I do not know whether I ought to attribute Dr. Paton's line of argument to a want of acquaintance with an elementary principle taught to every student when he is instructed to pay attention to the amount of urine voided in estimating the quantity of urea excreted, or whether this is another error from inadvertence, or whatever else it may be called, to be added to the list already given in my *Epicriticism*.

To argue against the relationship of sugar in the urine to that in the blood is to argue against that which is self-evident when in looking at the matter we take into account the unquestionable relationship existing in diabetes between the food taken and the amount of sugar passing off with the urine. Everyone knows that according to the food so is the elimination of sugar in diabetes. How can the carbohydrate ingested reach the urine except through the blood? If the relationship exists in the one case, it follows as a logical sequence that it must also exist in the other. I said upon this point in my *Epicriticism* (p. 117):

In addition to evidence of this kind, all experience in connection with diabetes conspires to show that the urine is most sensitive to, and speedily and proportionately influenced by, the ingestion of carbohydrate matter. For the urine thus to become influenced it follows either that the blood must become correspondingly so, or that the sugar must reach the urine in some mystic way without undergoing intermediary transit.

With the utmost confidence I reassert the proposition that according to the sugar encountered in the urine so has sugar pre-existed in the blood; and, conversely, in proportion as sugar is present in the blood so will it reveal itself in the urine, and this alike for the small quantities belonging to health and the larger quantities belonging to diabetes.

I do then in the most distinct manner affirm, and I am prepared to sustain the affirmation, that, in the words used by Dr. Paton, "whenever the amount of the sugar in the blood increases, a proportionate increase in the urine takes place." I further say that what is wanted to keep sugar, derivable from our food, out of the urine, is to keep it out of the blood, and that if the facts stood as they are assumed to stand under the glycogenic doctrine, the result would be universal diabetes. The difference between health and diabetes, in reality, is that in the one case the carbohydrate of our food is disposed of in such a manner as to prevent its reaching the general circulation in the form of sugar, whilst in the other the power of accomplishing this, in other words what may be spoken of as the assimilative power, is at fault. In proportion as it is at fault, so will the carbohydrate ingested be permitted to reach the general circulation as sugar, and thence the urine.

The fact that the experiments on the injection of sugar into rabbits showed that the sugar underwent modification, whilst within the system, does not affect the fact of elimination after introduction. The experiments simply show that in the healthy system there exists a power tending to convert carbohydrate matter into a lower cupric-oxide-reducing form. As I have already made known, I have found in some cases of diabetes that the sugar encountered in the urine has possessed a lower cupric-oxide-reducing power than that of glucose.

(To be continued.)

THE TREATMENT OF AORTIC VALVULAR DISEASE.¹

By ALEXANDER MORISON, M.D. EDIN., F.R.C.P. EDIN.,
Physician to Out-patients, Great Northern Central Hospital;
Physician to the St. Marylebone General Dispensary.

THE treatment of aortic valvular disease recurs from time to time as a subject of somewhat uncertain cogitation in medical literature. So long ago as 1829² Dr. Hodgkin, writing on Retroversion of the Aortic Valves, inferred from his experience that depletion in these cases, whether by blood letting or by other means, among which he seems to have included digitalis, was unsuitable for these cases. Sir Dominic (then Dr.) Corrigan, in his classical paper on Permanent Patency of the Mouth of the Aorta, or Inadequacy of the Aortic Valves,³ contrasts aortic with mitral lesions, and speaks very strongly of the dangers of digitalis in the treatment of such cases, remarking that cases injured by its use

