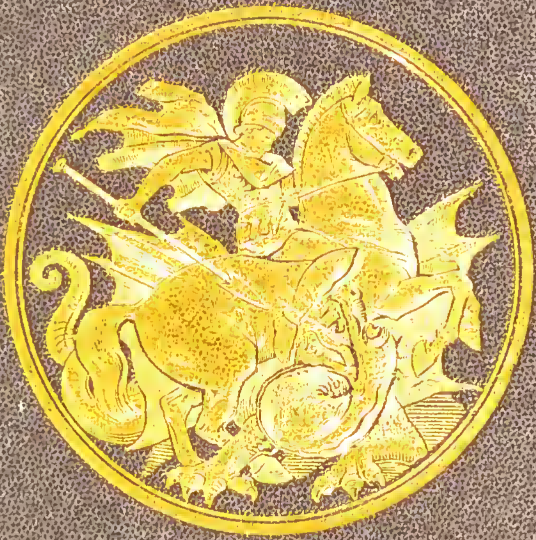
The Harveian Oration

W. H. DICKINSON, M.D.



HARVEY
IN
ANCIENT & MODERN MEDICINE

J. D. Dreher

From the Author.

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ON

HARVEY IN ANCIENT AND MODERN MEDICINE

BY

W. HOWSHIP DICKINSON, M.D., F.R.C.P.

HONORARY FELLOW OF CAIUS COLLEGE, CAMBRIDGE; SENIOR PHYSICIAN TO ST GEORGE'S HOSPITAL; CONSULTING PHYSICIAN TO THE HOSPITAL FOR SICK CHILDREN; CORRESPONDING MEMBER OF THE ACADEMY OF MEDICINE OF NEW YORK

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TO

SIR ANDREW CLARK, Bart., LL.D., M.D., F.R.S.

PRESIDENT OF THE ROYAL COLLEGE OF PHYSICIANS

IN TOKEN OF THE RESPECT

DUE TO ONE WHO WORTHILY REPRESENTS

THAT ANCIENT, HONOURABLE, AND LEARNED BODY

PREFACE

I fear that the only portion of this Address which aspires to be other than commonplace, that which relates to dropsy, may have but limited interest; while even the few whom it interests may find that, compressed as it necessarily is, it suffers from the want of detailed argument and corroborative cases. It is my hope to bring some of the considerations here briefly touched upon before the Medico-Chirurgical Society in a somewhat more expanded form. I may add that the association of renal dropsy with the dynamics of the circulation was no hasty invention on my part, put together for a temporary purpose, but had occupied my mind long before

I thought I should be honoured with this opportunity of asserting its connection with the Harveian scheme. Without claiming any other value for the views I have put forward than that they were the results of observation and thought, I judged it more fitting to lay them before the College as such rather than to offer that which had cost me nothing.

HARVEY

IN

ANCIENT AND MODERN MEDICINE

In submission, Mr. President, to your behest, but with sore discouragement when I call to mind the names of some of the 172 Fellows who have preceded me in this office, it devolves upon me to-day to exhort the Fellows of this College to concord; to commemorate by name our Benefactors; and to exhort our Fellows and Members 'to search and study out the secrets of Nature by way of experiment.' As to the first injunction, a general change in manners has made it less necessary than formerly; we no longer wear swords or use them, our language is more conventional and less emphatic, and there are fewer differences of opinion where all are better informed. As to the second, though there is no reason to think that Harvey meant the address to apply especially to himself, or be a repeated tribute to his own memory, yet in commemorating our benefactors it is not unreasonable to speak in particular of the greatest of them, one who was a benefactor not only to the College but to the race. And as to the third, the results of Harvey's work will without any special exhortation speak sufficiently in commendation of his method. Therefore I propose to make Harvey the subject of my discourse, and if I say again what has been better said before, my excuse must be that on so familiar a topic there can be little of novelty. Custom has sanctioned the use of this opportunity for the discussion of matters, often of the highest interest, but only remotely connected with Harvey or the purpose of his bequest. I shall not presume thus to wander, but gladly avail myself of the restrictions of a trodden path and a limited scope.

