last twenty-four hours; and there was considerable distension of the abdomen. A large blister was applied to the abdomen. The calomel and opium pills were ordered to be taken again every two hours; and a turpentine enema, containing two ounces of tincture of assafoetida, to be administered in the evening. He was also directed to have a pint of beef-tea daily; and the nutrient enemata were omitted.

June 17th. The bowels had been freely moved, with the expulsion of much flatus. The gums were slightly touched by the calomel. The pills were omitted.

Vespere. The bowels being copiously moved all through the day, he was ordered an anodyne injection at night, and to have two pints of beef-tea.

June 22nd. He was improving in every respect.

R Infusi quassiae ʒviij; tincturæ strychniæ ʒss. Fiat mistura ejus capiat ʒj ter die.

He was ordered common diet.

He was discharged cured on July 5th.

[To be continued.]

Original Communications.

OBJECTIONS TO THE STATEMENTS OF DR. FLEMING CONCERNING THE ACTION OF ATROPIA UPON THE EYE.

By HENRY LAWSON, M.D., Professor of Physiology in Queen's College, Birmingham.

THERE are few subjects within the very distant limits of physiological science, which have received so much careful consideration as that of the organ of vision; and on that account, it is of the greatest importance that, in adding to the already comprehensive information which we possess concerning the structure and functions of the eye, some attention to the views of the older writers should be paid by those who are about to advance new hypotheses.

In the *Edinburgh Medical Journal* for last month (March), an article has appeared from the pen of Dr. Fleming, in which he speaks of the action of atropia upon the eye. Dr. Fleming's merits as a researcher in the wide field of the science of *Materia Medica* are too well known to need any comment from me; it is, therefore, with a feeling of some reluctance that I come forward to controvert any statement made by so distinguished a physician. That it is my intention to question their accuracy, I frankly confess; not, however, from a spirit of opposition, but rather with the hope that their author may take further steps to maintain the truth of what he is pleased to term *his* theory.

The conclusions at which Dr. Fleming has arrived may thus briefly be summed up.

1. Adaptation of the eye to distinct vision at different distances is effected in great part by the iris.

2. The iris is an erectile structure.

3. The ciliary muscle, in contracting, presses on the veins returning from the iris, and so causes turgescence of the latter, decrease in the size of the pupil, and power of examining near objects.

4. Dilatation of the pupil is due to the contraction of the capillaries, diminution of the quantity of blood, and hence an absence of the necessary erectile fluid.

5. Contraction of the capillaries may be due to a stimulation of the sympathetic nerve.

With regard to conclusion 1, I may observe:—

a. That the view is by no means a novel one, having been proposed many years ago by Pouillet and Mile.

b. That it has been successfully refuted experimentally; and

c. That the case recorded by Von Graefe completely upsets it. In Graefe's case, the iris had been entirely removed; yet the power of adaptation was as perfect after as before the operation.

In proof of conclusion 2, the writer asserts, that the softness of the iris is evidence of its being erectile! It is not necessary to remark, that testimony of so vague a character is hardly testimony at all; but, apart from this, I take the evidence of Mr. Bowman on the matter, in preference to that of Dr. Fleming. The former writer observes: "From what has now been said of the vessels of the iris, it is evidently not an erectile structure. If a doubt still remained, it would be dispelled by the fact that the iris will contract and dilate several times after death under favourable circumstances." (*Lectures on the Parts concerned in the Operations on the Eye.* By William Bowman, F.R.S., F.R.C.S., etc.)

The third deduction seems rather hastily framed. It is very questionable whether the veins of the iris are ever compressed by contraction of the ciliary muscle; and, even on the author's supposition, the turgescence would remain to be accounted for, inasmuch as it is probable that the arteries would also suffer compression, thus leaving the quantity of blood in the iris the same as before.

