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For Dr. Bell - with the best regards of the Author. -

AN

# INQUIRY

INTO THE

# CLAIMS OF DOCTOR WILLIAM HARVEY

TO THE

**DISCOVERY OF THE CIRCULATION OF THE BLOOD;**

WITH

A MORE EQUITABLE RETROSPECT OF THAT EVENT.

TO WHICH IS ADDED,

## AN INTRODUCTORY LECTURE,

DELIVERED ON THE THIRD OF NOVEMBER, 1829,
IN VINDICATION OF HIPPOCRATES FROM SUNDRY CHARGES OF IGNORANCE
PREFERRED AGAINST HIM BY THE LATE PROFESSOR RUSH.

BY JOHN REDMAN COXE, M.D.

Professor of Materia Medica and Pharmacy in the University of Pennsylvania—Member of the Amer. Phil. Soc.—Of the Batavian Society of Sciences, at Haarlem—Of the Roy. Med. Soc., and of the Roy. Soc. of Sciences, of Copenhagen, &c. &c.

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TO

# THE STUDENTS OF MEDICINE

THROUGHOUT

## The United States.

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I know not that I could dedicate this essay with more propriety elsewhere, than to you, Gentlemen, the rising arbiters of the Profession. It is probable that but few of the elder members of our Science will change those opinions, from any thing herein collected, that have been cherished as Truth, from the earliest dawn of their medical studies. I do not suppose that this arises from any desire to sustain what is incorrect; but prejudices are not often abandoned, if adopted from those in whose dictates we are educated, and of whose correctness or truth, the learner has no immediate means of appreciating. Like the dogmas of our respective creeds, sucked in, as it were, with our mother's milk; so, the dogmas of our Profession, enunciated, ex Cathedra, with scarce a shade of doubt; are impressed too strongly to be subsequently removed by the mere opposition of an individual, who may venture to hazard some scruples on the subject; since all doubt has been so

long silent, as to the claims of Dr. Harvey, that the worst construction will probably be placed by many of my readers, on this attempt to diminish the value of the award, that has been for more than two centuries associated with his name.

To your charge, Gentlemen, I commit this Inquiry: if you find cause to believe it to be the mere attempt of a paltry desire to diminish the glories that have encircled his brow, you will treat it with the contempt it would so justly deserve: but should you be led by it to believe, that others have rights to a large proportion of Dr. Harvey's claims; a sense of justice will unquestionably lead you to investigate the subject, even more fully than I have been enabled to accomplish. Trusting that, in so doing, whatever may be your final verdict as to the conclusions I have arrived at; you will at any rate, appreciate the purity of my motives, and my desire to extend what I believe to be true,

I am, Gentlemen, most respectfully,

Your friend, &c.

JOHN REDMAN COXE.

*Philadelphia, September*, 1834.

# PREFACE.

It will, no doubt, at first sight appear extraordinary to the medical reader, and to the world at large, to hear of an attempt, at this late period, to divest the illustrious Harvey *of a part*, at least, of the honourable award that has so long been conceded to him, of the discovery of the circulation of the blood. Two hundred and twenty years have nearly elapsed since he read his course of lectures to the College of Physicians of London, in which, says his biographer, "he opened his discovery relating to the circulation of the blood," being then in his 37th year; and, after several years of controversy, and pretended claims from other sources, "the honour of the discovery has been sufficiently asserted and confirmed to Harvey,"—and, continues he, quoting from Friend, "*it was entirely owing to him.*" It may consequently be affirmed, that, from every quarter, this assertion has been so long reiterated, and so fully credited, that at present no one doubts the full extent, or even the smallest portion of this claim! The award thus granted has descended, as an heir-loom, from one medical generation to another; is accredited now, in its fullest extent; and to throw even a shade of suspicion upon it, to the medical world, will unquestionably seem a sacrilegious attempt, deserving of obloquy and unqualified reprobation! Attempts have, it is true, been made, at different times, to deny the circulation altogether, or to father it upon some other person; but, the manner in which this has heretofore been conducted, seems to have been but illy adapted to the end in view; since few, if any, pursued, fully and fairly, the fiction to its source; whilst the attempt merely added strength to the claim, which was seen to be so imperfectly opposed.

It will, necessarily, appear a hazardous undertaking, now, to

suspect the integrity of those statements that have reached us; to surmise any thing like plagiarism, or deception, in the proceedings; which, like fame, have augmented, as receding from their source: and this attempt, like its predecessors, though based on a different ground, and pursued in a different manner, will yet, probably, be regarded as undeserving of notice, except it should be that of reproach.

Satisfied, nevertheless, with the integrity of my intentions, which, indeed, have originated in circumstances that I could not control; I must leave to others, if they shall honour these pages with a perusal, the task of judging how far I am sustained in my opposition, by the facts or arguments adduced; whilst I with sincerity declare, that during a period of more than forty years from the time I commenced the study of my profession, no individual stood higher in my estimation than the illustrious Harvey. His treatises, both on the circulation and on generation, were amongst the first I read or possessed; and none considered with more absolute conviction than myself, that all opposition to his claims were founded in error or in prejudice: and it would, of all things, have appeared the most incredible to me, had I been told, only ten years ago, that I ever should have questioned the claims of the man I so long had venerated. What led to this, will appear in the course of the following pages,—in which I trust to show, that my opposition arises from no desire to disparage Dr. Harvey, or to underrate or undervalue his claims and discoveries; but altogether from an anxious wish to do justice to others, whose merits have in my opinion been entirely overlooked, or set aside, by awarding *solely* to Dr. Harvey the honour of the discovery of this great physiological fact,—on which so many, if not all others, absolutely depend,—instead of dividing it with several of his predecessors.

I shall not positively affirm, although it seems to me to be the fact, that *the same disposition*, (at the exaggerated estimate of a few individuals, who, at the period he wrote, gave Harvey the high and honourable title of discoverer of the circulation, although absolutely only attaching more firmly those connecting links of an extensive chain, which time had rusted, and possibly, also, adding slightly to its more full perfection,)—this same dis-

position, I repeat, at a subsequent period, robbed a native of America of the well deserved claim to the discovery of the quadrant! and placed those laurels on the brow of Hadley, that had so unjustly been torn from the humble and nearly unknown Godfrey. If Hadley, by his allowed improvement of the instrument, could fairly divest the latter of his claim to the discovery; then we may equally admit, that Harvey was the undoubted and sole discoverer of the circulation. If so great an injustice as that exhibited, and which is still persisted in, against the real discoverer of the quadrant, even only within the last sixty years, is tolerated, how can we now expect, when two hundred have elapsed, to substantiate, in opposition to Harvey, the co-equal claims of Galen, Servetus, Aquapendente, and many more? when, not only Great Britain, but all the world, alike yield to the testimonials in his behalf! Let me hope, at least, for an act of justice, by reading what I have to say, before condemning me! Let me say, in the words of Themistocles, "Strike, but hear me;" and then, perhaps, the real facts being made apparent, some may be induced to think with me, that the claims set up for Harvey, as to this great and glorious discovery, are far beyond their appropriate standard. Had he not, himself, most fully claimed *the whole* in various parts of his works, and especially in his 52d Exercit. de Generatione, where he speaks of it in the following words, "Circuitum sanguinis admirabilem, *a me jampridem inventum*," I should have thought the claims for him from other quarters, to be of less importance, and might probably have omitted altogether the inquiry it has led me into!

In order to comprehend fully his asserted claims, as well as the opposition to them, I regret that an unavoidable extent of extracts, from different sources, becomes necessary; and also, that, as the writings of those days were chiefly in Latin, when connected with scientific research, it has been equally necessary to quote from the original. The subject is sufficiently interesting to hope that this dead language will not discourage, or be overlooked by, the friends of truth. It may, in the present day, indeed, make the reference to a dictionary more frequent; but I have thought it more proper to give my quotations in the very words of the writer, than to put them into English; since, I

might be regarded as giving rather my own, than the version of the writer himself. I much fear, indeed, that it is this great neglect of Latin in the present day, that prevents many of our profession from investigating the treasures that abound in most of those huge volumes, in which our predecessors were wont to clothe their views; and of which they have been so frequently defrauded by the affirmed *novelties* of later writers.

I propose to precede the object in view, by giving the biography of this excellent man; together with the observations which Friend, in his History of Medicine, (Ed. 1725. vol. I. p. 227.) has introduced in his behalf. This history is in form of a letter to the celebrated Dr. Mead; and has always been in high and deserved esteem. The extracts from Harvey himself, and from the different authors, will connect themselves with the text when necessary; or be given as an appendix at the end. The editions of Harvey from which I quote, are "De Motu Cordis et Circulatione," a 12mo. edition of Glasgow, of 1751; and an English translation, of a former edition, printed in London, 1673, to which is added, a Discourse of the Heart, by Dr. De Back, of Rotterdam, and containing, also, two dissertations of Harvey to the younger Riolan, in vindication of his doctrines, and attempts to remove the prejudices of Riolan against them. It will be found that I have had occasion more than once to refer to these, and thus measure Harvey by a standard that others have neglected to employ, viz., by himself.

It may be well to impress here upon the reader, that although Harvey had not been idle in promulgating his opinions, previously to announcing them in his first course of anatomical lectures of 1616; yet, conforming to the Horatian rule of nonum prematur in annum, he did not commit them to the press until twelve years afterwards, viz., in 1628; so that he had every possible opportunity that time could afford, or animadversion could suggest, and friendship verify, of perfecting fully that opinion or discovery which he had so *long before* enunciated and proclaimed to be his own, or of giving to others that which was justly their due.

I am aware that numerous defenders have sprung up in behalf of Dr. Harvey's claims; but it appears to me, that a sufficient

distinction has not been drawn as to the facts, on which those claims are founded. Three different grounds have apparently been assumed against him by his opponents.

1. That others, before him, had discovered the circulation.

2. That, at all events, his assumption of the complete discovery, without any credit given to his predecessors, was unwarrantable, and contrary to fact.

3. That all that could possibly be granted to him, was that of more fully substantiating the fact, and demonstrating the probable route of the general circulation, which had previously remained in a state of uncertainty.

That Harvey claimed for himself, the *full and undivided credit* of this discovery, is clear from various circumstances, viz. :—

*a.* His own direct assertions, and those of most of his adherents and friends.

*b.* His almost entire silence, as to any competitor in this field of inquiry, either anterior to, or contemporary with him.

*c.* His total silence as to some writers, immediately preceding him, on the use of the valvular apparatus of the veins; the only part, nearly, that could be regarded as imperfect in the history of the circulation.

It will probably be affirmed, that all these particulars have been repeatedly disproved, and that Harvey's claims are absolutely unquestionable. But is this true? and how has the attempt to maintain his claims been conducted, and by whom? Very few have, indeed, taken up the question at all; and still fewer have conducted the investigation in the way it required. In vindicating or disproving his claims, strict justice demanded, that the inquirer should be guided chiefly, if not entirely, by his own views, *as at first* promulgated in his earliest treatise on the subject, rather than by subsequent editions, in which numerous alterations or modifications might be presumed to strengthen the imperfections of the first; for the observations or opinions of commentators present their own views, and not his alone. The facts he has himself laid down, are the only ones by which we should judge him:—from those alone are we entitled to reason, and not from the specious additions advanced by others. If we take, as our guide, the ardent attempts in Harvey's behalf by

his friend Dr. Ent, in his "Apologia pro Circulatione Sanguinis," printed in 1641, or 13 years after Harvey's treatise first appeared, have we any thing beyond assumption on his part, when he goes beyond the text of Harvey, and co-operates with him in all his invectives?—What if he has shown Primrose, Parisanus and others, to be wrong, and even to have outraged Harvey in their observations on his works; this by no means proves that Harvey was correct in all his assumptions, and to be sustained at all hazards, as the sole discoverer of the circulation.

If we look into the edition of Harvey's works, by the London college of 1766, we shall find therein an enumeration of between three and four hundred variations, of all descriptions, between it, assumed as the most perfect, and the first, printed by Harvey at Frankfort, in 1628. But the circumstance of Harvey being himself the editor, renders it essential to inquire into *its* merits, and not those of the college edition, at nearly an interval of a century and a half, when such numerous additions had been made in science. In reading the following remarks, this circumstance should be kept in view; and the question perpetually asked, at every position assumed by Harvey, or any asserted fact or demonstration; whether they were or not, known to others, previously to his publication? If acknowledged to have been known, what exclusive claim can he lay to them?—or, how can those, individually acknowledged to have been advanced by others, become the right of Harvey, because he may have collected them into a focus?

In comparing the strictures of the college edition, in the prefixed life of Harvey, with those of Senac in his treatise on the heart, on the opponents or predecessors of the Harveyan doctrines, we shall find them nearly the same; and differing but little from those of his earliest advocates. The claims of Harvey, awarded to him in his life-time, appear to have been so fully conceded, that scarcely has any inquiry into their correctness been since attempted. Assertions and conjectures, often unproved and unsupported, and sometimes contradictory, take the place of facts; and a chain has been riveted on free investigation, by tradition chiefly, which has continued unbroken to the present day.

In adverting to the circumstances which led Harvey to the inquiries he has presented to us in his treatise, De motu Cordis et Sanguinis, the college speaks in mere conjecture, as the following extract, p. xxiii, will probably prove; and which may or may not be founded in truth, for any thing determinate which can be gathered from it, that is not to be found in preceding writers.

"Haud injucundum fortasse erit quæsivisse unde primum Harveio injecta sit de sanguinis circuitu suspicio. In hac disquisitione, si ordo rerum in Harveii libello expositarum spectetur, nihil verisimilius se colligere posse quis autumet, quam Harveio musculosam cordis naturam contemplanti, ejusdemque valvularum formam atque nexum, sanguinis denique copiam ex corde dissecto cum impetu et celeritate singulis cordis contractionibus prosilientem, statim cogitationem de sanguinis circuitu mentem ejus subiisse. At aliud est verum invenire, aliud idem inventum demonstare: nam profecto sæpe fit ut id, quod primum animum inventoris percusserit, sit in docendo ultimum. *Boyleius* physicus ille celeberrimus, in libro, quem de finibus rerum naturæ conscripsit, narrat Harveium illi dixisse primam lucem sibi sanguinis itinera perlustranti a valvulis venarum Fabricio ab Aquapendente primum observatis effulsisse.* Cum enim ex forma atque nexu

* "Late experiments having shown the use of the blood's circulation, and of the valves in the heart and veins, (which, the famous Dr. Harvey told me, gave him the first hint of his grand discovery,) we at length acknowledge the wisdom of the contrivance, after it had escaped the search of many preceding ages."

*Boyle's Philos. Works. Shaw's ed. 4to. London*, 1725, *vol. I. p.* 11.

"I remember, upon asking our famous Harvey, what induced him to think of a circulation of the blood; he said, that observing the valves in the veins of many parts of the body, so placed, as to give free passage to the blood towards the heart, but to oppose the passage of the venal blood the contrary way; he imagined that so provident a cause as nature had not thus placed so many valves without design: and as no design seemed more probable than that, since the blood could not well, because of the interposing valves, be sent by the veins to the limbs, it should be sent through the arteries and return through the veins, whose valves did not oppose its course that way. Thus, though the ancient anatomists and physicians believed the parts were nourished by the venal blood; the modern writers teach them to be nourished by the blood in its passage through the arteries. Not that they think the blood, which runs through the veins, altogether unfit to supply the parts with that vital liquor; but because they judge the veins to be less fit for this purpose than the arteries; into the latter whereof the blood comes immediately from the left ventricle of the heart, agitated, and spirituous, and, by a brisk impulse, better suited to answer this end.—*Idem. Vol. II. p.* 179.

valvularum pateret sanguinem a corde venarum ductu in singulas corporis partes deduci non posse; nec naturam labore irrito et inani artificio valvulas illas construxisse certum esset; nihil verisimilius excogitari posse sibi visum esse, quam sanguinem a corde arteriarum ramulis quaquaversum deferri, venarumque finibus exceptum in cor rursus reportari; siquidem a venis in cor via pateat, sanguini vero nitenti contra valvulæ opponantur. Hac felici conjectura usus rem omnem experimentorum indicio patefecit."

It may here be remarked, that, although the college give Harvey the credit of telling Boyle the valves were first discovered by F. ab Aquapendente, yet that Boyle actually asserts no such thing in the part quoted. It is true, Harvey himself, in his treatise, as will be hereafter noticed, does ascribe the discovery to him, but denies his knowledge of their use. Is it reasonable to suppose the sagacity of Aquapendente, on seeing the valves, thus allowed to have been his discovery, and not seen merely at second hand, to have been inferior to that of Harvey? But admitting it, we shall be able to demonstrate that, what Aquapendente could not develope, viz., the use of the valves; is satisfactorily noticed by an individual, hitherto unnoticed in the discussion, by either friend or opponent, whose writings were published whilst Harvey was a mere child, and thus forestalled him even in this particular.

It is time, however, to proceed to the Biography of Harvey, together with Friend's remarks, above adverted to, as a preparatory step to the consideration of his treatise.

# BIOGRAPHY

OF

# DR. WILLIAM HARVEY.*

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William Harvey, an eminent English physician, who first discovered the circulation of the blood, was born of a gentleman's family at Folkstone, in Kent, upon the second of April, 1578. At ten years of age he was sent to a grammar school at Canterbury, and at fourteen removed from thence to Caius College, in Cambridge. At the age of nineteen, he travelled through France and Germany to Padua in Italy; where, having studied physic under Eustachius Radius, John Minadous, and the celebrated Hieronymus Fabricius ab Aquapendente, he was created doctor of physic and chirurgery in that university, in 1602. He had a particular regard for this last master; often quotes him, and in terms of the highest respect; and declares, that he was the more willing to publish his book De Motu Cordis, because Fabricius, who had learnedly and accurately delineated in a particular treatise almost all the parts of animals, had left the heart alone untouched. Soon after returning to England, he was incorporated doctor of physic at Cambridge, went to London to practise, and married. In 1604, he was admitted candidate of the college of physicians in London; and three years after admitted fellow. In 1615, he was appointed lecturer of anatomy and chirurgery in that college; and the year after read a course

* Biographical Dict. Vol. VI. London, 1784.

of lectures there, in which he opened his discovery, relating to the circulation of the blood. The original manuscript of these lectures is extant, in the valuable museum of the late Sir Hans Sloane, which was purchased by the parliament, and is entitled Prælectiones anatom. universal. per me Gulielmum Harvæium, medicum Londinensem, anat. et chirurg. professorem. Ann. Dom. 1616. Anno ætatis, 37. Prælect. Apr. 16, 17, 18. In 1628, he published his Exercitatio anatomica de motu Cordis et Sanguinis; and dedicated it to king Charles I. There follows also another dedication to the president and rest of the college of physicians, in which he observes, that he had frequently before, in his anatomical lectures, declared his new opinion concerning the motion and use of the heart, and the circulation of the blood; and for above nine years had confirmed and illustrated it before the college, by reasons and arguments grounded upon ocular demonstration, and defended it from the objections of the most skilful anatomists. This discovery was of such vast importance to the whole art of physic, that as soon as men were satisfied, which they were in a few years, that it could not be contested, a great many put in for the prize themselves; a great many affirmed the discovery to be due to others; unwilling that Dr. Harvey should run away with all the glory. Some asserted that Father Paul was the first discoverer of the circulation; but being too much suspected for heterodoxies already, durst not make it public, for fear of the inquisition. Honoratus Faber professed himself to be the author of that opinion; and Vander Linden, who published an edition of Hippocrates about the middle of the last century, took a great deal of pains to prove, that this father of physic knew the circulation of the blood, and that Dr. Harvey only revived it. But the honour of the discovery has been sufficiently asserted and confirmed to Dr. Harvey; and, says Dr. Friend, "as it was entirely owing to him, so he has explained it with all the clearness imaginable; and though much has been written upon that subject since, I may venture to say, his own book is the shortest, the plainest, and the most convincing of any, as we may be satisfied, if we look into the many apologies, written in defence of the circulation."

In 1632, he was made physician to Charles I., as he had been

before to king James; and adhering to the royal cause upon the breaking out of the civil wars, attended his majesty at the battle of Edge-hill, and thence to Oxford, where, in 1642, he was incorporated doctor of physic. In 1645, the king got him elected warden of Merton college in that university; but upon the surrendering of Oxford the year after to the parliament, he left that office and retired to London. In 1651, he published his book, entitled Exercitationes de generatione animalium; quibus accedunt quædam de parte, de membranis ac humoribus uteri, et de conceptione. This is a curious work, and had certainly been more so, but for some misfortunes, by which his papers perished, during the time of the civil wars. For although he had both leave and an express order from the parliament, to attend his majesty upon his leaving Whitehall, yet his house in London was in his absence plundered of all the furniture; and his Adversaria, with a great number of anatomical observations, relating especially to the generation of insects, were taken away by the savage hands of the rude invader. This loss he lamented several years after; and the reader will be apt to lament it too, when he considers the following pathetic words: "Atque hæc dum agimus, ignoscant mihi niviæ animæ, si summarum injuriarum memor, levem gemitum effudero. Doloris mihi hæc causa est. Cum inter nuperos nostros tumultus, et bella plusquam civilia, serenissimum regem, idque non solum senatus permissione sed et jussu, sequor, rapaces quædam manus non modo ædium mearum supellectilem omnem expilarunt, sed etiam, quæ mihi causa gravior querimoniæ, adversaria mea multorum annorum laboribus parta, e musæo meo summaparunt. Quo factum est, ut observationes plurimæ, præsertim de generatione insectorum, cum reipublicæ literariæ, ausim dicere, detrimento perierint." In 1654, on Michaelmas day, Dr. Harvey was chosen president of the college of physicians in his absence; and coming thither the day after, he acknowledged his great obligation to the electors, for choosing him into a place of the same honour and dignity as if he had been elected to be medicorum omnium apud Anglos princeps. But his age and weakness were so great, that he could not discharge the duty incumbent upon that great office, and therefore he requested them to choose Dr. Prujean, who had deserved so

well of the college. As he had no children, he made the college his heirs, and settled his paternal estate upon them in July following. He had three years before built them a combination-room, a library, and a museum; and, in 1656, he brought the deeds of his estate, and presented them to the college. He was then present at the first feast, instituted by himself, to be continued annually, together with a commemoration-speech in Latin, to be spoken on the 18th of October, in honour of the benefactors to the college; having appointed a handsome stipend for the orator, and also for the keeper of the library and museum, which are still called by his name. He died in June, 1657, and was carried to be interred at Hempsted in Hertfordshire, where a monument is erected to his memory. Not long afterwards a character of him was drawn up, and engraved on a copper-plate, which was put under his picture at college.

We will just mention that Dr. Harvey lived to see his doctrine of the circulation of the blood universally received; and was observed by Mr. Hobbes, to be "the only person that ever had that happiness."

# SOME REMARKS

## ON THE DISCOVERY OF THE CIRCULATION OF THE BLOOD, BY DR. FRIEND.

Extracted from his History of Physic, Vol. I. p. 227.

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I CANNOT omit saying something of one author more, whom we may reckon one of the ancients, though not properly a writer in physic. Nemesius, Bishop of Emissa, who wrote a treatise concerning the nature of man, near the end of the fourth century; because the Oxford editor ascribes two discoveries to him, one of which was the most considerable that ever was made in physic. The first is concerning the bile.*

But there follows a much more material point: and the editor contends, that the circulation of the blood, an invention which the last century so much bragged of, was known to Nemesius, and described in very plain and significant terms, which are these: "The motion of the pulse takes its rise from the heart, and chiefly from the left ventricle of it; the artery is with great vehemence dilated and contracted by a sort of constant harmony and order. While it is dilated, it draws the thinner part of the blood from the next veins, the exhalation or vapour of which blood, is made the aliment of the vital spirit. But while it is contracted, it exhales whatever fumes it has through the whole body, and by secret passages. So that the heart throws out whatever is fuliginous, through the mouth and the nose by expiration."

Upon this single slender proof does the Oxford editor attribute this great discovery of the circulation to Nemesius: and those who have insisted that it was known both to Hippocrates and Galen, have full as good arguments on their side. I will only say this, that from this very description, and from what the same author says of the liver in the same chapter, that it ministers

* This, not being connected with our subject, is omitted.—ED.

nourishment to the body by the veins, one may demonstrably infer, that Nemesius had no idea of the manner in which the circulation of the blood is performed.

I will not enter into a dispute upon this head; but shall only content myself with observing, that the true circulation was not at all rightly understood by a much later writer, and that a very elegant and accurate one, Columbus; who, as he was an excellent anatomist, above one hundred and fifty years ago, with the nicest exactness, explained not only the structure, but the use too, of every part belonging to the heart, excepting a little mistake about some of the valves: and did, in as clear a manner as words could express, show how, by the contraction and dilatation of the heart and mechanism of its vessels, the blood circulates through the lungs, from the cava to the aorta, (nobody, as he says himself, having either observed this, or written any thing of it,) and from thence, into all the parts of the body. In his language, (as to the sense, much indeed the same as we find in Servetus, a contemporary writer, though much more fully explained,) the lungs are for generating vital spirits, and this he describes in the following expressions:—"The windpipe diffuses the air into all parts of the lungs; the lungs mix this air with the blood, which comes from the right ventricle of the heart by the pulmonary artery. The blood by this continual motion of the lungs is agitated, attenuated, and mingled with the air, which air itself, by this collision and rarefaction, is so prepared, that both the blood and air, mixed together, are taken in by the branches of the pulmonary vein, and through its trunk conveyed to the left ventricle of the heart; and they are conveyed hither so well mixed and attenuated, that there is little more left to do for the heart; therefore, after a little further elaboration here, which gives as it were the last hand to the vital spirits, there remains nothing else, than that the heart, by the help of the aorta, should throw and distribute the blood into all the parts of the body." This is literally the sense of this inquisitive anatomist, and we see how exactly consonant to truth his doctrine is; only he stops short here, and does not at all explain how the blood flows from the arteries to the veins. Nay, it is evident from what he says in several places of those vessels, that he did not

in the least comprehend any communication between them. For besides that he assigns the carrying of vital spirits only to the arteries, in another discourse he tells us, that the veins convey the blood from the liver, to all the parts of the body. And *in this point chiefly*, that is, the intercourse between the arteries and the veins, is his doctrine of the circulation deficient; however little it has been understood by those who have writ for or against Harvey.*

Cæsalpinus indeed drops the word anastomosis, (copying, perhaps, from Servetus, whose word it is,) by which he supposes the native heat may pass from the arteries to the veins; but this in the time of sleep only: and, from the sentence immediately following, it is plain, that he had no notion of the circular progress of the blood; for he makes it only move like an Euripus, the very word he uses, in a sort of undulating motion from one extremity of the vessel to the other, which is, indeed, the very idea Hippocrates himself had of the motion of the blood; and Aquapendente, in direct terms, describes the blood as circulating by way of flux and reflux in the arteries. Were we, indeed, to reason from what these writers say concerning the circulation of the blood, both through the heart and through the lungs into the aorta, the conclusion must demonstrably be, that the blood which goes into the aorta must return back into the cava; else how could the constant current, which by their own account runs through the heart and lungs, be maintained? but it is as demonstrable, that they did not perceive this consequence, which naturally and necessarily follows from their own principles.