Harvey lived through one period which was favourable to intellectual progress, and through another which was not. Born (1578) under Elizabeth, ten years old at the time of the Armada, he was a spectator of English history until within fifteen months of the death of Oliver Cromwell. At the death of James I. Harvey was forty-seven years old; his great discovery had been made and expounded in this College, though not yet given to the world. Thus his most important work was done in a quiet time—a period not brilliant historically, but one not unpropitious to learning and research. The fetters of old superstition had been loosened, and the influence of the Elizabethan age was still in the air. It is worth noting that Harvey's long life made him the contemporary of the four greatest men whom England has produced. No one will dispute that this pre-eminence belongs to Shakespeare, Bacon, Milton, and Newton, each without rival in his own way. Shakespeare died in 1616, the year in which Harvey, then thirty-seven, began to lecture at this College. Bacon and Harvey were closely associated; Harvey was Bacon's physician, and had, as Aubrey tells us, but a mean opinion of his philosophy, though he esteemed him much for his wit. Harvey was widely separated from Milton politically, but he may have read his earlier works. Newton was fifteen when Harvey died.

It is no part of my purpose to describe Harvey's work in detail, or to vindicate his claim to the discovery with which his name is connected; the latter duty, as it recently became, has been ably performed by Dr. George Johnson. The *difficulty* of the discovery and its *magnitude* are the two considerations pertaining to it which cannot be passed over. To appreciate them we must take our stand at different points of view, and those widely separated. To see the *difficulty* we must place ourselves in the early part of the seventeenth century, and assume the state of mind belonging to Harvey's immediate predecessors. We who have passively imbibed from our infancy what was once a novelty and matter of dispute, cannot without effort realise what Harvey had to rid his mind of before he could see as we do. He had to get rid of a mass of time-honoured hypotheses, mainly false but not wholly so, in which an admixture of truth made the error enduring—so enduring as to influence to the present day the belief of the laity and the language of all.

In Harvey's time, as at the time of Galen, spirits were active in the human economy. There were three—the natural, the vital, and the animal. We hear of spirits even now, but they are irregular practitioners, without any recognised place in our philosophy. Up to Harvey's time spiritual essences, not implying the supernatural, but only aërial or gaseous, played a part which we now attribute to the blood in its liquidity. We can discern a sort of forecast of oxygen obscured in the old phraseology. The vital spirits were brought to perfection in the left ventricle from materials, air in particular, obtained from the lungs. As 'the fountain and workshop of the vital spirits,' the heart was the origin of vital heat and of the affections and emotions. From the left ventricle the vital spirits, together with blood, were conveyed by the arteries to the organs and tissues. But not only was the aërial element derived from the lungs, but the arteries also obtained air from the surface of the body generally, and discharged thence fuliginous vapours. Against this Harvey urged the impossibility of any such cutaneous respiratory process in the fœtus *in utero*, and in animals whose lives were spent in water.

Theories of vital spirits as directly connected with the mind, their origin in the heart, and the heart as the centre of emotion and character, still hold place in literature and conversation. We refer to high spirits and low spirits, regardless of the practical philosophy of the fool, 'I care not for my spirits if my legs were not weary.' We read without surprise that among the exhilarating effects of the apple Eve attributed to herself 'dilated spirits, ampler heart;' but when so modern and scientific a poet as the Laureate refers to the human heart as 'the holy secret of this microcosm,' we cannot but question whether the description would not better suit the brain, which, to quote the physiology of an older poet, 'some suppose the soul's frail dwelling-house.' That Tennyson meant the heart in a literal and anatomical sense is evident from his speaking in the same passage of dabbling the hand in it. Such expressions as 'warm-hearted,' 'cold-hearted,' 'chicken-hearted,' 'dishearten,' and the like are still common in our talk. Courage, by its etymology, should belong to the heart rather than to the hemispheres. We still speak of a man as 'having his heart in the right place,' as if we took our anatomy from a source older than Galen or Aristotle: 'A wise man's heart is at his right hand, but a fool's heart at his left.'[1] Thus, our language is behind our knowledge by hundreds, even by thousands, of years.

