BRITISH MEDICAL JOURNAL:

BEING THE JOURNAL OF THE BRITISH MEDICAL ASSOCIATION.

EDITED BY ERNEST HART.

LONDON: SATURDAY, JULY 5, 1873.

THE HARVEIAN ORATION

DELIVERED BEFORE

THE ROYAL COLLEGE OF PHYSICIANS,

On Wednesday, June 25th.

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MR. PRESIDENT AND FELLOWS OF THE COLLEGE OF PHYSICIANS:—A man whose lot it is to live away from London may well feel some diffidence in accepting an invitation to lecture before a metropolitan audience; and, sir, when you honoured me by requesting me to deliver this year's Harveian Oration, I felt and expressed this natural hesitation. I wish to record that you pointed out to me that my function in Oxford was to pursue and lecture publicly upon the very subjects with which Harvey occupied himself; and I suggested to myself that what could with any propriety form the substance of a course of lectures in the one place, could, *mutatis mutandis*, furnish materials for an address in the other. I felt besides that, as the President of the College of Physicians is, by virtue of his office, one of the five electors to the Linacre Professorship, the Linacre Professor might seem scarcely justified in declining an invitation to appear before the learned body to which in part he owed his position; and, though I mention it last, I felt first of all that a wish expressed to me, not so much by the official whom I am now addressing, as by the individual who now more than twenty years ago introduced me to Harvey's hospital, and has persistently befriended me ever since, was a wish which I ought not lightly to disregard. If now, sir, I follow an example which you have often set me, and, without needless preface or further personal allusions, address myself at once to the business before me, I shall thereby pay you the best of all compliments, by showing you that your teaching has not been wholly thrown away upon your former pupil.

The time allotted to me I propose to occupy, firstly, in expounding with all possible brevity certain advances recently made in our knowledge of the anatomy and physiology of the circulatory organs; and, secondly, in giving the as yet unrecorded history of one of the many attempts to rob Harvey of his rightful rank in the noble army of discoverers, which were made in the latter half of the seventeenth century.

Some of the last, if not the very last, of the many fruitful experiments which Harvey performed in the way of interrogating Nature as to the circulation, were experiments in the way of injection. If the writer of a work which appeared but some forty-three years ago—Guthrie, *On the Diseases and Injuries of Arteries*—had taken the pains to repeat those experiments which Harvey performed more than two hundred and twenty years ago, and when in his seventy-fourth year, we should not have had the following statement at page 8 of his book: "I have conceived that the arteries contain air in an uncombined state, which may assist in keeping them distended, and in facilitating the circulation; but I have not been able to prove it." The fact that Harvey performed experiments in the way of injection may be unknown to many persons who are too well informed to conceive that the arteries may or can, compatibly with the carrying on of any circulation, contain air in an uncombined state; for these experiments are not to be found recorded either in the treatise *De Motu Cordis* or in either of the two letters to Riolanus, which two compositions were, in the older editions of Harvey's works, printed as three parts of a single treatise, under the names of *Exercitatio Anatomica Prima, Secunda, and Tertia*, and were, till the appearance of the College of Physicians' edition in 1766, the only published, as they are still the best known, records of Harvey's work and labour upon the circulation of the blood.

