with a hot and dry skin, followed by profuse perspiration. These symptoms were now accompanied by obtuse pain in the right hypochondrium, and stretching upwards to behind the right shoulder. There was great tenderness and tumefaction in the hepatic region; pulse was 120, strong and bounding; appetite impaired; thirst urgent; tongue covered with a dark-brown fur; bowels constipated; urine of a dark wine colour, and depositing, on cooling, a bile-coloured sediment. He was bled to thirty oz.; twenty leeches were applied to the hepatic region, and he was ordered a purgative powder of calomel and jalap.

9th.—Had a restless night from severity of pain; febrile symptoms still urgent; pulse 110, wiry. The leeches bled freely; blood obtained from the arm buffed. The powder operated well; dejections bilious and very fetid. Swelling in right hypochondrium much increased, and occupying the entire extent of the liver. A blister to be applied to the hepatic region, and repeat purgative powder.

11th, Slept badly last night, having had several rigors. To-day the pulse is 100, and wiry; the tongue is cleaner and the thirst less urgent. The pain in the hepatic region is less severe; the swelling is more diffused, and presents obscure fluctuation. The blister rose well, and is discharging freely; the powder procured four large fetid bilious stools. He was bled again to twenty oz.; a poultice was directed to be constantly applied to the right hypochondrium, and he was ordered a morphia draught at bed-time.

14th, Tumefaction much increased, with evident fluctuation. An incision was made with an abscess lancet, and four saucerfuls of well-formed pus tinged with bile were evacuated. A seton cord was inserted into the wound, and the poultice was ordered to be continued; morphia draught to be repeated.

15th, A large quantity of matter was discharged during the night, and on withdrawing the seton to-day as much pus mixed with bile was evacuated as at the time of incision. Passed a bad night; pulse eighty, soft; thirst much abated. Quinine with a little wine was prescribed; and to have beef tea for dinner.

From this date the swelling and discharge diminished gradually, and the general symptoms improved. On the 17th a light bandage was applied round the abdomen. A colocynth pill procured two dark fetid stools intermixed with pus. In three weeks the discharge had completely ceased, and my patient, who was able to walk about, was rapidly regaining his wonted health. At the present time, April 1848, he continues well.

---

Part Second.

---

REVIEWS.

Principles of Medicine, comprising General Pathology and Therapeutics, and a Brief General View of Etiology, Nosology, Semiology, Diagnosis, Prognosis, and Hygienics. By Charles J. B. Williams, M.D., F.R.S., &c. Second Edition, considerably enlarged. London: 1848. 8vo. Pp. 533.

The actual state of pathology must be regarded as one of transition. Almost every school of medicine contains one or more enthusiastic labourers who are cultivating morbid anatomy on the basis of pathological histology. Almost every laboratory possesses men who are working out the problems connected with the chemistry of organic
tissues in a state of health and disease. At the present moment both pathological histology and organic chemistry are advancing with astonishing rapidity, and our own pages offer a convincing proof that scarcely a month passes without new facts being discovered and new ideas evolved of the utmost scientific importance. So quickly indeed does one new fact succeed another, that the time necessary for carrying a large work through the press is almost sufficient to vitiate its doctrines. Hence, all systematic books on pathology are necessarily premature. They are like infants born before their time; though newly formed they bear the characteristic features of old age, and rapidly sink for want of vital strength.

Five years have elapsed since the appearance of the first edition of Dr Williams' work on the principles of medicine. A copy of the second edition, now before us, is nearly twice the bulk of its predecessor. This is owing to the accumulation and addition of new facts and observations. We nowhere find, however, any important change in doctrine. On the contrary, the author tells us in his preface, "that in very few instances has it been necessary to retract or supersede the inferences and views set forth in the first edition." Knowing, as we do, how difficult a task it is to eradicate inferences and views once adopted, we are not surprised at this, although we are prepared to maintain, that very few of those regarded with so much favour by Dr Williams are capable of standing the test of time, if, indeed, they may not already be regarded as obsolete. It is, of course, impossible for us to criticise one tenth part of the doubtful and undecided questions treated of in this book; we shall, therefore, limit ourselves to the consideration of the author's opinions on subjects stated in the preface as having "been confirmed and extended by recent researches to a degree that has not less surprised than convinced him of their truth." These subjects are Congestion, Dropsy, Determination of Blood, Inflammation, and Deposits.

Congestion.

The older pathologists considered congestion to be connected either with an asthenic or sthenic state of the system, and to depend upon determination of blood on the one hand, or atony of the vessels on the other. These views are adopted by our author, who further speaks of these two kinds of congestion as being attended, the first with diminished, and the second with increased motion.

The chief causes of congestion with diminished motion are, according to Dr Williams, first, those of venous obstruction; second, those of atony of the vessels. Instances of the first from ligature or other obstructing causes are so common and self-evident, that we need not dwell upon them. Congestion from atony of the vessels may depend on numerous circumstances. It may be general, as in cases of debility, adynamic fevers, and the sinking which precedes death; or it may be local, from long continuance of a part in one position, from cold, the influence of malaria, and over excitement. Under the last head, we, for the first time, meet (p. 183) with an
allusion to a theory to which the author attaches great importance, not only in the production of congestion, but of inflammation also. We allude to the formation of colourless corpuscles in the vessels, which, combined with diminished tone of the vascular walls, causes them to be obstructed. This theory we object to in toto; but as we shall allude to it again under the head of inflammation, we need not enter upon its consideration at present. We need only cite a passage from Mr Erichsen's paper on Asphyxia, quoted by the author at length, who says, "Nor have I ever been able to discover any obstructions in the vessels in consequence of the adhesion of colourless globules to the sides,—a phenomenon that I especially watched for, and which has by several been supposed to occur."

Vessels which have lost their tone, according to the author, become inelastic and tortuous, and the very accumulation of blood in them opposes an increasing obstacle to its passage through them. This point he illustrates by some experiments in which water was forced from a syringe through brass tubes and coils of intestine, the latter being twice as long and twice the diameter of the former. The result was, that the small metal tube discharged from two to five times the quantity discharged by the larger but membranous tube. These experiments illustrate a principle that is too little considered in animal and general physics; the loss or neutralization of force by misdirection. In health the blood-vessels are so arranged as to make the most of the heart's propulsive power, and transfer it throughout their whole length; but when dilated, tortuous, flaccid, and otherwise altered, they misdirect and exhaust it; a sufficiency does not remain for the onward propulsion of the blood, which, therefore, stagnates and accumulates in the congested vessels.

DROPSY.