Cæsalpinus (1569) was apparently aware that the blood left the heart by the aorta and returned to it by the veins, passing, as he thought, by anastomoses from one set of vessels

[1] Ecclesiastes x. 2.

to the other, and re-entering the heart by the vena cava; but he supposed that spirit and blood made their exit from the heart chiefly during wakefulness, while the return was during sleep, and he repeated with strange confusion the old comparison of the movements of the blood to the ebb and flow of the tide in Euripus.

Before Harvey there appears to have been some difference of opinion as to whether the heart was muscular, but it was generally believed that the active agent in moving the blood was the act of respiration. Thus, although Harvey found the pulmonary circulation ready to his hand, and the term 'circulation' already attached to it, the course of the blood in the general system was so confused with tides and spirits, so destitute of any rational explanation of how the blood which left the heart got back to it, that, as he tells us, he was almost tempted to think, with Frascatorius, that the motion of the heart was only to be comprehended by God. That it was eventually comprehended by Harvey was due in part to the steps already made which helped to give him his standpoint. The hour was come as well as the man. While we give him his meed, let us not omit the honour due to his predecessors, especially Servetus and Fabricius—to the one for his deduction with regard to the pulmonary circulation, to the other for his accurate observations concerning the valves of the veins.

To recognise the *magnitude* of Harvey's discovery we must look at it from the latest attainable point of view ; test it by the knowledge of the present day ; whether it holds its ground ; and what are its issues and results. I propose thus to examine a discovery of the seventeenth century in the light of the nineteenth—not touching its truth, which is unnecessary, but in regard to what has sprung from it ; to judge of the tree by its fruit.

If the spirits of the wise, as they sit in the clouds, look down upon us with any recognition of what we are about, they can scarcely be indifferent to our estimation of their earthly labours. When we confirm their work and build upon their foundations, we appeal to the last infirmity of noble minds. We can imagine Dalton looking with placid satisfaction upon the superstructure which has been raised upon the atomic theory, scarcely less than the whole of analytical chemistry; we can imagine Newton as regarding with similar approval the marvellous illustrations of his law of gravity furnished by modern astronomy; and we can fancy Harvey as following the development of his discovery with an acquiescence which I fear he would not extend to the attitude of modern politicians with regard to his method. We can conceive Harvey as viewing with special regard those who have been most active in carrying on his work: Malpighi, Laennec, Hope, Williams, Corrigan, and I could add the names of some of our living Fellows were it not possible that they may be present to-day.

The immediate effects of Harvey's discovery were not beneficial to medicine, and were injurious to himself. 'His practice fell off mightily,' and practical medicine remained what it was before. This lack of early result has been more than once noticed by my predecessors.[1] Two observations apply to this discovery and to many others. First, practice advances independently of science, and often anticipates its conclusions. Secondly, what has been established by practice will hold its ground after the introduction of scientific knowledge by which it should have been abolished. Of practice which goes before theory we have many examples. Suction-pumps, as Sir George Paget observes, had been at work for hundreds of years before Torricelli showed the true cause of the suction. Gunpowder was invented by an ingenious ecclesiastic five centuries before anything was known of oxygen, carbon, or the laws of combustion.

Thus, before the circulation was understood, not only was venesection in use, perhaps obvious in any case, but the arm was constricted above the puncture, with results which would seem to hint at return by the

[1] Harveian Orations. By Sir G. Paget, p. 11, and by Dr. West, p. 45.

veins. Transfusion of blood, which should require some knowledge of its course, was certainly[1] practised in 1615, if not earlier.

As to the persistence of old practices after advancements by which they should have been set aside, it is easy to find examples in medicine and elsewhere. Christians were buried with a coin in their mouths long after the ferryman had ceased to demand the toll. The doctrine of signatures continues to guide popular medicine, and, even with the learned, infusion of roses still holds a place. We still sometimes apply local blood-letting as did our fathers who knew nothing of the circulation and little of the course of the vessels. The practice of the second century, and of Aretæus, is still occasionally seen in the leeching of the hypochondrium to deplete the liver, though there is no way through.

