

a little later. If a trial is made of these combinations on a proper basis and scale, I should very much like to hear what others can effect with my powder also.—I am, etc.,

JOHN A. GRAHAM, M.B.

Bethulie, O.F.S., South Africa, Aug. 27th.

HAEMORRHAGE FROM THE TONSIL.

SIR,—I have been greatly interested in this discussion, having anaesthetized for many thousands of tonsillectomies. I have observed that haemorrhage of a severe character is far more often associated with the use of the guillotine, and that it is most rare when the operation is carried out by dissection, owing, I suppose, to the tearing and twisting of the vessels with consequent clotting. Although ether is undoubtedly safe, there is, in spite of the great use of atropine, more haemorrhage than with chloroform. Some surgeons prefer the head low, and in this position, although the blood conveniently pools in the post-nasal space, there is more congestion (and bleeding) than when the head is level with the trunk. With a skilled assistant to swab, it is, as a rule, easy to keep pace with the surgeon. In my experience, most cases of severe haemorrhage have been in men with high blood pressure and fibrous tonsils who have been great smokers—notably soldiers during the war. Stitching the pillars has almost invariably stopped the haemorrhage, especially with a small plug of gauze placed in the tonsil bed.—I am, etc.,

GRAHAM SCOTT,

Late Anaesthetist, 4th London General, Great Ormond Street Children's, and Dreadnought and Central London Throat Hospitals.

Herne Hill, Sept. 17th.

CAPILLARY PRESSURE.

SIR,—Dr. Hill (September 10th, p. 417) tells us that he has "never failed to realize the obvious fact that there must be a greater pressure inside than outside the capillaries in order to maintain patency." My first letter (June 11th, p. 873) called attention to Dr. Hill's having taken no account of this obvious fact in his lecture, paragraph 2, last sentence. For the sake of brevity I did not call attention to his ignoring this obvious fact in paragraph 5 of his lecture, where he spoke of the pressure of the aqueous balancing the capillary pressure in the iris. And when Dr. McQueen (June 25th, p. 955) imagined he had proved that the pressure in capillaries was less than the pressure outside them by a manometric pressure of 10 mm. Hg, there was no protest from Dr. Hill that such a result must be wrong as being contrary to the same obvious fact.

However, Dr. Hill and I are now agreed that there must be greater pressure inside than outside the capillaries. But Dr. Hill says he maintains that the difference between the two pressures is very small, and in support of this contention he recapitulates some of the matter which appeared in his lecture, partly irrelevant, and the rest inconclusive, as I shall now show.

1. The fact that the pressure in the cerebro-spinal fluid is found to be about the same as that in the torcular Herophili gives no indication of the amount by which the pressure in the capillaries of the brain exceeds either of them.

2. The fact that the pressure of the brain against the skull is circulatory in origin, and is increased when the pressure in the cerebral blood vessels increases, is no proof that these two pressures are equal, or nearly so. If Dr. Hill will attach a small toy balloon to the tube through which he inflates his Roy and Graham Brown apparatus, so that the toy balloon lies inside the chamber, and connect the said tube with a separate manometer from that which records the pressure in the chamber outside the balloon, then close the chamber by tying on its covering membrane, and inflate the balloon, he will find that when the wall of the balloon is taut the pressure in the balloon is greater than that in the chamber outside it, though the latter is derived from it and increases with it. The smaller the balloon in comparison with the chamber, the more remarkable will the difference of pressure be.

3. The argument from the experiment of the cut finger inserted into a tube connected with a manometer is misleading. Every surgeon knows that when blood vessels are severed they both retract and contract. Owing to the contraction there is greater resistance to the flow of blood

through them, so that the blood emerging is probably at a lower pressure than there was at the same point before severance. Owing to free anastomosis, when the flow of blood in one direction is impeded it can get away by other channels, thus avoiding much piling up of pressure.

4. I have already (June 11th, p. 873) explained the fallacies of Dr. Hill's method of "measuring" the capillary blood pressure with the Roy and Graham Brown apparatus.

5. Dr. Hill says:

"The Roy and Graham Brown method has shown me that when the heart is stopped it takes the least pressure (1 to 2 mm. Hg) to drive the corpuscles along the capillary vessels as quickly as they move in the natural conditions of the circulation."

The corresponding statement in Dr. Hill's lecture was:

"When the web of the excised leg is compressed in the Roy and Brown apparatus, a momentary pressure of 2 mm. Hg will cause the corpuscles to rush along the capillaries and venules no less rapidly than in the normal flow."

Before giving an opinion on the argument from this observation I should like to know:

(a) What was the pressure in the Roy and Brown chamber to which the additional pressure of 2 mm. Hg was added?

(b) Was the additional pressure of 2 mm. Hg added by pumping more air into the chamber or by compressing the dome with the glass?

(c) Is the flow seen at the centre of the flattened portion of the dome, or only near the junction of the flattened and curved portions?

(d) It might also be of importance to know the magnifying power of the microscope used.

As I indicated in my first letter (June 11th, p. 873), I made no reference to several of the above points, because reference to them all would have made my letter too long. My object in writing was simply to point out that Dr. Hill was giving us as proofs of his statements things that were not proofs at all.

Another instance of this which I have not previously discussed is to be found in paragraph 5 of his lecture, where he said:

"That the pressure of the aqueous balances the capillary pressure in the iris is shown by the fact that on letting this fluid escape the iris bulges forward and may touch the cornea, and on compressing the abdomen the vessels burst and the blood comes into the anterior chamber. No such bulging or haemorrhage can be brought about by squeezing the belly when the eye is intact."

The irrelevancy of this argument is transparent. The pressure in the capillaries of the iris presses the posterior part of their walls backwards as much as it presses the anterior part forwards, and has nothing to do with the movement of the iris forward when the aqueous is let out. That the walls of the capillaries of the iris can stand the additional strain put upon them by compression of the abdomen when they have outside them the support of the aqueous in an intact eye, and cannot do so when deprived of that support, is no proof that the pressure in them is the same as that of the aqueous.

But Dr. Hill has me on the horns of a dilemma when he says (September 10th, p. 417):

"There is, of course, a difference of tension between the inner and outer surface of a capillary, but this is so small as to be negligible. Let Dr. Gillespie get out of his armchair and proceed by experiment to prove the opposite which he so dogmatically asserts."

If I take the first portion of this statement as it stands, and point out that it is nonsense, and that I have not asserted the contrary, I may be told that I am quibbling, as Dr. Hill meant something else which ought to have been plain to me; whereas if I attempt to amend it I may be accused of ascribing to Dr. Hill a statement he has never made. I had therefore better leave it. Dr. Hill's amendment of Dr. McQueen's nonsensical sentence fails to make a sensible statement out of it.—I am, etc.,

Knock, Belfast, Sept. 12th.

JOHN R. GILLESPIE.

SIR,—Those of your readers who are acquainted with Professor Leonard Hill's work and have at the same time followed this correspondence are cognizant of the fact—to give only one, though glaring, example of Dr. Gillespie's misrepresentations—that Professor Hill has never taken "the pressure of the brain against the skull as a measure of the pressure in the arterioles," yet Dr. Gillespie has stated this more than once. I submit, therefore, that the charge of misrepresentation is more than justified.