frequently recover on abandoning the drug and giving stimulants. Professor William Henderson, of Edinburgh, came to the same conclusion with respect to the use of digitalis in these cases, and agreed with Dr. Corrigan as to the cause of the unsuitability of the drug, namely, the prolongation of the diastolic interval. Dr. Easton, of Glasgow, to whose views Dr. G. W. Balfour, as recently as 1892,⁴ refers approvingly, contested Professor Henderson's views and endeavoured to maintain the opposite thesis, namely, that the longer the diastolic interval the less the opportunity for regurgitation. He appears to have ascribed aortic regurgitation to "contractions of the aorta." On this supposition he seems to have concluded that the more frequent such contractions were the greater was the regurgitation, and *vice versa*. Many subsequent observers whom it is not possible to enumerate on this occasion have either condemned or spoken hesitatingly of the benefit to be derived from the use of the digitalis group of remedies in aortic incompetency. Others again—a smaller body at present—have spoken with an emphasis in the inverse ratio to their number, as is the manner of minorities, of the unquestionable good to be derived from these remedies in these cases. Dr. Barrs, in a paper read before the Therapeutical Section of this Association so recently as 1891, maintained that there was no essential difference in the kind of failure in aortic and mitral disease, very justly held that digitalis could not be viewed as acting in one way in one case and in another way in another, and concluded that that drug was a valuable remedy in aortic valvular disease. It is almost too well known to be mentioned that the weight of Dr. G. W. Balfour's esteemed authority is cast in the same scale, but, as he very well observes,⁵ this is a question rather of "physics" than of "statistics." If, therefore, there be a difference in our conceptions of the physics involved, there will of necessity be a difference also in our interpretation of phenomena and in our views of treatment. That the mechanism both of failure and of relief differs in mitral and in aortic disease I shall endeavour with necessary brevity to maintain, and to explain, I hope satisfactorily, among other points the cause of the reliability of the digitalis group of remedies in mitral disease, and their still questionable reputation in the treatment of aortic, and especially of aortic regurgitant, lesions with lost or unattained compensation.

In the experimental investigation of the physics of the circulation, it is impossible to maintain in their entirety the conditions existent in the normal circulation, and this fact has an influence upon phenomena and upon deduction from an observation of them. Dr. Halford pointed out in 1860 (The Actions and Sounds of the Heart) that removal of the pericardium and evacuation of its contained fluid rendered tumultuous the beating of the heart which was acting smoothly and systematically, even after the chest was opened. From the days of Stephen Hales until now, the relations between artery and manometer have not been such as obtain in Nature, and the argument from quadrupeds to man as to the power of the heart and pressure in the blood vessels are fallacious, for those physical reasons which bring about a difference in the circulatory apparatus of animals habitually and respectively horizontal and erect. The latter is a point which I considered in some details in 1878, and Mr. Hill has recently read an interesting paper on the same subject before the Royal Society. Much more fallacious is it to assume that an experiment such as that by which Pascal burst a barrel is exactly applicable to the forces which tend to dilate the cardiac ventricle. It is in no spirit depreciatory of experiment that I make these remarks, but merely to emphasise the admitted fact that conclusions based on all such experiments have only an approximate value. It is true as Dr. G. W. Balfour and others have remarked⁶ that there is neither air nor a vacuum within the vascular system, but, in considering problems affecting blood pressure in the heart and blood vessels, sufficient emphasis has not, in my opinion, been laid upon the influence of respiration, and the consequence of the importation into the circulation of a greatly enhanced aspirative force after the commencement of aerial life. Not that the subject has been overlooked by physi-

¹ Read in the Section of Medicine at the Annual Meeting of the British Medical Association held in London, July-August, 1895.

² London Medical Gazette, vol. iii, p. 442.

³ Edin. Med. and Surg. Journ., April 1st, 1832.

⁴ BRITISH MEDICAL JOURNAL.

⁵ Diseases of the Heart and Aorta, 1st edition.

⁶ BRITISH MEDICAL JOURNAL, 1892, vol. i, p. 1132.

ologists. Marey, Foster, and others, have dealt with the subject, and more recently Germe⁷ has studied the matter in considerable detail. To myself it has appeared a question of the first importance in this connection for many years, and I have from time expressed my sense of its importance.

In the foetus the chief supply of blood to the heart is from the higher level of the umbilical vein on hydraulic principles, for even in placenta prævia the maternal source is always higher than the circulatory apparatus of the secondary organism. The chief propelling force is, at this time, the right ventricle which lies in the course of the principal blood current. The aspirative forces in the foetus are those of its heart and arteries and the respiratory and vascular movements of the mother. The blood supply from the umbilical vein being constant and sufficient, little effect is then produced upon that mere diverticulum of the circulatory apparatus under the circumstances—the pulmonary circuit. Hence the quiescence of the latter *in utero*. When respiration is established and the circulation through the cord arrested, a powerful and rhythmical aspirative force begins to exert itself upon a limited quantity of blood in a perfectly closed circuit. While, therefore, there is, as Dr. Balfour and others have said, neither air nor a vacuum in the circulation, there is both air and a vacuum around the circulatory apparatus, capable of very materially modifying conditions of pressure within it. These facts may be easily demonstrated on the following scheme:

Fig. 1.