With the same adherence to tradition and neglect of anatomy, we still cup the back to deplete the lung. We cup the loins to relieve the kidneys, and if we succeed in finding a

[1] Dr. J. W. Ogle, Harveian Oration, p. 110.

few insignificant vessels connecting the capsule with integument, we rejoice over them and flatter ourselves that we are the disciples of tradition and science too. As indicating the general belief in such measures in very early times—for they were not always misapplied—it is worth remarking that the same word in our language denotes an agent for local blood-letting and a medical practitioner.

A skilful leech is better far
Than half a hundred men of war.

Dry-cupping, not without its uses, though not always applied with due regard to anatomy, furnishes another instance of a practice which has survived the theory on which it was based. It apparently had its origin in the ancient belief that the arteries contained spirit which could transpire by the skin. Ambroise Paré quotes with approval the discrimination of Celsus: the cupping-glass draws blood if the skin be first scarified, 'but if it be whole then it draws spirit.'

To come to the results of Harvey's work, the first in time and in importance was the completion of it by the discovery of the capillary system by Malpighi, who was born in the year in which Harvey published his famous treatise. Harvey had never seen a capillary, nor did the state of the microscope in his time allow of it. He was fain to conclude that the blood passed from the arteries to the veins partly by anastomoses, but mainly by percolation, as water—to quote his own illustration—percolates the earth and produces springs and rivulets. Had it been possible, we may imagine the delight with which he would have witnessed the completion by vessels of his circular route. For a time the interest was for the curious, but wide and practical issues were wrapped up in it, some of which it will be my later endeavour to unfold.

The first improvement of immediate utility, so far as I can learn, which was the direct result of Harvey's discovery, was not made until more than half a century after his death in the invention of the *tourniquet*, by which arteries were compressed as distinguished from the veins. Then comes John Hunter's operation for aneurysm—tying the artery between the sac and the heart. Then —to take no note of lesser advances—auscultation comes into view. This, so far as concerns the inside of the heart and endocardial murmurs, implies a knowledge of the course of the blood, and could not have arisen without it. That the heart made sounds was an observation familiar to Harvey. The comparative recency of auscultation is almost a surprise, considering how large a proportion of our present medical knowledge is connected with it. Laennec's first memoir on mediate auscultation was published in 1818, within the lifetime of some of our present Fellows. How great has been the pace since! how great the multiplication of knowledge or of its semblance! But much as we know about cardiac murmurs, it is possible that we do not yet know all. There is one whose note has in it something of discord. All are agreed as to the change which it denotes, but not quite all as to the way in which it is made. Next, as a direct result of a knowledge of the course of the blood, comes that of the course of emboli—knowledge now in such habitual use that we scarcely realise that it is not fifty years since embolism became known. To recognise its value, it is only necessary to look at the brain and the writings of so great an observer as Abercrombie. How confused and inadequate are these, wanting the key which embolism gives! The theory of uræmia, somewhat of older date,[1] is the only advance of modern times which can compare with this in the light it has thrown into the recesses of cerebral pathology. As a corollary to embolism comes a special form of it known as 'pyæmia'—too well known before under other names, but not understood. Another Harveian development—if I may be allowed the expression—has no narrower limits than

[1] The theory of uræmia appears to have been first propounded in the year 1833 by James Arthur Wilson, in a paper read at the College of Physicians on 'Fits and Sudden Death in connection with Disease of the Kidneys.' (See *London Medical Gazette*, 1833.)

the whole extent of inflammation, or at least as much of it as concerns the capillaries. Suppuration, in particular consisting of the transit of corpuscles through their walls, is a simple pathological sequence of the theory of the circulation. John Hunter nearly predicted this recent advance when he said that pus was something separated from the blood in its passage out of the vessels, thus tracing the discharge to the blood, not, as continued to be thought long after his time, to the tissues.[1]

Now the scope widens and becomes too large for detail, for the whole of the vaso-motor system is opened to our view. Harvey ascribed the same motions to the arteries as to the heart, attributing to them a systole and diastole from powers inherent in their coats.[2] Were I to look only at a portion of the field

[1] President's Address, *Path. Trans.*, vol. xl. p. 3.

