective, agreeing with *Aves*, *Pisces*, *Genera*, *Species*, *Tribus*, or the like, with exception of but a few examples of simple names of animals, as *Accipitres*, *Sturiones*, *Polypi*, &c. Those appropriated to the divisions, sections, and orders of the Jussucuan, or natural system of botany, are compound adjective, agreeing similarly with *genera*, *species*, or *tribus*, excepting some simple names of plants, as *Fungi*, *Algæ*, *Filices*, *Lichenes*, &c. A Linnæan term itself is occasionally set aside by other authors, who substitute in its place a compound term indisputably adjective; as, for instance, the *Cryptogamia* of Linnæus are, by Richard, entitled *Inembryonata*, *Exembryonata*, or *Ahrizæ*; by Decandolle, *Cellulares*; and by Lamarck, *Agamæ*. In a modern work on botany which I have just looked into, this term *Cryptogamia* is translated, in perfect consistency with the opinion here advanced, “Cryptogamic, or flowerless plants,” a translation which can never be twisted out of a miscalled noun.

So far as opportunities of reference are available in a provincial town like this, they have not enabled me to find any authority for making nouns feminine out of these adjective terms, other than in the several editions of Hooper’s useful “Medical Dictionary,” to which I have before had occasion to allude. In other works of a like description, they merely appear as they did when first published by Linnæus himself. I must again, however, make exception of those of French authorship, in which, not only a Gallic noun, “*s: f:*” (*Cryptogamie*) is in each instance created as a synonyme of the name of a class, but also another term (*Cryptogame*) is had recourse to, and presented as “*adj: et s: f:*” This customary way of