PART II.
CRITICAL ANALYSIS.

I.

Philosophical Transactions of the Royal Society of London for the Year 1809. Parts I. and II.

This volume is full of interesting and important matter; but we must confine our attention, at present, to those papers which belong to physiology, and to subjects connected with medicine, reserving the review of Professor Davy's discoveries for another opportunity. The first paper is,

The Croonian Lecture. On the Functions of the Heart and Arteries. By Thomas Young, M. D. For. Sec. R. S.

The experiments of Dr Hales, and some mathematical calculations of the learned secretary, are here brought forward to elucidate some of the functions of the circulation. Dr Young thinks the only considerable resistance which the blood experiences, occurs in the extreme capillary arteries; and he infers, from a variety of reflections, that the muscular power of the arteries has very little effect in the progressive motion of the blood. The question about the powers which give motion to the blood, admits of very opposite and plausible statements. The little that is known on this subject, however, has not been derived from the mathematical school, but from the experiments of Hales, Haller, Senac, Hunter, and Jones; who, not content with calculating upon what was already known, determined to discover something new.

V. A Letter on a Canal in the Medulla Spinalis of some Quadrupeds. By Mr William Sewell.

"Upon tracing the sixth ventricle of the brain, which corresponds to the fourth in the human subject, to its apparent termination,
mination, the calamus scriptorius, I perceived (says Mr S.) the appearance of a canal, continuing, by a direct course, into the centre of the spinal marrow. To ascertain, with accuracy, whether such structure existed throughout its whole length, I made sections of the spinal marrow, at different distances from the brain, and found that each divided portion exhibited an orifice, with a diameter sufficient to admit a large sized pin, from which a small quantity of transparent colourless fluid issued, like that contained in the ventricles of the brain. The canal is lined by a membrane, resembling the tunica arachnoida, and is situated above the fissure of the medulla, being separated by a medullary layer; it is most easily distinguished where the large nerves are given off in the bend of the neck and sacrum, imperceptibly terminating in the cauda equina."

This is a modest and well written paper; it communicates an important discovery in a simple and unostentatious language.

VII. Account of the Dissection of a Human Foetus, in which the circulation was carried on without a Heart. By Mr B. C. Brodie.

This case of monstrosity is clearly and comprehensively detailed, and several peculiarities are neatly adverted to. The fact itself is sufficient to refute some of the speculations in the preceding Croonian Lecture, because the circulation must have been carried on by the muscularity of the arteries, as there was no heart. Similar cases are related by Lewenhoek, Hewson, Morton, and others; but none have been so satisfactorily examined as this by Mr Brodie.

IX. On the Nature of the intervertebral Substance in Fish and Quadrupeds. By Everard Home, Esq.

Another instance of beautiful structure and contrivance in some fish and quadrupeds, which will delight all lovers of philosophical news.

XII. An Anatomical Account of the Squalus Maximus of Linnaeus, which, in the Structure of the Stomach, forms an intermediate link in the gradation of Animals, between the Whale tribe and cartilaginous Fishes. By Everard Home, Esq. and F. R. S.

The fish from which this very accurate account is taken, was caught in the Channel on the night of the 13th of November 1808.
1808. Nearly about the same period, two other fish, of large dimensions, were thrown upon our coast. One at Penrhyn in Cornwall, and the other at Rotheslom in Stronsay, one of the Orkney isles. Mr Home endeavours to prove, that these were the same fish as that which he has described; but we shall have another opportunity of entering more fully into this disputed point of the natural history of our island.

XVIII. An Account of a Calculus from the Human Bladder, of uncommon magnitude. By Sir James Earle, F. R. S.

Sir Walter Ogilvie, Bart. of Dundee, the subject of this extraordinary case, received a blow on his back, at the age of twenty-three, which brought on a paralysis of the pelvis and lower extremities. During three months, he was obliged to have his water drawn off: for fourteen months he remained in bed, or in a horizontal posture; and, though he recovered the voluntary power of the bladder, and of his limbs, sufficiently to walk with the help of crutches, his general health continued many years in a weak and precarious state. About twenty years after the accident, he perceived symptoms of stone in the bladder; on being examined a stone was felt; the operation of extraction was recommended, but was postponed. At the age of fifty-three, thirty years after the accident, the spasms and fits of pain became so frequent and violent, that he was determined to have the stone extracted. The paralytic state of the lower limbs was considered no objection to the operation.