[2] 'We do not, therefore, deny everything like motion to the tunics of the arteries; on the contrary, we allow the same motions which we concede to the heart, namely, a diastole and a systole in return from the distended to the natural state; this much we believe to be effected by a power inherent in the coats themselves.' (Harvey's Works, Sydenham Society, p. 113.)

presented, and dwell upon the changes in the heart and arteries which Bright in part discerned, and which Johnson, Gull, and Sutton have done so much to amplify and elucidate, I should find enough to show that Harvey had worthy successors in his own College, though time would permit me to do little more. But I should at least show that the torch which Harvey lit was still being carried forward, and into dark places which he never dreamed of.

Hitherto I have kept, as I proposed, to beaten tracks; if I now for a short time leave them in the endeavour to carry a dim spark for another stage, I can but hope that my light may not prove to be of the nature of an *ignis fatuus*. I propose to touch upon dropsy as connected with the Harveian scheme. It is not needful to remind this audience that many forms of dropsy are simple consequences of impediments to the circulation, and require nothing more than a knowledge of the course of the blood for their complete explanation. Fluid accumulates behind an obstacle, and when we know where this is, and by what route the blood comes to it, the process stands revealed, whether we have regard to ascites from portal obstruction, or general dropsy from cardiac embarrassment.

Harvey's law has made simple what without it would be incomprehensible. But I will adventure further, and endeavour to show that *renal* dropsy, generally credited with a totally different mode of production, has its association with those which are purely mechanical, and is connected with the attitude of the blood-vessels as well as with the chemistry of their contents.

Some forms of dropsy are obviously connected—and others, perhaps, less obviously—with exaggeration of intravascular pressure. Increased pressure on the venous side gives lessened absorption; increased pressure on the arterial side should give increased transudation. Of the first condition we recognise examples every day in the common forms of obstructive venous dropsy, whether cardiac, portal, or femoral, and recognise at the same time that they are dynamical, not chemical. It will be my endeavour to show that renal dropsy is dynamical as well as chemical, that it is associated with arterial pressure as the others are with venous, as immediately, though not *ab initio.*

A distinguished predecessor in this office spoke of renal dropsy as a result of osmosis, not of obstructed circulation, and of Graham's researches with regard to diffusion through membranes as chiefly concerned in its explanation; but I think I shall be able to show that, whatever may be said of the laws of Graham, those of Harvey are not to be left out of consideration. It may even appear that the force of the heart and pressure of the blood have more to do with this morbid process than osmosis or dialysis.

It is probable that we have yet much to learn as to the passage of fluids through living membranes, and it would be presumptuous to assert that we know all about renal dropsy; but it presents such clear clinical relations with the physical conditions of the heart and vessels that these cannot be disconnected from it. Over-transudation, such as occurs in renal dropsy, may be hypothetically attributed to three causes—osmosis or dialysis; filtration from pressure; and, as Dr. Waymouth Reid would infer, an action on the part of the blood-vessels analogous to secretion.[1]

With regard to the last process, it is probable that we shall hear more; the operations of Nature are not always simple; where filtration ends and secretion begins would be a difficult question, if both are concerned. The secretive theory has been resorted to in conse-

[1] Dr. Waymouth Reid, to whom I must acknowledge my obligation in several respects, has published a valuable research on osmosis in the *Journal of Physiology* for 1890. In this he shows that the passage of fluid through animal membranes is influenced by many circumstances hitherto not sufficiently regarded. Among these the physiological condition of the membrane holds a prominent place. The transfer is influenced, as he considers, by the protoplasmic activity of the membrane, and is comparable to the act of secretion; it is influenced also by various toxic and medicinal agents, which depress or stimulate the vital activity of the membrane. It was not within Dr. Reid's purpose to compare the effects of pressure with those of osmosis, for which reason I have not given greater prominenco to his inquiry.

quence of the apparent inadequacy of osmosis to explain the phenomenon in question. But I think it cannot be doubted that the paramount agent in moving the liquids of the body, whether along tubes or through their walls, is pressure.