Congestion leads to an increased transudation from the distended capillaries, causing effusions of the watery and saline part of the blood, more or less impregnated with albumen, and sometimes even with fibrin, as exemplified in the fluids of fluxes and dropsies. Thus, congestion of the bronchi causes bronchorrhœæ; congestion of the intestines causes diarrhoeæ; congestion of the uterus, leucorrhœæ; congestion of the kidneys, watery and sometimes albuminous urine; congestion of the lungs and pleura, hydrothorax; of the heart, hydropericardium; of the abdomen, ascites, &c. Dr Williams considers this result to be purely physical, and the different characters of the effusion to depend not only on the amount of distension of the vessels, but on the condition of the blood. A watery state of this fluid promotes transudation, whilst a highly albuminous and fibrinous blood requires more pressure to make its watery parts pass through the coats of the congested vessels. In the latter case the fluid often contains self-coagulating fibrin, the "fibrinous dropsies" of Vogel. To this kind of dropsy the author refers the gelatinous masses of lymph often found in the peritoneal sac of the abdomen, the fluids of bronchorrhœæ, mucous diarrhoeæ, and leucorrhœæ, the
pseudo-membranous fibres occasionally effused on mucous surfaces, and albuminous urine. He considers also that hypertrophy may result from this form of congestion.

We are of opinion that Dr Williams has here confounded together two kinds of congestion, and that the phenomena he has latterly described, the fibrinous dropsies of Vogel, are erroneously ascribed by him to congestion arising either from obstruction or atony of the vessels. We will undertake to say, that there is no undoubted instance of a mere mechanical obstruction, either by ligature or otherwise on a vein, that has ever caused a true exudation. The author, indeed, refers to the experiments of Dr Robinson of Newcastle; but when we consider the injury to which the kidneys, in his experiments, were exposed, we can have no difficulty in attributing the effects to an inflammation rather than to a mere congestion. In short, we believe that two distinct lesions have been confounded together by Dr Williams,—that the pressure theory with respect to veins will not hold; and that, if it be applied to the capillaries, it ought to be considered as a result of inflammation, and treated of under the author’s next section, namely, congestion with motion increased, or what, following the older pathologists, he denominates

DETERMINATION OF BLOOD.

In discussing this subject, Dr Williams first refers to examples of local active hyperæmia in health, such as the face and neck in blushing, the uterus and breasts at the period of gestation and lactation, the gums during the period of dentition, and the antlers of the stag at the season of their development. In disease, he alludes to cases of what is commonly called “determination of blood” to the head, which, it is stated, have been seen to come on first with heating of the carotids, then flushing of the face and head, suffusion of the eyes, and sensations of distraction in the head.

With all deference to the author, we cannot help suspecting some fallacy in this observation. To suppose that congestion of the brain, and, as it is said, fits of epilepsy and hysteria, are preceded by throbbing of the arteries; or, in other words, that this throbbing of the carotids is the first in the sequence of changes leading to the congestion, is opposed to all known facts on the subject. The carotids have no such power of active contraction, independent of the heart on the one hand, or the capillaries on the other. No doubt, the arteries leading to inflamed or actively congested parts, do throb in an unnatural manner; but we believe that this is a result, and not a cause. In no place is this throbbing so well observed or felt as in the finger from a festered wound, yet this is evidently a propagation backwards from the injured part. It is the result of stimuli originally applied to the extreme vessels.

The author next alludes to the action of local stimuli to the vessels, such as snuff to the nose and eyes, spices to the mouth, and food to the stomach, which, he says, produce local determinations of blood. Here we cannot recognise any “determination.” We ob-
serve only an impression on the contractility of the vessels, the effect of which is to cause enlargement of them, and as a matter of course congestion or hyperæmia. Nothing more. But what does the author mean by this term determination? He tells us, that, on irritating a frog’s web, we see the arteries become enlarged, supplying a larger and more impulsive flow of blood to the capillaries and veins, which all become enlarged also; and the whole vascular plexus, including vessels which before scarcely admitted red particles, then become the channels of a much increased current. “This is determination of blood.”

But here again we must differ from our author on a matter of fact. We have performed the experiment frequently, but could never observe that the arteries were the vessels first affected. On the contrary, it always appeared to us, that the influence of the stimulus spread over a portion of the surface at once, that it was impossible to distinguish any difference between the effect on the capillaries and the effect on the minute arteries and veins, but that in all cases the smaller vessels (the capillaries and minute arteries) were influenced first, and the larger branches last. We hold, therefore, that the idea of “determination of blood” existing in the body is altogether erroneous, and that in no case do the arterial trunks take upon themselves the initiative in pouring blood into the minute vessels.

Dr Williams next enters into an argument to prove, that his determination of blood depends upon physical causes, upon purely hydraulic laws; but differing from him, as we do, with respect to the facts on which he rests his theory, it is unnecessary for us to attempt its refutation. The principal error, we think, lies in attributing too much to physical, and too little to vital causes, and in almost entirely overlooking that wonderfully great contractile and vital power of the capillaries, first demonstrated physiologically by John Hunter, and shown to depend upon a peculiar structure by Henle.

**INFLAMMATION.**

Dr Williams defines inflammation to be “too much blood in a part, with motion (of that blood) partly increased, partly diminished.” The increased motion is, according to him, dependent on “determination,” a subject we need only again allude to as a proof of the inconsistency of the whole theory. The “determination,” that is, the throbbing of the arteries, can never exist in an inflamed part. It is in the neighbourhood only, where the vessel is still pervious, that the phenomenon is manifested, and we have endeavoured to show that this is the effect and not the cause. All the facts with which we are acquainted prove that the condition of the vessels actually causing inflammation is one of obstruction, with complete stoppage of blood, and it is the cause of this stoppage which constitutes the only difficult subject of inquiry connected with the subject.

Here, again, we find that the author’s opinions regarding a tonic congestion and “determination,” are made the basis of a complex view
of inflammation, in which, of course, the contractility of the capillaries is denied, and the influence of hydraulic laws altogether depended on. Even these powerful aids, however, do not enable him to explain the stagnation of the blood in inflammation, and he has consequently recourse to another theory, said, like those which have preceded, to be founded on well observed facts, but like them, as we shall endeavour to show, unable to bear the test of close examination. This theory provides for another mechanical cause of obstruction, originating in the blood, by the formation of an increased number of colourless corpuscles, which stick to the sides of the vessels, and block them up. This would certainly be a very satisfactory cause did it really exist; but in this, once more, the author’s facts have been wrongly interpreted, and made subservient to an hypothesis which has no real foundation.