Neither is this so much to be wondered at; for Columbus and Cæsalpinus might as well go so far, and no farther, as that Aquapendente could discover and describe the valves of the veins, and yet be at the same time ignorant of the true use of them; as it is very plain he was, from his own description of them. As this great discovery *was entirely owing to our countryman*, so he has explained it with all the clearness imaginable;

* It is in this point, chiefly, that we shall equally find Harvey to be deficient; and, therefore, equally to be set aside, as the discoverer of the circulation, upon the same principle that Friend lays down for the rejection of his predecessor.

and though much has been since written upon that subject, I may venture to say his own book is the shortest, the plainest, and the most convincing of any; as we may be satisfied if we look into the many apologies written in defence of the circulation, or have the patience to read the tedious uninstructive treatise of Raymond Vieussens.

This new doctrine of the circulation, however proved beyond all doubt in a demonstrative way, met with great opposition; and the inventor of it was obliged to bear the attacks of numberless adversaries; who, generally, in their answers, showed more a spirit of contradiction, than any force of reasoning. The learned Gassendus, indeed, acted very differently, and behaved with that ingenuity which became a scholar; and though he had formerly very strenuously denied the circulation, and the communication of the chyle with the blood, yet at last was convinced of his error by Pecquet, the discoverer of the receptacle of the chyle, and the tracer out of the thoracic duct in a human body; and as soon as he was convinced, he expressed great joy, that, dying as he was, he had come to the knowledge of these two important discoveries; adding, that he looked upon these two truths, which prove one another, as the two poles, upon which all physic for the future ought to turn.

From this discovery of our great countryman, many improvements, even in the cure of distempers, might be made; he had thought of composing such a work himself, to show the advantages of this doctrine in relation to practice, but was prevented by sickness and death; the design of the architect was very noble, and I wish some of his successors might finish it. At present, I shall hint only at two or three particulars, which will convince us of what use a perfect knowledge of the circulation may be to us, if rightly applied in the practical part of our profession. For instance, this doctrine will let us see the reasonableness of tying up the arteries in amputations, as it is practised now by our surgeons, and how much preferable this method is, to that old, painful, and cruel one, of stopping the blood by cauteries, caustics, or escharotics alone. Besides avoiding an extreme torment in this case, we know that the blood, by the laws of its motion, must continually bound against the eschar of the divided vessel

with such a force, as nothing besides a ligature can well resist. The invention* of this method was owing to Parey, who, as he says himself, had never either seen or heard of its being practised before, but had taken the hint of it from a passage in Galen concerning wounds, and made the experiment of it with such success, that he thinks it came into his head by inspiration. And no doubt, without inspiration, if we would revolve often in our thoughts what the ancient physicians have written upon any particular subject, new hints would occur to us, not only in relation to that very case, but what may be applied, as in this instance from Parey, to some other. This practice of Parey was indeed *before* the discovery of the circulation; but yet I question whether or no it would have been so much in vogue, did not this doctrine evidently convince men of the usefulness of it: as we may have leave to infer from observing, that it was a practice but slowly received in several countries, nay, even in France itself, if we may judge by Vigierius's account of it, and but of late years revived, or rather introduced among ourselves. However, the Germans are but little acquainted with it; Hildanus himself speaks but slightly of it; and the Dutch, as Nuck informs us, entirely reject it. This doctrine likewise explains to us, how upon amputation, when the trunk of the artery is cut off, the course of the blood is nevertheless preserved; the lesser arterial branches in this case supply the defect, and by distending themselves gradually to a greater dimension, are able to furnish those parts with what is necessary for motion and nourishment. A problem which can never be solved by any other principles than those of the circulation; and is so far from being an objection, as some ignorant writers make it, against this doctrine, that it is one, and not the least, demonstrative proof of it. Once more, this doctrine at first sight shows us the true method, (as it is now practised amongst our own surgeons, who yield to none either in their skill of anatomy, or the ancient surgery,) of treating aneurisms, which arise upon a puncture; how, instead of

* Here is another *invention* claimed for a modern, who acknowledges he *took the hint* from Galen. Galen, however, gives more than a hint, of the familiar use of ligatures to stop bleeding, and that in more than one place of his writings.

using compression, which seldom stops the current in the artery, we ought, after having made proper ligatures, to divide the vessel; and that we ought not only to tie the artery above the puncture, but below it too, as in the case of a varix, in order to hinder any supply of blood from other branches, which every where almost in the body communicate with one another. It has been observed before, that the practice of another nation is very defective in this point. Galen, and all who follow him, order that revulsion should be always made on the same side, because it may be greater; and the reason they give for it, if it be reason, is because there is a greater consent of the parts on the right side with the right veins, and of the left with the left.

Accordingly, for many years, for almost two centuries, it was as warm a controversy as ever was in physic, whether in a pleurisy, a vein should be opened on the same or on the opposite side. I mention this chiefly to show, that they had no true notion of revulsion before the circulation was demonstrated, whatever some injudicious zealots for the ancients would pretend; and, indeed, it is impossible to understand any thing of this doctrine without a knowledge of the circulation. This, in one moment, lets us see where the strongest revulsion may be made; and as to the manner of bleeding mentioned in a pleurisy, it shows us, that bleeding on the same side does, indeed, somewhat more immediately revell, but that at the same time the difference is so minute, that one would wonder there ever could have been any dispute about it. I may add, in regard to bleeding in general, that the circulation has quite confounded and superseded all those rules, which had been before with so much pains and formality laid down, as to opening, in particular cases, this or that vein; and though the ignorant part of the faculty has lost a good pretence of driving on this way a trade in physic, and of making a mystery, where there is none; those surely who understand their profession must acknowledge, that they have this advantage at least from the circulation, of knowing exactly how indifferent it often is, which vein is made choice of; or if there be any preference, of judging without any hesitation, which vein to choose.

# AN INQUIRY

INTO

THE CLAIMS OF DR. HARVEY TO THE DISCOVERY OF THE CIRCULATION.

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The English translation of Harvey's treatise, de Motu Cordis, to which I have referred in the Preface; and which I have compared with the Latin copy, is only tolerably correct, and very imperfect in parts, as I shall hereafter point out, and it is, moreover, written in the quaint phraseology, and the peculiar orthography of the period. (1673.)—We have here the author's dedication to King Charles I., to whom he was physician, and by whom he appears to have been both highly and deservedly esteemed. To it, I refer, chiefly, for the purpose of introducing a single extract, which, as it so much resembles others, hereafter to be noticed, I shall not at present dilate upon; but only request the reader to keep perpetually in mind, that after *nearly* twenty years, from the *first public promulgation* of his opinions, to the time of printing them in 1628; and, when all opposition to the mere doctrine of a circulation may be regarded as having ceased; Harvey entreats the King, to "accept, according to his accustomed bounty and clemency, *these new things concerning the heart:*" and, as we proceed in the consideration of his book, let the reader equally ask, what are *these new things*, on which Harvey so continually dwells?—and whether *any of them*, to which he refers, were absolutely "unheard of," as he expresses himself, (ch. 8.,) before he made them known. Should it be acknowledged that he has not stated a single circumstance, that, individually considered, cannot be pointed out in previous writers, the question naturally presents itself, for the reply of every mem-

ber of the profession,—To what part, or proportion, of the discovery of the circulation, is Harvey truly entitled?

Following the preceding dedication, we find *another;* addressed to "the President of the College of Physicians in London—and the rest of the Doctors and Physicians, his most loving collegs." In this, we find him stating, that he had opened many times before, his "opinion concerning the motion of the heart, and circulation of the blood,"—that it had been "confirmed by ocular demonstration for nine years and more," before them;—had been "evidenced by reasons, and arguments, freed from the objections of the most learned and skilful anatomists, desired by some, and most earnestly required by others"—adding, that he had "at last set it out in this little book"—which "*only book* does affirm the blood to pass forth and return *through unwonted tracts*, contrary to the received way, through so many ages of years insisted upon, and evidenced by innumerable, and those most famous and learned men, I was greatly afraid to suffer this little book, *other ways perfect some years ago*, either to come abroad, or go beyond seas, lest it might seem an action too full of arrogancy, if I had not first propounded it to you, confirmed it by ocular testimony, answered your doubts and objections, and gotten the President's verdict in my favour; yet I was persuaded if I could maintain what I proposed in the presence of you and our college, having been famous by so many, and so great men, I needed so much the less to be afraid of others, and that only comfort, which for the love of the truth you did grant me, might likewise be hoped for, from all who were philosophers of the same nature," &c., with much more to the same purpose, and terminating a long paragraph, by affirming that all studious, good, and honest men, "seeing they very well know that all men may erre, and many things are found out by chance, which any one may learn of another, an old man of a child, or an understanding man of a fool."

I should not have thought it necessary to refer at all to this dedicatory epistle, except, to prove from it, his full assumption of the claim, in all its bearings, of the discovery of the circulation, without the slightest qualification of the rights of others; and also, for the following plainly expressed language, held by

him, and strongly reiterated in other parts; by which it is conspicuous, that he thought it unnecessary to oppose, by argument or additional proof, himself, any of the objections that had been advanced, either to the fact of a circulation, or as to its originating with him: so far from doing this, and thus sustaining his claims in any respect, it appears evident, that he altogether gave the go-by to all the adversaries of his system. Riolan seems the only individual of his opponents, to whom he addressed a single line upon so interesting and important a subject; and from his language, it would seem, that he really feared him, and was desirous of deprecating the criticism of one of, if not the first anatomist then living. The language of the dedication to which I refer, is as follows: "But, my loving collegs, I had no desire in this Treatise to make a great volume, and to ostentate my memory, and labours, and my readings, in rehearsing, tossing the works, names, and opinions of the authors and writers of anatomy, both because I do not profess to learn and teach anatomy from the axioms of philosophers, but from dissections, and from the fabrick of nature. As likewise that I do not endeavour, nor think it fit, to defraud any of the ancients of the honour due to them, nor provoke any of the moderns; nor do I think it seemly to contest and strive with those that have been excellent in anatomy, and were my teachers. Moreover, I would not willingly lay an aspersion of falsehood upon any that is desirous of the truth, nor blemish any man by accusing him of an error; but I follow the truth only, and have bestowed both my pains and charges to that purpose, that I might bring forth something which might be both acceptable to good men, agreeable to learned men, and profitable to literature."

Such then are the reasons assigned, at the very first printed promulgation of these "*new and unheard of things*," that were to subvert the medical literature of all preceding ages, for not engaging in the defence of his opinions against anatomists and others; many of whom, we may affirm, from his own subsequent confession, he neither had, nor intended to read! And, whether he has not actually defrauded any of the ancients; or, at least, taken no steps to "ostentate *their* memory or labours," must be judged of from the fact, that his work was printed long

after his opinions had been promulgated privately and in lectures; and when many pieces had appeared against them. Surely, possessing such veneration for truth, it might have been expected that Harvey would, at least, have done his "teachers" the simple act of justice, of giving to each one his due; but whether this is the case, will be best judged of by the sequel! In proof of what is above stated, that Harvey never read the writings of his opponents, we have his own testimony, in the following words, from his 2d Exercitation to Riolan, p. 131, when referring to the objections that had been made to his opinions; and which he appears to have severely felt. "It cannot be eschewed but *dogs* will bark, and belch up their surfeits; nor can it be helped, but that the Cynics will be amongst the number of the philosophers: but we must take a special care that they do not bite, nor infect us with their cruel madness, or lest they should with their dogs'-teeth gnaw the very bones or principles of truth!"

This language comports well, it must be allowed, with that which I have extracted above, from his dedicatory epistle to the College!—but he does not diminish in virulence as he proceeds!—nor vindicate his claim to his assertion, of "not blemishing any man." It is true, he does not "lay aspersions" upon any one in particular, by name; but, it will readily be admitted, that his "railing" is of a wholesale description, which might be made to apply in any direction! thus he goes on, "*Detractors, momes, and writers stained with railing, as I never intended to read any of them, (from whom nothing of solidity, nor any thing extraordinary is to be hoped for, but bad words,)* so did I much less *think them worthy* of an answer: let them enjoy their own cursed nature; I believe they will find but a few favourable readers; *neither does God give wisdom* to the *wicked*, which is the most excellent gift, and most to be sought for. Let them rail on still, till they be weary (if not ashamed) of it!"

Harvey, we perceive, here directly avows his not reading the writings of these detractors, momes, and railers; but having omitted to name them, conjecture alone can be advanced as to whom he refers! It is indirectly, therefore, that we may be enabled to fix upon them, and form some judgment respecting them. I cannot say with positive certainty, that I have looked

into their writings, which I unquestionably should have done, had Harvey named them, and I could by any means have procured them; not merely as matter of curiosity, but likewise of real importance, in order to ascertain what they could urge against the doctrines of a circulation, proved by experiments conclusive in their nature, and by no means in opposition, as we hope to show, with the opinions of different writers, through a long series of years. But, are we not entitled to ask, How did Harvey know, *if he never read* their writings, that these men, (mere opponents in a scientific research of infinite importance,) were really the unworthy characters he thus proclaims them to be, and altogether fools, as well as knaves? Was it heresy in them to differ from Harvey, on points, assumed by him, to be both "new and unheard of!"—Was his *ipse dixit* fully to establish the truth of *his* new opinions, without further investigation and research? And were his asserted facts not to be questioned by any one? Surely, when this conduct of Harvey is fairly considered, it will be regarded as arising from some apprehension of a due inquiry into his rights; rather than from that anxious desire of truth which he so much dwells on, whether from himself, or the hands of others! Does the reader perceive, in this fear, (or at least omission,) of consulting his opponents, a desire, fairly to throw down the gauntlet of inquiry? To me, with all my reverence and respect for Harvey, I think he has placed himself, at least, in a suspicious situation, as to the motives of his neglect; and whatever may be said to counteract this impression will, I apprehend, be more and more unavailing, the more it is contemplated! To keep back, from *any cause*, the sentiments of those opposed to him; but more so, if from one, which science has, perhaps, a right to estimate a false pretence, is surely no great recommendation; nor does it comport with the celebrated axiom of "Amicus Plato, &c. sed magis amica veritas!" Flattering the stern and inflexible professor of anatomy in the University of Paris, and equally, with himself, a king's physician; but who cared neither for Harvey, nor for his opinions, any further than, as an anatomist and physiologist, he considered them correct; to those of inferior standing, his harshness and intolerance is conspicuous, by the above quoted philippics, and still more so, when

coupled with the avowal, that he never read them! It is obvious, that he did not coincide in the justice of the adage, "Fas est ab hoste doceri;" and, if this acknowledged omission, under the futile pretences assigned by him, is any evidence of candour; or of modest and earnest desire to learn the opinions of others on a "new and unheard of" subject, of which he claims to be the discoverer; then indeed, it may be considered altogether useless to oppose any dogma in science, or any hypothesis in the discussion of truth! Whether, although so bland in his expressions in the dedicatory epistle; and apparently, so desirous of "not provoking any of the moderns;" he has not bestowed railing for railing, in language by no means equivocal, in that extract from his epistle to Riolan, will scarcely be denied by his warmest advocates! Will they, as they formerly did, advocate his writings in every respect? And may we not ask, whether, like himself, many do not, now, admire and judge of him from simple hear-say; and without having given his writings a single thought, much less a perusal!

This, however, is by no means all. We will admit those detractors, momes, and railers, (without knowing who they were,) on his own assertion, to have been deserving of neglect and scorn, from their strenuous opposition to the new discoveries and doctrines of Harvey. They were, it is to be remembered, his contemporaries, and might be presumed, felt jealous of his fame: but why has he not done a whit more justice to the great body of writers who preceded him? and who of course were opponents, if at all, by anticipation alone? Of this, I think no doubt can be entertained, when we come to mention individual facts in proof. Now, if Harvey knew them not, it proclaims an ignorance, which we cannot credit; and, if he knew, but chose to omit what they may have advanced in their writings, in relation to what he claims exclusively; is it too harsh to say, that it bespeaks a little mind, and a contempt of truth, which have not commonly been associated with his character? I shall be much gratified, if, by any explanation, this unpleasing association can be dissevered.

To the dedicatory epistle of Dr. Harvey succeeds a *preface*, by the apparent editor, Dr. Zachary Wood; wherein we find Harvey's claims thus summarily stated—and containing much other matter, that may be regarded as the echo of those claims; as well as of his complaints, which I have above noticed, and commented on; and which, perhaps, ought more fully to be reviewed, in as much as it is the production of one who appears to have seen nothing but perfection in Harvey; and nought but imperfection in those who differed from him! I shall, however, limit myself to one or two passages; for all will more or less be touched on, as we proceed to analyze the different chapters of Dr. Harvey's book.

Dr. Wood commences with some remarks on opposition to new inventions, and then proceeds thus: "Dr. William Harvey, king's physician, and professor of anatomy in the College of Physicians in London, has set out *a new and unheard of* opinion concerning the motion of the heart, and circulation of the blood, which is briefly thus: First, the ear of the heart contracts itself; in that contraction it thrusts out the blood contained in it, into the ventricle of the heart, which being filled, the heart is dilated, and straightwayes it contracts the ventricles, and makes a pulsation, by which pulsation it thrusts forth the blood thrown into it, into the arteries out of the left ventricle, and out of the right into the lungs through the vena arteriosa, from whence immediately it is snatched into the left ventricle through the arteria venosa, and by it driven out into the aorta, and so afterwards into the whole body through the arteries; the blood so driven out into the habit of the body, passes from the arteries again into the veins, and returns into the vena cava, and from it into the right ear of the heart, and then into the right ventricle, and so afterwards it passes through the same circle as before, and so continually, from whence he calls that motion of the blood, Circulation."—"Truly, (adds Dr. Wood,) a bold man indeed, O disturber of the quiet of physicians! O seditious citizen of the physical commonwealth! who *first of all* durst oppose an opinion confirmed for so many ages by the consent of all," &c. and, proceeding in a long train of remarks, to prove the propriety of what no one will doubt, that of advancing new opinions—

provided they are accompanied by adequate proof. "Time," says Dr. Wood, "will blot out the inventions of opinions, and confirm the judgments of truth,"—referring in proof of this to the fact, that Vopiscus Fortunatus Plempius, of Lovain, Doctor of Physic and of Arts, "having testified by speech and writing against the Harveian invention, and endeavoured to refute and explode it; was himself refuted and exploded by the persuasive and forcible reasons of Harvey." Now this is all as it should be; since few can withstand the demonstration of the circulation, as laid down by Harvey; and yet many, if they could be persuaded to enter fully on the inquiry, would probably be unwilling to grant to him the exclusive claim of this brilliant discovery, to the full extent in which it has been generally awarded! Whether I shall be successful in pleading for Harvey's predecessors, as dividing the merit with him, must be left to the decision of others: a failure in so doing will, in my opinion, rather prove the weakness of the advocate; but will by no means diminish their claims to a participation.

It may not be improper here, although only in connexion with the preface of Dr. Wood, to state some circumstances in respect to Plempius, who is thus brought forward by him; since Harvey has no where referred to him; and a better opportunity will consequently not present itself; and we shall begin by allowing Plempius to speak for himself. See his Fundamenta Medicinæ, 3d ed. in fol. printed at Lovain in 1654, or 26 years subsequent to Harvey's publication. In lib. 2. p. 128, De Sanguinis Circulatione, he thus expresses himself:—

"Nupèr Anglia novam peperit de motu cordisopinionem, quam invulgavit *Gulielmus Harveus* Medicus Regis Angliæ, et Anatomes in Collegio Londinensi Professor edito eâ de re peculiari libello." Here he gives the route of the circulation, and then continues: "Hoc suum commentum multis verisimilibus rationibus adstruit; adèo ut jam multis doctis hodiè adridere incipiat, nomineturque honoris causa à quodam populari suo, *circulator microcosmi* ad distinctionem alterius Angli, qui primus macrocosmum circuivit. Primùm mihi inventum hoc non placuit, quod et voce et scripto publicè testatus sum; sed dum posteà ei refutando et explodendo vehementiùs incumbo, refutor ipse et explodor; adeò sunt rationes ejus non persuadentes, sed cogentes: diligenter omnes examinavi, et in vivis aliquot canibus eum in finem à me dissectis verissimas comperi; hoc ut facerem, monitus quoque à Clariss. Walæo Professore Leidensi, cujus viri candido sedatoque

ingenio et judicio plurimum tribuo, multumque in hac ipsa materiâ acceptum refero."

I shall merely notice, for the sake of those who may wish to consult the views of Walæus on this subject; that they will find them in two letters addressed to the celebrated anatomist, T. Bartholine, in 1640; and printed in his treatise on anatomy; as likewise in the Institutiones Anatomicæ of Caspar Bartholine.

In the above quotation from Plempius, we find infinite reason to respect him, as yielding his prejudices to the conviction of truth;—in prosecuting the subject, he notices many particulars, indicative of the *previous and preparatory* knowledge that must have influenced the more correct exposition of the circulation as laid down by Harvey; and by which he appears to have subjected himself to the animadversions of some of the most active opponents of the Harveian doctrines, as we shall presently mention. Among the particulars he notices, is one, on which Harvey lays very great stress; viz. that in the arm, tied up for bleeding, the veins swell *below* and *not above* the ligature;—showing that the blood must ascend from the hand, and not descend from above; and he then adds, "Eâ certè re adversa Cæsalpinus convictus probavit sanguinem ad cor adscendere; sanguinis tamen circulationem ignoravit." Admitting this to be the case; nevertheless, if it also be admitted that from this established fact, Cæsalpinus judged the blood to ascend to the heart; surely it is to be viewed as an important link in the chain of the circulation, thus presented to Harvey, by a man who died (1603) before he had matured his views respecting it; and on which he founds some of his strongest arguments in support of his claim.

Whilst Plempius (p. 131) fully maintains the anastomoses of the arteries and veins, as one of their modes of communication, ("*Primo*, facillimè per anastomoses, quibus arteriæ venis junguntur: arteriæ enim quibusdam osculis in venas perviæ sunt," &c.) and mentions facts in proof; he very properly states it to have been known to Galen, and quotes him in the following words, (from lib. 3. cap. 15. de Nat. facult.) "Hæc venarum atque arteriarum anastomosis Galeno etiam nota fuit, nam inquit, 'Si multis amplisque arteriis præcisis jugulare per eas animal velis, invenies ejus *venas æ uè atque arterias vacuatas*; quod

*sanè nunquam fieret, nisi inter se* haberent altera in alteram ora reclusa.'" And again, lib. 6. de off. part. cap. 10: "*In toto corpore mutua est anastomosis* atque oscillorum apertio *arteriis simul et venis*, transsumuntque ex sese pariter sanguinem et spiritum per invisibiles quasdam atque angustas planè vias." A second mode of communication is maintained by Plempius, and it is that, which Harvey has adopted singly—divesting himself thereby, altogether, of the advantages of the *Galenian anastomosis;* which, we shall strive hereafter to prove, it was his great endeavour to avoid, so as to divest thereby Galen of one important support of *his* ideas of a circulation. "*Secundò*," says Plempius, "sanguis ex arteriis *per ipsam etiam carnem venas subire potest*," giving a very inadequate proof of it, I think, and, as I believe, he partly judged himself, from the expressions following: "Neque *hoc dictu absonum* est existimandum." But he had no doubt of the fact itself, since he repeatedly renews the assertion,—thus, p. 131, when speaking of the non-pulsation of the veins, he says, "Nec propterea Venæ debent pulsare, etsi sanguinem ex arteriis continenter accipiant; *nam non accipiant sanguinem* ex arteriis *per directos canales* et cum impulsu; sed per *modum transsudationis aut colationis per partium substantiam*, vel per angustas anastomoses."

If Harvey really accredited this porous infiltration of the blood, and this alone, as we believe was the fact; he, certainly, was but very partially acquainted with the most important part of the circulation; and has failed like others in the full attainment of his object: for, if the mode of intercommunication between the arteries and veins *was not truly determined* by him; how can he be hailed as the exclusive discoverer? For, it may with safety be affirmed, that even now, after two hundred years of controversy between the *anastomoses of Galen*, and the *porosities of Harvey*, the dispute is not settled; and probably never will be; since it appertains to a mere point as it were in the body, too small for even microscopic certainty; and, with Butler, we may well repeat, that

> He needs optics sharp, I ween,
> Who sees what is not to be seen.

Conjecture may be busy—but it is not proof: and, if Harvey has

rendered his no stronger; nay, not even as strong as Galen's, even *without* the co-operation of injections and microscopes, how can we possibly assent to his exclusive claims to a complete discovery? On the subject of the pores, much remains to be said, for it appears to have been one of the hobby-horses that Harvey rode, and we shall repeatedly have to revert to it.

At p. 149, Plempius, speaking of the veins, mentions, as worthy of note, certain membranes in their cavity, "quas valvulas vocavit *Aquapendens earum inventor*"—the which are not unlike those that are found in the heart, and they "impediunt ne sanguis refluat." That Aquapendente *was unacquainted with* their use, we are told by Harvey; but Plempius, with an opening so full, to give the whole credit to Harvey, says not a word that would suggest the idea of his ignorance in this particular. Although, amongst the particular points for which the ancients have been ridiculed, and their knowledge of a circulation absolutely denied; one is, that nutrition was ascribed by them to the *veins;* yet here, we shall see, that Plempius accredited this error, at least, in part; and that Harvey was, probably, quite as fully imbued with it, we shall show hereafter; evincing that, if even allowing him full credit for his discovery of the circulation; it added nothing to the correctness of his physiology! Plempius says, "*De usu venarum convenit inter omnes, quod sanguinem pro totius nutritione deferant et distribuant.*" And again, in this chapter, on the veins, he adverts to the passage of the blood *to them*, by the pores of the flesh: "Nam carnes non trahunt ex venis sanguinem, sed *venæ ex carnibus* trahunt. Hoc nos docet sanguinis circulatio: *impellitur nempe ex arteriis sanguis in carnes;* hinc in venas currit: ita ut carnes præcipuè nutriuntur sanguine, qui ex arteriis venit; est enim idem atque ille qui venis concluditur: sed tamen in transitu carnes ex venis quoque aliquod emulgent." I quote this, not to disparage either Plempius or Harvey; for I believe, that even now, we know but little more on the subject of nutrition than either of them: but merely to repeat, that a similar impression, on the part of Galen, of *venous nutrition*, has been one strong argument against his knowledge of a circulation. I shall, in another part of this essay, endeavour to prove, that

Galen, in this particular, knew better what he was about, than either of the individuals mentioned.