Fig. 2.

(a) is a manometer containing mercury, (b) a reservoir of water, (c) a closed tin box with three outlets for connection with the tubing f, g, h, (d) a bent glass tube, (e) a bottle capable of having the contained air rarefied by aspiration, and connected with (d) by the tubing (j).

The manometer and tubing represent the pulmonary circulation in the foetus. So long as the supply from (b) is constant, the aspirative effect of (e) has no effect on the manometer. The moment, however, the current in (g) is arrested, mercury falls in (a), and the direction of the current is towards the aspirator. The force of gravitation is thus modified in any section of the aerial circulation by the imported

and important force of aspiration. The argument, therefore, from the general laws of hydraulics and gravitation to these as expressed in the normal circulation of the blood is fallacious. Pascal's law of the equal pressure of fluids in all directions was deduced from observations on non-current fluids. The pressure of fluid in a river must at any point be greater on the up-hill side of that section, owing to the weight of a column of fluid being added to that of the atmosphere. Resistance is, therefore, least on the down-hill side, hence its flow. Were it possible to aspirate the river up-hill, pressure would be greatest on the down-hill side, and the river, against the force of gravitation, would go up-hill, that is to say, in the direction of that surface which experienced least resistance. Pascal's law, therefore, is not applicable, without important reservations, to the blood current in the ascending aorta and cervical vessels.

While the experimental study of the influence of respiration upon the circulation of the blood is open to the same objections of artificiality as direct investigations of blood pressure, physiologists are agreed that thoracic aspiration has an indubitable influence in attracting blood to the centre in the thorax, and from the head and neck.

It may be shown by a very simple experiment that the effect of inspiration is to attract blood towards the heart from the great vessels, venous and arterial, and that of expiration to drive blood from the heart into the vascular system. The former is centripetal, and the latter centrifugal in its tendency.

If a piece of rubber tubing, closed at its upper end and interrupted by glass joints, be placed in a bottle containing some water and the bottle be aspirated, air will escape from the end of the tubing under water. On allowing air to enter the bottle water will rise to a certain height in the tubing above the level of the water. If now the bottle be again aspirated, water in the tubing will fall towards the common level of the water in the bottle, and again rise in the tubing when the air pressure in the bottle is increased by readmitting air. The exhaustion of the air in the bottle by increasing the vacuum acts as inspiration does within the chest, and the readmission of air by increasing pressure within the bottle acts as expiration does by diminishing the thoracic vacuum. Germe⁸ has shown that the lung removed from the body and placed *in vacuo* with adjustments to its vascular inlets and outlets, and also when directly inflated *per tracheam* attracts fluids by way of the pulmonary arteries, and allows them to escape by way of the pulmonary veins. He admits that the inflow is greatly in excess of the outflow, as one would expect in the absence of the absorptive force of the heart and systemic arteries. A sphygmographic study of this question teaches the same lesson. Towards the end of

Fig. 3.—a, Normal sphygmogram of T. V. N.; b, the same in forced respiration with unobstructed nostrils.

inspiration the pulse wave is diminished in size, and the predicrotic evidence of increased arterial resistance is clear, while at the commencement of expiration the size of the wave is increased and the predicrotism falls, showing the lessened arterial resistance and greater ease of propulsion into the systemic vessels.

In addition to these forces there is another consideration

⁷ Marey. *op. cit.*

⁸ *Op. cit.*

¹ *Recherches sur les Loix de la Circ. Pulmonaire*, 1895.

which may, I think, be regarded as influencing the hydraulics of the cervical arterial column, namely, the physics of the intracranial circulation. The so-called "Edinburgh dogma," while incapable of being defended in all its details as originally promulgated by the second Monro and Dr. Kellie, of Leith, appears to me to be a valid factor in the problem of an easy maintenance of the cervical arterial column, and further modifies in this section of the circulation the operation of Pascal's law. The vault of the cranium must have some influence in diminishing atmospheric pressure on the cerebral vessels, and probably also prevents the collapse of the venous section of the intracranial circulation under the aspirative force of the thorax, a fact which is, moreover, argued by the anatomical characters of the bone-protected intracranial venous sinuses. The facts which appear to me to support this contention are read by Dr. Macewen, of Glasgow, in an exactly opposite sense.¹⁰ I am not alone in differing from him.