I take it, that the fourth conclusion has especial reference to the action of atropia; that, in fact, the dilatation of the pupil after the administration of atropia, is due to an absence of blood. Dr. Fleming has not shewn in any way that, in the case of dilatation of the pupillary orifice after the application of this drug, the vessels are constricted. It is quite true (indeed, I myself have observed it in the web of the frog's foot) that a local application of atropine does produce a temporary diminution in calibre of the capillaries; but this constriction does not continue for days, and may be owing to other circumstances.* On the other hand, it has been well ascertained that, in every case of poisoning with belladonna, the brain is fearfully congested—to use Christison's words, "*gorged with blood*"; and, as the vessels of the eye are merely offshoots from the cerebral arteries, we may fairly suppose that the ciliary vessels are dilated, rather than diminished in capacity, when under the influence of atropia.

I cannot see upon what facts conclusion 5 is based. I fancy that stimulation of the sympathetic is, in the instance referred to, exceedingly unlikely. It would be much more in accordance with the beautiful discovery of M. Claude Bernard, to suppose that the (functionally) cerebro-spinal nerves, and consequently the dilator muscles, are paralysed, and that therefore the constrictors, having no force to antagonise them, narrow the vessels. I do not for a moment assume that this is the real state of things; for I conceive that the vessels are either dilated or in their ordinary condition, and am of opinion that we are not yet in a position to draw very precise inferences relative to the control of the two sets of nerves over the movements of the iris. I merely throw out the hint as a suggestive one.

There are two or three other assertions of Dr. Fleming, which do not stand the test of a close examination. Thus, he states that the blindness or faint vision of epileptics is owing to a want of turgescence of the blood-vessels! If the statement be not intended to bear upon the writer's remarks, it means nothing, as it has been before put forward in another shape. But, if it be supposed that the faintness of vision results from an absence of blood from the iris, then I would say—

1. That when the blood has left the iris, vision is not impaired; and

2. That a much better explanation of the phenomenon might be sought in the diminution of the blood

* Such, for example, as the influence of the styptic acid, which is occasionally employed to dissolve the alkaloid.

supplying the cerebrum and retina, as Dr. Brown-Séquard has long since demonstrated.

The most startling fact which the writer announces is that, when the walls of an artery are softened, and deprived of contractility, *there is a much greater quantity of blood transmitted to its ultimate ramifications, than when it was in a healthy condition.*

I shall make no further allusion to this strange assertion, than a recommendation to my readers to consult their senses and their text-books on Physiology, for a corroboration (?) of Dr. Fleming's opinion.

In conclusion, I would submit the following queries, bearing upon the foregoing matter, to the careful consideration of the profession.

1. What becomes of the aqueous humour when the vitreous is pushed forward?

2. If the capillaries of the system can dilate without compression of the systemic veins by special muscles, why may not those of the eye act similarly?

3. If the capillaries of the eye can be dilated by the action of their coats, or owing to an increased demand for blood on account of organic change, what is the office of the ciliary muscle, which in the class *Aves* is composed of even striated tissue?

4. Do we always find that, during congestion of the eye, there is an inability to perceive distant objects?

5. If the vitreous humour be pressed on by the turgescence of the vessels, to an extent sufficient to cause the projection of the lens, will not the retina sustain some injury?

6. Can the pressure be sufficiently great to cause the elongation of the "arteria centralis retinae"?

I hope, in a memoir which I am preparing for publication, to enter fully into the subject of "adaptation", to review all pre-existing theories, and to enunciate my own opinions concerning the mode in which the refracting power of the human eye is increased and diminished according to the will of the individual. In the meantime, I shall be glad to learn the views of others, when based upon well-ascertained facts, and not merely speculative.

FÆCAL ABSCESS, TERMINATING SUCCESSFULLY.

By THOMAS HENRY SMITH, Esq.