At p. 170, Plempius takes up the consideration of the vital faculty, which he considers as chiefly residing in the heart, and as being of a double character. This leads of course to a notice of the motion of the heart and arteries; and to a view of the Harveian opinions in this respect—of which we shall elsewhere take notice. It is merely now adverted to, to state that we have in this chapter, a long and interesting letter of Feb. 15, 1638; from the celebrated Des Cartes to Plempius—which is worth perusal. Aristotle, it seems, ascribed the pulsific faculty to the heat of the blood; and, as Plempius says, he thus endeavoured to take it from nature. Plempius sustains the opinion that this pulsific faculty "in corde toto residet," and he notices this ancient dogma of Aristotle, as having been nearly subverted by Galen, and exploded from the schools, "until, of late," adds he, "William Harvey, physician to the English king, and the most ingenious Cartesius, a noble Frenchman, have attempted to restore it." It would appear that this led to a correspondence, and the Cartesian letters thus are introduced. I should not have thought of referring to these particulars, had it not been of high importance in another part of this essay, in relation to the consideration of an experiment detailed by Galen, and which is noticed by a vast number of writers, previous to the time of Harvey, when treating of the interesting topic of the pulse. The use I propose to make of it will be seen, when I come to mention it, as he speaks of it; when I shall give the experiment in Galen's own words, that no mistake may be made. At present I shall merely add, that it consisted in putting a pipe into an artery, and then tying the artery upon it, so as to still keep up the circulation through the included pipe, although the pulse will not be felt beyond it. A rude sketch is given of this arrangement by Des Cartes, and commented upon. With all this we have nothing to do at present: the reader will please to keep in mind the experiment, and the circumstance of the correspondence of Plempius with Des Cartes, for both will be the objects of future remark.

Before taking leave of Plempius (V. F.) it will not be altogether irrelevant to state, that Dr. James Primrose, of London,

who was one of the first, and most uniform opponents of Dr. Harvey, seems to have been also a thorn in the side of *Vopiscus* F. Plempius. We do not learn this from any notice from Vopiscus himself: but, he appears to have received from Harvey's and Plempius' advocates the most extraordinary castigation, in terms of reprobation, altogether unprecedented, and in language unfit for scientific researches! Whether he deserved it, I know not: for his writings I have never seen; excepting some of the quotations which are made from his attack on Plempius, in the works alluded to; and as a few of them are in a measure connected with our subject, I shall not hesitate to mention them. Among his writings, a catalogue of which is given by Vanderlinden; from which I should judge him by no means a contemptible or ignoble adversary; we perceive one, entitled, "Destructio Fundamentorum Medicinæ Vopisci Fortunati Plempii, ubi breviter 400 ipsius errores demonstrantur." This was printed in 1657, three years after Plempius, and is that which gave rise to two replies; *the one* by G. L. Blasius in 1659, entitled, "Impetus Primrosii in Plempium retusus." The other, by Francis Plempius, nephew to Fortunatus, entitled, "Munitio Fundamentorum V. F. Plempii, adversus J. Primrosium," and printed in the same year with the preceding. If Primrose was wanting in urbanity to the elder Plempius; who certainly was entitled to every respect from his character and standing; these, his advocates, are perfectly ferocious. Blasius, especially, much to our edification, not satisfied with calling him the Princeps Zoilorum, tells him in various places, "tua illatio nullius est considerationis"—"miror non erubescas"—"falsum est." What led to all this, may be difficult to say, and still more so to vindicate. In referring to that part of V. F. P.'s works, that have connexion with the circulation, Primrose says, "Nunc ad circulationem sanguinis est deveniendum, de qua sat multa scripsi, et cum rationibus multis et experimentis obruissem circulatores,* *illi mihi contumelias reponunt.*" This language would seem as if he had not been the first assailant. It serves also to explain in

* This term, was applied by Primrose, jocularly, to designate the advocates of the circulation: his castigators have however assumed it, as if he meant to speak of them as quacks, which the word implies.

some measure, the unmeasured words and language of Harvey, to which I have already referred.

Francis Plempius is not more mild than Blasius; and perhaps, as a nephew, may be more readily excused. He gives us, almost immediately, a philippic that partly opens the source of contention, in the following words, p. 2.: "Primum anno 1630, adgressus es Gulielmum Harveum Regis archiatrum, Angliæ decus et ornamentum, qui ut aquam in macrocosmo, ita in microcosmo sanguinem in orbem ire demonstravit. Quæ aggressio quam infeliciter tibi cesserit, omnes videmus; quidquid etiamnum manibus pedibusque contra agites. Quid vero Harvæus? quid? non pluris tuum scriptum fecit, quam Dionis gry. Imbecilla tua argumenta in contrarium adducta nullo responso dignatus est; despexit ut villaticum canem Cynthia noctu baubantem. Aquila cum cornice non congreditur." He proceeds to repeat much the same, as to his uncle not replying to Primrose; and then goes on to animadvert on his remarks with caustic acrimony. Yet, if I can judge, from what is said, Primrose was often correct; as I shall attempt to show, in that part only, however, where F. Plempius begins his observations on Primrose's animadversions on the circulation, which begin with the above quotation, "Nunc ad circulationem sanguinis est deveniendum," p. 133. We have already noticed V. F. Plempius' views of the return of the blood, "per anastomoses venarum et arteriarum;" adverting to this, Primrose says, p. 143, "Falsum est in brachio ab humero ad extremam manum dari ullum anastomosin inter venas et arterias. Idem dicendum de pede." To this Plempius replies, by an "Audax dictum." Much to the benefit of science, and of the particular object in question, doubtless! But is it not capable of explanation, so as to vindicate both Primrose and Plempius? We have seen that Plempius (Vopiscus) refers to Galen, as maintaining this anastomoses of arteries and veins. Now, the whole appears to me to rest on the different ideas that each party seems to have had of the meaning of the term. If by anastomosis in this case, is meant, such as is seen in the different branches of the arteries or veins, respectively; certainly this is not the fact. That is, the large branches of arteries and veins are not thus conjoined by mutual anastomosis; and Prim-

rose would be correct: but if it was only intended to mean, the ultimate connexion of the *capillary* branches of veins and arteries; then, anatomy seems to bear out the proposition; and Primrose would be wrong. I must, however, think, from the subsequent part of Francis Plempius' explanatory dicta on the point; that he at least, (and doubtless his uncle had similar ideas—see his Fundamenta, p. 131.) affirms this anastomosis of the *large branches* of these different kinds of vessels. "Vidit ne ergò ipse omnium corpora? natura variè ludit in corporum fabricâ; et vix ullum alteri simile omninò est, præsertim in vasorum distributione. Sit tamen ita; in quibusdam non reperiatur anastomosis media ab humero ad extremam usque manum: at in omnibus hoc non obtinet." This, he illustrates by a case he thinks in point: that of a person who fainted on losing a small amount of blood from the *right* arm; but which did not occur when the *left* arm was punctured. Seeking for the cause of this difference, he says, that "Compertum est, in dextro brachio subjacere venæ mediæ arteriam, quæ sive *per anastomosin seu transsudationem* spiritus vitales in venam mitteret, à quibus evacuatis subitus ille virium lapsus." Will the reader imagine that Primrose deserved obloquy for opposing such notions? or can it be asserted, that the knowledge of the circulation had enlightened that generation, even of those who most fully accredited it? All that these vindicators of Harvey asserted, they derived from him; whether, as in the above instance, or when he assumes the blood, "ex arteriis per ipsam carnem venas subire potest!"

In considering the reason, p. 145, "Cur venæ igitur non pulsant?" Primrose says, "Nonne sufficit ut sanguis tam rapidè in eas ex arteriis feratur, *quoquo modo accedat.*" From whence we might suppose that he was not partial to either opinion. But Plempius replies, "Nequaquam id sufficit; sed requiritur ad micationem illam, ut per directos canales et cum impulsu sanguis incitetur: venas autem sanguis ingreditur per modum transsudationis aut colationis per partium substantiam, vel per angustas anastomoses." There is then no mistake in this; nor can it be doubted, that this was precisely the sentiment of Harvey himself: which, if admitted, assuredly lessens his claim to be considered as the *sole* discoverer of the circulation; and in a measure

vindicates the rude manner in which his doctrines were by many treated! We must be allowed to pursue this a little further, in order to vindicate Primrose; and to render it probable that he actually comprehended this very particular, better, than either Harvey or Plempius: and that the desecration too commonly poured out upon him, for opposing Harvey, is both unjust and undeserved. It was the opinion of Hippocrates that the different parts, flesh, &c. derived their nourishment from the veins: (φλεβες.) Now by the term φλεβες or vein, was simply meant a canal, or channel of conveyance for fluids of any kind, and hence, equally applicable to both the arteries and veins. In order to distinguish them, however, the artery, from its motion, was called the pulsating phlebs; the vein, nonpulsating phlebs. We may, therefore, without any fear of an absolute contradiction, venture to suppose, that when Hippocrates speaks of the veins affording nourishment, he meant the pulsating veins or arteries: but, contrary to this, adds Primrose, p. 149, "Ait Plempius, venæ trahunt ex carnibus quod ego supra refutavi, nam nutritio impediretur." And may we not ask, was he not correct? Plempius replies with, "Et ego suprà refutationem illam refutavi." No ways discouraged, it would seem, Primrose, disbelieving the Harveian creed, goes on as follows: "Utut sit etiamsi vera foret circulatio, sententia Hippocratis maneret vera, ratione arteriarum, *nam saltem carnes trahent ex arteriis*, quomodò enim ex his in substantiam effluit sanguis, non expulsus, arteriæ enim per anastomoses expellunt in venas; sed carnes ex illis attrahunt sanguinem præparatum, idque per diapedesin, aliàs si expelleretur, in carnibus promiscuè foret quilibet sanguis, nam expulsio non ponit differentiam, sed utile cum inutili expellit." To which Plempius replies, or rather reiterates, "Arteriæ expellunt sanguinem in carnes: neque enim illæ per solas anastomoses adigunt sanguinem in venas, *sed etiam in carnes*, in quas inseruntur, *et ex carnibus hauriunt venæ*," &c. So little had the full developement of the circulation had effect, in determining the disputes on points of physiology, immediately dependent upon it, even after a lapse of several years!

No neutrality, it seems, was permitted in the consideration of the circulation! "He that is not with me, is against me!" was the war-cry of the Harveians; and hard names were hurled by

the opposing *inquirers after truth*, at one another, too often, it may be feared, at the expense of judgment and discretion! Few persons are fond of such unscientific warfare; and hence, of the vast numbers of the profession, comparatively few have left us any documents on either side: but the Harveians being most numerous, have carried their point; and raised an idol for the medical profession, that has been tolerated, without his claims having ever been, I think, fairly tested or examined. At p. 133, Plempius thus anathematizes all who may differ from the party: "Qui adversus hunc sanguinis motum commentaria ediderunt, umbras modò rerum dant, non lucem afferunt, et apponunt Promethei coria intùs inania." In all that I can gather from this bitter contest, I cannot but think, that, although erroneous in his views, Primrose was, as much or more, governed in his opposition by a desire of truth, than those who attempted to run him aground, by fair and unfair means.

Plempius proceeds in the next page to state, that "Quapropter *nulli* Harveus respondere dignatus est; res ipsa et natura patulo ore refellit omnes." Now all this is true, and need scarcely have been noticed by Plempius, since we have already seen, that Harvey was scarcely, if at all, inferior to him, or to Blasius, in the art of calling names! We shall merely add, that Plempius thinks these opponents ought neither to be noticed by writing or speaking; but that they should be left as irremediable: adding some caustic remarks, lest these poor wretches should too much rejoice at so unexpected an act of grace.

The refrigeration of the blood *at a distance from the heart*, as we shall find most strongly laid down by Harvey, so we find it no less powerfully enforced by this writer! The reader will here recollect, that the ancients supposed the lungs were formed, as bellows, to ventilate the blood and cool it. Harvey, we shall find, cools it at an antipodean distance from them. It appears extraordinary, but so it is, that whenever two extremes existed, in which, by assuming the one, he could thereby separate himself entirely from the views of Galen, he uniformly did so; without however canvassing the matter, or informing us, by substantial reasons, why he differed from his great predecessor; or wherein he was superior, or Galen defective, in proof of their

respective doctrines. Such is the case in the instance before us! Such is the case, also, in his selection of the passage of the blood from the arteries to the veins, through the porosities or parenchyma of the parts, which idea was reprobated by Galen; whilst he opposes Galen's views of an anastomosis of those vessels; or *makes use of them*, according to one of his strenuous advocates, Dr. J. De Back, "*only as it may further his purpose!*"

If a *complete and full discovery* of the circulation led Harvey and his followers to the absurdities so frequent in their writings; that discovery could be of but little importance, or must have misled them like an ignis fatuus! Primrose had opposed the circulation, from a belief, among other ideas, that "Nutritioni nocet, quæ quiete perficitur." "Not so," says Plempius, p. 135, "so far from hurting it, it tends to promote it; for without this motion of the blood, nutrition in many parts could not be accomplished:" and why? "Nam in *extremis artubus sanguis refrigeratur, crassescit, densatur*, fitque nutriendis illis membris ineptus, nisi *ad fontem caloris et focum suum* revertens, denuò incalescat!" This, we shall hereafter have occasion to touch upon, again, in regard to Harvey himself, the great luminary, around whom, as a minor satellite, Plempius revolves, with reflected light! I must, however, with one further remark, now leave Plempius. It is merely to show how grateful to his feelings was every possible weapon, by which he could oppose the unfortunate Primrose. The fate of St. Sebastian and of St. Lawrence combined; nay, if even that of Marsyas or of St. Bartholomew could have been superadded, the momes and detractors of Harvey would have experienced, had it depended solely on the will of some of his satellites! Even the gratification of criticising the Latin of Primrose is more than once greedily seized upon; thus, at p. 140, Primrose speaks of opening a vein, "intra duas *ligaturas* propinquas." Plempius says, "Quod attinet ad incisionem venæ factam inter duo propinqua *ligamina*: (*ligaturas dixit ipse barbaro vocabubo*," &c.); it may admit nevertheless of a question, which of the two is the preferable term; whilst the paltry criticism sufficiently indicates the malevolent feelings of the writer.

In this very passage, it may be remarked, that we again find

the idea upheld, of an anastomosis between the *large branches* of arteries and veins. "Nisi inter duo ista ligamina sit arteriæ et venæ anastomosis; quod interdum contingit: tum enim illa arteria suppeditabit venæ sanguinem emittendum."

I have thus, in advance, given some insight into the malevolent feelings and acrimonious replications of a few of the warmest and most intolerant friends of Harvey's opinions to those who thought fit to oppose them. Could it however be deemed a heresy in medicine, at that time, to canvass fully, and even with harshness, doctrines asserted to be new and before unheard of! doctrines subversive, as it was said, of Galen, who so long and so deservedly sustained the rank of prime minister in the Temple of Esculapius! If delivered from the fetters of Galenical rule; surely, it will not be found that the mind of man was much more free under the shackles of Harveian despotism! During a period of ten or twelve centuries, the doctrines of Galen bore unlimited sway. His faults and his perfections, are now, alike unknown: his interesting volumes are, indeed, (shame on the profession!) truly a dead letter! and what does that profession give in their place? I pause for a reply; and now proceed in my investigation.

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A Procœmium or Preface, by Dr. Harvey, follows that of Dr. Wood, which we have thus considered. In this, the author's intention appears, from the heading to it, viz., "By which is demonstrated, that those things which are already written concerning the motion and use of the heart and arteries, are not firm."

I must here entreat the reader's patience, for the apparent irregularity and repetition in which this book of Dr. Harvey's is investigated. It was, however, unavoidable. A hint gave me, perhaps, notice of somewhat that might be gained from other authority; and by whom again reference was given to another; prolonging thus my research, but giving some interesting detail, which I have not kept back: but which, whilst throwing some light upon a ground, held to be so sacred, as to have been but rarely trod; has yet from that cause been rendered almost in-

capable of being ranged in any thing like order, either of place or time; but we hope that its perspicuity and connexion with the subject will not be less apparent.

From the very beginning of the preface, Harvey says, it will be worth while, "seeing we are thinking of the motion, pulse, use, action, and utility of the heart and arteries, to unfold such things as have been published by others; to take notice of those things which have been commonly spoken and taught, that those things which have been rightly spoken may be confirmed, and those which are false both by anatomical dissection, manifold experience, and diligent and accurate observation, may be mended." This sounds admirably; but how the pledge is redeemed, is yet to be seen. "Almost all anatomists, physicians, and philosophers to this day," adds he, "do affirm with Galen, that the use of pulsation is the same with that of respiration, and that they differ only in one thing—that one flows from the animal faculty, and the other from the vital, being alike in all other things, either as touching their utility, or manner of motion;" and that, "because that the pulse of the heart and arteries is not sufficient to fan, and refrigerate, that the lungs were made about the heart," &c.

Here then we perceive, that Harvey admits that publications had been made on the subjects mentioned; but with one or two exceptions, he has made no mention of the writers or their works; no reference is given, by which to follow out their respective views and opinions, so as to enable us to judge for ourselves, and not with the spectacles of himself alone, of their real character and bearing. The clause of "almost all anatomists," &c. is one of wide extent; but who the individuals are to whom he alludes, he no where specifies, except it be Galen, Aquapendente, and Columbus, as we shall presently see—a small proportion, it must be admitted, of "almost all anatomists, philosophers, and physicians, to his day!" we must be content, however, to follow him as well as we can; and as he alludes to H. Fab. ab Aquapendente, we shall begin with him. His opinion, referred to, is given, he tells us, "in his book of Respiration, which he *has newly* set out." Something may probably be gained, in our estimation of relative views, by a regard to time, as developed by a

reference to dates. Now the first edition of Harvey's treatise *De Motu Cordis et Sanguinis*, was printed in 1628. He was appointed Professor of Anatomy to the College of Physicians in London, in 1615—and he delivered his first course in 1616, when he "opened *his discovery* relating to the circulation of the blood." See his Biography. But Fabricius' treatise, "de Respiratione et ejus instrumentis," was published at Padua in 1615, that is, thirteen years prior to Harvey's. Whether the term "newly set out," is quite appropriate, may be variously appreciated; we should scarcely, I apprehend, now call a book new, that had been thirteen years in existence! Be this as it may, why he has said no more about it, is very surprising; as is also his extreme brevity in the remark he makes, p. 76, respecting that same learned man's knowledge of the valves of the veins, that he "did not understand the use of them," especially when he was elsewhere so surprisingly prolix in his criticisms on the same writer, relating to his treatise "De formatione Ovi et Pulli." It must be here remembered that Fabricius was one of his teachers, whom, in his dedication to the college, he says he thinks it unseemly to contest and strive with! one would naturally be led to conclude, that Harvey would here have delighted to dwell on the merits of his excellent master in anatomy; and to have exposed the slightest claim which he might have, to any participation in a subject, which his various writings indicate to have had a large share of his attention. To appreciate this remark, I shall here refer to a list of them, as given by Vanderlinden, de Scriptis Medicis, p. 420. Some of which appeared in 1603, or about the period of Harvey's graduation, in the school in which Fabricius was professor of anatomy. So that one-fourth of a century preceded the event of his own publication! To say the least of it, there is something singular in this conduct of Harvey: nor can we, I think, doubt, that his master's publications, if not his lectures, which Harvey attended, must have afforded him many hints, that he may have matured and strengthened at a later period.

As to Galen, though he here refers to him, he does not enable us, by any notice, in what part of his voluminous writings, to make a fair estimate of his opinions, ourselves. I shall therefore

be excused for quoting a passage from him, which appears to me, to give a far greater idea of the importance of respiration, than is any where to be discovered in the writings of Harvey. It is from his book "De utilitate Respirationis"—without following up his reasoning on the subject, I shall barely say, that in this, one of his most ingenious and interesting treatises, he proposes the question "Quænam est utilitas Respirationis?" It is a physiological *morceau* of 1600 years' existence, scarcely equalled, and, I think, not surpassed by any answer or explanation that has been given. In replying to the above question, Galen says, "Unquestionably it is of no common character; since we cannot do without it a single moment; and, consequently, that it cannot appertain to any individual action; but must be considered as connected with life itself." This does not look like the confined and limited notion ascribed by Harvey to this great man: and, did time permit, I think it would not be difficult to show, that, with a difference of language from that of present times; he has actually forestalled the late ideas of a decarbonization of the blood by the agency of respiration! It is true, that Harvey goes on to point out some of the discrepancy of opinion of the above physicians and of Galen, which last, he tells us, "wrote a book, that blood was naturally contained in the arteries, and *nothing but blood; that there is neither spirit nor air*, as from reasons and experiments in the same book we may easily gather." Let this admission of Harvey not be forgotten by the reader! I do not think that Harvey has at all demonstrated *this fact*, better than Galen has done, in the book adverted to, by all the posita he assumes. In truth, some of his most powerful are taken from Galen; yet, in part, they are apparently perverted to the benefit of Harvey! Thus, when mentioning sundry facts to disprove the presence of air or spirits in the arteries, (although we shall hereafter find him speaking of them, as if they did contain them, and that repeatedly,) Harvey says, "and how comes it to pass, that if you tie the arteries, the parts are not only nummed, cold, and look pale, but at last leave off to be nourished? Which happens," adds he, "*according to Galen*, because they are also deprived of the heat which did flow from above out of the heart." Now this idea of Galen he apparently adopts; and yet

soon after, in reference to other particulars, he says, p. 4, that "these opinions seem to quarrel with one another, and to refute each other, *insomuch that all* are not undeservedly suspected." It seems as if Harvey was blowing hot and cold: for he immediately adverts to an experiment of Galen from the same book, to prove, "that by a great and forcible profusion, the *whole mass of blood* will be exhausted in the space of half an hour." "The experiment of Galen," [proving that blood *only* is contained in the artery,] says he, "is thus: bind the arterie at both ends with a little cord, and cutting it up in length, in the middle you shall find, in that place which is comprehended betwixt the two ligatures, nothing but blood, *and so does he prove* that it contains only blood." And he then adds: "We may argue likewise in the same manner; if you find the same blood in the arteries which is in the veins, being bound and cut up after the same manner, as I have often *tried in dead men*, and in other creatures, by the same reason we may likewise conclude, that the arteries do contain the same blood with the veins; and *nothing but the same blood*."* Is it not strange, that, impugning Galen for the idea of spirits in the arteries as well as in the blood, (although denied by that illustrious physician;) and drawing unfavourable deductions therefrom; that Harvey should, as I have above stated, advocate, himself, *the very same error*, if words have any meaning? See the continuation of this very paragraph, as well as numerous passages in the different chapters of his treatise. In imitating the experiment of Galen, we observe that Harvey arrives at the same conclusions; and this very experiment may indeed be considered as one, and a very important link in the chain of the circulation, which must necessarily be adjudged to Galen. At p. 6, Harvey details a very interesting experiment of Galen, from the same treatise above adverted to; and which experiment appears to have strongly excited the attention of many writers, both before and after Harvey, as we shall repeatedly show. It is intended to subserve sundry speculations relating to the pulsation of the

* Is it, however, *the same* blood that is found in these different vessels in all and every particular? that is, are venous and arterial blood identical? Surely not, nor has Galen asserted it, as Harvey has!

arteries. It is of some importance, in this examination of his claim; and of immediate interest, because it *deeply involves Harvey in an unjustifiable contradiction*, both in relation to himself, and likewise to Galen, if I have fully comprehended him; of which, indeed, I have not the remotest doubt; and I consider it just, both to him and to Galen, to prove this beyond a cavil. I shall quote, therefore, his own words, (p. 10,) and not those of his translator: "Arteriam nudatam secundum longitudinem incidit, calamumque vel concavam perviam fistulam immittit, quo et sanguis exilire non possit, et vulnus obturetur. Quoad usque, inquit sic se habet, arteria tota pulsabit, cum primum vero obductum filum super arteriam et fistulam, in laqueum contrahens arteriæ tunicas, calamo obstrinxeris; non amplius arteriam ultra laqueum palpitare videbis." I ask the reader if this be not a very plain statement of a simple experiment recorded by Galen? And what says Harvey respecting it? His words are as follows: "*Nec ego feci experimentum Galeni, nec recte posse fieri vivo corpore, ob impetuosi sanguinis ex arteriis eruptionem puto.*" Here it must clearly appear, that Harvey throws out a suspicion of Galen's accuracy, or rather of the impossibility of performing the experiment, for the reason he assigns; and by which, consequently, the veracity of Galen is called into question; for we can scarcely imagine that he would so circumstantially detail a mere fiction! At all events, Harvey expressly declares that he himself had never tried it. And yet, in his second exercitation to Riolan, (p. 209,) written a few years subsequently, we find him declaring that *he* had performed the experiment, and that Galen had not. Referring to the great authority of Galen with every one, and adverting to this experiment, he goes on to say to Riolan, "Hoc experimentum memoratur a Vesalio, viro anatomes peritissimo; sed neque Vesalius, *neque Galenus dicit, experimentum hoc fuisse ab ipsis, sicut a me probatum;* tantummodo præscribit Vesalius, *consulitque* Galenus veritatis indagandæ studiosis, quo certiores fierent, non cogitans aut intelligens difficultatem illius operis, neque, cum fit, vanitatem." Will this sentence admit of any other construction, than that *he had* performed this experiment, but *that neither Vesalius nor Galen had?* Such is the translation of the Latin,

at p. 132, of the edition I possess: and if this meaning cannot be set aside, how can it be reconciled with his former declaration, that he had not, neither did he think it could be done! Is there not something equivocal in this, to say the very least of it?