The experiments to which I refer are put upon record in a letter of Harvey's to P. M. Slegel, of date 1651 (see *Harveii Opera*, ed. 1766, p. 613; ed. Sydenham Society, p. 597). They were undertaken with the object of giving a final and happy despatch to all the quibbling objections of Riolanus, "omnes Riolorum circa hanc rem altercationes jugulare;" and they consisted, firstly, in forcing water from the cava into the right ventricle whilst the pulmonary artery, the "*vena arteriosa*" of those days, was ligatured—whereby Riolanus's suggestion as to the permeability or porosity of the interventricular septum was shown to be untenable; and, secondly, in forcing water from the pulmonary artery round into the opened left ventricle, whereby the lesser circulation was demonstrated, to use Harvey's own favourite word, *αυτοψία*; or, to use the very words employed by him upon this very occasion, by an "*experimentum φικτον* a me" (in his seventy-fourth year) "*nuper et collegis aliquot præsentibus exploratum*." Simple as this experiment may seem to us now, I do not think that any apology is required for the drawing of attention to it; for it is only twenty-eight years ago that Dr. Sharpey, so recently and so fitly decorated with our Baly Medal (see *Edinburgh Medical and Surgical Journal*, vol. lxiii, p. 20), had to perform the very closely similar experiment of injecting defibrinated blood into the thoracic aorta, with the very closely similar object of showing that the force of the heart was sufficient to account for the passage of blood through the intestinal and hepatic vascular systems—nay, to perform an all but identical experiment, adding on to it but the means for estimating and reproducing the force put out by the ventricle concerned. If such experiments as these were necessary in 1845, how much more necessary must have been the still simpler experiments of Harvey in 1651! At that time, the *prestige* of Riolanus the younger "pressed heavily upon mankind". Harvey himself had called that individual "*anatomicorum coryphæum*" in 1649; and, in the very year and letter we are dealing with, he calls him "*celebrem anatomicum*". And Pecquet, the discoverer of the thoracic duct, in his work, also of this selfsame year 1651, the *Experimenta Nova Anatomica*, a work spoken of by Haller (*Bibliotheca Anatomica*, i, p. 443) as "*nobile opus et inter præcipua sæculi decora*", has the following remarkable passage: "*Ita sentiunt non vulgaris peritiæ medici Harveius, Veslingius, Conringius, Bartholinus, alique complures; nec melior ipse Joannes Riolanus (quod mirari subito proxiimi viri, quâ in rebus anatomicis ceteros anteivit sagacitate). Audi hanc in rem illius sententiam*." This, I think, I will spare you; but I will remark that, after this singular—or perhaps, alas! not singular—instance of the blundering judgments which contemporary writers may pass upon each other, no young man, nor indeed any old one—for Harvey was in his seventy-fifth year when he first read Pecquet's work (see *Epistola Tertia*, Morison's, p. 620, ed. 1766; p. 604, ed. Sydenham Society)—if his own age, in his own estimation, do him scanty justice. Posterity ordinarily—I do not say always—rectifies these false judgments; it has done so, at all events, in the cases of the men so grotesquely grouped together by Pecquet. See also, I would add, Gregorius Horst, the father of Harvey's correspondent of the same name, in his *Opera Medica*, i, p. 83 (1661), where Riolanus is spoken of as "*anatomicorum hujus sæculi fere primum*"; and Bartholinus himself, who, in his work *De Lacteis Dubia* (1654), refers to "*multis Riolorum observationibus quibus rem anatomicam immortalis nominis celebritate auxit*". Haller, at all events, writing in 1774 (*Bibliotheca Anatomica*, i, p. 301), speaks of Riolanus as "*vir asper et in nuperos suosque coævus immitis ac nemini parcens, nimis avidus suarum laudum præco et se ipso fatente anatomicorum princeps*." The duty of attacking and abolishing such a man may, or, indeed, must have been, a disagreeable one to his contemporaries: they appear to have shirked it. It was their duty to have faced it, notwithstanding it might have been disagreeable.

Harvey used for these experiments a somewhat rough injecting apparatus, "*quemadmodum in clysteribus injiciendis fieri solet*." The modern experiment which I wish first to introduce to your attention rests for its accomplishment upon the employment of the delicate injection-syringe (for *Einstichung*) of Ludwig, and of the fine soluble Berlin blue for the substance to be injected. Here, as in many other instances, our superiority to our forefathers rests mainly or wholly

upon our possession of more delicate, or upon our command of more powerful, agents; and the delicate syringe and the penetrating soluble injection-mass help us to discoveries and demonstrations impossible in default of such means, just as the superior lenses of Malpighi and Leeuwenhoeck helped them to the discovery and demonstration of the capillary circulation, unknown to the discoverer of the circulation as a whole. The experiment to which I refer has its results fairly represented in the diagram to which I point, from a specimen prepared by myself at a class-demonstration. It gives a figure of the lacteals injected, by the means just specified, as they exist upon the terminal segment, here widely globular, of the ileum, upon a single disciform patch upon the commencing colon, and finally and chiefly upon and all around the walls of the colossal vermiform appendix of the Rabbit.

Fig. 1.—Vermiform Appendix and Cæcum with parts of Small Intestine and Colon of Rabbit (*Lepus cuniculus*). The Peyerian and also the mesenteric glands are seen to be injected.