Is it then a fact, as Dr Williams and Mr Addison assert, that a large number of colourless corpuscles are formed in the vessels of the frog’s web, on irritating it? Dr Hughes Bennett, in a paper published in this Journal for January 1847, says, No. We quote the passage in that paper which refers to this subject, so that our readers may judge for themselves:

"In the autumn of 1843," says Dr Bennett, "we carefully repeated all the experiments described by Mr Addison and Mr Williams, with Dr Redfern, now professor of anatomy in King’s College, Aberdeen. Since then we have repeated them four separate times, twice publicly in November before the class of pathology and practice of physic, and twice in June, in conjunction with the gentlemen attending the class on histology. We have just terminated a series of these experiments for the fifth time, and the results have been so decided and satisfactory, as to render it imperative upon us to publish them, and to demonstrate the errors into which the promulgators of this doctrine have fallen.

"It is true that in young frogs, and in many full grown ones fresh from the country, the colourless corpuscles are very numerous. It is equally true that in old frogs, and in many which have been long kept, they are few in number. It is in the web of such animals that, according to Dr Williams, the increase of the colourless corpuscles, as the results of irritation, are well observed. Now, we have applied salt, capsicum, and essential oils, and have never seen these corpuscles increased in number, relatively to the yellow corpuscles. When, by means of these irritants in young frogs, congestion is occasioned, all the blood globules within the vessel increase in number, and that of the colourless corpuscles is of course augmented. But the yellow corpuscles are also more numerous, and neither we nor any of the assistants (and more especially Dr Wilkinson, our assistant of this year), have ever seen that the former existed relatively in greater proportion.

"Again, in old frogs, in the vessels of which the colourless corpuscles are very few, we have frequently excited stoppage and exudation, without seeing any colourless corpuscles from the commencement of the process to the end. In other words, on the addition of acetic acid, or other irritants, all the phenomena we have described make their appearance, producing oscillation and stoppage of the current of blood, crowding together of the yellow corpuscles, without the appearance of one colourless one. It often results, however, from the action of re-agents, especially of acetic acid, that the nuclei of the epidermic cells covering the web are rendered very distinct. These nuclei are round colourless bodies, exactly resembling in size and appearance the colourless blood corpuscles. It consequently happens, that the vessels running between the two layers of epidermis present at different foci an appearance as if the
internal wall of the vessel was studded with colourless corpuscles. This, I am persuaded, is the phenomenon which has given rise to the idea of the calibre of the vessel being crowded with them. Under such circumstances, however, it may be seen, that the parenchyma of the web in the meshes of the capillary plexus is also studded over with similar nuclei, owing, of course, to the epidermis being spread uniformly over the whole web. Mr. Addison says, that colourless corpuscles escape from the vessel into the surrounding tissue, a circumstance which is physically impossible without rupture of the vascular wall, although the origin of the statement may probably be explained by the facts we have pointed out.

"If further proof is required, it may be found on the examination of the circulation in the tail of the tadpole. Here the vessels are so easily excited and paralysed by irritants, that before stoppage of the blood they frequently burst, causing extravasation of blood, and yet without any increase in the number of colourless corpuscles. Moreover, not only have none of the histologists of Germany, who have carefully watched the inflammatory process, ever observed an increase in the number of these bodies; but this supposed occurrence is directly negated by recent observations and experiments of Remak, who has shown that their increased number in the blood follows large abstractions of that fluid, and that the portions first drawn in inflammations contain very few. It may be concluded, then, that there is no increase of the white corpuscles in inflammation; no crowding together of them, so as to produce obstruction of the vessel; no escape consequently into the surrounding tissue; and that the observers who have stated these circumstances to have occurred, have mistaken the nuclei of the epidermic cells in the web of the frog's foot for these structures."—Monthly Journal, January, pp. 503-505.

The following is Dr. Williams' commentary on the above passage—

"The presence of the pale corpuscles in inflamed vessels has been questioned by Dr. Hughes Bennett, who hints that Mr. Addison and myself may have mistaken for them epithelium cells lining the vessels; but the plain description above given, and which I have repeatedly verified, admits of no such interpretation. The granular corpuscles may be distinctly seen to roll sluggishly, and with a dragtail before they stop; which they obviously do by adhesion to the interior of the vessel. I cannot understand why Dr. Bennett has failed to see so clear an appearance."—P. 266.

Now, a perusal of Dr. Bennett's statements will prove that he has seen all the appearances described by Dr. Williams. The facts themselves are correct as far as they go. What is denied is, that such appearances are the invariable forerunners of obstruction in inflamed vessels, and are the cause of the stoppage in the circulation. What Dr. Bennett asserts is, that he and his assistants have again and again watched the whole process of inflammatory obstructions in old frogs, without seeing any of those white corpuscles; and, on the other hand, they have seen all the phenomena described by Dr. Williams in young frogs, without inflammation or obstruction having occurred. The inference is, that Dr. Williams has mistaken an accidental phenomenon for an essential part of the process, and attributed a uniform effect to an occasional occurrence. On the other hand, the only explanation offered by Dr. Bennett of Dr. Williams' error, is the supposition that he had mistaken the nuclei of the epidermic cells running above and below the vessel (not the epithelium cells lining the vessels, as erroneously stated by Dr. W.),
which often, from the action of acetic acid, give to the vessels the appearance of being crowded with colourless corpuscles.

Our readers must judge for themselves as to the correctness of these opposing statements. Inexperienced persons, however, should not be misled by the diagram given by Dr Williams, p. 262, which he says "exhibits the appearance of a small portion of the capillaries." From the size of the vessels in relation to the blood globules they are probably small veins. For ourselves, we are by no means inclined to believe that the mechanical notions of Dr Williams, and the creation of a number of colourless corpuscles to stop up the vessels, can explain the nature of inflammation. But not to dwell upon the experiments of Remak, which appear to be unknown to the author, there is a series of cases on record, quite sufficient in itself to overthrow the theory. We allude to the four cases by Craigie, Hughes Bennett, Virchow, and Fuller, in which the blood was crowded with colourless corpuscles in every vessel, without any of the phenomena of inflammation being present, nor any of its usual appearances existing in any of the solid tissues. With these cases also, the author seems to be entirely unacquainted.

We hold, therefore, that this peculiar theory of Dr Williams' is not only not proven, but is extremely improbable; and think moreover, that the physical and mechanical causes to which the author ascribes the phenomena, are altogether inefficient for the purpose. Whether the views of the vitalists, and more especially the vital attraction and repulsion theory of Professor Alison, be better founded, we shall not stop to enquire. Certainly it is not free from objection: but on the whole we have more faith in it than in the mechanical notions of our author.