Strong as are the various grounds adduced by Harvey, in support of his assumed discovery; it appears to me, that *scarcely one of them*, individually considered, nay, *probably not one*, can be pointed out, as fully and solely appertaining to him, without doing injustice to others, whom he has scarcely, if at all, named or noticed; or when noticed, rather in some way in opposition to their observations! When endeavouring to prostrate the ancient opinion, of blood, and air or spirits, being distributed *separately* by the aorta, (for he supposes or admits that they are so distributed, when *united together*,) he says, (p. 5.) "Albeit the blood in *the arteries* do swell *with greater store of spirits*, yet those *spirits* are to be thought *inseparable* from the blood, as those which are in the veins; and that *blood and spirit make one body, as whey and butter in milk!* or heat and water in warm water," &c. (a goodly illustration, or assimilation!) When, I say, he endeavours to do this; he correctly asks, (p. 9,) "how it comes to pass, that spirits and fumes (fuligines) pass sometimes hither, sometimes thither, *without permistion and confusion?*" and yet, we find him in another place, (p. 88), actually falling into a greater absurdity, viz., where he is speaking of chyle and blood, attracted by certain veins, and returning "through the many branches of them into the porta of the liver, and through it, into the vena cava; so it comes to pass," says Harvey, "that the blood in these veins is imbued with the same colour and consistence, as in the rest, otherwise than many believe: for *we must needs believe, that it very fitly and probably comes to pass*, in the stem or branch of the capular veins, ('*Neque sic duos contrarios motus* in omni capillari earum propagine, *chyli nempe sursum* et *sanguinis deorsum* inconvenienter fieri, necesse est improbabiliter existimare,') that there are *two motions, one of the chylus upwards, another of the blood* downwards;" and so far from doubting this, in the slightest degree, as he had done that of the fumes and spirits; he even asks, whether this is not from a main providence of nature? But we must leave it to the reader to re-

concile these inconsistencies and determine which of the errors, that of the ancients, or that of Harvey, is the most to be pardoned. Even but a few lines further on, contending still against ancient errors on the subject-matter that he is considering, he contradicts, apparently, what he has just admitted—viz., opposite motions in the same vessel: thus, "They will have these (the lungs) to *send fumes from the heart*, and the other, [arteria venalis,] to *send air to the heart* by the same pipe, when notwithstanding nature did not use to *frame one vessel, and one way*, for *such contrary motions* and uses, nor is it ever seen to be so." Certain it is, that Harvey is fully as inconsistent as any of his contemporaries: which I would willingly conceal; had he not so often noticed it in others, and yet availed himself of the circumstance in prejudice to them, when opportunity presented: it is but justice, therefore, to measure him by the standard he himself establishes.

Much as is affirmed respecting Harvey's claims to the *sole discovery* of the circulation; his writings evidence fully, that, however he may have connected and strengthened the disjointed links of a mysterious chain, whose extremities *are yet unknown;* he has fallen into numerous errors respecting many of its most important details; and runs into contradictions in many places. In supporting very ably the passage of the blood by the *pulmonary artery*, from the right to the left side of the heart, instead of through the pores of the septum (mediastini cordis cæcas porositates), as was commonly taught—"By my troth," says he, "there are no such pores, nor can they be demonstrated." And yet this doctrine of porosities is Harvey's hobby, the chief, (nay, we shall prove, I think, it to be,) the only intermedium, by which he contrives to explain the passage of the blood, from the arterial extremities into those of the veins, not only in the lungs, but in every other part of the body.* "Truly," says he, "it is a wonder

* In his first exercitation to Riolan, p. 126, he says, "It is true, indeed, that I did find out of the authority of Galen, and by daily experience to be a refugium the anastomosis of the vessels, yet so great a man as he is, (meaning Riolan,) so diligent, so curious, so expert an anatomist, should have first laid open and shown anastomoses, and those visible and open ones and whirlpools proportionable to the impetuous stream of the whole blood, and the orifices of the branches, (from which he has taken away circulation) before he had rejected those which were most

to me, that they would rather *invent* (!) or make a way through the septum of the heart, which is gross, thick, hard, and most compact, than through the patent vas venosum, or else *through the substance of the lungs*, thin, loose, most soft and spongious." I at first supposed this idea, of making the blood to pass through the spongy substance of the lungs, was intended rather as a mark of comparison of its greater facility, than by the septum, which, he tells us, " is thicker and more compact than any part of the body, except the bones and nerves." (Let anatomists respond to this.) We shall, however, find Harvey attempting, (as well as most of his advocates,) to show, that the blood does absolutely pass, *mediately*, through the porosities or parenchyma of the parts; a doctrine to the full as incredible, we think, as that of the septum; if, especially, we recollect the probability then attached to it, by the well-known existence of the foramen ovale, in the fœtal state. I might readily extend my remarks on this proeme, but it is high time to proceed to the body of the work, and I shall conclude by stating my belief, that he has neither fully "demonstrated" in it, all that he undertook, as to the defects of the things already written, nor has he fully established all the particulars he so sedulously inculcates.

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In proceeding to consider the body of the work itself, it is proposed to follow as nearly as possible the respective order of its chapters; and I have earnestly to solicit the reader's forbearance, when he will probably discover, what he may at first

probable and most open," &c.; proceeding thus, he finally adds, as in derision of both Galen and Riolan: "But perchance I speak too boldly, for neither the learned man, nor Galen himself, could by any experience ever behold the sensible anastomoses, or ever could demonstrate them to the sense."

He tells us he had looked after them with all possible diligence, and was at no little charge and pains in the search of anastomoses, yet he could *never find that any vessel*, viz., the arteries, together with the veins, were joined by their orifices: that there are no anastomoses in the liver, milt, lungs, reins, or any other part of the intrals, with all the pains he was at for the purpose; and that he "dare, therefore, boldly affirm that neither the vena porta has any anastomoses with the cava, nor the veins with the arteries," &c. Yet with this absolute denial of them in every case, we shall find he occasionally employs them, as his friend Dr. De Back says, p. 87, "only as it may further his purpose!"

sight imagine, much useless repetition; but which, in order closely to follow up the subject, it was impossible always to obviate: besides, it is better, in a case like this, to exceed in prolixity, than to prove defective from brevity.

Chapter 1st. is headed thus: "*The Causes which moved the Author to write.*" In it, Harvey proposes the difficulties he had encountered "to find out the use of the motion of the heart," a thing so hard to be attained,* he adds, that, "with Fracastorius, he almost believed that the motion of the heart was known to God alone:" for, "neither could he rightly distinguish, which way the diastole and systole came to be, nor when, nor where, the dilatation and constriction had its existence." Neither did he wonder "at that which And. Laurentius writes, that the motion of the heart was as the ebbing and flowing of Euripus† to

* Hard as it was to Harvey, Galen had, fourteen or fifteen centuries before him, given an explanation of its use and of its motion, to which he was obliged to assent, although he has given no credit to his predecessor for it. In one particular they differ, viz., that Galen has not run into the absurdity that Harvey advocates, of the heart being the organ of hæmatosis.

† *Euripus.*—A narrow sea between Bœotia and Eubœa, which ebbed and flowed seven times in twenty-four hours. Pliny, 2. 47. Or rather oftener or seldomer, as the wind sate. Livy, 28. 6. Hodie, the channel of Negropont. Ainsworth's Thesaur. Ling. Lat. See also No. 70, May, 1833, of the Penny Magazine, p. 169, for a view of the bridge of the Euripus, with a description of the "Channel of the Euripus and the modern town of Egripos," in which the singular and irregular flow of the channel is referred to, and an explanation attempted.

"Moveri cor viscus nobilissimum nemo unquam, nisi amens et mente captus negabit." "Sed perennis illius motus natura et caussa, tot tantisque difficultatum involucris est implicita, ut soli Deo et naturæ cognitam existimarit doctissimus Fracastorius. Ego motus hujus naturam non minus admiratione dignam puto, quam Euripi angusti in Eubœa freti septies interdiu noctuque stato tempore reflui; cujus caussam dum in Chalcide exsularet Aristoteles, cum reddere non potuisset, mœrore contabuit, et mortuus fertur." *Laurentius, Histor. Anat. hum. corp. Frankfort*, 1599, *p.* 352. I cannot omit to remark here, that Harvey no where else mentions the name of Laurentius, nor refers to his writings; although there is scarcely another writer of equal eminence, who has so fully entered into a variety of particulars, connected with the anterior views of his predecessors, on subjects intimately associated with the circulation: I am greatly surprised, that at least a reference to him is pretermitted. His *Controversiæ Anatomicæ*, accompanying his description of the various parts of the body, are of infinite interest; and by no means is that portion of them the least, that treats

Aristotle." At length he did believe he "had hit the nail on the head"—and that since that time (what period he does not state) he had "not been afraid, both privately to my friends, and publicly in my anatomy lectures, to deliver my opinion," which, he adds, pleased some and displeased others. Some checked him, spoke harshly, and found fault with his departure from the precepts and belief of all anatomists. Some said it was new, worth knowing, profitable; and required it to be more plainly delivered to them; so that at length, moved partly by compliance with the request of friends, "and partly by the *malice* of some, who being displeased with what I said, and not understanding it aright, endeavoured to traduce me publicly; I was forced to recommend these things to the press, *that every man might of me, and of the thing itself, deliver his judgment freely.*" All this sounds well; and did not many parts prove, that Harvey gave the go-by to nearly all his opponents, and regarded them, and their "judgments on the thing itself," undeserving of notice, except that of vilifying them by the appellation of detractors, momes, &c., we might judge differently with respect to himself in several particulars! He was the more willing, he tells us, to publish this, "because Hyeronimus ab Aquapendente having learnedly and accurately set down in a particular treatise, almost all the parts of living creatures, left the heart only untouched."

We may be permitted to observe, in relation to this freedom of inquiry, which he seems here to invoke; that to none does he seem to have replied, except to John Riolan, the son, in two Exercitations, that are printed with his works. Of him, we should judge, he felt some apprehension. He dared not class him with those to whom he gives the opprobrious terms of momes and detractors. The younger Riolan appears to have answered him; if, indeed, he was not the person who had actually animadverted upon him. His remarks are entitled "Responsio ad duas Exercitationes anatomicas postremas ejusdem D. Harveii, adversus Riolanum de Circulatione sanguinis;" a work I have been unable to procure. He, or his father, if not both,

on the subject of the pulse and respiration! Harvey appears sedulously to have avoided any reference to any authors of his own period! Why so?

were physicians to the French king, and both highly esteemed for their anatomical attainments. They could not, therefore, well be overlooked by Harvey: but Primrose and others, who are probably the momes and detractors to whom he refers, were but private and humble practitioners.

*Chapter* 2*d.*, is prefaced by "*What manner of motion the heart has in the dissection of living creatures.*" Whilst the 3d. chapter considers the *arteries*, under the same circumstances—and being so closely allied, it will be altogether improper to separate them in our consideration. They are both of high interest, and bespeak great care and attentive observation on the part of Dr. Harvey; but whether absolutely new and unheard of before him, I am unprepared to say, unless I had an opportunity of comparing the writings of Columbus, Cæsalpinus, and others, his predecessors. If altogether his own, still there are one or two remarks which they seem to call for. If I am not under a misapprehension of his meaning, when he says that "in fish and colder animals which have blood, as serpents, frogs, at that time when the heart moves it becomes whitish; when it leaveth motion it appears full of sanguine colour," I should say, that his conception of this sanguine colour and its opposite which he speaks of, is, that it arises from the blood received into, or expelled from, the ventricle itself; and not that which must necessarily fill the muscular fabric itself, from the coronary arteries: and if I am right, I should apprehend his views to be incorrect, for it could scarcely be, that the blood in the ventricle alone could communicate a sanguine appearance through the thickness of its walls.

In reference, moreover, to his speculations as to the motion of the heart, whether belonging to its diastole or systole, the question is perhaps not yet conclusively settled. Harvey however has decided positively on the subject in the following words: "For that motion which is commonly thought the diastole of the heart, is really the systole, and so the proper motion of the heart is not a diastole but a systole, for the heart receives no vigour in the diastole, but in the systole, for then it is extended, moveth, and receiveth vigour." This subject was

matter of great diversity of opinion at that period; and if it is not satisfactorily settled, even now; it can neither strengthen, nor weaken the inquiry into the act of circulation. We might be led perhaps to contest the last clause of the 2d. chapter, thus proposed by Harvey. "Neither is it true which is commonly believed, that the heart by any motion or distention of its own doth draw blood into the ventricles, but that whilst it is moved and bended, the blood is thrust forth, and when it is relaxed and falls, the blood is received;" as he proceeds to point out in the next chapter. If he has accurately given us the result of his numerous observations on the motion of the heart; I cannot readily perceive, that he has enlightened us as to the cause thereof; and, perhaps, in denying the "commonly believed" opinion, "that the heart by any *motion or distention of its own*, doth draw blood into the ventricles," he has deprived himself of at least a possible collaborateur of this unceasing phenomenon. And it is likewise somewhat inconsistent with what he states in the next chapter, p. 26, of the hearts of eels, and some fishes and living creatures, "being tane out, beats without ears, nay, though you cut it in pieces, you shall see the pieces when they are asunder, contract and dilate themselves," &c. Is it possible to imagine, that what thus takes place in the divided fragments of the heart, should not occur in its perfect state? We shall find hereafter that he ascribes an independent power of motion to the blood itself; a fluid devoid of nervous, or any other absolutely direct communication with any part of the body, though essential to the whole: so as even, by some physiologists of that period, to be denied as a part of the body. If then this be the case, or if it actually possesses an innate power of motion, why may not the heart equally possess such power? It is obvious, however, that if the blood possesses it, it would be enabled, of itself, to fill the heart, without any other assistant cause. Now, considering the mode of explanation Harvey adopts, for the passage of the blood from arteries to veins, *mediately*, by the porosities of the flesh, I much wonder he has made no use of such admitted locomotive powers in the blood itself! But what then actually "draws blood into the ventricles," he does not make clear and apparent. Let us however suppose

the heart to be distended by any cause, *apart from merely the impulse* of the blood; would not that fluid necessarily rush into a vacuity thus produced, as air into the expanding lungs; or water into a bladder or bag of caoutchouc, as it expanded by its elastic property? If the blood has not any independent motion, (which Harvey however maintains, but makes no use of;) and if the heart has none either per se, as he affirms to be the case; I really can see but little reason in the explanation he has afforded; for, as he admits that there is no pulsation in the veins, so no power on their part can be presumed to co-operate: and we are as much in the dark on the subject, after his full developement of the circulation, as before that event! He concludes, as Galen had equally, long before him, that the heart impels the blood into the arteries, and that the "pulsation of the arteries arises from the impulsion of blood from the left ventricle;" stating, in proof, some similitudes, that seem not very happily chosen, "as when one blows into a glove, he shall see all the fingers swell up together, and assimilate this pulsation." This idea he soon after repeats, and refers to Aristotle, (3. Anim. c. 9. and de Respiratione, c. 15.) who says, "the blood of all living creatures beats *within their veins*, (meaning the arteries, says Harvey,) and with a continual motion moves every where. So do all the veins beat together, and by turns, because they have their dependence upon the heart," &c. I copy this for the purpose of adding, that Harvey here says, that "We must observe with Galen, that the arteries were named veins by the ancient philosophers," and that, as by his own allowance, the pulsation of the arteries, even by Aristotle, *was ascribed to the impulsion of the heart*, he can have no claim to this link of the chain of circulation! I have another reason, moreover, for adverting to this notice of Harvey, *that the arteries were called veins by* the ancient philosophers; although he, in another part (p. 52.), gives an erroneous view of Galen's explanation, as will there be shown. The notice itself is, however, highly important, and deserving the attention of all those who may feel disposed to undervalue the riches of former writers, too often, probably, from not comprehending their meaning accurately. It was *this very remark* by Harvey, which was a primary cause to lead

me to examine the works of Galen, so soon as I could procure a copy of his immortal writings; in consequence of some remarks, by my respected preceptor and friend, Dr. B. Rush, in his Introductory Lecture of 1806, "On the opinions and modes of practice of Hippocrates." In it, after the warmest panegyric on that venerable sage; he undoes the whole, by sundry objections to him, that may readily be shown to be altogether unfounded. I well know the estimation in which Dr. Rush always held Hippocrates, and the writings ascribed to him; and which, indeed, he fully expresses throughout the lecture itself; especially when he says, "His writings were among the first books I read in medicine; and, as a proof of my partiality for them, permit me to mention, that I translated his Aphorisms into English, before I was twenty years of age."

Without entering fully into a confutation of my venerable master's attack, if so it can be called, on Hippocrates; but, which, if living, he would be the first to approve, if persuaded of its correctness; I shall only remark on that part of it that has reference to the subject immediately on hand, viz., the arteries and veins. "He confounds," says Dr. Rush, "the offices of the arteries and veins, and afterwards the offices of both, with the nerves and ureters." In the year 1829, my Introductory Lecture was intended to vindicate Hippocrates from these misapprehensions, which had been made public, in the same place, twenty-three years previously: and from it, I shall be excused, I hope, for extracting the defence I made in his behalf. It is too long to be here embodied, and will therefore be found in the Appendix. I should not have considered it proper to affix it even there; but for the close connexion it maintains with the subject in question. I shall therefore conclude my remarks on this chapter, with the words of Harvey, in order to identify more fully his precise ideas, that "the pulse of the arteries is nothing but the impulsion of the blood into the arteries."

The 4th chapter proceeds to consider "*What manner of motion the heart, and the ears of it have, in living creatures.*"

Harvey tells us, that, according to G. Bauhin, and J. Riolan, "men very learned, and skilful anatomists, there are four

motions, distinct *both in time and place.*" He differs however from both, and affirms that "there are four motions, *distinct in place*, but not in time; for both the ears move together, and both the ventricles move together, so that there are four motions distinct in place, only at two times, and it is thus," &c.; which is correct, in this, a matter of mere accuracy in observation? I should not have thought it necessary to dwell on this, for one moment, had not Harvey, in a sentence or two following, mentioned as a fact, in the dying away of the heart, what appears to me rather to confirm the opinions of Bauhin and Riolan, than his own: "So the heart first leaves beating, before the ears, so that the ears are said to outlive it: the left ventricle leaves beating *first* of all, *then* its ear, *then* the right ventricle, *last of all*, (which Galen observes,) all the rest giving off and dying, the right ear beats still: so that life seems to remain last of all in the right," &c. Why should not this admitted distinction in death, be equally the truth, in the perfect state of life; as Bauhin and Riolan have asserted? Would not those long-established consecutive movements, independently of other circumstances, be more likely to occur in death, if under the influence of association from habit, than if an opposite state prevailed in the normal condition of the body? Be this as it may, we at all events perceive, that Harvey acknowledges this link, (so far as it may be so deemed,) of the circulation, the *ultimum moriens* of the right auricle, to have been known to Galen: and, consequently, this cannot be a part of the novelty of his newly claimed discovery. One thing of infinite interest, he seems to have observed, that was apparently overlooked by Galen; at least Harvey takes no notice of it; nor have I met with it in my examination of Galen's writings; it will, however, probably be found there; since the fact, by Harvey's admission, is noticed by Aristotle; and could scarcely have failed of attracting the attention of an observer so acute as Galen. It is at p. 27, in the following words: "But besides all these *I have often observed*, that after the heart itself, and even its right ear, had at *the very point of death left off beating, there manifestly remained in the very blood* which is in the right ear, *an obscure motion*, and a kind of inundation and beating." I know not a fact in the whole history of the blood

and its circulation, if really the case, more interesting than this solitary one of Harvey, relating to its independent power of motion! It is not however absolutely insulated as a fact; at least, Harvey immediately mentions one, of a congenerous nature, viz., "A thing of the like nature, in the first generation of a living creature, most evidently appears in a hen's egg, within seven days after her sitting; *first of all there is in it a drop of blood which moves*, as Aristotle likewise observed, which receiving increase, and the chicken being formed in part, the ears of the heart are fashioned, which beating there is always life," &c. Here again we perceive, that this *primitive and independent motion of the blood* itself, as a link in the chain of circulation, was known to Aristotle, nearly two thousand years before Harvey was born, or his "new and unheard of things" were promulgated; and cannot therefore come within his claim. I may, incidentally, here remark, that Aristotle, apparently astonished at the wonderful and mysterious character of this fluid, is led to affirm that it is not a part of the body, and is devoid of feeling, on that very account: "Nec ipse sanguis sensu præditus est: quippe *qui nulla pars sit animalium.*" (De part. Anim. lib. 2. ch. 10.) We shall find one of Harvey's warm advocates speaking in the same manner of the blood—as not belonging to the body: a position unnoticed altogether by Harvey, as well as that of its entire insensibility. Could the wonders of the circulation be altogether unsuspected and unknown by a philosopher, who has, in so many instances proved himself to be of the most observant and inquiring character? and who is here shown to have forestalled Harvey on several important points; and also to have noticed several particulars relative to the blood, of which Harvey was ignorant, or deemed it prudent to suppress them in his writings! What follows the preceding quotation that I have given from Harvey, and more especially in connexion with it, would seem to *impress the character of absurdity* on what he subsequently assumes in opposition to Galen, as to the locality of sanguification. I have already noticed as a remarkable fact, that whenever two explanations could be given on any subject, *Harvey invariably opposes that to which Galen inclines;* even at the risk of choosing the worst of the

two; and of yet, nevertheless, advancing nothing new upon the subject. The present is of this character; and if all these aberrations from the Galenic views are purely accidental, I must deem it still more remarkable. "Within a few days, the body beginning to receive its lineaments, (says Harvey,) then likewise is the body of the heart framed, but *for some days it appears whitish and without blood*, nor doth it beat and move as the rest of the body; as I also have seen in a child after three months, the heart to be also formed, *but whitish, and without blood;* in the ears of which notwithstanding, there was great store of blood, and of a crimson colour:" and again, "It is doubtful too, whether or no before them also (the ears and heart) the *spirit* and *blood* have an obscure beating, which to me it seemed to retain after death;" &c. with these, and other facts admitted by him; how he could venture to fix on the heart, as the organ of hæmatosis; and to oppose Galen's more probable, hepatic location, is certainly curious; it is still more remarkably apparent, however, in his treatise on generation, than in this on the blood: and most of his adherents appear to have advocated the like opinion.*

In this chapter, we perceive one of the circumstances which ought indubitably to have given Harvey a vast superiority over his less fortunate predecessors; viz., in the use of "an optic glass, made for the discovery of the least things." p. 28. Whether it had this influence in his hands, may admit of great doubts; especially in relation to the only point, on which the full and complete idea of a circulation could be considered as incomplete. That is, the *mode of intercommunication* between the extremities of the arteries, and the veins; whether by immediate anastomoses of these vessels;—or mediately through the porosities of the different parts?—Hippocrates appears to have been fully persuaded of an anastomosis, as the following from his treatise *de loc. in homine* (Ed. Fœsius, 409.) will perhaps evince: "Hæ autem omnes venæ (remember that

* "Whether the blood be moved or driven, *or move itself* by its own intrinsical nature, we have spoken sufficiently in our book of the motion of the heart and blood," &c., 2d Exercit. to Riolan, p. 147. Some of these additional ideas of the heart being the organ of hæmatosis, will be found in the Appendix.

venæ mean both arteries and veins,) inter se communicant, et mutuo confluunt."—Galen accredited an anastomosis of the vessels, but objects to a credence in the porosities, as Harvey seems to maintain. Yet Galen, *without glasses*, has better established his views, than Harvey, with them!—This chapter is a very interesting one; yet still, whatever stress may be placed on it in upholding the claims of Harvey, it will leave him, in my opinion, but a small proportion of the extensive, the exclusive demand!

In the 5th chapter, to which we now proceed, we have considered, "*The action and office of the heart.*"

If we are to be governed by words alone, we may probably incline to the opinion, that the chapter commences in error. Harvey here follows up the intent of the last chapter, by an attempt to point out the manner of the motion of the heart, which, says he, will be found to be "after this manner."—"First of all, the ear contracts itself, and in that contraction, throws the blood, with which it abounds, *as the head spring of the veins*, and the cellar and cistern of blood, into the ventricles of the heart, &c." It does not appear from any part of his writings, that Harvey regarded the term, veins, as a generic one, including therein, as the ancients did, both arteries and veins; but that, on the contrary, he limits it to the veins, now, strictly so called. If so, the auricles, or ears, can, with no regard to accuracy, be regarded as the head springs of the veins, as here stated; since they, the veins, receive the blood, mediately or immediately, from the arteries; and pour their contents into arteries, by the intermedium of the auricles and ventricles. And again, the same objection may be advanced to what he says in p. 32, "that it is sufficiently evidenced, that in the beating of the heart, the blood is transfused, and drawn out of the veins, into the arteries, through the ventricles of the heart," since he here altogether omits the intervening auricles! "But," continues he, "this, *all do in some measure grant*, and gather, from the fabric of the heart; and from the figure, place, and use of the portals (valves)." Now, if all agree in this, surely he cannot claim such position as his own: it is true, he qualifies this acknowledgment, of the admission of others, by

affirming, "that divers things are clampered up, which are contrary and inconsistent;" which he ascribes to their stumbling in a dark place, and being dim-sighted. The chief cause of doubt and mistake herein, he ascribes, however, to "the contexture in a man of the heart and lungs; for when they did see the vena arteriosa, and the arteria venosa, coming likewise into the lungs; and there to disappear, it could not sink with them, either how the right ventricle should distribute the blood into the body, or how the left ventricle should draw it out of the vena cava." This Galen's words do testify, adds Harvey, in his book de Plac. Hipp. et Plat. 6. cap. 6. "where he inveighs against Erasistratus, concerning the beginning and use of the veins, and the concoction of the blood." He then quotes from Galen, what he conceives is adequate to prove, that he had rejected an opinion that carried reason with it, "because he could not find a vessel which, from the heart, should distribute the blood into the whole body." It is easy, by taking insulated passages, to make even Galen appear to know less than he did. If the whole of this 6th chapter were accurately translated, I am disposed to think that Harvey would be found to have underrated his views. But, be this as it may, he assumes this opinion, "*as now his own*, and in all things else agreeable to reason, by Galen's own confession," and asks, if at that time, any one "should with his finger have pointed out the great arterie, dispensing the blood from the heart, into the whole body, what would that divine man, most ingenious, and most learned, have answered? I wonder whether he would have said that the arteries distributed spirits, and not blood?" No wonder Harvey should have so promptly seized this argument, in his own behalf! and yet, methinks, it might not have been amiss, for him to have recollected, that but a few pages antecedently, he had himself shown, that the observant Galen had fully demonstrated that the arteries carried blood, and not spirits; and consequently, that he had not altogether comprehended him, from thus limiting his object to a short extract, or from a determination to overlook, what Galen had so fully demonstrated! at least, so far as was requisite to bolster up this intended assumption of Galen's opinion, as his own! for he

immediately adds, that Galen here contradicts himself; and basely (turpiter) denies that, which in *one of his own* books he *stiffly maintains* to be true, *proves it* by many, and strong arguments, and by experiments *demonstrates* it, that *blood is naturally contained in the arteries, and not spirits.*" There is certainly, as Harvey propounds the matter, some apparent discrepancy in the remarks of Galen, which might lead us to apply to him the *aliquando dormitat;* were not the passages so contradictory, as to induce the possibility of misconception, of the one or other. It is not my intention to defend Galen, right or wrong; nor can I indeed undertake it here, in the least; owing to Harvey's neglect, as usual, of affording us reference, directly to the passages he quotes: so that, to follow up a short remark, it becomes requisite to pore over pages of his writings. We perceive, however, which was what I principally had in view, that Harvey cannot claim as new and unheard of, what he acknowledges Galen had demonstrated; and, therefore, that it cannot be considered as a link in the chain of circulation, exclusively his own. Justice to Harvey requires that I should here add the part immediately following that, which has led to these remarks, in order to estimate fully his peculiar views and explanations. "But if that divine man, as he does often in the same place, do grant that all the arteries of the body do arise from the great arterie, and it from the heart, and professing likewise that those three pointed doors, placed in the orifice of the aorta, do hinder the return of the blood into the heart, and that nature had never ordained them for the best of our intralls, unless it had been for some special office; I say, if the father of the Physicians should grant all these things, and in the same very words as he does in his forementioned book, I do not see how he could deny that the great arterie was such a vessel as did carry the blood, after it had received its absolute perfection, out of the heart, into the whole body: or perchance he would still continue to be doubtful, (as all the rest since his time to this very day,) because, not *seeing the contexture of the heart with the lungs,* he was ignorant of the ways by which the blood could be carried into the arteries, which doubt does not a little perplex the anatomists, when always in dissections they find the

arteria venosa and the left ventricle full of thick knotty black blood, so that they are forced to affirm, that the blood swets through the encloser (septum) of the heart from the right ventricle to the left; but this way I have sufficiently refuted already, therefore there must be another way prepared and laid open, which being found, there can, I imagine, be no difficulty, which can hinder any body from granting and confessing those things *which I propounded* before of the pulsation of the heart, and dispensation of the blood by the arteries in the whole body." p. 34.