Sir James Earle and Mr Cline were consulted as to the practicability of extracting this large stone. After mature consideration of every circumstance, these gentlemen were of opinion, that the possibility of extraction must depend on the consistence of the stone: if it proved soft, it might be taken away, but if too hard to be broken it would be too large to be extracted whole, and must be left. The usual lateral operation was judged to be the only safe and probably means to be attempted. It was performed by Mr Cline. The staff could be passed in no farther than the neck of the bladder. The division of the urethra and prostate gland was made with the scalpel and probe-pointed bistoury. When this was accomplished, it was found impracticable to introduce any kind of forceps; but, on pressing hard with the finger, part of the stone felt soft, gave way, and made some room for the forceps, which brought away several portions, and with the assistance of a scoop, as much stone was extracted, as would have filled a large tea-cup; but the great mass beyond what the finger could
could reach on either side, still remained hard and impenetrable; and after repeated trials with forceps of different kinds, and of the strongest powers, it was found impossible to reduce the size of it, or take it away. Of course it was left, and the patient died ten days after the operation. On opening the abdomen, the bladder was found much diseased and thickened, firmly embracing a stone of extraordinary magnitude, and appearing to be completely filled with it. When taken out, the form of the stone appeared to have been moulded by the bladder; the lower part having been confined by the bony pelvis, took the impression of that cavity, and was smaller than the upper part, which having been unrestricted in its growth, except by the soft parts, was larger, and projected so as to lie on the os pubis. The weight of the stone was forty-four ounces, or three pounds four ounces, (apothecary's weight;) the form of it elliptical, the periphery on the longer axis, sixteen inches, on the shorter, fourteen. Its chemical composition was proved to be ammoniacal-magnesian phosphatic, the fusible calculus of Dr Wollaston.

We have given the above abstract of this case, because its interest depends upon considerations of fact and practice. As a question of fact, it is short and easy to decide upon; but it is of great importance, as it affects the practical rule, which ought to be followed in similar cases. The internal structure of the stone, which was fortunately exposed by the finger, during the operation, (though it ought to have been shown more completely, by sawing the stone through its whole length) appeared to consist of distinct calculi or nuclei, consolidated into one mass, and covered with innumerable layers of earthy matter. In this respect, it is an instance of the correctness of the opinion entertained by Albinus, and other authors, who thought that a number of small stones might be cemented together, so as to form a large calculus; yet that opinion is contradicted by the most common structure of calculi, which are formed upon one nucleus only. This stone is not the largest ever seen in Europe, though it exceeds the dimensions of all those huge concretions described in this country. Dr Heberden has described one weighing more than thirty-three ounces, (Phil. Trans. 1750) and Lister, in his journey to Paris, mentions one, which, he says, was taken from a monk, and weighs fifty-one ounces.

Now, as to the practical precept to be deduced from this history. Far be it from us to indulge that unreasonable and illiberal habit of blaming a piece of practice, because it has not succeeded; but we must view this operation as an experiment, and one which we should not willingly have tried upon ourselves, and consequently not have advised another to submit to. Be-
cause there could be no doubt of the stone being very large, of
uncommon magnitude; because the texture of large stones is
known, from their chemical composition, to be very hard, and,
therefore, not easily broken; because uniform experience shows
the difficulty and danger of extracting large stones, even from
healthy subjects; and lastly, because Camper has shewn, that
calculi, whose circumference is eight inches, require a wound in
the bladder of four inches, and cannot be extracted at any one
time, to preserve the patient's life; he has likewise shewn, that
the forceps of Le Dran for breaking stones, cannot be introdu-
ced into the bladder, without tearing the prostate gland.

XIX.—On Expectorated Matter. By George Pearson, M. D.
F. R. S.

From many experiments and observations made on this intri-
cate part of animal chemistry, Dr Pearson concludes, that the
various kinds of expectorated matter do not differ in the ingre-
dients of their composition, but merely in the proportion of
them one to another. His experiments seem to shew, that the
circulating and secreted fluids contain neutralized potash, and
not soda, as commonly imagined. He does not state those pro-
properties by which expectorated secretion may be distinguished
from expectorated pus, although he thinks he has discovered ma-
ny. For the minute details, our readers must consult the origi-
nal paper.

XXI. Observations on Albumen, and some other animal fluids, with
remarks on their analysis, by electro-chemical decomposition. By
Mr William Brande, F. R. S. Communicated by the Society for
the Improvement of Animal Chemistry.

These experiments prove, that the coagulation of albumen is
owing to the separation of alkaline matter, which keeps it in
a fluid state; and electrical decomposition will separate it
from such states of combination as are not to be detected
by the usual tests. Some new ideas are suggested concerning
the composition of mucus; and the whole paper deserves an at-
tentive perusal.

XXII. Hints on the Subject of Animal Secretions. By Everard
Home, Esq. and F. R. S.

A society has been instituted in London for the improvement
of animal chemistry, and every one must anticipate important
discoveries
discoveries from the combined exertions of so many able anat-
omists, physiologists, and chemists, whose knowledge of these
branches of science renders them peculiarly fitted for the lau-
dable purpose of such an undertaking. This is one of their
papers, published in the Philosophical Transactions; and it seems
to call for particular notice, as it is supposed to throw light
upon the principle of secretion, and to serve as hints for future
inquiry, beneficial to medical science.

After detailing five experiments, made by Mr Brande in five
succeeding months A. D. 1809, Mr Home concludes with the
following observations:

"By these experiments it is ascertained, that a low negative
power of electricity separates from the serum of the blood an alka-
line solution of albumen; that a low positive power separates albu-
men with acid and the salts of the blood. That with one degree of
power, albumen is separated in a solid form; with a less degree, it
is separated in a fluid form. From these facts the following queries
are proposed:

"1st, That such decomposition of the blood by electricity may
be as near an approach to secretion, as could be expected to be pro-
duced by the artificial means at present in our power.