We might dwell at considerable length on the author's account of the results of inflammation, in which the same imperfections in appreciating facts, and the same unacquaintance with the labours of modern pathologists, are every where observable, as in the points we have just noticed. But, confining ourselves to those matters, which, according to Dr Williams, have been most perfected by recent researches, we have next to notice what is said on

DEPOSITS.

The subject of deposits in or upon textures, constitutes section 6 of chapter iv. on structural diseases, or diseases of nutrition. Here we cannot refrain from observing, that nothing can exhibit so clearly the faulty arrangement of a work on pathology, and the confused notions of its author, as separating the phenomena of inflammation from diseases of nutrition. Surely that process which perverts the growth of a part, leads to formations of pus, lymph, ulceration, and gangrene, must, more than any other, be regarded as

1 Edin. Med. and Surg. Journal, October 1845.
2 Frorieps, N. Notizen, No. 780.
3 Lancet, July 1846.
a lesion of nutrition. Yet Dr Williams makes inflammation and its phenomena constitute one section, and diseases of nutrition another. It is under this latter head the author describes hypertrophy, atrophy, induration, softening, transformation, and degeneration of textures, deposits, non-malignant growths, malignant growths, and disorders of mechanism. On each of these subjects we have much to find fault with; indeed none of them may be considered as communicating to us the actual state of science regarding them. What is said as to deposits will serve to illustrate this statement.

The term *deposits* is applied by the author "to matters which result from an overflow of the nutritive material beyond what is necessary to nourish the textures themselves." These matters he has divided into *euplastic, cacoplastic, and aplastic*, by which barbarous words he means substances that exhibit a perfect, imperfect, or absent organization. We are surprised to see that the euplastic deposits are formed of cicatrices, which in our opinion ought, according to the author's views, to be considered as cacoplastic. We find, however, that Dr Williams does not mean cicatrices properly so called, but regenerations of perfect tissue.

For an account of the healing of wounds the author is indebted to Carpenter's physiology, and it is to be regretted that he should have had recourse to a mere compilation, when so many masterly accounts of the process, founded on original observation, are to be obtained in the works of British and continental pathologists. But in this, as well as throughout the work, there is to be discovered a very deficient knowledge of the labours of his contemporaries, and especially of the investigators in Germany.

Under the head of cacoplastic and aplastic deposits, there is a description of true cicatrices, which we are told "exhibit various degrees of organization, some being vascular, and some not." Here, as in many other places in the work, the author confounds organization and vascularity with each other, an error that no one acquainted with the present state of physiological-pathology could have committed.

The structure of tubercle is described by the author as consisting of a "predominance of minute and often irregular granules, and the comparative absence of fibres and cells, of which mere traces are seen, at least in the older specimens. The granules are aggregated together by an amorphous material, the solidity of which gives hardness and some translucency to the mass; for acetic acid or alkalies, which dissolve this cement, loosen the granules and render them distinct. The chemical nature of granular tubercle is albuminous, with some gelatine and a little fat, the latter in very minute proportion, and occupying the centre of some granules; and the gelatine being probably the amorphous cement just noticed," p. 391. Now, these so called "granules" are, we presume, the tubercle corpuscles of modern pathologists. They cannot be granules properly so called; for the author tells us, p. 269, that a
granule appears "as a light spot, surrounded by a dark circle." Calling, then, these bodies granules, is not only contradictory, but, in the case of a structure having contents, is faulty in the extreme. Why has not the author given us the admirable description of these corpuscles by Lebert, instead of an account so vague and confused, that even few histologists can understand what is meant, not to speak of the generality of his readers? But here, again, no allusion is made to the accurate researches of Lebert, with whose labours the author seems to be wholly unacquainted. As to the amorphous matter which is associated with tubercle being gelatine, we know of no fact which can support such a statement. Most assuredly the author has not given us one. Neither has he noticed the numerous analyses of tubercle by Glover of Newcastle.

There are numerous speculations in this section with which we cannot agree, and which a more extended knowledge of the writings of other pathologists would have prevented the author from forming, especially the idea that tubercular matter may form within the vessels, p. 398; that tubercle is most common in the lungs, because they are the chief seat of the formation of fibrin, and so on.

Dr Williams tells us, that cod liver oil is assuredly the most efficacious of all medicinal agents in the treatment of cacoplastic and aplastic deposits, and is one which, after two years' constant experience in its use, is still frequently surprising him by the wonders that it occasionally works even in aggravated and advanced cases of scrofula, mesenteric disease, pulmonary consumption, chronic pneumonia and pleurisy, and chronic rheumatism. He conceives it to act as fatty matter. The nuclei or rudimental molecules of all structures appear to consist of fat, which the oil in its highly divisible state supplies and renews in the manner most conducive to active and healthy nutrition. We are surprised to find that this explanation of the action of the oil is put forth by Dr Williams as his own, and alluded to both in the preface and in the text (p. 404) as the result of the author's reflections on the subject. Though unacquainted with modern researches made so far off as France and Germany, he might surely have known that this very theory had been formerly published by Dr Hughes Bennett in 1841, and publicly taught by him at no further distance than Edinburgh, for the last seven years.

Our limits prevent us from proceeding further. We have confined ourselves to a notice of those portions of the work considered by the author himself as being most perfect; and we need not say that if such be the correctness of those inferences and views, which in the opinion of Dr Williams have been confirmed and extended by recent researches, the other portions we have left unnoticed are open to even more objections. And such is the fact. Now, as stated at the commencement of the article, this is not altogether owing to Dr Williams, but to the imperfect state of our knowledge. The error consists in speaking too positively on subjects which are
still doubtful, and in writing a systematic book on what cannot be systematized. We had reason to expect, however, a candid and fair statement of the opinions of others; but we regret to say, that either owing to inadvertence, or to a want of information, this has not been furnished. Had the author perused the works and memoirs of Remak, Gruby, Rokitansky, Engel, Henle, Lebert, Virchow, Bruch, and other members of the physiologico-pathological school of Germany, it would doubtless have merited a different notice from us.

Notwithstanding this work contains so much that is deficient in observation, research, and correct theoretical deduction, we have no hesitation in saying, that it also possesses merits of a very high order, which will not only render it acceptable to the medical practitioners of this country, but probably make it valuable to our continental brethren. In Germany, indeed, it is likely to be even more useful than in England; for whilst its errors and deficiencies will be palpable to the foreign pathologist, the chances are, that if these do not induce him to throw the work down too soon, he will obtain from it a degree of information that none of the books published in his own country will afford him. We allude to that portion of Dr Williams’ “Principles” which refers to treatment. In all that is said on this subject, we recognise the enlightened and practical physician. No doubt we owe to Germany much of what is known of pathology as a science. There, it is pushed forward with a rapidity far too great for a London practitioner. But it is destitute of that great aim which can alone make it valuable, namely, the cure of disease. In uniting theory with practice, and in thus following the great examples of the Hunters and the Bells, Dr Williams has done well. Such, we trust, will always be the chief characteristic of British works on medical science. Dr Williams has many claims to the gratitude of the cultivators of medicine; but the work we have just noticed, is sufficient to prove that these claims do not so much rest upon his knowledge of pathology, as on his powers of diagnosis and skill as a practical physician.