These considerations point, in my opinion, to the conclusion that there are other forces and conditions besides propulsion by the heart which tend to maintain the circulation in various positions of the body. It is interesting to note in this connection that Mr. Hill¹¹ found that the vasomotor regulation of the circulation in different positions of the body was greater in erect and semi-erect animals than in those habitually horizontal. In the latter, as I pointed out in 1878,¹² the circulatory apparatus has adapted itself by certain characteristic features of a more permanent nature, to the persistent difficulty of propelling a preponderantly horizontal circulation.

According to these observations, therefore, the chief dilating force acting upon the left ventricle in aortic valvular incompetency is not the pressure of the cervico-cerebral blood column acting upon the static principles of Pascal's law, but the active aspirative force of the left ventricle itself, added to the generally increased difficulty in propelling the systemic arterial circulation. This general difficulty is greater in the recumbent than in the erect position. Compelling a patient, therefore, who is suffering from aortic valvular incompetency to lie down, when Nature prompts him to sit up, is not only unnecessary but contrary to the posture indicated by the physical conditions involved, and therefore unscientific.

The orthopneic position assumed in cardiac dyspnoea produces comparative thoracic comfort, not only by aerating the blood as some have contended,¹³ but also, and I believe chiefly, by increasing the respiratory aid afforded to the circulation by exaggerated breathing. While the erect or partially erect position assumed by cardiac patients in orthopnoea tends to promote the systemic arterial circulation, it at the same time tends to limit the inflow from the inferior caval system which represents in the adult the umbilical inflow in the fetus. By thus limiting the inflow cardiac aspiration is enabled to act with greater advantage, as may be shown by experiments with the schema already referred to.

The effect of digitalis upon the heart and blood vessels is tonic not clonic, continuous not intermittent. Its ultimate phase is tonic systolic contraction too powerful for the inherent resiliency of the circulatory apparatus to overcome, and its injurious influence commences when this resiliency is impaired. Cardiac systole is a rhythmical and active event; cardiac diastole is a rhythmical and passive event, but dependent upon and a consequence of systole. The tonic effect of digitalis is continuous and passive, and aids systole, not by increasing rhythmical contraction, but by controlling dilatation and allowing the active force of rhythmical systole to deal with and project a more convenient volume of blood. Such a tonic of cardiac wall, however, even when associated with some diminution of the cardiac chamber, involves increased aspirative power by that chamber. This may be shown experimentally. If two india-rubber balls be taken, the one smaller and more elastic than the other, it will be found that the former rebounds when dropped higher than the latter, and that if a nozzle and tubing be attached to it it also has a greater aspirative power. Digitalis, there-

fore, acting passively and producing the smaller and more elastic cavity in the heart and vessels, aids the circulation of the blood, in my opinion, not so much by increasing the contractile power of the heart as by increasing its resiliency, not so much, that is, by strengthening systole as by increasing the absorptive power of cardiac diastole and vascular elasticity generally. That the cardiac ventricle possesses this power is now acknowledged by most physiologists, is an ancient doctrine long ignored, and was established afresh by different methods in 1878 by Golz and Gaule and by myself.¹⁴