The following facts must be had regard to in any explanation of renal dropsy which the future may have in store for us. If we look at the character of the effusions in different kinds of dropsy, certain broad facts present themselves which appear to have significance with regard to their mode of production. The chief components of such effusions differ from each other as crystalloid and colloid; the salts crystalloid and highly diffusible, the albumen colloid and slightly diffusible, except under pressure. Under osmosis, salts should transude without albumen; under pressure, both together. Now, whatsoever be the cause of the dropsy, the effusions in the same place are essentially the same; not without differences, but without any such differences as would seem to imply an essentially different mode of origin. The effusions vary much with situation, little with the disease. Those into the serous cavities are always rich in albumen; those into the cellular tissue, poor in albumen with equal constancy. The mineral salts vary scarcely at all. I have made many observations on these points, but dare not trouble the College with more than a compendium of results.

I have here a table, which I propose to print, but will not venture to read. I may say, briefly, that as regards the mineral salts in ten cases of pleural effusion under a variety of disorders, chiefly renal and cardiac, these salts were found to vary only from ·71 to ·89 per cent. In fourteen cases of peritoneal effusion presenting similar varieties of origin, together with several of cirrhosis of the liver, these salts ranged only from ·75 to ·95 per cent. As to fourteen cases of œdema, similarly various in their origin, the range was from ·75 to ·89 per cent. Thus, whatever process gave rise to the saline exit, it appeared to be much the same whatever the disorder. As to

AVERAGE CONSTITUENTS OF DROPSY FLUIDS IN 100 PARTS.

Disease.	No. of Cases.	Specific Gravity.	Solids.	Albumen.	Mineral Salts Soluble	Mineral Salts In-soluble	Mineral Salts Total	Specific Gravity.	Solids.	Albumen.	Mineral Salts Soluble	Mineral Salts In-soluble	Mineral Salts Total
		PERICARDIUM						PLEURA					
Renal—nephritis and granular kidney	6	—	—	—	—	—	—	1011·8	1·84	·840	·795	·029	·82
Renal—lardaceous	4	—	—	—	—	—	—	1011·9	2·05	·742	—	·029	—
Diabetes mellitus, simple . . .	2	—	3·60	†	·61	·03	·64	—	—	—	—	—	—
Diabetes + fibrotic kidney . .	1	—	—	—	—	—	—	—	—	—	—	—	—
Diabetes + saline injection into veins *	1	—	5·02	—	·81	·05	·86	1016·7	4·77	2·10	·80	·045	·84
Valvular disease of heart . . .	7	1018·5	—	3·08	—	—	—	1013·3	3·32	2·11	·75	·031	·78
Emphysema and bronchitis . .	2	—	—	—	—	—	—	1013·5	3·35	1·91	·78	·041	·82
Hepatic — Cirrhosis	4	—	—	—	—	—	—	—	—	—	—	—	—
		PERITONEUM						CELLULAR TISSUE					
Renal—nephritis and granular kidney	6	1012·3	3·36	2·24	·885	·036	·92	1008·3	1·30	·09	·84	·025	·86
Renal—lardaceous	4	1011·6	1·92	·49	·755	·03	·78	1008·3	1·38	·152	·80	·021	·82
Diabetes mellitus, simple . . .	2	1018·2	4·48	2·41	·86	·047	·90	1012·2	1·99	·362	·86	·023	·89
Diabetes + fibrotic kidney . .	1	—	—	—	—	—	—	1009·3	1·76	·23	·83	·031	·86
Diabetes + saline injection into veins *	1	1013·5	3·49	1·38	·835	·036	·87	—	—	—	—	—	—
Valvular disease of heart . . .	7	1014·7	3·75	2·63	·742	·04	·78	1008·5	1·56	·359	·78	·026	·81
Emphysema and bronchitis . .	2	1017·9	4·59	3·13	·81	·03	·84	1008·1	1·30	·235	·74	·024	·77
Hepatic — Cirrhosis	4	1013·4	—	2·25	·812	·029	84	1009·1	2·53	1·39	·77	·03	·80‡

* Of 456 ozs. of saline liquid during 32 hours in a case of diabetic coma (see *Transactions of Clinical Society*, vol. xxiii. p. 130). † Much. ‡ The œdema fluid was examined only in one case.

the albumen, it will be seen, as I have said, to vary more with situation than with the disease.