From this long quotation it clearly appears, that Harvey had no intention of admitting any person whatsoever, into partnership with him, as to this pulmonary portion of the circulation: yet, even allowing all he asks, does it not prove his admission, however, of Galen, and others, having taught and suspected, a *passage of the blood through the septum* of the heart, from the right to the left ventricle;—thus acknowledging a circulation, even if incorrect in the route?—It must be admitted, however, that some excuse existed for this error, in the well known foramen ovale; which sometimes continues open long after birth, and, according to Harvey himself, p. 39, "in some, for many years, if not all their life time, as in the goose, and very many birds." Admitting that Harvey has adequately proved the ignorance of Galen relative to the pulmonary circulation, (which I am not, however, prepared by any means to acquiesce in,) through the pulmonary artery; still, he cannot, by any elucidation given, prevent the rights of others; of one or two of whom, it may be proper to advance their claims to this particular. As this will equally answer in the appendix, to it I must refer the reader, in behalf of Servetus, Cæsalpinus, and others; and continue here to remark, that if Galen erroneously supposed the blood to flow through pores in the septum of the heart, it can scarcely be regarded as a greater error than the view of Harvey in the passage of the blood from the pulmonary artery, to the pulmonary vein, "incessantly through the porosities of the lungs;" a point to be more fully noticed, when we come to the 7th chapter. We must be permitted to repeat, however, that either by anastomoses or

pores,—the pulmonary circulation having been known prior to the time of Harvey, he has no claim to it—nor, by his own acknowledgment, even to the idea of the necessity of the blood reaching the left, from the right side of the heart, and of its then distribution over the whole body, by the great artery, or aorta.

It is essential, in order to comprehend Harvey's account of the union of artery and vein, to enter more into detail on the subject, by giving the particular statements of different writers, both of his own day and subsequently, when the doctrine of the circulation was fully and uniformly accredited; and as it is of importance to know what he himself says in explanation, I shall quote the following *short* exposition, (I cannot call it luminous, or very intelligent, but it is the only one I can discover in his writings; it is in his second letter to Riolan, p. 170.) "Lest it should seem a difficult business, how the blood should *pass through the pores of the parts, and go hither and thither*, I will add one experiment. It happens *after the same manner* to those that are strangled and hanged with a rope, as it does in the tying the arm, that beyond the cord, their face, eyes, lips, tongue, and all the upper parts of their head, are stuffed with very much blood, grow extream red, and swell till they look black; in such a carcase, untying the rope, in whatsoever position you set it, within a very few hours, you shall see all the blood leave the face, and the head, and see it, as it were, fall down, with its own weight, from the upper to the lower parts *through the pores of the skin, and flesh, and the rest of the parts*, and that it fills all the parts below, and the skin chiefly, and colours it with black matter: how much more lively and sprightly the blood is in a living body, and by how much more penetrating it is through the porosities than congealed blood, especially when it is condensed through all the habit of the body, by the cold of death, the ways too being stopt and hindered, so much the more easy and ready is the passage in those that are alive through all the parts!"

Although I cannot perceive any striking analogy between this explanation of a so called *experiment*, and the passage of the blood from the arterial extremities, through the pores of the

parts, into the veins; no doubt, it will be clear as daylight to the partisans of Harvey!—I have only to request the reader to bear in mind, continually, this wonderful demonstration;—whilst I proceed to notice other authorities on the same subject.

The first I shall mention is taken from the "Introductio in universam medicinam" of Doctor M. Alberti, printed at Magdeburg, 1718. [He was a celebrated Professor, "in Regia Fridericianæ Med. Publ. ordin. et Philos. Natur. Extraord. Academ. Cæsar. Nat. Curios. Collegia.] In chap. 5. p. 58. "de Motibus vitalibus," he considers as the principal, the "motus progressivus sanguinis, qui vulgo vocatur circulus sanguinis"—and he refers to Stahl's Schediasma de Æstu maris microcosmici,—a work I have not seen. He proceeds as follows, in short paragraphs:

No. 28. Hic priscis medicis non fuit cognitus, sed recentiorum industriæ et inventioni adscribi debet.

29. Est vero hic motus progrediens, neque in ulla corporis regione subsistens, sed continuo per sanguinem circumiens.

30. Propterea vocatur circulus, quia ex uno puncto effluit, nimerum é corde et in illum iterum influit.

31. Unde subjectum hujus motus esse debet fluidum et non solidum, uti sanguis est: quam primum vero hic spissior redditur, tunc ad hunc progressum ineptior evadit, et lædit proportionem motus ad humores.

32. Ordo vero hujus progressus est, ut é corde sanguis per arterias propellatur, ex arteriis ad universum corpus tam sursum intra caput quam deorsum in reliquum truncum, imprimis vero *in organa cribrosa, aut partes porosas* effundatur, per has partes profluat, ex iisdem intra venas transfluat, et per venas ad cor iterum refluat.

33. Particularis circuitus sanguinis notandus est, quod e dextro cordis ventriculo sanguis intra pulmones per arteriam pulmonalem et per illos in venam pulmonalem, ex hac denique in sinistrum cordis ventriculum profluat.

34. Ob id progressui sanguinis, usus pulmonum per respirationem imprimis maximopere favet, cujus defectum in embryone peculiares viæ supplent.

35. Dignitas hujus motus inter alia momenta ex co elucessit quod in œconomia vitali continuo duret, siquidem cordis motus, nunquam cessat, sed diu noctuque continuat.

No. 36. Sanguis itaque ex sinistro cordis ventriculo per systolem in arterias propellitur, et ad partes advehitur, qui quando rapide per arterias transfluit easdem tunc momentanee extendit, cujus extensionis aut elevationis perceptio medicis pulsus audit.

37. Pulsus itaque non est motus arteriis proprius, qualis vulgo supponitur systalticus, siquidem sensitivæ perceptioni contrariatur, quod in pulsu arteriæ non constringantur, sed dilatentur, quæ dilatatio provenit a sanguine momentanee arterias transeunte, et allidente, indeque distendente.

38. Expellitur itaque sanguis ex arteriis *in partes porosas et reliquas organicas cribrosas: dicuntur vero nobis pori, interstitia fibrarum quæ permeabilia sunt.*

39. Fibræ vero sunt pars solida, flexilis tamen et consistens, non vero cava aut pervia.

40. Unde sanguis non per fibras quæ quandoque cavæ supponuntur, sed per prædicta interstitia sive poros progreditur cujus asserti diversa prostant argumenta.

41. Interea motus cordis systalticus tantum proficit quo sanguis singulas partes ingrediatur.

42. Arteriæ habent suas absolutas extremitates quoad plurimas propagines è quibus sanguis intra partes effluit.

43. Et quo ipse per porosas imprimis partes progrediatur, non efficit, neque sufficit pulsus cordis, sed Tonns partium sive motus alternans constrictorius et relaxatorius.

44. Hac contribuit fluiditas sanguinis et motus voluntarius corporis.

45. Sanguis enim, qui ex arteriis affluit, fluidior est, quam qui in venis deprehenditur, quæ fluiditas et subtilitas sanguini conciliatur, dum per pulmones progreditur, et e ventriculo cordis pellitur.

46. In partibus porosis vero sanguis adhuc magis attenuatur et fluidus redditur, dum per easdem transprimitur et in iisdem conquassatur.!

47. Quid quod in illis partibus magis concalescit, dum inter fibrillas atteritur et proprimitur.

48. Ita calor sanguinis non a nudo motu intestino molecularum sanguinearum, imprimis inflammabilium provenit, sed ab illa allisione, attritione, collisione, appressione et transpressione in et per poros fibrillarum, tanquam per partes solidas.

49. Hic tonus, qui ad promotionem sanguinis per partes porosas concurrit, proportionato ordine alternat cum pulsu: unde qualis pulsus, talis etiam est tonus: loquor vero de gradu et ordine.

50. Sanguinis reditum e capite promovent membranæ capitis, quibus vasa sanguifera intertexta sunt: illæ enim ut nervosæ continuo subtili constrictorio motu gaudent, et vasa venosa comprimentes adjuvant refluxum sanguinis ad cor.

51. E *partibus porosis vero progreditur sanguis in* venas, harumque minutissima ostiola ingreditur.

No. 52. Et quanquam *hic ingressus difficilis* esse ab adversariis et anastomoseos autoribus præsumatur, tamen ille per diversa commoda adjuvatur et promovetur.

53. Et enim commemoratæ difficultates longe magis premunt suppositam totalem anastomosin.

54. Per venas refluit sanguis a partibus ad cor, nisi sola vena portæ excipiatur, per quam sanguis a partibus ad partem iterum progreditur, interea tamen ad refluxum versus cor omnino respicit.

55. Hic progressus sanguinis per venas videtur adhuc majoribus difficultatibus premi, cum in toto spatioso corporis humani trunco per ascensionem profluat, propterea etiam eminenti et evidenti motu indigeat.

56. Interim ad promovendum hunc regressum sanguinis diversa adminicula contribuunt, inter quæ nominari etiam debent *valvulæ venarum*, quarum præsentia tam in corde, quam in venis Harveum permovit, quo circulum sanguinis agnosceret.

57. Quando in aliqua vena valvulæ tales deficiunt, tunc alia subsidia earundem defectum supplent, quod imprimis valet de vena portæ et azygos.

58. Sub hoc paulo tardiori refluxu sanguinis per venas ad cor, ipse sanguis in spissiusculam consistentiam redit.

59. In hac motus progressivi consideratione physiologica, hodiernis ævis floret sententia quædam inter medicos, anatomicis imprimis subtilitatibus inhærentes, de anastomosi vasorum sanguiferorum absoluta.

60. De hac anastomosi traditur, quod sanguis ex arteriis immediate in venas profluat, neque in partes vel porosas vel cribrosas alias effluat.

61. Supponitur enim quod arteriarum fines fint venarum initia, aut per interpositas valvulas in venas continuent, ex qua connexione et convolutione glandularum texturam et compagem credunt, fingunt et statuunt.

62. Et qui vulgo anastomosi in anatomicis et theoreticis favunt, mox a particulari ad universale argumentantur et totalem in corpore humano anastomosin supponunt.

63. Licet vero ex mero, sic dici solito, lusu naturæ ob copiam vasorum in aliquibus partibus, anastomosis occurat, tamen eadem non pertinet ad essentiam corporis, neque absoluta et universalis est.*

64. Hic motus, quem progressivum appellamus non est actus simpliciter mechanicus ex se pendens, aut a corpore et causis atque qualitatibus corporeis proveniens.

65. Sed est actus non alterius principii et causæ efficientis, quam solius naturæ hominis, quod assertum variæ rationes comprobant.

* Here, the author adds in a note, "Conf. Stahlius de mechanismo motus progressivi sanguinis," thes. 3. which I have not seen.

No. 66. Præter sanguinis circulum et progressum, differunt etiam Physiologi et Anatomici de circulatione lymphæ, quæ cum sanguine effluit, per ductum thoracicum vero refluit in sanguinem.

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From this interesting compend, a century later than Harvey's promulgation of his alleged discovery, we find the doctrines of porosities and of anastomoses equally unsettled and undecided! Can that be considered as a discovery, (if it was not negatived by other considerations,) in the solution of which, not only at the time, but ever since, and up to the present period, the *most important*, nay, *the only link*, is still defective! Galen believed in a circulation; but his claims have been contested, because he is said to have advocated the passage of the blood from the right to the left ventricle of the heart, through the unproved and undetected pores of its septum! If Harvey's demonstration is equally unproved; if the pores he employs in every part are yet undetected, although two hundred years have elapsed, and microscopes and injections carried to the highest perfection; why are we to crown this imperfect product, to the entire prostration of every other claimant?

In copying these concise views, I have been thereto induced from the very imperfect manner in which Harvey has treated the subject, both of pores and anastomoses. As the learned Alberti was a warm advocate of Harvey, we may safely presume that Harvey's ideas are here more fully developed; and we notice, that both the doctrines alluded to, had their respective partisans, as at the present day. Which is right, is yet undetermined, as in the days of Harvey; may we not say, of Galen himself? In advocating anastomosis he opposes porosities, as we shall see hereafter: and yet we claim for Harvey an undisputed honour, for an unproved discovery! It is to be hoped that the above short exhibit will not be overlooked by the intelligent inquirer; even should it compel him to *rub up* his Latin! Certain it is, if Harvey has not fully demonstrated the *quo modo* of the passage of the blood from the arteries to veins, the discovery was, in his hands, incomplete; and whatever foundation there may be for his claim in other particulars, this *hiatus maxime deflendus* will as effectually shut him out from the

true character of the discoverer, as it has done in the case of one, infinitely his superior, the illustrious and neglected Galen.

This subject of pores and anastomoses is one of too much importance, in connexion with the Harveian claims, to be concisely passed over; and having, in the course of my investigation, met with much unexpected information from numerous sources, I cannot but imagine that it will interest the reader; and perhaps enable him the more correctly to appreciate the real character and extent of Harvey's rights. We are to recollect, that Harvey is to be judged of, by *then existing* circumstances, and not by that false brilliancy of long continued approbation, without duly weighing either side of the question. His claims have been so long admitted by the profession at large, in every quarter of the world, as incontestible; that its members, now, are but slightly impelled by any motive to run over details that have received the sanction of time; and the unlucky individual who hazards a doubt on the subject, under the deliberate persuasion that he is merely performing an act of justice to neglected merit, will probably be rewarded by the castigation of reviewers, who will not take a step in the inquiry themselves. Without any particular reference to the order of time, I now proceed with some other authorities on this subject.

The Fundamenta Medicinæ reformatæ physico-anatomica, of F. Zyphæus, printed at Brussels in 1683.—The second edit. of 1687, from which I quote, after pointing out, in his chapter "de circulatione Sanguinis," several facts in proof of a circulation, thus proceeds, (p. 190.) to demonstrate the "modus circulandi;" and after stating its usually admitted route from the right to the left side of the heart, then proceeds to notice its dispersion by the aorta,

"per totum corpus, ex qua denuo in correspondentes Venæ Portæ et Cavæ ramos, ad auriculam dextram, velut ante, ducitur,—idque *secundum dicta quatuor modis*, nempè *immediatè*, *per inosculationem ramorum*, *et transudationem ex arteria in adjunctam venam*; vel mediatè *per carnem mediam* inter ramos venosos et arteriosos, et per sinus, corumque ramos in cerebro, ubi sanguis ex arteriis carotidibus et vertebralibus, a sinibus absorbetur, et ex his ad jugulares amandatur."

Believing thus, that the blood "circuletur per carnem," he attempts to prove it by sundry facts, which although admitted, yet scarcely answer the object; he concludes his remarks by saying,

"Intrat verò sanguis carnem, *vel* ex tenuibus, simplicissimis, et *porosis arteriarum capillarium tunicis*, *vel* per illarum extremitates."

So very indefinite, it appears, had the *full discovery* of the circulation by Harvey, sixty years before, left it in the minds of his adherents! I mean this most important part, the mode of its vascular intercommunication. Surely none will doubt—or, if they do, we shall soon have a chance of removing those doubts by good authority; that at all times, a circulation was accredited, even admitting the route to be misunderstood. And shall an imperfect lucubration be considered as having a better claim, because advanced by Harvey? Surely no one of the profession would now desire to be measured by a standard so unsatisfactory; and to claim the whole, when scarcely meriting a part! I cannot but think the exclamation of Anthony over the body of Cæsar is here appropriate:

"Oh! judgment—thou art fled to brutish beasts,
And men have lost their reason."

But we have much more to lay before our readers; and as Alberti, in his fifty-sixth paragraph adverts to the valves in the veins, as leading Harvey to the important truth; we shall take the opportunity to say, "il n'est que le premier pas qui coute." If Harvey or his adherents had any where given us a satisfactory elucidation of the mode, by which nature contrived to make the arterial blood, thrown out of the general circulation into the asserted porosities of the flesh, reach even the first of the venous valves, we should have no difficulty in comprehending its path to the second, third, and so on: but the more the subject is considered, by so much the more, does this doctrine of the pores appear too poor for nature to have adopted: and I must say, I infinitely prefer Galen's proposition of the "mutua anastomosis, atque oscillorum apertio arteriis simul

cum venis" to that of Harvey; even coupled with the notion that they "transumunt ex sese pariter sanguinem et spiritum, per invisibiles quasdam atque angustus plane vias." (*De usu, part. lib.* 6. *cap.* 10.) It is by no means impossible, that these invisible and narrow passages may have led Harvey to the idea of pores, which Galen reprobated. Be all this, however, as it may; admitting the blood to be thus thrown into the parenchyma or porosities of the parts; it is then left in the dark, to find its own way, to scramble as it may, perhaps, like *Euripus*, backwards and forwards, and find the orifice of the vein it is to enter. We have no explanation of it, that the mind can rest on, as likely to answer; nay, Harvey has not even taken advantage, as he might have done, to explain it, of the *self intestine* movement of the blood, of which we before took notice. But apart from this, is it not a fact, that in some of the lower orders of animated nature, a mere intestine movement of the blood—a backward and forward motion, alone, constitutes the real character of the circulation? If so, this simple doctrine applied to man, would have some analogy in its support; whilst that, which throws the blood entirely beyond the reach of vascular control, can scarcely be deemed to possess the slightest resting-place.

This doctrine of the porosities is not new, as we have already noticed, even in the hands of Harvey; and it will not be misplaced, therefore, further to explain how it was formerly comprehended. We quote from the *Tractatus physico-medicus de homine*" of Theodore Craanen, a former professor in the University of Leyden, where the work was printed in 1689—4to. He appears a warm friend to the doctrines of Harvey, and ascribes the discovery of the circulation, exclusively, to him; at the same time noticing, that it had been, by others, attributed to a certain monk, P. Sarpa,—from whom Harvey was supposed to have learned it, and more accurately examined and experimented thereon. However this be, he adds, p. 128. "Nos Harvæo non detrectabimus gloriam hac in parte, et ille apud mortales, pro hac inventione, quidem retinebit æternam laudem." With this eulogium, we might, apparently, safely presume that his account of the porosities would accord

fully with the ideas of Harvey. But no! at p. 273, when speaking of erysipelas and inflammation, he adverts to the ancients, as supposing those diseases to originate from an extravasation of the blood; and that also, the parts were nourished by means of the blood, which they considered as being naturally extravasated and deposited; but, adds he, if this be true, "quod sanguis extravasetur, tunc non potest esse causa Erysipelatis, aut si est, deberent semper omnes homines Erysipelate tentari;" and, therefore, says he, they contradict themselves, as this is not the case. In order to give a clearer idea of this business, he proceeds to consider the nature of a tube or canal, and the pores, necessarily left by their formation; a plate accompanies this, wherein are to be found a variety of forms of pores, round, quadrangular, &c.; all harmonizing admirably with his doctrines, which are quite as well sustained as any present dogma. His own words will sufficiently illustrate this.

"Obstructiones quoque omnes hic crepant, sed nullam injiciunt mentionem pororum, qualium, non considerant *tubulos partium earumque poros;* vidimus enim antea, quod tubuli erant exigui canales, relicti à tribus vel pluribus fibrillis sibi incumbentibus; quæ non possunt tam prope ad se invicem accedere, *quin spatium aliquod relinquunt*, quod nos tubulum vel canalem vocamus; tales autem innumeri dantur in quolibet musculo, et membranâ, vel aliâ nostri corporis parte, qui omnes constant ex meris fibrillis inter se contextis." "*Pori autem sunt illa spatiola, quæ relinquuntur a fibrillis inter se* vario situ intertextis, *non aliter ac videmus* spatiola inter contextus linteamentorum. Dantur adhuc alii et multò exiguiores pori in ipsa fibrillarum substantia—"

Thanks to this good man for his most learned exposition of tubuli et pori! Enlightened by his eloquence, we shall no longer find it difficult to follow up the route of circulation; now, that the nature of the pores, porosities, or sieve-like vacuities, may be presumed to be fully comprehended by every reader, even should he be unable to detect them by the microscope! This is of no consequence; it is sufficient to detect them with the eye of faith—the only detection they ever have, or will receive! But, without full credence in them, it is

more clear than themselves, that no one can ever fully follow up the Harveian circulation!—I cannot exactly determine, if the learned author was an exclusive Porist: or whether he had not likewise a leaning to anastomoses. At p. 290 he refers thereto, and justice to all requires its insertion. He is about to speak "de transgressu sanguinis in venas:" and he says that much has been written on the subject of anastomoses; of which, Bartholine has noticed three species, viz.:

"Primum, quando osculum arteriæ ingrediatur osculum venæ. 2. Quando transversè ductus arteriosi transeant in venas. 3. Quando vena et arteria communi latere sint connatæ inter se, inter cujus lateris fibrillas est fissura seu rimula quædam, per quam iret sanguis ex arteria in venam."

This triplicity of anastomosis is not less a matter of faith, if we are to credit Craanen; for he immediately superadds to the above, that

"Multi Neoterici hæc tria genera negarunt, sed *argumento planè negativo, quia non poterant ostendi in cadaveribus.*"

In general, this would be deemed a pretty sound argument! and more applicable to the doctrine of porosities, probably, than to the other. Our excellent author seems puzzled what to think of all this; yet is perfectly assured that a passage of some kind is absolutely necessary.

"Certum autem est, *quod debeat esse aliqua via*, cum de facto nemo amplius dubitat; (circumstantial evidence, however, only exists in its behalf, so long as the mode of intercommunication is absolutely defective!) circulatio enim sanguinis, omnibus est manifesta, factum constat in hepate, corde, cerebro, in pulmonibus clarissimum est, quomodo enim sinister ventriculus acciperet sanguinem ex dextro, nisi ex arteriis pulmonalibus transeat in venas cognomines, et sic deferatur in sinistrum cordis ventriculum? Deinde quomodo possibile esset, ut sinister ventriculus pulset eodem momento, quo dexter, nisi continuo sanguis iret ex dextro ventriculo ad sinistrum per vasa pulmonalia? *Quæstio hic igitur tantum esse potest de modo*, quo sanguis transeat ex arteriis in venas." "Illorum argumentum nullum esse scimus omnes, cum negativa argumentatio non procedat; ergo non video, ergo non datur; si enim multa non deberent dari in rerum natura, quæ tamen sunt, licet sensibus crassis non queant detegi: de hoc maxime conqueritur *Cartesius*, homines inquit ubi nihil vident, aut sensibus percipiunt, illic nihil esse

statuunt, quod tamen in philosophando præjudicium est, sane maximum et abominandum; multorum enim existentiam negare non audemus, quæ tamen nullis sensibus existere percipimus: hoc ipsum etiam in medicina locum habet maximum."

These remarks are undoubtedly just, and will apply equally well to the porosities of Galen, in the septum cordis, as to those of Harvey in every part of the body! But let us hear him asking for information on the subject, of Swammerdam.

"Rogavi quandoque piæ memoriæ Dominum Swammerdamium *an arteriæ non essent continuæ cum venis?* hoc est, an non sibi inoscularentur, et principium venarum sit ab extremitate arteriarum, quarum principium oritur a corde; affirmavit hoc ipsum subtilissima anatomia expertum esse, *omnia vasa esse continua.*"

Here then we find the testimony of the most indefatigable of microscopic experimenters, in his reply to Craanen, to be altogether favourable to anastomoses. And, lest Craanen may be supposed to have misrepresented him, we may confirm that testimony, directly, from the writings of Swammerdam himself, in a letter to Boerhaave of Nov. 5th, 1716. from vol. 1. p. 286. 4to. Ed. Leyd. 1722.

"Omnia enim corporis vasa, *quantævis sint exilitatis*, non sunt nisi unius et ejusdem continuatio vasis. Quippe si quid sanguinis ex vase quopiam in quamlibet corporis partem effusum esset, illud nulla via posset remisceri sanguini; sed tractâ corruptione putresceret." "Medicus quidam (Qu? did he mean Harvey?) paucos ab hinc annos in scriptis suis tradidit, *sanguinem per fibrillas carneas* in orbem circumire, sive circulari: et ille quidem hanc opinionis suæ rationem allegat, quod caro ob illam sanguinis circuitionem rubescat!" "Sed frivola est ipsa probatio," &c.

If more is wanting to prove the uncertainty of opinion as to the particular mode of sanguine communication, between the arteries and veins; and that nothing was demonstrated or proved by Harvey or his followers, so as to give him a decided superiority over his predecessors in the same field of anatomical investigation; we may refer to Albert Keyper, in his "Institutiones medicæ, ad hypothesin de circulari sanguinis motu compositæ." 4to. Amsterdam, 1654. In p. 29. we find the following.

"Communicant arteriæ cum venis, non solum vicinià, verum etiam anastomosibus tum in medio tum in extremis, in his tamen minus evidentibus, ut *hodie in controversiam* vocentur (that is, one-fourth of a century after Harvey professes in his printed works to have settled the question), quandoquidem aliqui per porositates carnium tantum in extremis communionem fieri existiment."

May we be permitted, here, to ask anatomists and physiologists whether this business is even yet settled, two hundred years since Harvey promulgated his doctrines? and which is preferred generally; the anastomoses of Galen, or the porosities of Harvey?

*Ruysch*, in his 15th Epistle, 4to. edit. of 1724, printed at Amsterdam, on the subject of the extreme branches of the blood-vessels (De vasorum sanguinorum extremitatibus) replies to Dr. A. H. Grætz—who had stated to him, *that Harvey had left unexplained*, the "Usus verus vasorum, et cumprimis porum extremitatum in visceribus æque ac in reliquis corporis humani partibus," &c. adding, moreover, in a further part, that if the mere office of the veins and arteries was correct, as held by the mass of physicians (vulgaris medicorum opinio,) viz. as consisting alone in the veins returning back the blood, which the arteries had carried forth;

"Haud opus, credo, fuisset providæ naturæ, diversum ab alioque distinctum arteriarum et venarum cursum ac reptatum unicuique visceri largiri, nisi præstantior arteriarum extremitatum esse usus, quam quidem in hunc usque diem a medicis est creditum."