In this latter place, it is but what the Germans call, and have called, a *Kinderspiel*, to insert the point of the fine *Einstichung* syringe, charged with the soluble blue injection, just beneath the peritoneal coat at the cæcal end or elsewhere, when, upon pressing the piston, a reticulation of blue will spread itself over the surface of the tube, inclosing as islands the solid substance of the Peyerian follicles. It needs but a little perseverance in the way of gentle pressure to cause superficial tubular lymphatics to arise into view, and to declare their true character by their contrast with and distinctness from the blood-vessels, as well as by their moniliform character, speaking of their richness in internally placed valves. Passing over the convex walls of the appendix, they join larger trunks which run along its mesenteric border; then larger trunks in their turn enter the mesenteric glands, and form in their substance reticulations strikingly like those formed previously in the walls of the intestine around the solid substance of the Peyerian follicle—speaking thus to the naked eye of the similarity, and by consequence of the homology, which the microscopic examination enables us to prove to exist between the lymph-masses and the solid masses which they surround in the Peyerian follicles and in the mesenteric glands respectively.*

* I take this opportunity of expressing my surprise that Henle has not seen his way towards accepting this view of the real nature or *Bedeutung* of the Peyerian follicles. In his *Gefässlehre* of 1868 (p. 404), he refers us back to his *Eingeweidelehre* of 1862, where the absorbent character of these structures is denied just as it was about the same date by Hyrtl in his *Handbuch der Topographischen Anatomie*, 1860, p. 646, and by Teichman, *Das Sanguadersystem*, 1861, p. 88-91. The view which I have adopted was accepted by a distinguished Fellow of this College, Dr. Burdon Sanderson, in the *Eleventh Report of the Medical Officer of the Privy Council for 1868*, p. 96.

It is the demonstration of the relation of the lymphatic or lacteal vessels, or sinuses, as the case may be, in different animals, to the solid ampulla-like masses in the Peyerian follicles, which the modern method of puncture can claim as being eminently its own attainment; for many years ago—in 1784, in fact—and three years before the appearance of Mascagni's splendid work with similar figures and histories of similar experiments (*Vasorum Lymphaticorum Historia et Iconographia*, 1787), the continuity of the lacteal radicles upon the walls of the intestine with the "lymph-paths", to borrow a word of later coinage, in the mesenteric islands, and finally, after passing through successive lines of these apparently solid structures with the thoracic duct itself, had been demonstrated by Sheldon, then Professor of Anatomy in our Royal Academy of Arts. These are his words (from page 49 of his work *Of the Absorbent System*, 1784), describing his plate No. 5, a copy of which I have had made and suspended there: "In the fifth plate of this work, upon a portion of human jejunum from an adult female subject, seventeen lacteal vessels are injected with quicksilver, by inserting pipes into them upon the intestine. They were remarkably large and varicose in this subject; and, as the quicksilver was poured into the lymphatic injecting-tube to fill these vessels, it frequently ran out in a full stream by the jugular vein which was opened. This circumstance rendered it evident that the mercury had passed through the whole course of the lacteals and thoracic duct, and had penetrated even into the venous system. It is, I believe, the only instance in which the thoracic duct has been injected from the lacteals on the intestines."*

Sheldon's first plate, I may add, when compared with his letterpress on p. 37, appears to show that what he calls "ampullulæ" were really Peyerian glands, and that he had repeatedly seen these glands distended in the way of natural injection with chyle, as it is easy enough to see them distended in an animal, such as a rat, which can be got to feed on fatty food, and can be killed at a proper interval of time afterwards. He appears to have had very serious as well as reasonable doubts as to the existence of any foramen in the apices of these ampullulæ; but the authority of Lieberkühn, whose *Dissertatio Anatomica* (p. 18) he had himself edited, appears to have weighed with him more than his *αὐτοψία*. Near, therefore, as Sheldon came to seeing the whole truth, he just failed of doing so entirely and completely; and the views which Lieberkühn had put forward (p. 19, *l. c.*) as to the great number of the Peyerian glands in the lower segment of the small intestines, being a proof that they held relation to secretion or excretion rather than to absorption, prevailed, and have prevailed, even into our own day. These are Lieberkühn's words: "Quare ad finem ilei plures quam in integro intestino positi erunt? Nonne propter feces jamjodium exsuccas et indurescentes ut lubricatæ valvulam facile transeant nec lædant?" In Henle's ordinarily and marvellously excellent *Generelle Anatomie*, of date 1841, I find (p. 895) the excretory character of the Peyerian follicles taken as something certain; the only thing left uncertain being the question as to whether their contents found their way into the cavity of the intestine by a constantly patent, however small, duct, or by dehiscence, as ova from an ovary. In 1850, the real meaning, the true physiological import, of these glands was proved by Brücke. The method of injection, of which I have spoken, enables us to demonstrate or exhibit what was thus proved, and that with the greatest ease. It is difficult to understand how any one can now doubt that the Peyerian glands are really but the *piloerhizæ* of the roots, the glands the tubera, and the thoracic duct the trunk or stem of the absorbent tree.