Thus the similarity of the effusions cannot fail to suggest that renal dropsy is made in much the same way as those which are obviously due to vascular obstruction. Osmosis, as measured by the salts, is nearly the same in the two kinds, while the albumen in both implies a process other than osmosis, which is certainly pressure in one case, and may be so in the other. Any view, therefore, that renal dropsy is especially a result of osmosis must be abandoned, and our inquiries directed anew to the nature of the process.

When renal dropsy sets in, the urine is usually below par, but the relation of dropsy with the quantity of urine is not constant enough wholly to explain the process. With obstructive suppression dropsy is usually totally absent, while it is frequently present under the diuresis of diabetes. Such facts must find their explanation in any adequate theory of such dropsies as are not coarsely mechanical; and I venture to submit that, whether coarsely mechanical or not, modifications of blood-pressure are largely, if not mainly, concerned in the dropsical process.

Anæmia has been thought to be concerned in renal dropsy; but we often see intense, and even fatal, anæmia under the name pernicious without any such effusion, whence it may be concluded that anæmia alone is not enough to cause it.

With obstructive suppression arterial tension, so far as can be learned by the finger, is conspicuously wanting, while death occurs by asthenia or syncope. Low blood-pressure, with no dropsy where it might otherwise be expected, is at least suggestive of the need for more facts. In acute nephritis, where the urine is only scanty, not suppressed, we see high blood-pressure, and, together with this, dropsy. Want of urine does not, alone and of necessity, cause renal dropsy; increased arterial tension would seem to be a necessary intermediary, at least of this variety of it. An increase of arterial tension beginning with the earliest stage of nephritis, accompanying the œdema, and increasing with it and the disease, has long attracted notice. I could, if it were necessary, adduce a large number of tracings in proof of this assertion, which I think will not be doubted. If further evidence be needed on this point, it can be found in perhaps the most conclusive shape in the hypertrophy of the left ventricle even in the acute forms of the disease.

After death, within ten weeks of the onset of acute renal dropsy, I have found decided hypertrophy both of the left ventricle and of the muscular coat of the arteries. Hypertrophy of the left ventricle has long been accepted, and hypertrophy of the arterial muscle must be equally accepted, as evidence of an obstacle ahead—in front, that is, of the major part of the arterial system, which on this evidence, as well as on that of the sphygmograph, must be overcharged and overtight. The next question is where precisely is the arterial exit impeded, in the arterioles or in the capillaries? Upon this much turns. Renal dropsy, presumably, depends on excess of exudation, not deficiency of absorption; this can scarcely occur elsewhere than from the capillaries, whose walls alone are adapted to the process. If the primary obstacle is in the arterioles, as Johnson supposes, the blood must be in a measure shut off from the capillaries, the pressure on them reduced, exudation from them lessened, and the view of renal dropsy which I submit abandoned. But the doctrine of capillary obstruction preponderates, and, to use a political phrase, holds the field. It has been urged that the capillaries are not muscular; on the other hand it has been shown by Roy and Graham Brown that they are contractile; beside which we cannot doubt, what has long been inferred, that the blood may acquire peculiarities which add difficulty to its transit. That the obstruction is beyond the radial artery is sufficiently proved by the fulness of that vessel; that the obstruction is beyond the minute arteries of the retina and the brain is shown by their often bursting from the pressure to which they are subjected by their contents. We have no choice but to go further, and attribute the impediment to the capillary system, whether from change of blood *per se*, or from vascular contraction engendered by it. With stoppage in front, and accumulating systolic force behind, increase of transudation would seem to be inevitable. Whatever be the nature of the hindrance in the capillaries, we may presume from the phenomena of inflammation that it is consistent with an increase of transudation from them. So far, I have shown that the form and stage of renal dropsy under discussion is at least associated with what may be called the mechanics of the circulation, which are necessary corollaries of the simple laws which we owe to Harvey. Whether the increase of pressure is complicated with any such secretive process as Dr. Waymouth Reid infers must be left for the future, but the more simple action seems too obvious to be excluded.