T. J., aged 18, had been delicate from birth. He was first seen on June 18th, 1862, when he had smart febrile symptoms, with deep-seated pain, increased on pressure, over the region of the cæcum; there was swelling, and some dulness on percussion. He was much relieved by small doses of calomel and opium for a few days, with salines and linseed poultices, with an opiate ointment smeared over the surface.

On June 28th, tenderness had subsided, but there was apparently some thickening of the bowel. A blister was applied with benefit; and he seemed to be getting well. Oleaginous aperients had brought away a very large quantity of feculent matter.

About a fortnight from the commencement of his illness, on lifting a slight weight, he felt a sudden acute pain in the right lumbar region. This pain was relieved by sedative applications from time to time, but it never left him; and on July 8th, I found an ill-defined elastic swelling, without any distinct fluctuation, midway between the last rib and the crest of the ilium. Dr. Habershon saw him with me, and advised leeches, poultices, and perfect rest.

On July 19th, Dr. Habershon saw him again. The swelling had decreased; there was less pain; and we agreed that there was nothing to warrant an opening being made.

He fluctuated for two or three weeks, taking bland,

easily digestible food; but, on the whole, decidedly losing ground, emaciating a good deal. During all this time the bowels acted naturally.

On August 16th, I found that matter was evidently coming to the surface, and pointing just above the junction of the middle with the posterior third of the crest of the ilium.

On August 17th, I made a free opening, and out rushed a full pint of intensely foetid matter, with so much flatus, that there could be no doubt of communication with the bowel; in fact, it was just like opening the distended colon itself.

On September 4th, Dr. Habershon saw the patient for the third time; and the prognostication was very gloomy. Fluid feculent matter was passing in abundance through the opening, though also naturally by the rectum; emaciation was extreme; the pulse very rapid; and the tongue irritable. He was being well supported with wine, strong animal broths, milk, eggs, etc. Charcoal poultices agreed best with the wound; and, with great attention to cleanliness, prevented fætor. He took ammonia with chloric ether, and decoction with compound tincture of bark.

From this time, however, he slowly but steadily improved. The appetite kept up; he took abundant nourishment; the discharge lessened; flatus ceased to pass; finally, the discharge was no longer feculent; and he was able, by the third week in September, to take carriage exercise.

On September 26th, he went to Brighton, and returned on October 24th, so wonderfully improved in health, strength, and amount of flesh, as hardly to be recognised; in fact, he was in much better condition than he had been before his illness. The discharge has now nearly ceased; there has been no ill smell to it ever since September; and he is the picture of health.

There are some points of interest about this case apart from the rarity of its termination.

1. Was the original affection a low form of peritonitis in the cæcal region? or inflammation, running on nearly to abscess, external to the peritoneum, in the abdominal parietes? The diagnosis between the two is at first by no means easy. My first impression was, that the cæcum was alone involved; and, during the first fortnight of his illness, very abundant evacuations passed by the bowel; treble the amount of food taken; showing that there had evidently been a considerable accumulation in this region. But, after the more decided swelling appeared externally in the posterior lumbar region, some effusion and thickening of the cellular tissue between the anterior and posterior parts could be traced: so that the irritation would seem to have been communicated from the cæcum to the cellular tissue in the parietes of the abdomen adjacent to it.

2. Did the bowel give way at first when the sudden pain was felt? or did the perforation take place from without, matter remaining for some time in the walls of the abdomen, and then making its way into the intestine? The latter suggestion is the most probable; for had the bowel given way so early in the case, in all likelihood there would not have been the long period of suspense, and, I may say, of uncertainty, as to whether there was matter or not; it would not have been quiescent so long; there would have been more pain and more constitutional disturbance. Could the matter have been evacuated early, the communication with the bowel might possibly have been prevented; but at the first consultation there was a doubt as to whether there was matter at all; and at the second, the swelling had so lessened that an opening seemed uncalled for.

3. The opening into the intestine was probably *valvular*; had it been *circular*, there would have been less chance of its healing up so kindly.