If any apology be needed for my dwelling so long upon a point of anatomy which has not merely much historical, but also much practical, interest—the Peyerian glands being the part of the organism especially affected by the poison of typhoid fever, which I see has, amongst other aliases, that of "Peyerian fever" (Walshe, *On Diseases of the Heart*, 3rd ed., p. 208)—I would add that I was till recently under the impression that the actual demonstration, the doing, of that which that diagram represents as done, might have been a fitting exhibition for me to go through upon the present occasion, following herein the example of Harvey, "*villiora animalia in scenam adducentis*". I have, however, learned that this very demonstration on the appendix vermiformis of the rabbit has been often performed in Germany, and, indeed, also in England; and I judged, consequently, that it might be superfluous, as it would not be novel, to exhibit it here and now.

* I have some pleasure in pointing out, by the aid of another diagram, taken from the plates of the venerable Professor Arnold, of date 1838, that the quicksilver injection could sometimes give as correct results as the "silver method" of modern microscopy for the detection of lymphatics by their epithelium. The diagram shows the fourth ventricle plexus without, the velum interpositum, on the contrary, with, lymphatics injected with quicksilver. The use of the silver method has enabled me to prove that this representation is correct: abundance of choroidal villi can be procured, and very beautiful objects they are when treated with 0.25 per cent. of nitrate of silver from the plexus in the fourth ventricle, but no lymphatic vessels. These can be shown from the velum interpositum by the use of the same reagent.

Having been thus disappointed in my intention of demonstrating something new in this direction, I cast about in another for something of the same character. And in the heart of a bird, the Australian Cassowary (*Casuarus Australis*), killed at Rockingham Bay, lat. 18 deg., on the east coast of the continent, and sent me by my former pupil, J. E. Davidson, Esq., I came upon a structure which I am well assured has never been either described or figured before. It possesses upon this ground some claim upon our attention; but it possesses stronger claims than any which mere rarity could give it, being a structure which, though it has never been seen in any other member of the class Aves, is largely developed, and, indeed, exactly reproduced in the hearts of certain mammals, and does not fail to be represented, at least rudimentarily, in our own. The structure in question is a "moderator" band, holding precisely the same relations to the other parts of the right ventricle in this bird which the band so named by Mr. T. W. King in the *Guy's Hospital Reports*, vol. ii, p. 122, 1837, holds in many, if not in all, Ungulate mammals. This, I presume, is made plain by a comparison of the two diagrams placed side by side, showing, one of them

us to speculate as to the nature of the secret bond which brings us into relations of affinity not only to the mammalian class, but with an older stock, the many-sided potentialities of which embraced not only mammals, but all warm-blooded animals, and not only all warm-blooded creatures, but cold-blooded animals and reptiles also? The valves of the heart in the higher Vertebrata, when regarded from this point of view of development, the safest if not the sole criterion of homology, may be spoken of as being but trabeculæ flaked off from the inner surface of the wall of a muscular sac, and subsequently made more or less membranous in the way of specialisation and its correlative economy. Thus as Gegenbaur (*Vergleichende Anatomie*, 2nd edition, p. 836) has

Fig. 2.—Heart of Sheep. The right ventricle is laid open. The letter M indicates the moderator band. A probe has been passed between the chordæ tendineæ of the two main segments of the tricuspid valve and the outer wall of the ventricle.

the heart of this bird, the other the heart of a Sheep, with the right ventricle similarly laid open in each case. The advantage, which in the struggle for existence, and specially in that very common phase of it which takes the form of a race for food or from an eater, which an animal with such a muscular band passing directly across the cavity of its right ventricle from its fixed to its movable wall must possess, is not a difficult thing for any man to understand who has ever either watched in another or experienced in himself the distress caused by the over-distension of any muscular sac. A band of similar function—I do not say definitely of precisely the same morphological importance—has often been figured as existing in the hearts of most or all Reptilia below Crocodilina; and it serves in them to close up and expel the blood from the pulmonary compartment of their imperfectly divided ventricle.