"2d, That a weaker power of electricity than any which can
readily be kept up by art, may be capable of separating from the
blood, the different parts of which it is composed, and forming new
combinations of the parts so separated.

"3d, That the structure of the nerves may fit them to have a low
electrical power, which can be employed for that purpose; and as
such low powers are not influenced by imperfect conductors as animal
fluids, the nerves will not be robbed of their electricity by the sur-
rounding parts.

"4th, That the discovery of an electrical power, which can se-
parate albumen from the blood in a fluid state, may explain the mode
in which different animal solids and fluids may be produced, since,
according to Mr Hatchett's experiments, albumen is the principal
material of which animal bodies are composed.

"5th, That the nerves of the torpedo may not only keep the elec-
tric organ under the command of the will, but charge the battery,
by secreting the fluid between the plates that is necessary for its ac-
tivity.

"6th, As albumen becomes visibly coagulated by the effect pro-
duced from twelve four-inch double plates of copper and iron, a
power much too low to affect even the most delicate electrometer,
may not this be occasionally employed with advantage as a chemi-
cal test of electricity, whilst the production of acid and alkali, effect-
ed by still inferior degrees of electricity to those required for the
coagulation of albumen, may likewise be regarded as auxiliary tests
on such occasions?"—p. 390.

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These queries seem to confound two subjects, in their nature essentially different; the production of nervous energy or nervous power, and the separation of the different substances from the mass of blood. Both these subjects are enveloped in great obscurity, and many hypotheses have been invented to explain them, but in aiming at generalization we should not overlook the great distinctions between them. There can be no doubt that the laws of chemical affinity act in living as in other bodies; and as different salts dissolved in the same vessel, crystallize apart, so in the animal body all the component parts of the secretions are mingled in the blood, and go to be deposited each in their respective and proper parts: the osseous matter to the bones, the synovia to the joints, the bile to the liver, the urine to the kidneys. The vital functions are carried on by secretions, that is to say, by chemical combinations and transmutations; which are regulated directly by the muscular power of the vessels, but indirectly influenced by the nerves through the medium of the muscular power. The vessels are the apparatus in which these chemical changes take place, but there is no proof of the nerves being the principal agents in the process of secretion. That the nerves assist the secreting organs in performing their functions has long been known; and nobody can have seen a patient in hysteria without observing how much the action of the kidneys is increased in that disease. It has not escaped the watchful eye of medical theorists, that animals possess the power of generating something analogous to electricity and galvanism. This opinion has been gaining ground ever since the experiments of Mr Walsh on the torpedo, which were communicated to the Royal Society some years ago; and the brilliant discoveries made by Mr Davy, naturally excite physiologists to attend to this extraordinary faculty. But there is nothing analogous to secretion in muscular contraction, nor in the shock which the gymnotus electricus can give to any animal approaching it. The argument that a low degree of electrical power may influence the secretions, is nugatory, because some secretions have been imitated out of the body without employing any such power. All animal substances are convertible into fat; having every property of the natural animal oils. Both the fibrin and the albumen of the blood are convertible into gelatine, and the experiments of Mr Hatchett show in what manner crassamentum or fibrin can separately be converted into a matter similar to the adipo-resin of the bile. Mr Home seems to consider the nerves as the chief instrument of secretion; and an experiment of Dr Berzelius, Professor of Chemistry at Stockholm, is quoted to show that the agency of the
the nerves in secretion has been disregarded. There is no foundation for either of these observations. The nerves have quite enough attributed to them without any additional work. It is their office to give sensations; it is the office of the muscles to give the power of accommodating the objects of the material world to our convenience: it is the office of the blood-vessels and glands to separate various substances from the circulating fluids. How can we account for secretions going on, and parts living, after the nerves and muscles leading to those parts have been destroyed, if the structure of the nerves fits them for the exercise of this supposed electrical power? As to the nerves of the torpedo keeping the command of the electric organ, they act like our own nerves; for besides the sensations derived from external impressions, there is one, little attended to, though very important, and that is, the sensation of muscular motion, which serves to regulate the force and quantity of contraction to be expended on any particular occasion. It is gratifying to see the old notion of a vital principle influencing secretion entirely abandoned by these enlightened inquirers into animal chemistry; but there seems some danger in overrating the influence of electricity, for however much we must admire the splendid exercise of its power in analysing different bodies, we ought to be cautious to avoid the visionary theories of the French and German school.

XXV. On Respiration. By William Allen, Esq. and William Hasledine Pepys, Esq. F. R. S.

In this elaborate paper, we find a series of accurate and well-contrived experiments, conducted without reference to any particular theory, which furnish a variety of important facts for the consideration of physiologists. The experimentalists conclude with a philosophical wish, in which they will be supported by all the lovers of nature and truth. "Confident, however, that all those who repeat the experiments with the same care, will arrive at the same results, we shall rest satisfied with stating the facts, not without a hope, that those brilliant discoveries of Professor Davy, which have already given us new views of the operations of nature, will, in their progress, furnish us with that explanation, which it is in vain to expect at present."