And he asks, as a great obligation, to be enlightened on the subject, by Ruysch's researches. It is obvious from all this, that Harvey, who taught his doctrines so early as 1616, and printed them about ten or twelve years afterwards, viz. in 1628; after having had ample time, by his own confession, to render that complete, which had been "perfect some years" before; and who tells us, moreover, that "this *only book* does affirm the blood to pass forth, and return through unwonted tracts, contrary to the received way, through so many ages of years insisted upon," (see Dedicatory Epistle); it is obvious, I repeat, that his elucidations were imperfect: since up to the time of

Ruysch's reply to Grætz, in 1704, that is, nearly a century from Harvey's public demonstrations, this junction of the vessels had never been satisfactorily demonstrated. Even Ruysch, with all his exquisite anatomical skill, aided by his superior attainment in injecting the vessels, and by improved microscopes; even he, could not clearly resolve the problem! as the following extract from his reply will prove.

"Sanguis autem refluus à venis recipitur idque *per anastomoses fieri existimo*, quantumvis *eæ quoque videri nequeant*."

If after a century, then, the first anatomist of the age had not been able to satisfy his mind as to this most essential point, the actual mode of intercommunication of the vessels; if Harvey himself had never demonstrated it, or, in attempting it, failed; if, even now, the subject-matter is still *sub judice*, and merely the object of suspicion, whichever side be adopted; wherein do we actually differ from the ancients, who believed in a circulation of some sort, as essential to the animal economy, from circumstantial evidence alone, and uncertain of the manner; since, as the mode of vascular communication being still undetermined, circumstantial evidence is also that alone on which we are obliged to depend!

I now take up another writer, the warm advocate of Harvey and his doctrines; from whom I shall make some extracts; and I have the advantage here of an English translation by Salmon. The author alluded to is *Isbrand de Diemerbroeck*, in his "Anatomy of the Human Body," ed. of 1694., Lond. fol.

In the preface to this work, we find the following words of the editor in relation to Harvey: "No less than immortal glory can be due to the renowned Harvey, our countryman, *for finding out* the circulation of the blood," so that the full credit of the discovery is awarded to him, with no reference to any other person, either directly or remotely. It might, perhaps, admit of inquiry, what influence the circulation could be supposed to exert in the human body, by Diemerbroeck, when he lays it down as manifest, that "the blood and spirits and other humours, *are not parts* of the body." (p. 5.) Hypothesis can scarcely go beyond this. Under the head of "The true mode of the Circu-

lation," p. 320, he says that, "It is apparent, that the blood does not only circulate by the said anastomoses, but *through the substance itself of the parts.*"—"Now that the blood flows *into the* pores of the parts, *and returns through those* into the veins, is apparent."—"This opinion of ours is confirmed by Harvey, Plempius, Pecquet, and Charlton."—"There is no reason to fear tumours, inflammations, aposthemes, &c., because the blood is *poured forth without the arteries into the substance of the parts:* for by reason of the narrowness of the arteries ending in the substance, no more flows in, *than can pass conveniently through the pores*, and be *again suckt* in by the orifices of the veins."!! This is the first attempt that I find, to explain the rationale of nature in this extraordinary step she is asserted to adopt, in order to transfer the blood from the arteries to the veins: and we owe him thanks for the elucidation it affords! No doubt he saw it clearly, if not with corporeal, assuredly with the eyes of an unqualified faith! and with such we may not venture further to contest. He delivers, however, this doctrine of the pores so explicitly, that I could not well omit to mention it here.

Like Harvey, he makes the heart the source of heat; and, as the blood (p. 322),

> "The further off it flows from the hearth of its fire, is so much the more refrigerated, and less a part of nourishment; there is a necessity of its return *to the fountain of heat*, the heart, *to be again new warmed* and attenuated therein, which return is occasioned by the circulation."

And at p. 329, he makes the heart likewise, as its "*chief* and *primary* action" to form or make blood. See also p. 33, "Now blood is a red juice, *made in the heart* out of the chylus for the nourishment of the whole body." This was the ancient opinion of Hippocrates; *and had not Galen seceded* from it, and attributed sanguification to *the liver* or to *the veins*, or to both conjointly, (in which Vesalius, Columbus, and many others coincided,) it is ten chances to one, that Harvey would never have adopted it! but, says Diemerbroeck, "in this our age, the ancient truth, that lay long wrapt up in thick clouds; again broke forth out of darkness into light;" and he soon after adds:

"This sanguifying duty, the most famous philosophers at this day allow the heart; so that there are very few left that uphold the Galenic sentence of the liver any longer."

Let us, nevertheless, seriously reflect, whether, if Galen's idea of the liver being the organ of sanguification, be thrown aside; we shall gain any thing in correct physiology, by adopting Harvey as our guide? The heart seems clearly intended, as a powerful forcing-pump, to propel the blood, when formed, to every part of the body; and, having a double part to act, in all such animals as are provided with lungs. Without such an hydrostatic force, by what means could this wonderful and most mysterious fluid, this vital intermedium, if we may so speak, between mind and matter; ever have been effective in the animal economy! Requiring continual elimination, as in its progress its important duties were performed; was it nothing in the economy of nature, that, contrary to her usual course, she employs the largest gland in the body; perhaps, in size, equal to all the others conjoined; was it nothing that she employed this, to secrete perpetually from *venous blood*, (not arterial,) a large amount of matter in the form of bile, before that venous blood could safely pass to the heart and lungs! and if we have just grounds for believing this elimination to be a most important preliminary step to the pulmonary *arterialization* of the residue, wherefore may we not adopt the Galenic views of hæmatosis, at least in part? But what function does the heart possess, by which, in even the most remote manner, it can be supposed to aid this process? Can one be mentioned? Yet is this most wonderful of all wonderful transformations, that of chyle into blood, ascribed to the power or function of the heart, by Harvey and his early followers! Was his physiology to be maintained at all hazards, without objection or reply! were his opponents to be converted, or set down, by sarcasm and idle ribaldry; or to be considered as altogether undeserving of reply? Let any one compare the physiology that Harvey taught, as he assures us, founded on the great discovery of the circulation made by him, with that which now prevails; and judge thereby, of the little value, that mere route of circulation has really conferred upon the medical profession. It is not improbable that the blood,

as an organized and living mass, (although Diemerbroeck and others deny it to have life and spirits—and, as he again repeats in p. 343, that it is not a part of the body,) possesses functions sui generis; and that amongst these, by some process of vital chemistry co-operating with the various emunctories, is that of resolving into its own character, the heterogeneous matters presented to it. Something like this appears to have been the opinion of the celebrated Glisson; who concludes, (says Diemerbroeck, p. 330,) "that the blood is not generated and moved in the heart; but that the heat and blood are generated by the spirit, or vivifying juice which is in the blood itself." Now, if neither the heart, nor liver, are sanguifying organs; surely, the almost inanimate and elastic tubes, either arteries or veins, can scarcely be considered as better calculated for that important function! If our doctrines in this point are equally unproved with those of Galen or Harvey, wherefore may not that of Glisson be tolerated, until one more probable or plausible may be presented to us! Diemerbroeck, in sustaining the Harveian opinion, has endeavoured, of course, to weaken or destroy that of Glisson; but I think he has altogether failed therein. There is no modification of the opinion by him; he holds it as being that of Harvey, and at p. 337, he repeats it thus: "In the mean time certain it is, that the chylus passing through the heart, and therein dilated, loses the form of chylus, and at the very same moment assumes another, that is to say, the form of blood."

In proceeding to consider (p. 344) "whether parts are nourished by veiny or arterious blood," he adverts to the ancient idea of its being veinous,

> "Because the blood was thought to be made in the liver, and carried thence through the veins to the parts. *Which error being discovered* by the circulation of the blood, since which time, it has been observed, that the blood is made only in the heart."

Admitting thus the error of *venous nutrition*, he yet immediately falls into the very same, by telling us that "whilst the blood returns through *the veins* to the heart, some small part of it *sweating through the pores of the vessels* or tunicles, are fixed up and down to various parts and nourish them; and that the

*tunicles of the veins* themselves are nourished *by the blood* which they carry; and that the *greatest part of the liver* receives its nourishment from the *veiny blood*, as is apparent from the vast number of veins, and small quantity of arteries that creep through it." Arterial and veinous nutrition are here, we perceive, both admitted. Can it be said that the real character or use of the circulation was fully comprehended by Harvey, or that they have been correctly unfolded, either by him or his followers? What function has Harvey better explained, than has been done by Galen? Is it that of respiration? As Diemerbroeck's views scarcely differ from those of Harvey, although at nearly half a century difference; I shall here mention them, to prevent repetition, when I come to consider the opinion of Harvey in its immediate place: It will corroborate what is above said, as to the little real importance that Harvey's discovery actually conferred on the doctrines of physiology! At p. 357, he says, the office of the lungs is to be serviceable for respiration, and then explains the various steps of the process as intended to refrigerate and condense the hot spirituous blood, so as to enable it to pass more readily into the left ventricle, "and there be dilated and spiritualized, and be wrought to a greater perfection." In explaining "its end" he is no less absurd;

"For because the blood breaking forth from the right ventricle of the heart into the lungs, is *much dilated*, *very light*, and *requires twenty times a larger room than condensed blood*, *which the left* ventricle cannot afford, hence there is a necessity that *that same vapour sealed up*, be again *condensed into the thickness of blood*, and so become heavier."

Such are the important physiological deductions from the Harviean doctrines of the circulation! It is doubtful, whether any of Galen's opinions, even supposing him altogether ignorant of the circulation, were quite so absurd! Nor, in explaining syncope, did Galen reason so ridiculously as either Diemerbroeck or Harvey. Speculating on the important advantages of this great discovery, Diemerbroeck says, p. 358:

"Hence it is apparent why in a stove that is overheated, many times we fall into a swoon; because the air being suck'd in, cannot sufficiently condense the vaporous blood, for want of cold; so that the lungs become filled

with that blood, and afford but little or no condensed blood to the left ventricle, to be dilated anew !"

And yet this discovery, which was to rectify all ancient errors in physiological research—nay, in every other department of medical science—has brought forth this, and similar fooleries in the most ardent advocates of Harvey; and they, men of candour and unexampled erudition! Well may it be said of it, "Mons parturiens nascitur ridiculus mus." Those who still think, that the mere discovery of the circulation by Harvey, (admitting him to the undisputed claim;) was the basis of all *true knowledge* in our science, are earnestly requested to reflect on how much truth it actually has accomplished, in the above or other particulars! Is the physiology of our time more settled or more satisfactory? What do we know, beyond all dispute, as to nutrition, sanguification, animal heat, &c.? Is the oxygenation or decarbonization of the blood, exclusively agreed on? Is even the absorption, or non-absorption of nitrogen, in the process of respiration, definitively settled? and, although the chief knowledge we possess of fever may be considered as derived from the blood and vessels, has the mere discovery of the circulation unfolded its locality? Is it not, indeed, itself the locality of fever! if any individual part can be so accounted, in an affection of a character so universal? Nay, even in this last respect, what is it, of the body, but the blood alone, which is universal in its distribution; and which has never yet been found wanting in the system, whilst *every other part*, without exception, has been found defective or absolutely wanting! Now, if nothing has been gained, not even probably, in practice, as I shall attempt to prove in another part; wherein does the unbounded praise ascribed to this asserted discovery consist? And the high standing of Diemerbroeck, as a practitioner, and as an anatomist, precludes any idea, that he is not a fair example, from whence to judge; for, in fact, we shall find almost every one of his views reflected from those of Harvey himself.

I must however, now, point to an author anterior to Harvey, that I may give a fair statement of the views entertained, on several of the particulars we have noticed, before the supposed

discovery of the circulation. If Harvey never saw the writings I am about to quote, it is to be regretted, as he might, possibly, have thereby avoided some of those imperfections with which he abounds; and if he did know them, 'tis pity he did not profit by them.

Vidus Viduus, a name of high and deserved reputation, was a Florentine by birth, who *died* in 1567, about nine years prior to Harvey's birth. His works in fol. were printed by his son, in 1611, at Venice; he practised at Pisa with great success, and, as professor, lectured there for twenty years.

In order to attain an idea of what was known or thought at that period respecting the veins and arteries, and also, we may add, of a circulation, I shall refer to p. 119, book 6th, where will be found his chapter on the veins (De Venis). He refers frequently to Galen and his writings: and he begins by asserting the *universal distribution* of the vessels; (φλεβες) signifying, among the ancients, both arteries and veins, but having then a distinctive meaning. Speaking of the veins he says,

"Finis ob quem venæ factæ sunt a natura, est ut sint Vasa inquit Aristoteles, (l. 3. de part. Anim.) et per eas *quasi per canales sanguis in omnes partes corporis distribuatur:* sunt etiam factæ venæ ad procreandum sanguinem, quam facultatem obtinent a jecinore; (Gal. de us. part. lib. 1. ch. 16.) quo nomine referuntur venæ inter partes multiformes, et *ponuntur non secus ac nervi et arteriæ in communibus instrumentis totius corporis.*"

Hence may be deduced, a strong presumption of an accredited circulation, even if imperfectly explained; and requiring but little to render it complete. Again,

"Superficies levis, cava patent naturaliter quantum necesse est ad sanguinem distribuendum, sicut et *extrema ora per quæ impertiunt sanguinem arteriis*, et ab illis spiritum accipiunt."

And subsequently, showing the proportion of blood carried to a part, to be in the ratio of its required nutrition, he says,

"Cor quamvis durum sit, ob calorem tamen ingentem plurimo eget alimento, et idcirco a venis grandioribus nutritur, at renes venas habent latas, non quo plurimum alimenti postulent, sed *quo per eas sanguinem expurgent.*"

Surely, a man expressing himself thus, must have had a conviction of the importance and existence of a circulation, although he might not have distinctly known its route; and was imbued moreover with the Galenic ideas of an hepatic origin of blood. It would seem to prove an adequate conception by him, both of its actual existence and high importance, when he subjoins, p. 120,

"Constat autem venas nullam propriam actionem præstare, sed tantum usum canalium, per quos ut diximus sanguis fertur ad alendas omnes partes corporis."

And when speaking of the arteries, p. 121, he says,

"Habent *et extremæ arteriæ ora, sicut et extremæ venæ, quæ se mutuò contingunt*, atque ubi res postulat aperiuntur ut utræque ex se mutuò attrahunt quod utile est."

The connexion is here fully maintained, even though an incorrect explanation is given for it. And in the sixth book, de Pericardio, et materia cordis, p. 261, he speaks of the vena arterialis (pulm. artery) arising from the right side or ventricle, through which

"Quandoquidem non exigua pars ejus sanguinis *ex dextro ventriculo cordis, penetrat in sinistrum*, unde per arteriam magnam dividitur in arteriis totius corporis."

With these, and many similar evidences of his penetration in the subject before us; can the mere fact, if fully admitted, of a more correct and experimentally proved route of the blood, be considered as *a discovery of the circulation*, with that perfect candour, and with that due degree of justice, to which others are entitled? Had it been denominated a confirmation, or verification, or by some analogous term; which, whilst surrendering to Harvey every fair claim that could be asked; would, at the same time, render it probable that without all these preliminary links, Harvey would not have dreamed of claiming the complete chain; we should have deemed him deserving of every praise: but, with the evidence already adduced, and more that will yet appear, I should do violence to my conscience, if I did not

explicitly avow my belief, that he is not entitled to the supreme distinction he so long has retained.

I must be pardoned for still further trespassing on the patience of the reader, by presenting for his consideration a few extracts from the "Dissertationes Medicæ" of the celebrated Dr. A. Pitcairn; a man of the first eminence in his time; an undoubted believer in the circulation; and a warm friend to the claim of Harvey. I quote from the fourth 4to. ed. of his works, printed at the Hague in 1722. In p. 15, he thus begins his dissertation:

"De circulatione sanguinis per vasa minima."

"Docuit nos, prorsus novo et divinitus invento systemate, Harvæus, sanguinem e corde per arterias excurrere, perque venas ad cor redire: unicumque *hoc dogma fidei medicorum* commendare contentus, *cætera in obscuro reliquit.*"

This deserves attention; for if Harvey was content to commend this dogma of medical faith; and yet, beyond the mere general proof, has left the other parts in a state of obscurity; can he with perfect justice be considered as having discovered the circulation; and that without acknowledging the claim of others to the smallest part?

"At, cum compertum esset hoc usibus medicis non sufficere, cœperunt homines disputare, an *sanguis ex arteriis effunderetur in partes corporis* aliquas, per quas arteriæ et venæ hiantibus osculis dispergantur: an vero arteriæ minimæ non sanguinem crassum ad partes nutriendas, sed tenuiorem veherent et non rediturum, sanguisque omnis reliquus par arterias majores exiret in venas anastomosi junctas. Patet attendenti utrumvis horum dogmatum circulationi sanguinis adversari. *Primum* enim magnam partem sanguinis crassi (i. e. qualis in majoribus vasis continetur) emittit in *partes corporis*, aut *potius interstitia partium*. *Secundum* vero emittit partem sanguinis tenuiorem (id est, partem inclusi arteriis minoribus) dicatam partibus nutriendis; hoc est, docet magnam partem sanguinis non circulari, sed in viseribus, ut loquuntur, partiumque poris stagnare et hærere. Atqui, cum sanguis omnis in gyrum agatur a cordis et arteriarum impetu, ita ut hoc vigente non possit quiescere sanguis, patet stagnare eum in vasis minimis non posse, quin semper accidente sanguine hæc disrumpantur, aut præter modum sanguine non redituro per venas intumescant, quod in animali sano non fit: patetque in poris hærere non posse, ob incrementum quod sequeretur perpetuum eadem ratione: quo plus enim ejus in poros effunderetur, eo minus facile rediret ob *occlusionem venæ a circumfluo fluido*, ut postea ostendetur."

Such, and so uncertain, seem to have been the opinions of physicians, all warmly espousing the general doctrine of a circulation; in regard to the facts of this great "dogma fidei medicorum," which Harvey is supposed to have so fully illustrated and discovered! but which, at the expiration of more than a century, to the time of Pitcairn, was apparently as little comprehended, (that is, truly, satisfactorily, and uniformly,) by the profession at large, as they profess to esteem it to have been in the days of Galen! Is it not indeed the fact, (and I appeal to those who have read and pondered the works of Harvey,) that his views do absolutely, as laid down by himself, lead to this character of uncertainty; and, consequently, are to the like degree imperfect! I principally allude to his doctrines, so far as we can gather them; whether in physiology or in practice. In this last particular, its general influence may, perhaps, be judged of, by the following extract from Pitcairn; who shows, I apprehend, both with candour and with justice, how little, if at all, it superseded the practice of the ancients! At p. 17:

"Omnes vero medici, qui methodum ullam, quamvis circulationi, ut credi volunt, convenientem tradiderunt, *uno ore docent*, sanguinem, aut in partibus, aut glandulis hærere: et quia sanguis, sive crassior, sive subtilior, in partium interstitiis detentus, eadem omnia symptomata et inferre et pati potest, quæ veterum sanguis circulandi nescius. *Idcirco eadem medendi methodus a recentioribus est ubique fere adhibita, quæ antiquis placuit, quamvis plerumque experientiæ et legibus circulationis contraria. Unde non est mirandum non majorem factam esse mutationem in arte medica*, cum morbi plerique oriantur vitio circulationis in vasis minimis, quam multi recentiorum non melius Hippocrate et Galeno intelligere se demonstrant."

These are strong, but perfectly just estimates, of the very slight degree of real practical improvement, beyond that of former experience, which Harvey's discovery of the circulation, admitting it to be both new and perfect, had actually introduced. They are stated thus forcibly by one who fully appreciated its importance, and gave all due credit to Harvey himself; defending him, moreover, from several "apud quos Harveana demonstratio fidem non invenit." Like the boy in the fable, we have continued to cry, Wolf! Wolf! without perceiving, like him, that we were, however, proclaiming what was unfounded, or,

like the eulogies on the perpetual meteorological tables, committed to almost all philosophical transactions, &c., their high value is asserted, but has never yet been proved, by any just and accurate deduction drawn therefrom! such was even the case, apparently, in the time of Hippocrates, such is the case at the present day!* as will be acknowledged by every candid reader. I profess not to review the different opinions that have successively appeared: I notice them, merely to show the weakness of the Harveian claim, since it is not perfect; but many things therein, as he says of Galen, are strangely clampered up: or if admitted even to be perfect, its real utility is not yet unfolded, beyond a few of its first pages, so far as we may judge from the discordant lights it has elicited; for we must object to consider as such, the singular and discrepant ideas that have sprung up from mistaken or imperfect conceptions of its real character! and if, as Pitcairn proves, the ancients, unaided by a knowledge of the circulation, yet had adopted a mode of practice, which Harvey's discovery has scarcely modified; surely its advantages,

* The following observations are given as a note by the editor (of "Traduction des Œuvres Medicales D'Hippocrate." Toulouse, 1801. Vol. I. p. 70.) at the § 45 on the Humours. "What Hippocrates says in this treatise, as to the constitutions of the atmosphere, and his frequent repetition of the same doctrine throughout his writings, suggests to me an idea that has frequently presented itself to my mind, viz., It is principally from the observation of the phenomena of disease and health, that we are enabled to collect the most important information, respecting the state and variations of the atmosphere.

"Hippocrates appears to have made his meteorological observations on the human body alone; yet how curious and interesting are his deductions from them! I perceive, that for a great length of time, the atmosphere has been observed by means of barometers, thermometers, hygrometers, anemometers. Immense collections of such observations have been made, which are both very dry and very useless (bien secs et bien steriles.) Every academy has its own: numerous individuals have theirs, likewise. To these have since been joined, electrometers and eudiometers. Much attention *has not* been given to the variations, in the number and state of hospital diseases: yet there would be found the means, in my opinion, the most adapted to make known, what is the most desirable to discover, relating to the effect of atmospheric variations. I do not wish to undervalue the use of physical instruments and meteorological observations. I am not entirely as yet convinced of their inutility: but assuredly the little benefit that has yet accrued from them, authorizes me to compare the majority of those who make meteorological observations, with children, who innocently amuse themselves in arranging pictures, or playing with toys."

as he gave it to the world, are at best problematical, and yet to be demonstrated. I must be allowed to think, until the opposite is clearly shown, that the *mere discovery* of the true route of the circulation, if due to Harvey, has been greatly overrated! In itself, it is a most interesting point of physiology; but more particularly so, when considered in its intimate and absolute connexion with others no less interesting; such as nutrition, sanguification, and animal heat, &c. It is, in this connected view, pre-eminent; and yet, will any one contend, that we are perfectly acquainted with those functions? It has been attempted to demonstrate, that the ignorance of the route of the blood by no means made the ancients less attentive, or less judicious practitioners, than were those of Harvey's period; and even to present times. Few would probably affirm the contrary, even of themselves, individually, unless at the expense of a little appropriate humility. And when we read such passages as these I am about to quote, we may probably admit the writers not to be altogether in the dark, as to a circulation; or unobservant of practical experience in the operation of blood-letting!

"Locus autem naturaliter continens, (sanguis) sunt *venæ et arteriæ, et concavitates cordis;* aptus ad generationem spirituum, et ad omnia membra laudabiliter nutrienda." "Phlebotomia cum cautela debet fieri, si perfecto vis esse medicus semper time mensuram,—semper ad pondus fac *secundum vires, ætates, tempus, calores, immutationes temporum.* Si sanguis à principio *niger extiterit, fac extrahi donec deveniat rufus.* Si spissus quousque veniat tenuis."

Such, and similar remarks, prove an experience, to which a mere knowledge of a more probable route or mode of circulation, would have added nothing; with the exception, possibly, of enabling the writer to modify some of his hypotheses, founded on the philosophy of the age; though very likely quite as correct as any of those of present notoriety. And who, the reader will ask, is this person, who thus admits the natural locality of the blood, to be both veins and arteries? and who is so well acquainted, *practically*, with the influence of *the non-naturals* on the effect of blood-letting? and who, *practically*, also knew the effect of bleeding, in giving a *brighter tint to a*

*dark-coloured blood?** It was a man who lived in 1300, that is, nearly three hundred years before Harvey; the celebrated physician and alchymist, *Arnoldus de Villa Nova*, who wrote a treatise on the philosopher's stone; but whose eccentricity in this respect seems not to have diminished in any degree his qualifications as a practitioner, as his writings amply testify. His works were printed, under the care of N. Taurellus, at Basil, in fol. 1585, or at the period of Harvey's birth; and I may take the opportunity of stating, from his writings, p. 1853, in a chapter amongst his Questions "de mala complexione diversa," that the doctrine of porosities was a prevailing and a favourite one, apparently, long before Harvey. The subject is discussed, in the part referred to, under the title or question, "Utrùm ibidem sit ponere vacuitates in corporibus" which, the author assures us, both Aristotle and Galen reprobated. As it was Harvey's business to differ from Galen and his anastomoses; so it would appear that he had no other way of making a complete circulation or passage of the blood from the arteries to the veins, but by either adopting those anastomoses; or by repudiating them altogether, and adhering to the pores denied by Galen.

Is this too extravagant a position to be assumed, all things considered? Why, in mentioning Galen and his anastomoses, does Harvey say nothing of his opposition to the pores? If he knew it, and he was too well acquainted with his writings to doubt of this, it was at least reprehensible; and if he really

* It is this *practical* experience, that, at all times, without any reference to the mere route of circulation, has enabled the judicious physician to test the utility of bleeding, in removing this dark colour of the blood, and replacing it by blood of a florid hue. We see that Arnoldus de Villa Nova practised on this plan in the fourteenth century; and we have seen the same exemplified in the recommendation of Professor Chapman, in his well known letter to Dr. Tyler, in 1832, on the subject of the pestilential cholera, then existing in Philadelphia. See National Gazette of Sept. 4th of that year. "Let a vein be then opened, and if the blood flows freely, take a large quantity, and especially should the pulse rise, and *the blood become florid.*"—Is the former physician entitled to no credit in this case? he stands, in the instance before us, precisely as the *predecessors* of Harvey, in relation to him.

See his life, and extraordinary qualities, amply developed in an excellent biography of the Med. Biog. of Dict. des Scien. Med. vol. I. p. 352.

knew it not, it evinces unpardonable ignorance on a subject so deeply implicated in the consideration of his exclusive claim.

In one of the letters, constituting a part of the correspondence with the United States Gazette, dated Washington, March 22d, 1834, is an outline of Mr. Calhoun's speech, in relation to the plan of a Bank, as presented by Mr. Webster, and that which he proposes as his own: a remark therein made, and which struck me at the moment, I believe first suggested to me the above suspicion! assuredly it is perfectly consistent with our knowledge of human nature, and may therefore equally apply to Harvey, as to Calhoun! "The restrictions (says the writer, meaning those in Mr. Calhoun's plan) were not greater than those proposed by Mr. Webster; and it would appear that the *only motive* which could have induced Mr. Calhoun to dissent from the plan of Mr. Webster, was to *obtain the paternity of the scheme.* His views, continues the writer, in arriving at this object, I leave others to determine." Which intention, it will be most prudent for me likewise to adopt in regard to Harvey.