Such being the function of this moderator band, what is its morphological bearing, and what traces can we find of it in ourselves, tempting

Fig. 3.—Heart of Australian Cassowary (*Casuarus Australis*). shewing M, the moderator band.

remarked, the intervalvular space in these animals corresponds to the entire cavity of the spongy walled heart of Fishes and Amphibia; and the sinuous intertrabecular cavities in the spongy walls of these latter animals correspond with the chief part—viz., the extravalvular part of the ventricular space—in Mammals, Birds, and Crocodilina. Now, the musculi papillares represent the disposal or destination of the innermost layer of the right ventricle, according to Dr. Pettigrew (see his paper, *Phil. Trans.*, 1864, p. 479); and I would submit that the moderator band is but a specialisation of the next layer in order from within outwards—to wit, Dr. Pettigrew's sixth layer, which he has figured (pl. xiv, fig. 33) as proceeding in a spiral direction from right to left, much as the fibres of the moderator bands I have figured do. A study of the heart of the Rabbit will put this matter in a very clear light, and further open our eyes to see and recognise the rudimentary representation of this moderator band in our own hearts. If we look at the outer aspect of that very constant musculus papillaris, which passes in Man from the outer and movable wall of the right ventricle to distribute its chordæ tendineæ to the two more anteriorly placed of the three segments of its auricular valve, we shall frequently see that its longitudinal fibres are crossed nearly or quite at right angles by a slender fibrous band, so that we have before us an appearance not

wholly nor essentially unlike that presented by the striæ longitudinales of Lancisi and the fibres of the corpus callosum when viewed in mutual connexion. This band of fibres can sometimes be traced up towards the conus arteriosus, and be seen not to die away until close upon the point of origin of the most anteriorly or upwardly placed chorda tendinea arising from the septum to pass to the hindermost of the three

Fig. 4.—Human Heart. The right ventricle has its cavity exposed. The moderator band M is seen to pass up from the base of a musculus papillaris arising from the outer or movable wall of the ventricle towards the conus arteriosus. Close to this, its upper end, a chorda tendinea arises, and passes up to the most posteriorly placed of the three cusps of the tricuspid valve.

segments of the tricuspid. The points between which this line of fibres lies may be observed to be the very same as those between which the moderator bands in the Cassowary and the Sheep stretch as free columns in the diagrams before you. It is not altogether rare to see this band raise itself from the position of fusion, like the ventricular wall, and assume the character of a cylindrical band for a lesser distance, but with no less distinctness as a column, than in the Ungulata. Such a case I had actually before me whilst writing this, and you have it now figured before you.

Every gradation, in fact, exists between the entire obsolescence of the moderator band, which we sometimes see in the human heart, through the typical, and, I should anticipate, constant, but not functionally important, representation of it in the Rabbit, up to the important and structurally prominent development attained to by it in the Ungulate Mammal, and this solitary instance for the class of Birds, and the subclass with such generalised affinities, of Struthionies.

And speaking of the method of gradations, I take this opportunity of saying that its application in the case of the muscular right auriculo-ventricular valve of Birds will, in my judgment, put an end to the disputes which have taken place as to its homology with one or other of the two valves in the Crocodiles. The two portions of the valve in the Casuarius Australis are so nearly equal—the larger being but 1.7 inch and the smaller 1.4 inch—as to do away with the difficulty which might be felt in holding that both Crocodilian valves are represented here. There are other reasons for this view, which I reserve for another occasion. But, whilst speaking of the heart of the Bird, I cannot forbear pointing out how the structural arrangements of its auricle, dif-