Traité de l’Art de Formuler, ou Notions de Pharmacologie appliquée à la Médecine. Par M. Mialhe, M.D., Pharmacien, Professeur agrégé à la Faculté de Médecine. Paris: 1845. 8vo. Pp. 518.

A Treatise on the Art of Prescribing. By M. Mialhe, M.D., &c. Paris: 1845.

The ordinary run of treatises on the subject announced in the title of this work are, for the most part, characterised by the absence of original matter; the book before us is distinguished by the opposite quality. In Mialhe’s work we are furnished with an extensive series of original experiments, having for their object, on the one hand, the
explanation of the action of medicinal substances, and, on the other, the improvement of the formulæ made use of in their administration. The "formules rationelles" of this writer are founded on a careful consideration of the chemical reactions that medicines undergo in the stomach and intestines, and are devised with the view of securing, as far as possible, the absorption of the whole dose of the remedy prescribed, in every instance where its entrance into the blood is essential to its therapeutic action.

Five chapters are occupied with general considerations on the absorption of medicines. As the fundamental proposition of his treatise, our author contends, that "every substance capable of exerting a remote action on the animal economy is soluble, or susceptible of becoming so in the fluids of the body"—pp. 17 and 22; and his researches have served to solve several apparent exceptions to this law. His experiments show that sulphur, phosphorus, metallic arsenic, the insoluble compounds of lead, antimony, mercury, silver, &c., the action of which is often cited to prove an opposite opinion, are all chemically influenced, and rendered wholly, or in part, soluble by the agency of the gastro-intestinal fluids; and to the change thus effected, we can have little hesitation in attributing their physiological action.

If, however, the recent observations of Oesterlen are to be relied upon, solubility does not appear so exclusively essential to absorption. This writer has observed that finely powdered charcoal, when administered by the mouth, finds its way into the blood, in which fluid the carbonaceous particles may be detected by the microscope. Admitting Oesterlen's observation to be correct, it is improbable that charcoal or any analogous substance could, in this way, be introduced into the blood in sufficient quantity to act upon the economy.

In relation to absorption, Mialhe divides the articles of the materia medica into two classes. The first class comprehends those substances which are soluble, and hence capable of immediate absorption;—it includes potassa, soda, ammonia; all the soluble vegetable acids; some mineral acids; arsenious and arsenic acids; all alkaline salts; and of the metallic salts, all those which do not coagulate albumen, as the cyanides of potassium, iron, mercury, &c.; some vegetable alkaloids, and indeed we may say all, as there are none entirely insoluble in water.

The second class comprises all insoluble drugs which require the intervention of the gastro-intestinal fluids to effect their absorption, and is divided into—A, substances which are rendered soluble by the acids of the gastric juice. This division includes all the metals except silver, gold, &c., which have no action on the economy when taken in the metallic state, all the insoluble metallic oxides, and the sparingly soluble vegetable alkaloids. B, substances which are rendered soluble by the alkalis of the intestinal canal. Sulphur, phosphorus, iodine, almost all the mineral acids, tannic acid, and all the
insoluble organic acids; the electro-negative metallic oxides of antimony, tin, zinc; resins, fixed oils, &c., belong to this division. C, substances requiring for their absorption the intervention of alkaline chlorides. This division includes all the oxides and salts of lead, mercury, silver, gold, and platinum. Their absorption may be effected throughout the entire length of the alimentary canal, or wherever alkaline chlorides are present in the secretions.

That a classification of this kind has considerable practical value is at once evident. In administering a drug belonging to division A, very little fluid should be given along with it, as a large quantity not only weakens the solvent power of the gastric juice, but also hastens the passage of the drug into the intestines, where it meets an alkaline reaction. Of course, alkaline drinks should be here avoided; while it is equally evident, that to obtain the maximum effect of a medicine included in division B, acid drinks should not be taken.

But as the quantity of acids, alkalis, and of salts contained in the fluids of the body, is limited, it follows that the action of insoluble bodies is also limited, and often bears no relation to the amount of medicine taken. We are thus enabled to understand why sixty grains of calomel should not act with twelve times the energy of five grains of the same medicine, or why so little difference should be observed in the remote effects of small and large doses of such drugs as the carbonate of the peroxide of iron, nitrate of bismuth, oxide of silver, &c.

In the use of soluble medicines which are not decomposed by the gastro-intestinal fluids, we can predict with considerable accuracy the effect which a given dose will produce, and can increase or diminish that effect by increasing or diminishing the dose. But in the case of insoluble drugs, many circumstances may interfere to prevent the chemical reaction necessary to render them active; and consequently we find that the same dose may in different individuals, and at different times, produce very unequal results. Calomel, oxide of antimony, and other insoluble medicines, afford constant illustrations of this remark. In many diseases, the secretions undergo important changes in their chemical composition, and in their power of acting on insoluble bodies, which present in consequence unusual inertness or activity, as the case may be. Many anomalies of this kind have been hitherto referred to idiosyncrasy. It is further clear, that, beyond a certain limit determined by the solvent power of the gastric and intestinal fluids, we cannot increase the remote action of an insoluble drug by augmenting the dose. When this limit is exceeded, a portion of the medicine remains undissolved, and is either expelled with the faeces; or what is very likely to happen, at least if the use of the drug be continued for some time, it accumulates in the stomach and intestines, and afterwards leads to unpleasant consequences. On this subject our author makes the following useful remarks:
“When,” he observes, “an insoluble medicinal compound is introduced into the system, and cannot be entirely dissolved by the gastric and intestinal fluids, the insoluble portion of this body, or the insoluble whole, either traverses the entire length of the alimentary tube, to be expelled with the faces, or is arrested in its course, and lodges for a time in some of the folds of the intestinal mucous membrane. Thus, the inconsiderate employment of caustic magnesia has given rise in the stomach of the gouty to extraordinary magnesian incrustation; insoluble preparations of iron, and especially the subcarbonate of the peroxide, administered in too large doses, often occasion intestinal concretions. The accumulations may even occasionally be the direct consequence of the exhibition of too great a quantity of the medicine. But the insoluble matters thus accumulated do not all present the same dangers; those on which the vital fluids exert no effect, act on the surfaces with which they are in contact only as foreign bodies—that is, by causing irritation and symptoms of inflammation; while matters capable of becoming soluble in consequence of a change in the quantity, or in the composition of the visceral humours, may thus become active, or often poisonous, and by their absorption give rise to severe or even fatal effects.

