(Graham-Brown that they are contractile, beside which we cannot doubt, what has long been inferred, that the blood may acquire peculiarities which add difficulties to its transit."

Upon this statement I would remark first, with regard to the blood pressure in the capillaries, that Dr. Dickinson apparently overlooks the fact that the resistance offered by the hypertrophied arterioles is counterbalanced by the increased force of the correspondingly hypertrophied left ventricle, so that the intracapillary pressure probably remains practically unchanged whatever may be the degree of cardiac and arterial hypertrophy.

Then, with regard to contraction of the capillaries, admitting the possibility that, as Roy and Graham-Brown suggest, "the individual capillaries contract or expand in accordance with the requirement of the tissues through which they pass," surely no physiologist maintains that capillary contraction, or any acquired peculiarity of the blood, can cause such an impediment to the circulation as will account for the enormous hypertrophy of the left ventricle, which occurs in cases of chronic Bright's disease.

With respect to the contraction of the capillaries, Dr. Michael Foster remarks:³ "In a young animal, at all events, the capillary wall is to a certain extent contractile; the epithelioid cells, which then appear to contain a large amount of undifferentiated protoplasm, seem able, under the influence of stimuli, to change their form, passing from a longer and narrower shape to a shorter and broader one, and thus influencing the calibre of the tube of which they form the walls. How far such an active change of form takes place in the capillaries of the adult body has not yet been definitely determined."

In opposition to Dr. Dickinson's theory, I maintain that the explanation of the hypertrophied left ventricle, which I first published in the fifty-first volume of the *Medico-Chirurgical Transactions*, "holds the field."

When Drs. Alison and John Reid were investigating the phenomena of asphyxia, the hypothesis of capillary obstruction appeared to be consistent with the facts; but since the function of the muscular arterioles, as distinguished from that of the larger elastic arteries, has been revealed by the researches of Bernard, Brown-Séquard and others, the older hypothesis has been shown to be untenable.

It is now admitted by all physiologists that the function of the muscular arterioles is to regulate the blood supply to the capillaries. In the words of Professor Huxley: "While the small arteries lose the function which the capillaries possess of directly irrigating the tissues by transudation, they gain that of regulating the supply of fluid to the irrigators or capillaries themselves. The contraction or dilatation of the arteries which supply a set of capillaries comes to the same result as lowering or raising the sluice gates of a system of irrigation canals."⁴

The hypertrophy of the muscular arterioles, which I first described in the paper referred to, and the hypertrophy of the left ventricle are joint results of the increased arterial contraction (what I have ventured to call their "stop-cock function") excited by the abnormal condition of the blood in cases of Bright's disease.

Dr. Dickinson admits that in those cases of Bright's disease in which cardiac and arterial hypertrophy with resulting arterial tension are most conspicuous, namely, cases of contracted granular kidney with copious secretion of urine, dropsy is commonly absent, and seeing, as he could not fail to do, that this is inconsistent with his theory of capillary obstruction, he says: "The arterial thickening cannot fail, so far as I see, to obstruct the access of blood to the capillaries and dam it back in the heart and larger vessels with the result of increased tension and increased cardiac hypertrophy. Here comes in the stop-cock action and with it lessened capillary pressure and exudation."

It will be seen, then, that Dr. Dickinson, denying the influence of the arterioles as a cause of the obstruction which results in cardiac hypertrophy, and explaining the occurrence of renal dropsy by increased capillary obstruction and pressure, is driven to admit the obstructive influence of the arterioles in

those cases in which, the cardiac hypertrophy being extreme and dropsy absent, his theory of capillary obstruction is utterly inadequate to explain the facts. How does Dr. Dickinson explain the supposed transfer of the obstruction from the capillaries in one stage or form of disease to the arterioles in another?

It would be interesting to know whether Dr. Dickinson still maintains the view of the function of the arterioles which he has expressed in his work on *Albuminuria*, Ed. ii, p. 328, where he says "the only feasible view of the mechanism of the circulation is that the wave of blood which arises in the ventricle is helped on by an advancing zone of arterial contraction, which follows swiftly in its rear, and by a sort of vermicular action continues the ventricular impulse as far as arterial muscle extends."

He dismisses the stop-cock theory as improbable and implying an unnatural conflict between the heart and the arteries. He further states: "We revert to the older view of capillary hindrance, and in this, it is scarcely possible to doubt, we find the key of the whole mystery of cardio-vascular hypertrophy."

It is evident that at the time of this publication Dr. Dickinson believed that the hypertrophy of the arterioles is the result of an effort to assist the heart in overcoming an obstruction caused by the capillaries in front.