fering as they do strikingly from those of the same compartment in the mammalian heart, help us by that contrast to get a true idea of the working of this latter. Firstly, the walls of the Bird's right auricle are relatively thicker, not only as compared with the walls of its own ventricle, but also as compared with the walls of the corresponding auricle in the Mammal, the muscoli pectinati standing out in as sharp relief as the similarly working muscular ridges in a hypertrophied bladder, and inclosing anfractuosities and recesses almost as deep. But, secondly, and what is of more importance, the Bird's auricle is furnished with a large and functionally active valve, protecting the entrance of the great veins, and preventing regurgitation into those vessels just as the auriculo-ventricular valves prevent regurgitation from the ventricles. It is fair to argue *à priori* that, if the mammalian auricle had counted for as much in the action of the heart as the Bird's, its force would have been economised by the placing of a large and functionally useful valve in the site of the rudimentary Eustachian—a structure altogether absent in many mammals, and variable, as rudimentary structures very often are, in ourselves. The *à priori* argument of comparative anatomy is abundantly borne out by the appeal to experiment. Marey, in his *Physiologie Médicale de la Circulation du Sang*, 1863, whilst referring (p. 36) to other evidence from comparative anatomy than that which I have adduced, cites, in support of the view that the auricle has but an accessory and subordinate rôle in the functions of the heart, an experiment of Chauveau's, in which the auricle of a horse, being exposed and irritated, lost its contractile power for a time, during which, nevertheless, the ventricles continued to contract and the circulation to be maintained. Colin, again (*Traité de la Physiologie Comparée*, vol. ii, p. 257, 1856), found that the left ventricle continued to be filled with blood, even when the corresponding auricle was prevented from contracting by the insertion into it of a finger. And further, Magendie had long ago noted, in experimentation, what many here present may have noted in pathological or clinical observation—viz., that the auricles may remain extremely distended for hours, and, like other muscular sacs similarly conditioned, unable to contract and empty themselves, without the circulation for all that being brought to a standstill. It was Dr. Pavy's paper treating, in the *Medical Times and Gazette* of November 21st, 1857, of the case of a man (E. Groux) with a congenital fissure of the sternum, which first drew my attention to these points; and his summary of what takes place in the dog is so clear that I herewith reproduce it.

"In the dog, the contraction of the ventricles is sharp and rapid, instead of prolonged, as in the reptile, and does not appear to occupy nearly so much time as half the period of the heart's action. The ventricular contraction communicates a sudden impulse to the auricles, occasioning in them a distinct pulsation, which is instantly followed by a peculiar thrill, wave, or vermicular movement, running through the auricular parietes down towards the ventricle. This thrill or wave is coincident with the passage of the blood from the auricle into the ventricle, and takes place so instantaneously after the ventricular contraction, that the one movement appears to run on to continue itself into the other. There is then a pause, which seems comparatively of considerable duration, and which is succeeded by a recommencement of the heart's action, beginning with the ventricular contraction."

Dr. Pavy has very kindly gone to the trouble of repeating the experiment upon which these statements are based; and from a letter with which he has favoured me, I gather that the auricular contraction detectable by the cardiographic tracing immediately preceding the ventricular contraction, is also detectable, of course during the pause just mentioned, by the eye, unassisted by the cardiograph, and turned simply upon the exposed heart, in which the auricular appendix is seen to become redder or more flesh-coloured at the moment in question (see Walshe, *l. c.*, p. 65). And he further remarks that this auricular contraction, difficult though it be to be observed under physiological conditions, may be exaggerated into considerable prominence in disease entailing contraction of the auriculo-ventricular orifices, and may then make itself known by a præ systolic murmur.

I should now be glad to draw attention shortly to a few memoirs which have appeared comparatively recently, and which treated of matters of considerable interest, not merely as scientific problems, but also as practical questions. First among these, I would name the paper which appears in the third volume of Professor Ludwig's *Arbeiten*, 1868 (having previously appeared in vol. xx of *Bericht Math.-Phys.-Klass. K. S. Gesellsch. Wissensch.*, Leipzig), by Professor Ludwig himself and Dr. Dogel. In this paper, we have a number of experiments recorded as performed with the hearts of dogs removed from the body, and as nearly as possible emptied of blood; and the conclusion to which the authors come is, that the heart of the dog, when removed from the body and emptied of blood, still produces a sound during the systole of the ventricles, which is not essentially different from that which is recognised as the normal first sound of the heart. The authors add, how-