“It is thus that calomel, given as a purgative in large doses, occasionally causes ptialism, and deeply affects the system; so also basic or official sulphate of quinine, administered in the dose of several grammes daily, though at first producing no remarkable physiological effect, has all at once given rise to symptoms of poisoning, followed by death. A few glasses of tartaric lemonade have occasioned vomiting and diarrhoea in a patient who had taken protoxide of antimony some days before, it is scarcely necessary to add from the formation of tartarized antimony. Ioduretted water, given to a patient affected with a cutaneous disease, who had a short time before been taking calomel as an alternative, has given rise to most profuse salivation, occasioned by the biniodide of mercury, resulting from the action of the ioduretted water on protochloride of mercury still remaining in the system.”—P. 260, et seq.

An important practical conclusion to be derived from the foregoing is, that insoluble agents should not be exhibited in single large doses, but in frequently repeated small doses, so as to expose the medicine to the solvent power of a large quantity of the fluids of the body. In practice this precept is frequently followed; but its rationale is, we believe, not generally understood.

The proportion of water taken into the stomach along with an insoluble medicine, exerts considerable influence on its therapeutic action; the solvent power of the secretions being diminished by large dilution. Theoretically, this is exactly what we should expect; but the following experiments will enable the reader to see clearly the full import of the proposition that we have just laid down. Under the influence of alkaline chlorides in solution, calomel undergoes partial conversion into corrosive sublimate and metallic mercury, to which change, as will be afterwards seen in the course of our remarks, we have good reason to attribute the physiological action of this important remedy. In four experiments calomel, six decigr. common salt and muriate of ammonia, of each six decigr, and a different proportion of distilled water, were placed in contact during twenty-four hours, at a temperature of from 40° to 50° centig. The quantity of distilled water used, and the amount of corrosive sublimate formed, in each experiment, were as follows:
Distilled water 5 grammes.  |  Corros. subl. produced 24 milligs.
"   " 10 grammes.  |  "   "   " 19 milligs.
"   " 20 grammes.  |  "   "   " 12 milligs.
"   " 40 grammes.  |  "   "   "  9 milligs.

Clearly showing, that the quantity of corrosive sublimate produced is greatest when the alkaline solution is most concentrated.

Of the sixth chapter, two sections are devoted to the consideration of the changes which medicines are supposed to suffer in the blood, and the mode in which they act on the economy. They contain much ingenious speculation, and merit careful perusal.

The greater portion of the work contains a detailed examination of the individual members of the materia medica. We have space to notice only a few of the more important.

**Sulphur.**—In this drug we have an excellent illustration of an insoluble body capable of exerting a remote or constitutional action on the economy, and the question arises, How is its absorption effected? Mialhe's experiments show that, when exposed to the action of alkaline carbonates in solution, sulphur is transformed in part into the sulphuret and hypo-sulphite of the alkali, compounds which are soluble, and hence capable of absorption. It is therefore probable, that sulphur enters the circulation only in virtue of the changes which it undergoes by contact with the alkalis of the intestinal fluids; in favour of which view, it may be urged that the most active preparations of sulphur are the alkaline sulphurets, or those which contain them. Its action is much promoted by the addition of an equal part of carbonate of magnesia, which probably serves to saturate the free acids of the stomach, and so enable the alkalis and their carbonates to operate more effectually.

Mialhe advances, as another argument in favour of this view, the fact, that the energy of sulphur ointment as an anti-psoric remedy is much enhanced by the addition of the carbonate of potassa; but the latter remedy alone forms perhaps the most active application in scabies that we possess.

**Iron.**—In France it is a common opinion, that in the treatment of chlorosis and other affections demanding the tonic powers of iron, those ferruginous preparations only are efficacious which contain the protoxide, or the metal itself, which in the stomach is converted into protoxide; further, that the protoxide must be united with carbonic acid or some organic acid that can be assimilated, as the citric or lactic; and lastly, that all the salts of the peroxide of iron, and all ferruginous combinations with the strong mineral acids—as the sulphuric, phosphoric, or muriatic—are not assimilated, and are only useful as astringents (Bouchardat, Gélis, and Conté, *Annuaire de Thérapeutique*). Mialhe maintains, and very justly, that clinical observation is altogether opposed to these conclusions, as is shown in the recognised value of such preparations as the subcarbonate and citrate of the peroxide, the hydrated peroxide, and the permuriate.
of iron. On his part, he advances the following propositions:—

1. "That all ferruginous preparations, soluble or capable of solution in the acids of the gastric juice, and susceptible of decomposition by the alkalis or their carbonates contained in the blood, may be advantageously employed in the treatment of chlorosis." This includes all the preparations of iron in common use; as the pure metal, its combinations with oxygen, the chloride, bromide, iodide, and, without exception, all the oxisalts of iron. All these suffer decomposition in the blood, the alkalis of which unite with the acid, and the base is set at liberty. This is the property that, according to Mialhe, constitutes the essential condition of the tonic (or, to speak more precisely, "reconstituant"1) powers of a ferruginous preparation. As the theory indicates, salts of this class are slow to appear in the urine, and if given in small quantity, cannot, with one or two exceptions, be detected in that fluid.

2. "That none of the martial compounds, soluble or capable of solution in the acids of the stomach, but not susceptible of decomposition by the alkalis of the blood, can be of any service in the treatment of chlorosis." This class comprises only a few compounds; as the ferro-cyanide of potassium, the sulpho-cyanide of potassium and iron, &c. These traverse the circulation unchanged, and soon appear in the urine.

We know, that in chlorosis the constituent parts of the blood which are deficient are the iron and the blood globules, and that these are promptly restored by a course of chalybeates. Whether, in producing this effect, the iron acts simply as a general tonic, or whether it is assimilated, and enters directly into the formation of the blood globule, the present state of our knowledge does not enable us certainly to determine. The latter hypothesis is, we think, very probable; but Mialhe, without giving any satisfactory reason, assumes it as an established fact. The arguments in favour of it are these, (1.) That the other general tonics have not the same effect as iron in chlorosis; (2.) That those chalybeates which, from their nature, are detained in the blood, are the most efficacious.