In my *Medical Lectures and Essays*, page 38, I have suggested the following explanation of renal dropsy: "When, in consequence of acute inflammatory disease of the kidney, the urinary crystalloids and water accumulate in the blood, they pass thence by an exosmotic process through the walls of the capillaries into the subcutaneous areolar tissue and into one or more of the serous cavities. On the other hand, when the kidney has reverted to its normal condition and recovered its functional activity, the blood being first freed from excess of urinary solids and water, a current of dissolved urinary crystalloids sets in from the tissues into the blood and out by the kidneys. Thus, by a copious flow of urine the dropsical accumulation is soon swept away."

These phenomena, like the increased secretion of diabetic urine, the result of an excess of a saccharine crystalloid in the blood, occur, as I believe, quite apart from the influence or the existence of increased capillary pressure.

AN ADDRESS

ON THE

SURGERY OF THE TONGUE.

Read before the Liverpool Medical Institution.

By JONATHAN HUTCHINSON, F.R.C.S., F.R.S., LL.D.,
Late President of the Royal College of Surgeons.

THE surgery of the tongue has been greatly developed within the last five-and-twenty years. I well recollect the time when operations on the tongue were regarded with the utmost dread on account of the fear of hæmorrhage. Removal of the entire organ was never thought of at the London hospitals during the early part of my familiarity with their operating theatres. Even partial operations were rare, and, although so infrequent, I remember witnessing several in which the patient almost died on the operating table. They were never undertaken excepting in cases in which the disease was so far advanced that the diagnosis of cancer was obvious to everyone; and it is needless to add that they were seldom attended by results which offered encouragement to the performance of an operation in itself attended by so much hazard. The return of the disease in the glands was always a matter of certainty and promptitude.

I shall never forget the first case in which I was induced to operate on the tongue. This was in 1858, and about two-thirds of the organ were removed. The case was one of obvious cancer in a man of middle age. I operated in a lodging at Reigate, where I had induced the man to go, partly because I had myself a house there, and partly that he might have the benefit of pure air. Such was my fear of bleeding that I had had

² *Journal of Physiology*, vol. 2, p. 358.

³ *Textbook of Physiology*, Ed. v, p. 192.

⁴ *Mental Physiology*, 1886, p. 25.

constructed two small but powerful screw torsion instruments, by which I was enabled to tighten ligatures which had been passed through, one on each side well behind the diseased part. These were left in his mouth. Having in this way, in the course of forty-eight hours, produced a condition of complete gangrene, I then cut away the offensive mass with scissors. The plan succeeded perfectly, but it is needless to say that it was a most disagreeable one for the patient. He recovered well, and for some months was in great delight at what he thought the cure of a disease which he had supposed incurable. His well-deserved triumph, however, was not lasting, for in the course, as far as I remember, of about six months the disease returned in the glands behind the upper third of the sterno-mastoid. It was impossible to remove them, and in the course of a few months the man died.

In 1858 I was present in the theatre of the Royal Infirmary in Edinburgh, when Mr. Syme performed, for the second time, a complete excision of the tongue. It was done before a very large concourse of spectators, and it excited the utmost interest.¹ Mr. Syme divided the lower jaw, and held the two halves apart, thus gaining free access to the root of the tongue. He then cut it away, tying the arteries as they were divided. The operation was most skilfully performed, and without any very alarming loss of blood. For a few days it was hoped that the patient would recover, but he eventually sank, and with his death ended, I believe, Mr. Syme's advocacy of the operation. It was his second trial of it, both patients had died, and Mr. Syme was a man who never put aside the interest of his patients in the procedures which he undertook. He had previously abandoned excision of the knee-joint under very similar circumstances. Not long after Mr. Syme's operation we received at the Pathological Society, from Mr. Fiddes,² of Jamaica, a tongue which he had removed for cancer. It was his second case, and the successful issue of both was the means of giving a great stimulus to English surgeons in reference to the operation. Not long after this, Chassaignac's *écraseur* came into vogue, and with it an era of much increased boldness in dealing with cancer of the tongue. This organ, indeed, seemed to be exactly the one best suited for the use of the new instrument, since it is of soft structure, contains no bone, and is peculiarly liable to bleed. The enthusiastic inventor of the instrument had actually gone the length of first fracturing the bones of limbs, and then dividing the soft parts with the chain of which he was so fond. In the case of the tongue no such barbarous preliminary was necessary.

From the date of the introduction of the *écraseur* to the present time we have to record steady progress in reference to operations on the tongue until they have now come to be regarded as amongst the easiest and safest of those which we undertake. Our advance has been in several different directions, and it has resulted not only in a diminution of the risk attending complete excisions, but in a great reduction of the number of cases in which such operations are requisite, and in far better results as regards the subsequent immunity of the patient. We now diagnose early and operate promptly, with the result that many of our patients remain well for long periods, and more than a few permanently so.