ever (p. 85), that they do not think these experiments entirely exclude the possibility of the tension of the auriculo-ventricular valves entering as a factor into the production of the first sound; and hereby they would be guarded from coming into contradiction with most English authorities—as, for example, Dr. Walshe (*Diseases of the Heart*, 3rd ed. 1862, p. 62). Dr. Guttman, however, in a paper of no great length, but of considerable merit, published subsequently to the one just mentioned, and in Virchow's *Archiv* for 1869, points out with much acuteness what, when once pointed out, is ever thereafter obvious—viz., that it is, in the nature of things, impossible, with all possible precautions in the way of emptying the heart of blood, to empty the complex phenomenon made up by a systole of the heart of the condition of tension of the auriculo-ventricular valves. Surely the musculi papillares will contract with the rest of the ventricular walls, and, contracting, will they not stretch the chordæ tendinæ and the valves? For myself, I would say that we are more likely to overrate the share taken by the valves than to underrate that taken by the muscular walls. I need not say to this audience that the fact with which we are all familiar, of the alteration in the first sound produced by disease of the auriculo-ventricular valves, does not absolutely prove that they produce any part of it during health; and, finally, to my own ear at least, a modification of Wollaston's experiments, which anybody can try for himself by making his temporal and masseter muscles contract at any time of perfect stillness, appears to produce a sound which is scarcely, if at all, different in quality from the first sound of the heart. A judgment, however, upon the nature of a sound, or, indeed, an aggregation of sounds, as in music, is one upon which two observers may very well differ, as neither of them can lay his proof of supposed identity or difference alongside of that which the other may possess, or suppose he does.

It is with much pleasure that I refer to Dr. Rutherford's paper on "The Influence of the Vagus on the Vascular System", which appears in the *Edinburgh Royal Society Transactions* for 1870, vol. xxvi. In that year, having to deliver an address to the Biological Section of the British Association at Liverpool, I made bold to say that the results to which Dr. Rutherford had come, and which were then only known to me in an abstract in the Cambridge and Edinburgh *Journal of Anatomy and Physiology* (May 1869, p. 402), would prove to be of the highest value and importance. His memoir now published *in extenso*, and extending over forty-two pages, as fully justifies my prediction as it will fully repay anyone who will take the pleasant trouble of reading it. The most important result in a practical point of view is the demonstration which Dr. Rutherford has given of the nerve-circle, whereby, in the way of reflex action, the all important secretion of gastric juice is called forth. The sensory impulse caused by the ingestion of food into the stomach, is propagated upwards by the vagi to the medulla oblongata, where it throws into abeyance the vaso-motor nerve-cells, which, whilst the stomach is empty, keep the blood-vessels of the gastric mucous membrane constricted, but which, when their activity is inhibited, allow the zonular fibre-cells of these blood-vessels to dilate, and allow the increased afflux of blood thus called for. That relief will result to some of the countless martyrs to dyspepsia out of the demonstration of this physiological relation of vagus, sympathetic, and peptic glands, I do not doubt. Possibly, I would add, Owsjannikow's observations as to the working of hydrate of chloral as a depressor of arterial tension (Ludwig's *Arbeiten*, 1872, p. 32) may prove valuable to persons engaged in practice, by pointing out, in however shadowy a fashion, the road to a more rational and systematised, even if less general, use of this drug than that which I am told is now made of it. It may seem a paradox, but it is none the less true for all that, to say that, for the activity of many organs, a paralyzing and inactivity of certain nerve-centres in connection with them is a prerequisite. The activity of such, indeed of most, organs is but intermittent and occasional, being but intermittently and occasionally called for, whilst the constringing activity of the sympathetic has to be constantly at work to prevent waste of force; the phenomenon of the distinction of the corpora cavernosa, a phenomenon used by Harvey himself in the way of illustration (of p. 129 of the *Epistola secunda ad Riolanum*), I may adduce in the way of illustration also, being, as it is, dependent upon a similar nervous mechanism.

Owsjannikow's paper (also to be found in Ludwig's *Arbeiten*, 6th year, 1871, and in the *Bericht Math.-Phys.-Klass. K. S. Gesellsch. Wissensch.*, Leipzig) just referred to, and published two years subsequently to Dr. Rutherford's, gives, as the result of a number of experiments performed in Professor Ludwig's laboratory at Leipzig on rabbits, and independently at St. Petersburg on cats, the conclusion that the ganglionic centres of innervation for the entire sympathetic system occupy but a small space at the base of the brain, two strips, to wit, one on each side of the median fissure in the floor of the fourth ventricle; of, in the rabbit, a length of about four millimeters, beginning about four or five millimeters anteriorly to the calamus scriptorius, and ending

about one or two millimeters behind the level of the corpora quadrigemina. The title of such a book as Eulenberg and Guttman's *Die Pathologie des Sympathicus auf Physiologische Grundlage* (Berlin, 1873), is an encouragement to those who hope to see fruit arise from such researches as these in the way of additions to our means for meeting, or at least understanding, human disease and suffering.