Mialhe goes on to consider the condition in which iron enters into the formation of the globule. Liebig had already shown that the iron contained in the blood was combined with oxygen, and Mialhe observes, that the probabilities are all in favour of the peroxide being the active agent; it is unalterable in the air, and its salts are in general stable, and present a red colour more or less resembling blood. If iron were assimilated only in the first degree of oxidation, it is very unlikely that the persalts would form such energetic preparations. He believes that the protoxide only becomes active by passing to the higher degree of oxidation. His ingenuity carries him a little further, and offers us the following hypothesis

---

1 Trousseau and Mialhe mean to indicate by the use of this term, that the iron is retained in the blood and assimilated.
respecting the formation of the blood globule. The ferruginous salt and the albuminate of soda contained in the blood suffer mutual decomposition; there are formed a new salt of soda and the albuminate of iron, which, according to our author, constitutes the base of the blood globule. An experiment, advanced in support of this opinion, merits attention. When to a solution of albumen we add a perfectly neutral persalt of iron, no precipitate forms; but on adding to this mixture a certain quantity of chloride of sodium, a copious precipitate falls. Now, we know, that while the globules of the blood are soluble in distilled water, they are not so in a solution of common salt.

When an insoluble preparation of iron is used, it should be administered in frequent small doses, and its use continued for a considerable period, as it acquires activity by solution in the acids of the gastric juice, which exist only in small quantity. Among the insoluble preparations, he gives the first place to the metal in fine division and the carbonate of the protoxide, then come Ethiop’s mineral and the hydrated peroxide. The insoluble protosalts are preferable to the insoluble persalts, on account of the greater facility with which they are acted on by the acids of the gastric juice.

The soluble preparations of iron are incomparably more active than the insoluble. Among these there are some, as the sulphate and muriate, the absorption of which is incomplete on account of their active astringent power. “Of the soluble compounds, those which have the least styptic taste are richest in iron, and are most readily absorbed, ought to be preferred; and, on these grounds, no ferruginous compound can be compared with the tartrate of potassa and the peroxide of iron.”

Remarks on the Employment of Anaesthetic Agents in Midwifery.

By G. T. Gream, one of the Medical Officers of the Queen Charlotte’s Lying-in Hospital; Fellow of the Royal Medical and Chirurgical Society, &c. &c. London: 1848.

In December last, immediately after the anaesthetic effects of chloroform in midwifery were announced, a notice against it was published in the Medical Times, averring stoutly that anaesthesia could not be of so much advantage, as had been reported, in first deliveries, because in first deliveries the principal impediment “is,” said the writer, “found in the pelvic bones and rigidity of joints.” Of course, every tyro in obstetric knowledge was capable of detecting the intense absurdity of the allegation, that the bones and joints of the pelvis were the cause of delay in first labours. The author of this amusing averment was Mr Gream, a London “Late Lecturer on
Midwifery and the Diseases of Women and Children." He has stated his objections against anaesthesia in midwifery at a greater length in the present publication; and we have no doubt that, a century hence, Mr Gream's pamphlet will be prized and quoted as a literary medical curiosity in its way, and another proof of the absurd and lamentable lengths to which medical prejudices will sometimes lead medical men, whenever any thing new is proposed in practice.

We believe it is needless to encumber our pages with quotations from a work exhibiting such extraordinary unacquaintance with physiological and pathological science as this pamphlet betrays; but lest any of our readers, perchance, deem our verdict too harsh, we will give an extract at random. At p. 26, on the "latent effects" of ether and chloroform, Mr Gream illustrates these effects by the case of the young druggist at Aberdeen, Arthur Walker, who poisoned himself in February last with chloroform. The italics in the following extracts are, we beg to say, Mr Gream's own:—

"Dr Jamieson, who examined the body of Arthur Walker, in his report says: 'Lungs turgid, loaded with dark blood, and extensively adherent to the walls' of the chest, and the 'heart extensively adherent to the pericardium.'

'The walls of the right ventricle thin, cavity enlarged.' That we find these appearances in individuals not unfrequently, it will be allowed; but the combination of them in a person habitually a drunkard, through the effects of etherization, at once gives rise to the suggestion that they have arisen from this cause."—(P. 26.)

These adhesions, Mr Gream proceeds to show, were probably "the effect primarily of venous congestion," produced by the chloroform inhalation; and he thinks "it is impossible to be certain," that those who have inhaled ether or chloroform "have not at this moment similar morbid adhesions."

We feel almost ashamed to offer a word of commentary upon such statements and reasoning as this. But surely no other man in the educated medical profession of the present day, except Mr Gream himself, could possibly believe that old and intimate adhesions between the opposed surfaces of the pleura or pericardium, such as were found in Walker's case at Aberdeen—adhesions evidently of very long standing—could have been produced by chloroform, the anaesthetic effects of which were only first discovered in Edinburgh a few weeks before Walker died.

1 Mr Gream gives this and the other extracts from Dr Jamieson's report, both within inverted commas and in italics, thus doubly marking his wish that they should be regarded as actual and true quotations. Dr Jamieson's published report of the case (Medical Gazette for February 25) was the official one given in to the law authorities. Mr Gream has completely changed its meaning. The actual words of Dr Jamieson's report, regarding the state of the heart, are these, "The pericardium was generally and closely united to the heart by old adhesions, which had to be forcibly torn asunder with the finger." &c. Mr Gream has carefully omitted the word "old," as that word would have totally destroyed all the arguments he wished his "fair" readers to draw from the case. We need not say that this is a most unwarrantable and unjustifiable proceeding, and, we fear, must seriously compromise Mr Gream's character as a medical author.
If Mr Gream places any faith in his own reasoning (though, for the sake of Mr Gream's medical knowledge, we charitably hope he does not), he must further believe that Walker, for some few weeks before his death, was going about his usual occupations with acute or subacute Pleurisy and Pericarditis marching on in full career within him, and without producing a single symptom. Certainly Mr Gream must have uncommonly innocent and strange ideas of pathology and disease.—Patients operated on under anaesthesia die sometimes, like patients operated on without anaesthesia, though by no means so frequently. If Mr Gream will inquire at any of his surgical friends—or any one who knows pathology at the London hospitals—he will find that these surgical patients do not exhibit appearances of pleurisy and pericarditis more frequently, after ether or chloroform, than those who die without having inhaled ether or chloroform.