Digitalis, therefore, is on this showing of most use in those affections of the heart in which the aspirative power of left ventricular diastole is advantageous. This power is not advantageous, for the reasons already given, in aortic valvular incompetency, although the absorptive power of the systemic arteries is of use in that and all other forms of valvular disease. Experience, therefore, has caused physicians, as a body, to use with less confidence than in mitral disease digitalis and the digitalis group of agents in cases of aortic regurgitation. When the drug acts favourably in these cases it does so, in my belief, chiefly by increasing the aspirative power of the systemic arteries, a view which is strengthened by an observation of Dr. W. G. Balfour, one of the chief advocates of the use of digitalis in aortic incompetency,¹⁵ which causes him to remark, that the drug is unsuitable for such cases of this lesion as are associated with sclerosis of the systemic arteries. He maintains, also, that digitalis, by diminishing the capacity of the left ventricle, benefits those cases, and he and other advocates of the drug ascribe its failure in other hands to timidity in its use. But that degree of persistent tonic power which will safely reduce the cavity of the ventricle, must, it has been shown, increase also its aspirative power, and, if pushed to such a degree as relatively to diminish this power, must at the same time, by excess of tonic power, lessen the aspirative power of the systemic arterial system. Now, thus notably and quickly to diminish the capacity of the left ventricle involves danger, and requires a constant supervision and skill not always available. It has further been maintained by its advocates that retardation of the heart does not favour regurgitation into the ventricle, but a study of bradycardia in a young subject with elastic vessels seems to show that this view is not physiologically correct. The tracing of such a pulse shows that the site of dicrotism may be steadily marked by three elevations of constant size at the site of normal dicrotism as in the example I now show you. (Fig. 4.) If this does not argue a greater possibility

Fig. 4.—a, Post-pneumonic bradycardial tracing in a man, aged 18 years; b, the same ten days later when bradycardia had passed off.

of regurgitation from increased oscillation, I know not how otherwise to interpret the phenomena.

For all these reasons, the use of digitalis and the digitalis group in the treatment of aortic incompetency seems to me to require greater care in its use than in mitral disease. The "physics" of the circulation appear to me to support this view, and the "statistics" occasionally published, with however great authority, have not hitherto been sufficiently convincing to overcome the general distrust of the drug in these cases. A much abused and frequently unsound maxim seems to me, under these circumstances, to find application, namely: "*Securus judicat orbis terrarum*."

The use of digitalis is less objectionable when the aortic lesion is obstructive, because the influence of ventricular

¹⁰ *Pyogenic Infectious Diseases of the Brain and Spinal Cord*, p. 33, et seq.

¹¹ *Lancet*, February 9th, 1895, and elsewhere.

¹² *Dextral Valvular Disease*, p. 26, et seq.

¹³ Balfour, op. cit.

¹⁴ *Pflüger's Archiv f. Physiolog.*, 1878; *Dextral Valvular Disease*, p. 36.

¹⁵ *BRITISH MEDICAL JOURNAL*, 1892, vol. i, p. 1182.

aspiration on the aortic blood is checked by more or less perfect valves, while the increased tone of the arterial system brought about by the drug promotes the resiliency and aspirative power of the latter. What, then, is the rational treatment of aortic incompetency?

Marey and Chauveau demonstrated by experiment on the horse that destruction of the aortic sigmoid valves caused a rise in intraventricular pressure due to the reflux of blood, and that this was associated with increased frequency of the heart's action.¹⁶ Ottomar Rosenbach¹⁷ also showed that no artificial destruction of valves, or even the introduction into the heart of various obstacles, materially affected the blood pressure in the arteries. Marey's and Rosenbach's observations are not incompatible. The increased frequency of action noted by the former in the presence of increased intracardiac pressure, together with the constant action of the forces of gravitation and aspiration, serve to maintain arterial pressure, notwithstanding the greater labour thrown upon the heart. The therapeutic indications given by these results is that the aortically incompetent heart should be quickened, not retarded, until, after acquired compensation, the organ is able to act in a more leisurely manner, and the patient, abandoning the aid of the orthopneic position, to recline in comfort. It should, moreover, be remembered that the average frequency of the pulse in aortic regurgitation is above the normal rate, Corrigan¹⁸ placed it at 110 per minute. Belladonna, strychnine, and the nitrites are the agents which I have found most useful under these circumstances, and are to be regarded as cardiac stimulants, not depressants. When cardiac propulsion has to be increased with as little increase as possible of cardiac aspiration, I use accelerants of the heart's action such as those named; when the main indication is the increase of cardiac aspiration I use the digitalis group of remedies, and most frequently digitalis itself, over which I fail to find any advantage in its substitutes, for the vascular effects of digitalis need not be injurious provided the case is one for treatment chiefly by the digitalis group. In the paper to which I have already referred Dr. Barrs speaks of the out-patient department of a considerable hospital as the best field for testing the effects of remedies, because in the cases which present themselves the other circumstances remain largely unaltered. With this opinion I entirely agree, and it is just in such a field that I have had an opportunity of convincing myself of the correctness of the views I have expressed. It is a matter of common experience that compensation will become established in many cases without any treatment. Unimpeded in its action by drugs, the overburdened heart tends as a rule to ease itself by quickened action, which may pass into a morbid tachycardia regular or irregular in its rhythm. Skill in the treatment of heart disease consists in following the indications thus afforded by Nature in promoting compensation, and in checking a dangerous preponderance either of tachycardia or of bradycardia during the process. Lesions requiring increased propulsion, I repeat, will, as a general rule, need cardiac acceleration, those requiring increased aspiration cardiac retardation. The indubitable representative of the former class is aortic valvular incompetency; of the latter, mitral regurgitation. The pathological relations of these valves thus differ as essentially as their anatomical situation and physiological function; hence also the different treatment each requires when its normal action is disturbed.