It has long been known (Budge, 1855) that the sympathetic nerves which supply the vessels of the head and iris do not pass directly or by the shortest possible route to this their distribution, but pass down the spinal cord for a greater or lesser distance, and then turn outwards, and pass from the anterior nerve-branches to bend upwards, much as the recurrent laryngeal nerve does. That other vascular regions receive their vaso-motor supply by this apparently circuitous route and, till the history of development is taken into consideration, paradoxical route, is from time to time being demonstrated. Dr. Pavy, to whom I have already referred, many years ago identified and mapped out one segment of the road along which nerve-force passes to the liver, and prevents or allows the occurrence of diabetes. Further exploration of this route we owe to Cyon (*Bulletin de l'Académie Impériale des Sciences de St. Petersburg*), cited in the *BRITISH MEDICAL JOURNAL*, December 23rd, 1871; and this same investigator, working still in the same line of investigation, as it is in these days usually necessary for an investigator to work if he will make himself a name as a discoverer, has also shown us (Ludwig's *Arbeiten*, 3rd year, 1868) the track along which the vaso-motor nerves of the anterior limbs pass, proving that these nerves pass down in the spinal cord as low as the mid-dorsal region, before leaving it to turn upwards in the sympathetic chain to join the brachial plexus. Of all the results, however, which have been attained to in the line of experimentation now under consideration, those come to by Brown-Séquard and demonstrated by him at the meeting of the British Association held at Liverpool in 1870, and subsequently published in the *Lancet* of January 7th, 1871, seem to me to be certainly the most striking and possibly the most important. Could anything have been more surprising to him whose memory we here this day commemorate, than to have been told that an injury to a particular part of the brain, the pons, called after the excellent anatomist whose life ended in the very year in which his had begun, would produce hæmorrhage in certain parts of the lungs, and anæmia, œdema, and emphysema in others? This is an easy experiment to repeat; it is one which might have been done in the days of Harvey as easily as in those of Bernard, of Budge, of Ludwig, and of Brown-Séquard. But easy though it would have been to perform, I am bold to say it was well for Harvey that he never happened to perform it. For considering that, like Haller, he knew nothing of the contractility of arteries; considering that Hunter had not performed his now well-known experiments with the umbilical arteries; considering, sir, that in that excellent work on *Physiology*, the translation of which in 1838, by our late and never sufficiently to be lamented friend Dr. Baly, we owe to your suggestion, I find several pages (p. 202-206) devoted to disproving the muscular contractility of arteries; considering, that it was not till three years later, in 1841, that Henle's work, already referred to, appeared with its still unsuperseded figures, plate iii, figures 8, 9, and 10 of the arteries, with their circular muscular coat, and with its excellent summary in letterpress of the whole subject, pp. 518-526, and especially pp. 524, 525; when I consider that nothing of all this had been done, to say nothing of other advances connected with names of men yet living to speak for themselves and for us—I say it may have been well that Harvey never came upon the facts relating to the alterations of lung-substance being entailed by destruction of brain-substance, not difficult to be observed and reproduced, which we owe to Brown-Séquard. For if he had come upon them, how could he have explained them in the absence of the entire chain of connecting facts, in the forging of which chain so many successive workers—Purkinje, Valentin, Weber, Burdach, Stilling, and others—have all contributed links? Might not even Harvey, often as he withstood such temptations, have, nevertheless, in default of power to assign the real causes of such a phenomenon, been driven back upon some of those explanations which he himself forcibly denounces in the words (*Epistola secunda ad Riolanum*, p. 116), "Vulgo sciolium causas assignare haud norunt dicunt statim a spiritibus hoc fieri et omnium opifices spiritus introducant, et ut mali poetæ ad fabulæ explicationem et catastrophem ἔθεν ἀπὸ μηχανῆς advocant in scenam." It is a hard thing for any man to abstain from speculating as to the cause of any well established phenomenon, especially if it be of striking interest and importance; it is a hard thing for any man to do more than keep pace with his own generation; and those who have spent any time in reading the works of Harvey's contemporaries, will best appreciate the difficulty he must have had in setting himself free from the influence of the *idola theatri* referred to.

[To be concluded.]