Mr Gream's other illustrations of the "real hazards" of ether and chloroform are all precisely of the same kind as the above, and all betray a similar want of temper and of knowledge. According to his catalogue of their "real hazards," almost every corporeal disease is the result of their inhalations, and a few mental also; for he humanely suggests to the "heartless mother," who seeks by anaesthetic means to avoid her own sufferings, whether this practice may not make an 'idiot' of her child; and that, no doubt, on grounds as good and just as Dr Rowley—another fashionable practitioner in London, and "Lecturer on the Practice of Physic"—gravely suggested half a century ago to the "heartless parents" of his day (when vaccination was first introduced), that an artificial cow-pox might certainly, in the human subject, ultimately lead to the growth of cow's hair upon the human skin, and cow's horns upon the human head. Nay, a coloured drawing of a case, in which he averred this last transformation to be taking place, was published by him as a frontispiece to a pamphlet against the horrors of vaccination, written in the exact spirit of Mr Gream's publication. We would suggest Mr Gream's own portrait as a fit frontispiece for his next edition. And perhaps some of his readers will recollect Byron's commentary on the poet, who, like Mr Gream, selected and sung for his hero of "An Idiot Boy":—

"A moonstruck, silly lad, who lost his way,
And, like his bard, so mingled night with day,
That all who viewed the Idiot in his glory,
Conceived the bard the Hero of the story."

There is one subject prominently brought forward in Mr Gream's pamphlet, intended, as its pages are, to "fall into the hands of persons not belonging to the medical profession," on which we would make one parting remark. He avers that the use of chloroform is calculated to excite improper sexual feelings and expressions in women, when given during labour. Some time ago, in consequence of seeing this gross mis-statement loathsomely repeated over and over again, by the same hand, in a London journal, we made extensive inquiries among
our obstetric friends in Edinburgh, who were constantly, and with unvarying success, using chloroform in midwifery, as to whether they had, in any case, observed any thing that could give rise to such an idea. Not one of them had ever remarked any thing of the kind, or any thing approaching to it. But to those who are themselves impure in thought, every thing, in others, looks and sounds impure. And, on the present point, we quite agree in the opinion which we heard expressed by Dr Simpson at the Medico-Chirurgical Society a few nights ago. Persons, he stated, sometimes, when imperfectly under chloroform, carry on for a time the same train of thoughts, and even of conversation, as they were engaged with when the inhalation was begun; and if some obstetricians find their patients talking obscenely when chloroformed, the fault does not depend upon the chloroform, but upon the coarse and improper previous conversation of themselves or their assistants.

After what we have stated it is almost unnecessary to add, that Mr Gream makes very unscrupulous and distorted misquotations throughout his pamphlet, and revels very largely in a number of very frightful but very apocryphal cases and facts, which if inquired into, would, we doubt not, turn out as groundless as his own attempts at reasoning. Indeed, in glancing over his pages, we have sometimes felt at a loss whether to imagine that Mr Gream really believed in what he stated, and was so unacquainted with pathology and medical reasoning as his observations seemed to show; or whether the logical ignorance of the author was not real, but merely affected and assumed for the purpose of enabling him to make broader misstatements. At all events, this is evident from the pamphlet, that if it is to be taken as a criterion of the arguments that can be used against ether and chloroform, the success of anaesthesia in midwifery is as certain as the success of vaccination in medicine,—seeing that the lovers of pain must be desperate, and that Mr Gream, with all his anxiety to retard and decry the new practice, has not been able to adduce against it a single case or argument that any educated medical man would place the slightest degree of value upon.

The pamphlet, in fact, is only fit (to use his own expressions) for "the hands of persons not belonging to the medical profession;" and we owe an apology to our readers for noticing a production like the present at such length. We need only add, that an author who, like Mr Gream, is so defective in other forms of knowledge, is sadly deficient also in the knowledge of his own and of other languages. He writes, for example, more than once, "ebriety," for "inebriety,"—conjoins plural nouns with singular verbs,—spells forceps "forceps"—sets at defiance all rules of punctuation; and translates the French expression "salive écumeuse," by "squamous saliva," instead of "spumous;" "rushing (raptus) of blood to the head," he translates "ruptures of blood," &c. &c. He is, in short, himself apparently much in the state in which he pathetically describes
a patient under ether. "He has undoubtedly lost his reasoning power, for his conduct is most outrageous. He closes his eyes" (to the truth?) "and foams at the mouth."—(P. 12.)

---

**Part Third.**

**MEDICAL NEWS.**

**MEDICO-CHIRURGICAL SOCIETY OF EDINBURGH.**

**SESSION XXVII.**

**MEETING XI.—MAY 17TH, 1848.—DR HAMILTON, PRESIDENT, IN THE CHAIR.**

**ON THE PHYSIOLOGICAL ACTION OF CHLOROFORM. BY DR GUNNING.**

This paper consisted of illustrations, by experiment and observation, of the theory of the author, that in the mode of action on the nervous and vascular systems, the effects of chloroform were similar to those of a modified asphyxia. The conclusions of the author were as follows:—

1. That there is, after inhaling chloroform and similar substances, an impediment to the transit of the blood through the pulmonary capillaries, which impediment reacts, through the right side of the heart, upon the nervous centres; in which the pressure of the venous, as well as the want of arterial blood, induces the changes which produce the striking effects upon voluntary motion and sensation.

2. That, in all essential particulars, the full effects of chloroform are the same as those of confessedly asphyxiating causes, such as carbonic acid, hanging, drowning, &c.

3. That the same treatment is applicable to those suffering from any of these causes, namely, to renew the relation between the blood in the lungs and the atmosphere by artificial respiration, and to take off the pressure from the right side of the heart by blood-letting.

4. That, therefore, the distinction of substances into "Stimulants" and "Sedatives" is, in many cases, not scientific, seeing they produce similar symptoms and similar material effects.

Dr Simpson said, that he sincerely believed Dr Gunning's theory of anaesthesia to be entirely incorrect; but it was difficult to state, on the spur of the moment, the reasons which might be adduced against so new and strange a view of the subject as Dr Gunning had taken. But the state of anaesthesia was assuredly not a state of asphyxia, any more than the state of narcotism from opium, &c., was a state of asphyxia. Dr Simpson had often in midwifery seen the state of complete anaesthesia from chloroform kept up for one, two, three, four, or more hours, and the person immediately and perfectly recover as soon as the inhalation of chloroform had ceased. If you induced and maintained asphyxia, simple asphyxia, by closing the glottis, compressing the windpipe, or any such direct means, for as many minutes, you would have a very different result. Yet, if Dr Gunning's doctrine were true, and anaesthesia was asphyxia, you ought to be able to induce and maintain it by merely drawing your patient's neckcloth tight for the time being, or by covering over his mouth and nostrils by a handkerchief or towel. Surely Dr Gunning would not venture to reduce his theory to practice. Ville and Blandin, in their experiments on ether, had actually found the elimination of carbonic acid from the lungs increased during anaesthesia, instead of decreased as in asphyxia, and as they a priori expected. Dr Gunning believed the more immediate cause of chloroform anaesthesia, was an increased degree of pressure on the brain from impeded return of blood in the jugular veins. But mere increas.