To return from this digression on Villa Nova to Pitcairn, I may be permitted to say, that his Essay, entitled "Solutio Problematis de inventoribus" is a most admirable one; and, although intended to oppose, in *favour of Harvey*, the idea of the circulation having been known to Hippocrates; yet there is so much candour and ingenuity displayed in it, that I trust I shall be excused for a small extract or two from it, even if it had not really a strong bearing on the subject before us: and although the arguments are forcible against Hippocrates, they yet do not appear to be absolutely conclusive; since it may be affirmed with great truth, I apprehend, that there is no certainty that he ever wrote, *expressly*, on the pulse and circulation; or, that if he did, it may very probably have perished with hundreds of other interesting medical documents, in the unfortunate conflagration of the Alexandrine Library. There can be no doubt, but that he has alluded to the pulse, and likewise to a circulation of some kind: now, if any writer should chance to mention a circulation, without expressly explaining its route, must it necessarily be concluded that he knew nothing about it? But this is, really, the chief foundation

of the arguments against Hippocrates and other ancient writers, not only in this particular, but in a variety of others. The reader will pardon me, if, in connexion with this opinion, I earnestly recommend to his perusal, the interesting and learned preface of *Hercules Saxonia*, to his Treatise "De Plica," published in 1600, at Padua, in 4to, wherein he attempts to show, that many diseases, upheld as new, were known to the ancients; whether successfully, must be for the reader to judge of: but how? By symptoms alone, for names are often changed, or are deceptive from other circumstances. Should, however, a train of symptoms be clearly pointed out in an old writer, which can apply only to some one particular disease now known, and perhaps considered as new, will any candid man deny them to be one and the same?

In justice to Harvey, I might with some propriety introduce the whole of the essay of Pitcairn, to which I have above adverted: but my object is not to advocate the claims of Hippocrates; but to endeavour to prove, that no one person, individually considered, is entitled to the high and prescriptive appellation of Discoverer of the Circulation! Confining myself therefore, to the short extract which follows, I shall only say, that I think the reader will find it both agreeable and useful to read the whole. It forms one of the best and most temperate vindication of the modern claim to the discovery of the circulation that I am acquainted with: of the *route*, I mean: for Pitcairn, and every other writer, pro or con, invariably admit, directly or indirectly, that the idea of a circulation was common to the ancients. I shall begin with one, at p. 97, wherein it will be seen, how strongly favourable he is to the Harveian claim.

"Eodem modo, non debuisset a viris doctis quæri nimis sollicite, an Hippocrates quædam dixerit, quæ circulationem sanguinis nobis ipsam ab aliis edoctis redolere possint; quamvis ne hoc quidem verum sit: sed, an aliquod ejus rei argumentum sit ab Hippocrate allatum, quo alii ad suscipiendam circulationis prius ignoratæ fidem permotos esse se profiteantur; id vero nemo est mortalium qui unquam fuerit professus. Nam, quod aliqui hodie, re tota nempe per alios demonstrata, dicunt Hippocratem clare tradidisse valvularum cordis artificium atque usum, nihil juvat: quam enim multi fuere, qui

earum artificium atque usum, ab Hippocrate traditum, melius ipso tradiderint? Omnes profecto post Hippocratem Anatomici et Medici; quibus tamen adeo erat ignota circulatio sanguinis *vera*, ut eorum aliqui, quibus noti fuerant Columbus, Cæsalpinus, Servetus, aliique, lectique ipsorum libri, adversus illud eorum dogma libros conscripserint. Concludo denique, usum valvularum cordis *verum* Hippocrati fuisse ignotum. Patet hoc ex ejus libello *De Corde*, ubi hæc habet de dextro ventriculo ejusque vase: 'aperitur quidem in pulmones, ut iis sanguinem ad alimentum præbeat: in cor autem clauditur, non confertim tamen: quo aer quidem ingrediatur, neque tamen admodum multus.' Unde patet, usum valvularum Hippocrati eum fuisse, ut tantum sanguinis non rediturі posset egredi, quantum *pulmoni alendo* sufficeret,* dum per easdem vias ingrederetur ex pulmone aer, quæ a circulatione sanguinis *vera*, et a vera ratione respirationis sunt alienissima nimisque abhorrentia."

In a preceding p. 92, he had thus expressed him, not less strongly against any claim of Hippocrates.

"Ex quibus concludo, ubi quæritur an Hippocrati cognitus fuerit sanguinis circuitus, (in qua quæstione auctoritas Hippocratis spectatur tanquam, nulla nulliusve utilitatis, cum nemo circuitum ei notum dicat; ideo, quia fuerit infinitæ vir peritiæ) licere ejus dicta, non minus quam imperiti cujusvis, ita explicare, ut falsa et absurda sint, neque in hac quæstione auctoritatem ejus debere adduci, ut quæ absurda et circuitui sanguinis adversantia protulit, molliori interpretatione diluantur. Hic etiam infero, in secundo casu, nempe, cum auctoritas inventoris non ingreditur conditiones problematis, (Pitcairn, like Dr. Mead, a mathematical physician, alludes to certain problems and theorems, previously laid down, and on which he founds his demonstrations) theoremata duo esse necessario vera: nam, quoniam eo in casu auctoritas illa est nulla, non debet assumi inventor, sive philosophus sive medicus, ejus esse ingenii, ut plura intellexerit quam quæ disertissimis verbis tradidit: unde fluit primum theorema. neque ejus peritiæ, ut non et falsa atque absurda potuerit proponere, et sæpe proposuerit: unde fluit theorema secundum."

Much as we may perhaps smile at an attempt to demonstrate who is or is not the author of the discovery, by theorems and problems; it will be found here treated of in a very ingenious manner, by the most celebrated of the physico-mathematical sect; thus he proceeds.

"Ut quæ jam dicta sunt, facilius intelligantur, adverti velim quo pacto ea ad quæstionem de inventore circuitus sanguinis solvendam applicari pos-

* This idea was equally entertained by Harvey.

sint. Quæstio in eo versatur, ut inveniamus, an Hippocrati cognitus fuerit sanguinis circuitus. Vocem autem circuitus eodem modo ab Hippocrate usurpatum, quo ab aliis multo recentioribus, liquido debet constare, ita ut invenienda sit in Hippocrate descriptio hujus circuitus, *satis clara et distincta.*" "Ego vero affirmo, circuitum sanguinis nunquam diserte ab Hippocrate describi, *nihilque in ejus Scriptis contineri*, quod suadeat motum illum ei cognitum fuisse; sed tantum, cognosci ab eo potuisse: nam, *quamvis nuspiam circuitus sanguinis perpetui meminerit, ea tamen sæpissime commemorat*, ex quibus deduci potest iste circuitus, quem quidem nunquam exinde colligit, licet inventum sit longe majoris momenti, quam sunt ea omnia, quæ ex sibi notis intulit et prolixe inculcavit Hippocrates."

Adverse as he is to Hippocrates' claim, he yet seems here to strengthen it, but again proceeds to demolish it, on his principles; or at least some of those particular points, on which a claim has been set up for Hippocrates of a knowledge of the circulation; and then continues to uphold the claim for Harvey. At p. 94,

"Deinde advertatur, Hippocratem de motu sanguinis nunquam aliter loqui, quam locuti sunt postea alii, quos manifestum est, *circuitum perpetuum* non agnovisse, quorumque aliqui etiam ab Harvæo declaratum et demonstratum negarunt. Legat qui volet Scriptores medicos Harvæo ætate superiores, et aliquos etiam æquales, comperiet profecto verissimum esse quod assero; piget quippe eorum sententias exscribere. Hæc cum ita se habeant, licebet concludere, *circuitum sanguinis verum* fuisse Hippocrati ignotum."

In thus acknowledging that a circulation, although not the *true and perpetual one*, as declared and demonstrated by Harvey, was really advocated and upheld before him, the dispute seems to be greatly circumscribed: and if it is found, that Harvey has not fully illustrated it; nay, that even now, this *circuitus verus* is not absolutely determined and settled beyond dispute, to the satisfaction of every one, wherein can it be affirmed that Harvey has the supreme claim to perfection beyond his predecessors! Much more, however, and in parts, perhaps, stronger than above, does Pitcairn advance, in opposition to Hippocrates, and in full credence of the rights of Harvey. And this is all as it ought to be, in this excellent vindicator, whose motives and strength of argument are fully appreciated by me. Not acquiescing however in all his views; I shall now quote him in one further extract, to prove the generally received idea of a

circulation: for, if not accurately comprehended, its necessity in the system, most unquestionably was uniformly felt and adopted. At p. 99, then, we find him thus speaking.

"Nemo unquam fuit medicorum, quamvis *circulationi veræ* adversantium, qui non *aliquem motum sanguini* tribuerit; sed, per eadem semper vasa, Euripi in morem: quare hi idem dicere et possunt et solent, quod hic ab Hippocrate dicitur. Hujus enim verba motum sanguini concedunt, at, *nullo modo circularem*, cum flumina non redeant in orbem ad fontes suos, veluti sanguis hodie redire statuitur per alveos continuos atque continentes: mirumque est, tot eruditos viros, ubicunque vident Hippocratem motui sanguinis ascribere periodum, credidisse ex eo patere, Hippocrati notum, et ea voce declaratum fuisse sanguinis *verum circuitum;* cum ea vox illi (ut et Geometris Philosophisque sæpius) denotet solam in iisdem vasis, per stata tempora (ut hic locus testatur) in partes, nunc has, nunc hisce contrarias, fluctuationem, quæ tamen aliquando majori sanguinis copia et celerius, aliquando minori et tardius absolvitur."

In thus attempting to show, and indeed proving, we may say, that physicians all agreed (nemo unquam medicorum) in admitting "aliquem motum sanguinis," he is probably correct in his object, which is to prove that the Harveian circulation was unknown to Hippocrates: and yet his quotations from the writings of that great man very strongly corroborate an important extent of view, as to its character and existence. Thus, from the treatise "De alimento," Pitcairn quotes the following, "venarum radix hepar est: arteriarum radix est Cor. *Ex his per omnia* sanguis et spiritus *pervagatur*, calorque *per ea* permeat." And could such expressions mean ought, or any thing; if not implying all that is now implied or understood by the Harveian route? Is not the blood distinctly characterized as flowing to every part, by this vascular apparatus, and the heat also; and is much more now implied, than was fully appreciated by Hippocrates; although not speaking exactly in similar terms; or denoting that particular route, which was more correctly laid down by Harvey, but without completing it? can this alone confer the exalted privilege awarded him? and shall not an iota of credit be allotted to others! Well may these neglected worthies, when viewing their birthright and blessing surreptitiously bestowed on a younger brother, like Esau to Isaac, exclaim,

"Hast thou but one blessing, O my father!" Can we draw no probability of an individual view of a particular subject, except it be clothed in one peculiar form of speech! Can the following extract by Pitcairn from Hippocrates "De Corde," admit of reasonable doubt as to a full conviction of a circulation, and that *perpetual;* although the precise route was then, and *is yet*, not conclusively settled! "Hi sunt humanæ naturæ fontes; hinc flumina excurrunt, quibus corporis alveus irrigatur." Surely the above, and others that might be adduced, might well establish a prior claim to the doctrine of a circulation, without being weakened in its fair construction, by that which follows from Pitcairn; viz., that both the above extracts, compared together, show that Hippocrates "eadem modo ex hepate, quo ex corde, credidisse motum celebrari fluidi versus extimas partes corporis *per eadem vasa redeuntis.*" This position is, I think, unfairly assumed by Pitcairn; it is, however, one that has been so long repeated, as to be fully adopted;—it amounts apparently to the opinion ascribed to Galen, of the analogy of the flux and reflux of Euripus;—and which I have already attempted to weaken; whether successfully, must be determined by others. I must, however, dissent from this especial part; for if even indirectly, such a stand might be assumed, I cannot think the construction is a natural one, nor the comparison of the two passages, by any means establishing that, thus gratuitously adopted by Pitcairn. Moreover, I think the difficulty by no means lessened, if we take Hippocrates, in his different writings, to be his own best commentator; even here, too, we might arrive at different conclusions, from the same original, or by consulting different translators: for they do not always agree; and error consequently creeps in, on the one side or the other. I must be permitted to illustrate this, by one instance that occurs to me. It is in *Lib.* 2. *de Morbis Mulierum*, p. 638. Foesii. Ed. 1624. Frankf., wherein the Greek word σιχυαις, which is uniformly by others rendered cucurbitula, cucurbita or cups, is by a French Editor translated "sangsues" or leeches, which were not at that period employed in medicine. We might likewise, did time admit, adduce many parts, which insulated, speak loudly of a circulation; we shall state but one, from the book above mentioned by Pitcairn, De Alimento;

"Alimentum in pilos, et in ungues, et in extimam superficiem ab internis partibus pervenit." It must be a thorough partisan, who can see in this, a mere flux and reflux of the blood! Can we possibly imagine the sagacious mind of Hippocrates to have exercised itself on these, and other points of nutrition, without admitting him to have had more than an imperfect view of the circulation; when we find him affirming that the very hair and nails of superficial parts are nourished from the interior! Surely it is too much to ascribe to the circulation, as the principal value of its discovery, the mere knowledge of its route, if we should even admit this to have been the work of Harvey alone! and forget that the most important part, in the whole, is altogether unsettled and unknown! His assertion that the blood reaches the veins from the arteries is true: had not Galen equally shown it? but which is correct, the anastomoses of Galen, or the porosities of Harvey? The world is yet divided between them; and the *discovery* of the circulation is not yet complete!

We proceed at length to the 6*th Chapter* of Harvey, headed as follows, "*By which ways the blood is carried out of the Vena Cava, into the Arteries, or out of the right ventricle of the heart into the left.*"

The different chapters so nearly involve repetitions, and that repeatedly, from all pointing to one object, the route, or passage of the blood, that it is impossible to avoid the same, in considering them successively. We have already had occasion to notice the pulmonary route: and here, again, whilst we could wish to spare the reader, we find it necessary to advert to it; and probably may still further trespass on his patience.

In this chapter are to be found assertions, correct in some respects, but which are in a measure opposed to the very point they are intended to sustain. It is very generally known that objections of no trifling nature have been thrown out by many, but especially by *Vesalius*, as to Galen's anatomical attainments; and assertions made, that he was acquainted only, or chiefly, with the anatomy of animals, and not of man. Admitting this in its fullest extent, let us see what it would lead to, conformably to the declarations of Harvey, as it regards Galen's knowledge of

the "connexion of the heart with the lungs;" and whether he ought not really to have comprehended accurately wherein that connexion consisted; and consequently have some idea of the nature and existence of the pulmonary circuit. Speaking in p. 35, of those, who, "Whilst they desire to give their verdict, to demonstrate, and understand all parts of living creatures, look *but into man only*, and into him, being dead too, and so do *no more to the purpose* than those, who go about to frame universal arguments from particular propositions;" "were they *but as well* practised in the *dissection of creatures*, as they are in the anatomy of men's carcasses, this business, which keeps them all in doubt and perplexity, would, in my opinion, seem clear without all difficulty."

The dissection of brutes, then, it appears from Harvey, would settle this business (the connexion of the heart and lungs), better than that of man: if so, and if, according to Vesalius, Galen dissected animals alone; he was the very man to clear up this difficulty, and detect and discover the pulmonary passage! It may not be improper here, however, to render it probable, that Galen was an extensive dissector of the human body, as well as of brutes: and the first step in this proof, is that of lessening the credit of his accuser, even of Vesalius; as must be the case, if the following unexceptionable and diversified testimony is to be relied on: and, whilst hearing both sides, strike a balance between them. C. N. Jenty, in the historical compendium prefixed to his anatomical lectures, printed in 3 vols. Lond. 1757, thus speaks on the subject, after stating that he (Vesalius) was born at Brussels in 1514, and, that at the time Vesalius appeared, anatomists were so much blindfolded with the authority of Galen, that to have contradicted him had been looked upon as heresy: that Vesalius ventured to expose the mistakes, and correct the errors of Galen, both in physic and anatomy: which led to the censures of some distinguished authors, who charged him "with ignorance, want of honour, vain-glory, and plagiarism." Such is what I am now to present to the reader's notice. Jenty gives at p. xciv. an extract, translated from *Piccolhominus*, whom I shall again refer to; but, as I have the original, and it is much more definite in the charge, I shall prefer it here. See

his "Prælect. Anatomicæ," Lect. 3d. p. 207. fol. Rom. 1586. In this chapter, speaking of the fœtal heart, he claims for Galen the discovery of *certain parts;* and refers in proof to the 6th book, de usu partium, cap. 20, 21, and also to the 6th ch. of 15th book, which, says he, Vesalius "in magno illo de re anatomica volumine," has not mentioned; and he thus continues—

"Qua ab eo prætermissa, duo perspicuè indicantur; alterum, *se in fœtubus dissecandi segnem et ignarum fuisse*, cum hanc neque invenerit, neque prodiderit; alterum, *se libros illos Galeni* quos modo commemoravi, *nunquam legisse*. Nec minus mirari subit Fallopium, qui passim Vesalium divinum appellat. An divinitatis nomen meruerit quòd rei anatomicæ, omniumque corporis humani partium, fuerit inventor primus et observator? Si mihi aliquando per otium licebit, *luculenter commenstrabo*, *quæcumque bona scribuntur* à Vesalio in illo volumine, *omnia* ex Hippocrate, Aristotele, Galeno, aliisque antiquioribus *esse transcripta*, *horum virorum*, *nulla prosus facta mentione: Quæcunque verò falsa*, ab eodem scribuntur, *quæquam plurima sunt*, ex suo furibundo marte prodidisse." And soon after—"Ex duobus itaque illis Galeni libris, et locis, in quibus admonet, horum vasorum coitionem in fœtu, *nonnulla*, veluti problemata eruam, quò res obscurissima, tractatur dilucidè et maximè perspicuè."

These are serious charges, we must admit, yet they do not rest on Piccolhomini's assertions alone. What follows, as I have not the originals, I extract therefore from Jenty, who thus proceeds—"The censure of *Caius* upon Vesalius, is still more remarkable. 'We both lodged,' says he, 'in the same quarters at Padua, at the time when Vesalius wrote and prepared his book *De Corporis Humani fabrica.* One Aldinus Junta, a Venetian printer, employed him to correct the anatomical works of Galen, both Greek and Latin; and, for that purpose, several emendations were sent him: but he rendered Galen's text more corrupt than it was before, with no other view than that he might have somewhat to find fault with.'—"And though *Fallopius* owns him to be the father of anatomy, yet he carps at his opinion almost everywhere. *Columbus* talks thus of him: 'I cannot but be surprised that he, who on all occasions, lashes and chastises Galen for his having described apes and brutes, instead of men; should yet, himself, be so ridiculous, as to describe the larynx, tongue, and eyes of oxen, and not of men; without so much as ever giving a caution with regard to it.

He also ascribed muscles to the epiglottis, which are only found in brutes.' *Eustachius* has also observed of him, that 'he described and delineated a dog's kidney, instead of a man's.' "*Arantius* styles him the common master of anatomists, but accuses him of having delineated the pudenda of brutes, on account of the scarcity of the bodies of women; whereby it happened that *Valverda*, and those who immediately followed him, taking things upon trust, split upon the same rock." "*Johannes Baptista Carcan. Leon.* speaks of him thus: 'It is surprising that Vesalius, whilst he accuses Galen, the chief of physicians and anatomists, of so many blunders and errors, should yet himself, be so justly liable to censure in the same respect: and, what is still worse, by these his accusations, he seems widely to have mistaken Galen's meaning; ascribing to him things he never so much as dreamed of; and affirming, that he denied those very things that he insisted on in the most distinct and explicit manner: and whilst he so often wonders at, and finds fault with Galen; he himself deserves to be wondered at, and found fault with.'"

These are, I repeat, heavy charges against Vesalius, both as to integrity and information; yet, if correct, it is right they should be fully known: and, as they were nearly coeval with him, and adduced by men of high repute; we can have no reasonable cause for doubting them. My object, in quoting these remarks against him, has been to show, how grossly he has acted towards Galen; and thereby to render it probable, that Galen's dissections were by no means confined to brutes, as so commonly insisted on, though powerfully resisted by Riolan; but that also, if even admitted, the fact should prove him, agreeably to Harvey's rules above-mentioned, to be the more qualified to investigate the subject treated of in the chapter under consideration.

That Galen was a bold and enterprising surgeon, can scarcely admit of a doubt, and whether he attained that profound skill which he seems to have reached, by mere dissection of brutes; will probably be judged of differently, according to the opinion of the reader, as to the importance he may attach to anatomy as the ground-work of surgery. In endeavouring to vindicate him

from the obloquy of Vesalius and others, I need not apologize to the reader; for if he knows but the tithe of Galen's attainments, I am sure he will be gratified by the removal of one stigma that has thus been attached to his memory. And it is perhaps singular in the annals of our profession, to find two cases, very closely allied in some particulars, though very different in others; and probably the only ones of the kind recorded, if not solitary in existence, reported in detail by Galen and Harvey at an interval of probably fourteen centuries! The difference between them consists in this; that of Galen's was the product, if we may so say, of his unrivalled anatomical and surgical skill; that related by Harvey, was the effect of disease, and is to be regarded solely as his report; yet, from its analogy, we *may well be surprised*, that Harvey has not even referred to the more extraordinary case of his great predecessor! I quote Galen first, as having the priority of time; and indeed Harvey's is of no importance in the actual object I have of vindicating Galen as an anatomist. The intent of each, in his report, is, moreover, different in most respects. Galen's case to which I refer, is mentioned in more than one part of his writings; but more particularly in the 7th book of his work, "*de Anatomicis administrationibus*," and the whole book will be found more or less connected with the subject of the circulation, and it will prepare the way, for the case adverted to, to cursorily notice its contents. In this 7th book, then, Galen treats of the heart, lungs, and arteries, as seen both in the dead and living. He makes the lungs, the heart and thorax the principal organs of breath, (spiritus,) and notices the two-fold kind of artery, viz., that arising from the left ventricle, spreading through the body, and beating in unison with the heart: the other, the arteria aspera or wind-pipe. This is followed up with an account of the pleura and pericardium, of the heart and arteries, with some notice of the different opinions respecting the vessels of the lungs, of the pulse in the heart and the arteries in every part of the body which arise from the aorta; also the non-pulsation, or at least the non-perception of it, in the lungs; he remarks in relation to these pulmonary vessels,

"Verum inde, *quod sinistro ventriculo sint continuæ, conjecturam aliquis fecerit:* et si quidam *non conjecturam solum*, vel probabilem spem, sed certam functionis ipsarum scientiam habere arbitrantur."

This we consider highly interesting, unless mistaken in its import, viz., that it had been conjectured that these vessels were continued into the left ventricle; a conjecture not merely probable, but certain, judging from our knowledge of their functions: and this is subsequently still further urged. We moreover find him, in explaining some of the differences between an artery and a vein, thus speaking:

"Quales igitur *toto corpore* existant arteriæ, tale vas ex dextro cordis sinu procedens, in totum pulmonem ramorum serie diffunditur. Quales autem venæ, tale ex sinistro: ut ex tribus vasis pulmonem intertexentibus, quod à sinistro cordis ventriculo proficiscitur, *arteria venosa* nuncupetur, quod à dextro, *arteriosa vena*," &c.

He now, in the 13th chapter of this book, comes to the case we have in view: and of which we shall give but the outline—sufficient, however, to enable every one to form an estimate of both his anatomical and surgical attainments. It will moreover be well to recollect, that as Galen had no precedent to direct his judgment; so neither has there been any other similar case recorded: even that of Richerand is not to be compared with it. It stands isolated in the records, as he himself does, in the myriads of our profession!* The son of an actor, he tells us, in some gymnastic sport received a blow on the sternum. It was at first neglected, and was supposed to have got well. About four months after, suppuration appeared, the part was incised to discharge the matter, and it quickly cicatrized: inflammation again ensued, and suppuration. The part, again divided, could not be healed up. A consultation took place, to which Galen was invited; when the sphacelated appearance of the part, and the diseased bone became obvious; and even the pulsation of the heart was apparent. No one dared to remove this diseased bone; and, at length, Galen, without however promising a cure, undertook to remove it, uncertain of the state of the parts be-

* Richerand's Excision of the Ribs, &c., by no means equals the case of Galen. See "Histoire d'un resection des Cotes et de la Pleure. Paris, 1818.

neath. He accordingly cut away the affected bone; together with the vertex of the pericardium, which was in a state of putrefaction; *thus leaving the heart entirely bare.* It is sufficient to remark, that the person perfectly recovered in a short time; which, says Galen, could never have happened, if no one had been bold enough to cut away the bone, and which no one would have attempted, unless well instructed in anatomy; ("nisi in administrationibus anatomicis *præ-exercitatus.*") I cannot but add, moreover, that in this chapter Galen evidently evinces his knowledge of, and actually employed ligatures, to stop hæmorrhage. But let me ask, whether the mere dissectors of brutes would have performed, and that successfully, the operation I have above noted?* Let us now, however, proceed to the case detailed by Harvey, in his 52d Exercitation of the *Treatise "de Generatione."* The son of Viscount Montgomery of Ireland, he informs us, had a severe fall when young, whereby the ribs of the left side were fractured. An abscess formed, and discharged much matter, which trickled constantly from a large cavity. About the age of eighteen or nineteen, he travelled through France and Italy, and finally returned to London, the immense opening in the breast still remaining, so that the lungs could be seen and touched, [adeo, ut pulmones (uti creditum est)

* Under the article Anatomistes, vol. I. p. 223 of the Biography of the Dict. des Scien. Medic. when speaking of Galen, the editor says, "That as to the physician of Pergamos, it has been maintained that he opened human bodies: without denying the fact, we shall only observe, that whenever he enters upon anatomical details, it is from among animals that he derives them. In fact he dissected a great number of animals, many of whom were very similar to man, particularly monkeys without tails. This is a truth which Vesalius had, before, placed beyond doubt, when the learned and fine researches of Camper came to confirm it, and proved that Galen dissected orang outangs." All this may be true; and yet no one has, I think, decisively shown, that his dissections did not reach to man! If thus limited, of what use can it be to the surgeon; since we find one of the most brilliant operations on record, performed by a man ignorant of the parts to be cut! The very fault which Galen finds with many of his own contemporaries. It is, at all events, placing us between the horns of a dilemma; which, incapable of being extricated from, by direct and positive proof; must be settled by evidence of a circumstantial nature; which, all things considered, are favourable to the idea of the dissecting of both, whatever Vesalius may affirm to the contrary; and, remembering his own disingenuity of composing his descriptions of man, from that of animals, he ought to have been guarded in throwing a stone!

in eo cernere, ac tangere liceret.] This miracle being stated to King Charles, he sent Harvey to inquire into it. He tells us he found the young man "vegetum, et aspectu quoque, habituque corporis laudabili præditum;" and acquainted him with the king's commission; on which every thing was exhibited, and the pledget being removed, which was used to guard the part from injury; he says he perceived a large cavity, into which he could readily thrust his three first fingers and thumb: a species of fleshy protuberance being extruded and drawn back by turns, which he was able cautiously to handle. Astonished, and repeatedly examining every part, he concluded, that an old and ample ulcer had by nature been healed; but that the part he and others had regarded as a mere fleshy protuberance, or a part of the lungs; was really the extremity of the heart (cordis conum), surrounded by a fungous excrescence; this was daily cleansed, and a covering supplied, and he appeared to enjoy both health and exercise. He carried the youth to the king, that he might also inspect so great a wonder: and it seems that the heart was touched by both, without any sensation experienced by him, or knowing this to be the case, unless he looked. From all which, Harvey concludes that viscus to be devoid of feeling.