It may be noted as a somewhat unexpected result in reference to the removal of cancer of the tongue, that experience has proved that it is by no means necessary to excise the whole of the organ in cases where the disease is limited to one part. This is contrary to what might have been inferred, for since the substance of the tongue is vascular, succulent, and well supplied with lymphatics, it was only reasonable to fear that infiltration of its substance with infective elements to a considerable distance from the apparent limits of the disease would be very likely to happen. Experience has

shown, however, that wide infiltration of adjacent tissues is not common, whilst, on the other hand, it has revealed the fact that infection of more or less distant lymphatic glands may be produced by ulcers of the most insignificant size and of the briefest duration. We now know beyond the possibility of doubt that the real risk is of gland infection, and that it begins almost from the very day that the sore assumes suspicious features.

I shall have to advert presently to the very interesting fact that cancer germs may remain latent in the lymphatic glands for periods of several years, and may then, after both patient and surgeon have been willingly believing that the case was a permanent cure, evoke disease. My own experience on the points adverted to has been most definite. Although I am still an advocate for very liberal excisions, I do not now think it necessary to recommend the removal of the entire organ in many cases in which in former years I should, on principle, have insisted on that measure. The cases are indeed but few in which I deem it necessary to remove absolutely the whole organ. They will become, I trust, less and less frequent as years advance and early diagnosis is better understood. With rare exceptions it may be said that the case must have been most grievously neglected in its earlier stages which ever comes to require removal of the whole tongue.

In justification of the partial excisions which I have practised and now recommend, I must state that in the whole of my experience of the surgery of the tongue, I have had but four cases in which the disease returned in the organ itself, or, indeed, in the mouth. One of these was a case of very advanced disease, in which I had removed the whole organ, but in which the tissues were already infected so far back that the disease sprouted again before the wound was well healed. Another was a case in which the floor of the mouth was infected; and in a third, that of the late Earl of —, I had foolishly allowed myself to be influenced by entreaties not to take more than was necessary. With these exceptions my partial excisions (and they have been very numerous) have, I believe, remained perfectly and permanently sound so far as the scar of the operation and the stump of the organ were concerned. The picture changes, I am sorry to say, when we come to speak of the lymphatic glands.

My impression is that in at least three-fourths of my cases sooner or later the disease has recurred in the glands. In many the interval has been very considerable and the gain to the patient from the operation has been very definite. But experience compels the admission that however early the stage at which the operation for cancer of the tongue is performed the probability is very great that enlargement of the lymphatic glands will sooner or later follow. It is to be added to this admission that in but few of the cases in which the glands become enlarged will the surgeon be able to operate upon them with much chance of permanent success. I make it an invariable rule to excise lymphatic glands if they are movable and accessible, and to do it in the freest possible manner; but I am sorry to say I can quote but very few cases in which it has been followed by protracted immunity.

(To be concluded.)

¹ Mr. Syme's case will be found recorded in considerable detail (by myself) in the *Medical Times and Gazette*, August 14th, 1858.

² The description of Mr. Fiddes's operation will be found in vol. xii of the *Pathological Society's Transactions*, p. 223. He divided the symphysis of the jaw, and used a pair of blunt-pointed scissors, and ligatured one lingual before cutting the other. His first case was recorded in the *Edinburgh Medical Journal* for 1859. Both were successful. A report on Mr. Fiddes, specimen by Sir Henry Thompson, Mr. Lawson and myself, records that the cancerous patch not so large as a shilling, and that the substance of the tongue everywhere else was quite free from infiltration. It was therefore not a case in which, in my opinion, excision of the entire tongue was necessary.

DEATHS IN THE PROFESSION ABROAD.—Among the members of the medical profession in foreign countries and the colonies who have recently died are Dr. Carl Christian Liman, Professor of Medical Jurisprudence in the University of Berlin, aged 74—he was formerly assistant to Casper, whose classical treatise he edited after the death of the author, each successive edition making the work more and more his own; Dr. John Singleton, of Melbourne, the well-known physician and philanthropist, aged 83; Dr. Eugène Bouchut, *agrégé* of the Paris Faculty and Physician to the Hôpital des Enfants Malades in that city, aged 73—Dr. Bouchut seems to have been the first who suggested intubation, but he failed to carry his idea to a successful issue; Dr. Moriz Kuh, a well-known physician and prominent sanitarian, of Brünn (Austria), aged 74; Dr. Weber-Liel, formerly Professor of Otology in the University of Jena, whom illness some years ago compelled to retire from active professional life; and Dr. John Clarkson Jay, of Rye, New York, distinguished for his researches on conchology, aged 83.