Now let me turn to a later phase of renal dropsy in which also the action of the heart and arteries is to be reckoned with. As the hypertrophy of these structures increases, the effusion independently of medicine tends to lessen and disappear. It would almost seem that this hypertrophy, whether of the heart or arteries or both together, is the natural cure for the dropsical state. This at first sight is difficult to reconcile with the early increase of dropsy, together with increasing arterial tension; for now with advancing hypertrophy the tension becomes greater but the dropsy less. The hypertrophy is little of the right ventricle, mainly of the left ventricle and the arteries, and appears to militate against the dropsy so long as the cardiac charge is unaccompanied by dilatation. Whether the heart or arteries have the more to do with this mitigation must be taken into question. 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 stopcock action, and with it lessened capillary pressure and lessened exudation. Now let us glance for a moment at the left ventricle and the change of proportion which renal disease accomplishes for it. Look at the thickness and solidity of the wall and the comparative smallness of the cavity as yet undilated. Beside the obvious increase of contractile power which the increase of muscle entails, the expansile power must be increased even in a greater degree, since it must have to do not only with the thickness of the wall, but also with its extent; in other words, with the size of the cavity. The smaller the cavity in relation to the thickness of the wall, the greater the rigidity of the chamber and its consequent expansile power. The suction thus exaggerated must tend to pump out the waterlogged tissues by drawing first upon the lungs, then upon the systemic veins and their tributaries. The hypertrophy of the right ventricle, so far as it goes, will also have an effect in aiding the circulation in the lungs and systemic veins, but it does not seem to go far enough to be of much use.

It is worth noting, as bearing upon its

nature, that renal dropsy, like cardiac, is acted on remedially by the horizontal posture; not only is the œdema moved, but often removed, and that, as we must infer, by reducing the pressure in the veins and capillaries of the dependent parts.

I now approach the last aspect of renal dropsy in relation to the circulation. After a time the hypertrophied ventricle begins to stretch, and then not only loses expansile power, but valvular adaptation. Dropsy reappears or increases and is now as much cardiac as renal. Mitral regurgitation ensues through the widened orifice, though the valves may be healthy, and a murmur is often heard which may mislead the unwary as to the origin of the disease; and as a more striking consequence, pulmonary apoplexy with hæmoptysis presents itself as a direct and simple issue of the cardiac changes of advanced renal disease. Did time permit, I could show by examples that this complication, so generally associated with disease proper to the heart, may be primarily and essentially renal—

a result of the advanced granular kidney by way of vascular tension, cardiac hypertrophy, and cardiac detriment.

The time at my command will not allow me to include in this consideration of renal dropsy that of lardaceous disease, which is far from simple. I hope to refer to this at some other time and place; but, apart from this, I venture to think that I have shown that the dropsy of kidney disease has an association with the dynamics of the circulation which, though it may be less obvious and more complicated than that of cardiac dropsy, is not less real. All present themselves as connected with the movements of the blood, not in the remote or indirect manner in which almost every operation of the human body can be so traced, but under the immediate control of whatever impedes or adds force to the current.

I have finished, Sir, the honourable task which your kindness imposed upon me. If I have been wearisome in recalling much that is familiar, I have at least shown how much we owe to Harvey of what is now rudimentary and commonplace. Knowledge has been advancing since his time in many and independent lines; the achievements of Bell, Bright, and Addison had no direct connection with his, but it is not too much to assert that the medicine of to-day is scarcely less permeated with the results of Harvey's discovery than is the human body with the circulation he discovered. It does not make him small to say that what he found out must have come to light had he never lived. If Columbus had not discovered America, someone else must have done so before now. The law of gravity might even have been revealed in the fulness of time to another if not to Newton. But the discoverer is before his age: in this lies one measure of his praise; another, and a more important one, is in the results of his discovery.

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