¹⁶ *Circul. d. Sang.*, p. 674-75.

¹⁷ *Archiv f. exp. Path. u. Pharmac.*, Bd. ix, p. 1 et seq.

¹⁸ *Loc. cit.*

DR. ALLAN MACFADYEN, Lecturer on Bacteriology at the British Institute of Preventive Medicine, has been appointed to act as Honorary Secretary of the Institute.

YELLOW FEVER IN BRAZIL.—A telegram from Rio de Janeiro dated March 1st states that there is serious yellow fever there which is likely to last. In consequence of the non-completion of a promised hospital of isolation, the only available quarantine station of Brazil there is now used as a yellow fever hospital for the crew of an Italian ship of war which has, out of 280, had 225 cases, 84 having proved fatal. Several suspected cases have occurred at Belgrano, a suburb of Buenos Ayres.

NOTES ON CERTAIN CHANGES IN THE CARDIO-VASCULAR SYSTEM WHICH ARE INDUCED BY TREATMENT ACCORDING TO THE SCHOTT METHODS.¹

By W. BEZLY THORNE, M.D.

THE changes which may be observed to take place in some of the physical characters of the heart and blood vessels under the several and combined influences of position, rest, and pharmaceutical treatment, have been the object of well-deserved interest. The modifications of form and alterations of function which these organs undergo as the result of treatment by physical methods consisting of mineral baths and remedial movements, are so rapidly effected, and yield results so striking, that they may be regarded as equally noteworthy. They have been observed mainly by auscultatory percussion and with the aid of the sphygmograph. It is not possible to claim for either method the merit of un-failing exactitude. Nevertheless, they are capable of yielding information of both clinical and physiological value.

Auscultatory percussion is practised, it is scarcely necessary to say, by so adjusting a binaural stethoscope to the patient's chest, that the percussion note is conveyed a relatively undiminished and unblended sound to the observer's ears. The stroke may be delivered in a variety of ways. My own practice is to touch the chest lightly with the tip of the round-pointed ferrule of a long clinical pencil, and to let it travel in a radial line, from a point well outside the region to be defined, to the point at which a rise of a semitone is observed to take place in the percussion note. (The diagram illustrates the proceeding. The circles indicate points at which the chest piece may be placed, and the dots mark the line in which the ferrule is made to move.)

Certain advantages may be claimed for this method. Extraneous sounds being to a great extent excluded, and the note itself being clear and definite, the faculties of hearing and of mental concentration are spared a measure of strain and fatigue which is inseparable from the more usual proceeding. Moreover, inasmuch as the ferrule impinges at each stroke on a surface not much more extended than that which is designated in the geometrical definition of a point, it may be claimed that the resulting demarcations will bear comparison with others in respect to precision. The factor which introduces the element of uncertainty is the observer's appreciation of sound. It may be defective or exhausted. Nevertheless, it is possible to educate it to almost automatic action.

The tests to which I have subjected auscultatory percussion in defining the position of abdominal viscera, more especially of the liver, the lower border of which is frequently appreciable to the touch, have convinced me that

¹ Read in the Section of Medicine at the Annual Meeting of the British Medical Association, held in London, July-August, 1895.