To recur to the chapter we are considering; Harvey proceeds to notice the *single character* of the heart in fish, ("as having no lungs,") as clearly showing his proposition: and he adds that "you may likewise see the same afterward easily in all other creatures, in which there is but one ventricle only, or something answerable to it, as in the toad, frog, serpents, *house-snails*, which, although they are said in some manner to have lungs, because they have a voice," &c. Here we perceive, that Harvey's impression was, that the lungs are principally intended to supply a passage for the blood "by the pulsation of the heart; which is thus "brought out of the *veins into the arteries*, the way of it open, patent, manifest, no occasion of doubt or difficulty at all." If correct in my idea, that this is the principal view of Harvey in his explanation of this important organ and its functions; I must continue to think that his conceptions were not sufficiently enlarged! That animals without lungs had no voice, was familiar to Aristotle

and Galen—[but whether *house-snails*, as Harvey asserts, have a voice, is a doubtful proposition. I mention this, the rather because the translator has rendered the word, *house-snail*, from the Latin, *lacerta*, which it will not, certainly, very readily bear; but it is an additional proof of what I before have mentioned, of the difficulty of really comprehending an author, owing to defective translations!] Be all this as it may, the important end of the function of respiration being that of the regular and continued arterialization of the venous blood circulating through the lungs; the voice follows secondarily, by the anatomical structure of the parts, and their action: now, as voice is not essential to life, it is merely superadded to the other important donations of our Creator, to the higher orders of animated nature; but since the process of arterialization is absolutely essential to life, so, even in those inferior animals, who have no lungs, and but a single heart, some correspondent organization effects the purpose. That I am not incorrect, in thus supposing Harvey's ideas too limited, as to this great function of the animal economy, is, I think, demonstrated by his subsequent remark, that, "seeing there are more creatures, which have no lungs, than there are which have; and more which have but one ventricle, than there are which have two, we may very well aver for the most part, and almost in all, that the blood is transfused out of *the veins into the arteries*, through the bosom of the heart by an open passage." This is a broad position, and might afford room to much remark; I shall, however, merely suggest, that the difficulty of the circulation is connected with the mode of conjunction of the extreme branches of the two systems of vessels. It is apparent how it gets from the large and patent mouths of the venæ cavæ into the heart; and from thence into the pulmonary artery: this is "patent, manifest, and no occasion of doubt or difficulty." But here the *quo modo* is at a stand, or, at least, the intercommunication from hence to the pulmonary veins, may be regarded as having a triple party, viz.: those who, with Harvey, contend for the parenchymatous porosities; those who consider the anastomoses of Galen adequate; and those who think, that both united are scarcely adequate to explain

the circulation, and its accompanying phenomena of nutrition, &c. Can that, then, however plausible and reasonable, and admissible by circumstantial evidence alone, be regarded as discovered; and that, too, by one man alone, when the very point at issue is absolutely unsettled and undetermined! We might as well affirm the absolute discovery of the longitude; because we know its existence as a matter of fact, and act upon it accordingly; navigating in every direction under rules deduced from uncertain premises, but not less useful to the interests of mankind; but where is the man to whom the award has been made of its actual discovery? And, supposing Harvey now arraigned before the present generation of physicians, with all the well known facts, from Galen downwards, satisfactorily demonstrated to them: acknowledging the estimated conviction of the ancients as to a circulation, yet equally assured of its not being exactly that of present times; but noticing their practical information as to the pulse and blood-letting; with other particulars that will present to the reflection of the reader! Suppose all this, I repeat—and let each individual, as if on oath, declare, whether he would vote in favour of the full and undivided claim of Harvey?—It is much to be doubted! Receiving the impression of *his* rights in the earliest dawn of our reading, &c., we adhere to it as fact in after life; as we do to our religious and sectarian principles, without any particular reflection on, or examination of their truth! Is this not correctly stated? and can we believe that Harvey actually comprehended the vast importance, and real character of the circulation, either of the pulmonary or the larger division, when we find him, (as we presently shall,) affirming that "the *left* ventricle was made for the lungs' sake."*

* I have left this as I first wrote it, that I might do justice to this excellent man. After some reflection, I was so amazed at the apparent error of Harvey, with all his acumen; that I thought it proper to consult his original Latin. And I again call upon the profession to pause, when making second hand extracts without referring to the original. Certainly, the intention, on my part, was to draw deductions unfavourable to Harvey's claim, as may be seen; but it is not requisite to go beyond the limits of truth to do this, as I shall show when I come to this very place in the 7th chapter; and I am not altogether sorry that this circumstance

In continuance of the subject, Harvey adverts to the hearts of "embryons," and here again, in justice to him, I must recur to the inaccuracy of his translator, "consideravi (says Harvey) autem mecum, quod etiam *in embryonum eorum quæ pulmones habent*," &c. p. 61. "But, (says his translator, p. 37,) I conceived with myself that it is plainly seen too in *those embryons which have hearts*." Surely it is requisite to inquire into the accuracy of all our translations, from the Greek, to the Latin, or French; and from these last to the English, to know precisely what we are about! It is more with a view to these remarks that I have adverted to this part of Harvey's writings; although I could wish, that the statement he gives was accurately analyzed by some distinguished anatomist; for I cannot, myself, think him correct. In order to facilitate this, I here give, in separate columns, a portion both of his own words, and the translation.

"Consideravi autem mecum, quod etiam in embryonum eorum quæ *pulmones* habent, sectione apertissime constat, in fœtu vasa cordis quatuor (viz. venam cavam, venam arteriosam, arteriam venalem, et aortam sive arteriam magnam) alio modo uniri, quam in adulto, id quod omnes Anatomici norunt satis.

"Primus contactus et unio Venæ Cavæ cum arteria venosa (quæ fit prius quam cava in dextrum ventriculum cordis se aperit, aut venam coronalem emittit, paululum supra egressum ab

"But I conceived with myself, that it is plainly seen too in those embryons which have *hearts*. In a birth, there are four vessels of the heart, the vena cava, the vena arteriosa, arteria venalis, and the aorta, or arteria magna; and are otherwise united than in one come to age, which all anatomists know well enough.

The first touch and union of the vena cava with the arteria venosa, which comes to pass *before* the vena cava opens itself into the right ventricle

has presented itself, as I mention, as a caution to all who are really desirous of correctness in their researches. I present the reader with Harvey's original, and with the translation from the edition which I have adverted to; the work of an ardent (but careless) friend!

"Natura tamen cum voluerit sanguinem ipsam per pulmones transcolari, dextrum ventriculum superaddere coacta fuerit, cujus pulsu, per ipsos pulmones, è vena cava in sinistri ventriculi locum, sanguis compelleretur. Et hoc modo, *dextrum ventriculum* pulmonum causa factum esse et ob translationem sanguinis, non ob nutritionem duntaxat, dicendum est." *Ed. Glasg.* 12*mo.* 1751. p. 80.

"Yet nature desiring that the blood should be strained through the lungs, was forced to add the right ventricle, by whose pulse the blood should be forced through the very lungs, out of the vena cava into the receptacle of the left ventricle: and so it is to be said that the *left ventricle* was made for the lungs' sake, and not for nutrition only." Lond. 12mo. Ed. 1673, printed for Richard Lowndes. p. 48.

hepate) anastomosin lateralem exhibet, hoc est, foramen amplum patens, ovali figura, pertusum e cava in arteriam illam pervium, ita ut per illud foramen tanquam per unum vas sanguis e vena cava in arteriam venosam et auriculam cordis sinistram, usque in ventriculum sinistrum liberrime et copiosissime dimanare possit. Insuper in illo foramine ovali e regione, quæ arteriam venosam respicit, operculi instar membrana tenuis dura est, foramine major, quæ postea in adultis, operiens hoc foramen et coalescens undique istud foramen omnino obstruit et prope obliterat. Hæc, inquam, membrana sic constituta est, ut, dum laxe in se concidit, facile ad pulmones et cor via resupinètur, et sanguine a Cava affluenti cedat quidem, at ne rursus in cavam refluat, impediat; ut liceat existimare in embryone sanguinem continuo debere per hoc foramen transire e vena cava in arteriam venosam, et inde in auriculam sinistram cordis; at postquam ingressus fuerit, remeare nunquam posse." p. 61.

of the heart, or *sends out* the coronal vein, a little above its outgoing from the liver, displays unto us its orifice, sideways, that is to say, a hole, wide and large, of an oval figure, made through passageable, from the vena cava into that arterie; insomuch as through that hole the blood may freely and abundantly pass out of the vena cava, into the arteria venosa, and the left ear of the heart, and so to the left ventricle. There is, moreover, against that place which looks towards the arteria venosa, a membrane thin and hard, like a cover, which afterwards, in those which grow to riper years, covering this hole, and growing together every way, does quite stop it, and takes away almost all sign of it. This membrane,* I say, is so ordained, that hanging loosely with its own weight, it makes way into the lungs and heart, and is turned up, giving passage to the blood which flows from the vena cava, but hinders it from flowing back into the cava again. So that from hence we may imagine in an embryon, that the blood ought continually to pass through this hole into the arteria venosa, out of the vena cava, and so into the left ear of the heart, and after it is entered, that it can never return." p. 37.

If this account be correct, it is at least somewhat ambiguous, and that in more than one place. The statement that the coronal vein is *sent out by* the vena cava! &c. Unquestionably, Harvey, according to the very principles of his great discovery, if granted to be his, considered the venæ cavæ to be the great and ultimate *recipients* of the venous branches, not the outlets; and the above noticed error must be set down to indiscretion, or any thing but ignorance of the subject. It must however be admitted, that errors or faults of less magnitude are made the object of severe reproof by him, when occurring in others; and, therefore, he himself should have been peculiarly cautious of tripping: had it not been for this, I should probably have omitted any remark on this particular; and recollected the apostrophe, "Non offendar paucis maculis." Other parts of this quotation seem to be equally open to criticism, as when speaking of the foramen ovale,

* This is called *septum* in a note.

he says, the "blood of the embryo ought continually to pass through this hole, into the arteria venosa out of the vena cava and so into the left ear of the heart." If I understand him correctly, I cannot think he is accurate; but I leave it for the determination of others; merely stating, that, at first, I did suppose he was giving an imperfect detail of the passage of the blood through the ductus arteriosus from the pulmonary artery to the aorta: but he has pretty accurately noticed this in the next page, wherein he speaks of a third trunk or arterial conduit pipe from the vena arteriosa and arteria magna; and of its subsequent attenuation and fading away "until it is quite dried up, and lost, like the umbilical vein." And from sundry considerations enumerated by him, he comes to the conclusion, "that in an embryon, when the heart contracts itself, the blood must always be carried out of the right ventricle into the arteria magna by this way." All this seems to have been known to Galen; and unaccompanied by some of the errors or obscurities of Harvey's explanation; which I deem also obvious, from a subsequent assertion, in the following page, when explaining the necessity of motion in the fœtal heart, &c. he says of the *passages* that they are "open and free, (as well in men, as also in other creatures,) not only to the time of the birth, which anatomists have observed, but likewise many months after; yea in some for many years, if not all their life time, as in the goose and very many birds." It is this, he thinks, that led Botallus to affirm, "he had found a new passage for the blood, out of the vena cava into the left ventricle of the heart:" and he adds, that when he himself "first found this in a rat of full growth, he did imagine some such thing."

Upon the whole, it is to be wished, that this chapter should be fully investigated, and followed up in detail, by an accurate anatomist, either to verify Harvey in his different statements, or to rectify him wherein he may have gone astray. He has very justly noted that character of the fœtal circulation, by which its double heart has the function of a single one, alone; and that "the condition of embryons that have lungs and make no use of them, is like to the condition of those creatures that have none at all." He then enters into some attempt *to explain why* nature closes

these passages, and establishes others after birth: rather choosing to "have the blood to be *squeezed through the strayner of the lungs*, than through most patent passages, as in other creatures." But how he has effected this, a few lines will show! "Whether this be, says he, because that greater and perfecter creatures are hotter, and when they come to be of age, their heat is apter to be suffocated and to be inflamed, and therefore the blood is strayn'd and sent through the lungs that it *may be tempered by breathing in the air upon it*, and freed from overheating and suffocation, or some such other thing. But to determine and give a reason of this is *nothing else but a search for what the lungs were made*," which he has by no means done. He terminates the chapter, by stating his intention to prove, "that in the more perfect animals, and those come to age, as in man, the blood may pass from the right ventricle of the heart, by the vena arteria, into the lungs, and from thence through the arteria venosa into the left ear, and from thence into the left ventricle of the heart, and then that it is so." And here we may demand if all this was known before him, why has he taken such pains to demonstrate it, but without giving credit to any one? for, although he largely quotes from Galen, yet it is pretty evident, that it is cited (as his advocate, De Back, says elsewhere,) by the venerable man "only as it may further his purpose;" and that, not crediting Servetus or others with the slightest knowledge on this part of the subject, he bends Galen to his own ends.

Chapter 7th. This chapter is thus headed, "*That the blood does pass from the right ventricle of the heart, through the streyner (parenchyma) of the lungs, into the arteria venosa and left ventricle.*"

Harvey sets off in proof of this position, by a singular attempt, (although perhaps a legitimate deduction from his premises of the pores, &c.) to assimilate the passage of the blood through the lungs, to the way in which "the water *passing through the substance of the earth*, doth procreate rivulets and fountains!" Can any of his advocates acquiesce in this similitude? or in what immediately follows, when he attempts to compare this same passage of the blood to the mode in which "sweat passes

through the skin, or urine flows through the parenchyma of the reins!" If such resemblance cannot be found, we must surely admit his views to be imperfect; although he may, as others had previously done, demonstrate, that the blood does *somehow* get from the right to the left side of the heart, through the intervening lungs: but, *how*, is the question; a question that he has no more resolved, to the satisfaction of all, nay, not even of himself, than his predecessors or his successors in the attempt! From all that has been and is yet to be said, it seems clear, that the doctrine of porosities, as the intermedium of communication of arteries and veins, is that which Harvey principally advocated; at the same time it will be found, that he also appears occasionally to lean to that of anastomoses, and even to both of them. This might at first sight appear extraordinary to the reader; but his darkness will be removed when we come to find out, in a future part, that by the distinct avowal of one of Harvey's most devoted advocates, that "venerable man (Harvey) cites that place (on the subject of anastomoses, from Galen) *only as it may further his own purposes!*" He considered the blood then, as passing through, not the vascular, but the parenchymatous structure of the lungs; and it will scarcely be contended that the vessels, arterial or venous, constitute the part, to which anatomists, either then, or now, gave the term of parenchyma. If, then, this discrepancy, common to Harvey and his immediate advocates, and to all their successors to the present day, exists; can Harvey be said to have comprehended and explained; much less to have demonstrated beyond all controversy, the actual nature of that passage, more correctly than Galen and his successors? In both cases, it is but circumstantial evidence of that, which all before him admitted, viz., a Circulation! But, if Galen was wrong in his conclusions, has Harvey absolutely set us right, in a manner no longer subject to doubt? If he has not, how can he have a better right than others, to appropriate to himself the sole and exclusive discovery of this *true passage*, when, even now, we remain ignorant of it! By Harvey's own admission, Galen actually maintained such a passage from the right to the left side of the heart. Now we have already seen the attempt to prove this to be by the septum

cordis; which, admitting its incorrectness on the part of Galen, yet evinces, nevertheless, his firm persuasion of the necessity of a circulation, if not of its absolute existence; since he had before shown, that the arteries contained only blood; that if a single artery, even a small one, were cut, the whole blood of the body, *venous* as well as arterial, would be evacuated; and having maintained the anastomoses between the arteries and veins on the one hand; nothing further was requisite to render the circulation in his hands complete, than to point out, on the other side, some way by which the blood, thus brought into the veins, should find a passage into the arteries, to renew its course. I repeat, that Harvey has attempted, with all his followers, to fasten upon Galen the presumed passage by the septum; and in so doing, he confirms Galen's *discovery* of the circulation, in a manner fully as perfect as his own; that is to say by channels or passages not proved, or fully admitted. Both therefore are placed on an equality. But, besides this, we have now to mention in favour of Galen; that Harvey has absolutely admitted that he, Galen, advocated *the pulmonary route* from the right to the left ventricle. I think this is sufficiently clear, and request the reader to attend to the proof.

Harvey seems to wish to strengthen his own views, by calling to his aid the authority of Galen, in a quotation (from his sixth book, ch. 10. de usu partium) relating to the anastomosis of the veins and arteries, and the use he attributes to the valves of the heart. Thus feeling, says Harvey, that, "there are some persons which admit of nothing, unless there be authority alledged for it; (and which in the present instance relates to "this passage of the blood through the lungs;") and, adds Harvey, "with Columbus, a most skilfull and learned anatomist, believe and assert the same from the structure and largeness of the lungs; because that the arteria venosa, and likewise the ventricle are always full of blood, which must needs come hither out of the veins, by no other path, but through the lungs; as both *he* (Columbus, and why was not Servetus mentioned?) and *we*, from our words before, our own eye sight, and *other arguments*, (which he does not deign to bestow on us,) do believe to be clear." p. 44. "Let them (they who admit nothing without

authority) know, that the *very same truth may be proved from Galen's own words;* that is to say, *not only*, that the blood may be transfused *out of the vena arteriosa into the arteria venosa, and thence into the* left ventricle of the heart, and afterwards transmitted into the arteries; *but also*, that this is done by a continued pulse of the heart, and motion of the lungs whilst we breathe. There are in the orifice of the vena arteriosa, three small shuts or doors, made like a Σ or half moon, which altogether hinder the blood sent into the vena arteriosa to return to the heart, which all know." Surely, after this candid avowal by Harvey himself, of a knowledge of this pulmonary passage of the blood, not only by Columbus, but likewise by the illustrious Galen, 1400 years before; it may be hoped, that none will hereafter pretend to claim *this part* of the circulation for Harvey, which he thus indirectly disclaims! He has partially strengthened some of the arguments, and rendered the fact more indisputable: but it is possible, that some may think his explanation of the passage of the blood, in some particulars, to be even less correct than that of Galen.

I must here beg leave to make a remark that may not be altogether useless, when reading translations, or even transcripts, of the ancient writers. It is, that from the *stops* in the copy of Galen* to which I refer, being in many instances *different* from those given in the extracts by Harvey; a *difference of meaning* seems sometimes to exist between them. A word is occasionally omitted; sometimes one word is placed for another, whose meanings might be variously understood; and at other times, words and stops are introduced that are not in the text itself. It is possible that this may arise from Harvey's employment of some previous edition; but not very likely, since the eighth, the one I advert to, was so near the time at which he wrote. But, at any rate, it would have been but right that he should have noticed the edition he employed. I have, indeed, the more I look into Galen, reason to deplore, that such *partial* extracts have been made by Harvey, when every part of a chapter is really so closely linked, and explanatory of each other, as to

* Eighth ed. Venice, 1609, p. 151 b.

render it almost criminal in an investigation of this kind, not to give the whole complete! A good English translation by the side of the Latin would be one of the highest favours that could be bestowed on the profession; especially if compared with the original Greek!

To proceed, Harvey continues thus: "Galen expresses the use and necessity of those shuts (valves) in the following words." Here he gives a long extract from the book and chapter above noticed, tending to confirm Galen's opinion of the necessary passage of the blood, from the very fact, of the valves of the vena arteriosa preventing its retrogression to the heart, after once passing those portals—which are more closely shut, as the necessity of the case requires; adding, moreover, [that is Galen,] a *threefold* [pray mark this well!] inconvenience that would have followed, had not these valves been provided. And here, in the very *first* of these inconveniences as pointed out by Galen, and copied from him by Harvey, we find sufficient evidence that he never did nor could believe in the mere flux and reflux of the blood in the same vessel, like that of Euripus, as has already been noticed. His words are,

"Nisi valvulæ essent; triplex sequeretur incommodum; ut sanguis ipse frustra longum hoc curriculum subindè emetiatur; in diastolis quidem pulmonis adfluens, et quæ in ipso sunt, venas omnes refarciens; in systolis vero, quasi æstus quidam maritimus, instar Euripi, motum identidem, huc atque illuc reciprocans, *qui haudquaquam sanguini conveniat*," &c.

If I do not entirely misunderstand his meaning, I should absolutely wonder that any person should have ever associated with Galen the ideas of Euripus, as laid to his charge; probably, to divert us from the examination of his claim to a complete idea of a circulation; and not merely of having sustained a simple flux and reflux in the same vessel. Be this as it may, the *second* inconvenience stated by Galen is, that in the mean time it might weaken the benefit of respiration, a point of some importance; the *third*, and that by no means a slight one, is that

"Retro sanguis in expirationibus remigrasset, nisi opifex noster membranarum epiphysin fuisset fabricatus," and he concludes that, "communis ipsarum omnium (valvularum) est usus, ut materias retro remigrare prohibeant."

From all this, we may be led to suppose, that had Galen been so fortunate as to have seen the valves of the veins, he would naturally and legitimately have ascribed to them the same use that he has here ascribed to the valves of the heart. Now, this is a very interesting chapter of the works of Galen, evincing his skill, both in anatomy and physiology, but which is not now my object to discuss; and I shall content myself with Harvey's own commentary on these very parts of Galen's works which he has so largely quoted: "It does therefore clearly appear," says Harvey, "from the words and places of Galen, a divine man, father of physicians, both that the *blood doth pass from the vena arteriosa into the little branches of the arteria venosa*, both by reason of the pulse of the heart, and also because of the motion of the lungs and thorax." This is all apparently very candid and open in Harvey; and it would seem incredible that, with this admission on his part, he should take the slightest merit to himself, respecting any part of the pulmonary circulation, save that of simply strengthening it! yet is it not the fact, that this part of the circulation is usually attributed to him by the majority of the profession, even admitting that he does not, himself, *directly* lay claim to it? Indeed, if here, he appears unequivocally to award the palm to Galen; it is not less true, I apprehend, that, indirectly, it is his object throughout, to claim it as his own. Should any one ask, why then has he so very explicitly afforded us the above views of Galen; by which he must strip himself of a part of his assumed discovery, when he might with equal ease have passed him by, as he has the most of his contemporaries: I reply, that one great object of most of those contemporaries, was that of awarding to Galen the merit which Harvey so sedulously claimed for himself; and he could not well avoid that reference to Galen's writings, which were then so commonly in every one's possession: although those of his commentators, being far less common, might be more readily passed by, as we find was really the case, seeing that he has scarcely mentioned one of them; so that we can with difficulty refer to them, or to their remarks! It would seem, however, from a short paragraph at the close of the last quotation I have made from him, that some one had probably called his attention to the writings of a man,

whom he could not well omit to notice, who had apparently done that justice to Galen, in so public a manner, as to compel him in a measure to do the same: it is however evident, that, as it respects the individual alluded to, Harvey has barely noticed him; and in such a manner as to leave it questionable of whom he speaks, or where to look in relation to the subject he is pursuing. "See also," says he, "the commentary of the most learned Hofmannus, upon the 6th book of Galen, de Usu part., ☞*which book I saw after I had* written these things." Which Hoffmann he alludes to, is not stated; if it was Caspar, as I apprehend, who was born several years before Harvey, I shall barely remark, that his commentary on Galen's books, de Usu partium, according to Vanderlinden, (p. 157 de Scriptis medicis,) was printed in 1625, that is, three years *before* Harvey committed his writings to press; and both were printed at Frankfort. Is it possible that Harvey saw this work of Hoffman *only after* his own was written! Admit it to be so, why has he said nothing respecting his observations, or never again noticed him in any part of his work?* I should greatly wish to know what Hoffman has said; and hope the treatise alluded to will not be overlooked by those who may possess it.

The priority of Galen to *some other parts* of what seems so closely and intimately connected with the pulmonary circulation, should the friends of Harvey contest the point, appears sufficiently evident from his own concession: thus, quoting Galen as above mentioned, he adds, that Galen writes "for which cause there being four orifices only, two in either ventricle, one takes in, the other draws forth," &c. Harvey amplifies this concise statement as of himself, in nearly the same language, viz., that "for this reason it was necessary that it (the heart) should be served with four locks or doors, whereof two should serve for the intromission, and two for the emission of blood, *lest either the blood like an Euripus*, should be inconveniently driven up and down, or go back thither from whence it were fitter to be drawn, and flow from that part to which it was needful it should have

* A letter to C. Hoffman from Harvey, is given in the College edition of 1766, dated 1636, or nearly ten years after his own treatise was printed.

been sent," &c. We have here, then, another indirect admission by Harvey himself, that Galen could never have accredited the backward and forward flow of the blood in the same vessel, as constituting the circulation; at least, it so appears to me, by the language held both by Galen and by Harvey.

Immediately following, we find Harvey affirming that his "assertion appears clearly to be true, that the blood does continually and incessantly flow through the *porosities of the lungs*, out of the right ventricle into the left." Enforcing still further the fact, by arguments, &c., that this passage does take place, he properly affirms (p. 48) that the *right* ventricle (here improperly translated *left*) was made for the lung's sake—and I mention this on account of the strange mistake of the translator, as well as another, at the close of the chapter, wherein Harvey is made to ascribe the nourishment of the heart to the coronary *vein;* whilst his own words expressly are, *arteriam* coronalem. What translation may be depended on, without comparing with the original writing of an author?

Before concluding this chapter, it may be well to state, that we find here an example, to which I have before adverted, of Harvey falling into the same absurdity, which he so heinously reprimands in poor Galen; by speaking of the blood as, "tanto puriori et *spirituosiori*," here and in various other places, he unquestionably, if words have any meaning, maintains the existence of spirits, (whatever he meant by them,) in the blood; and had no right to reprimand Galen for that which he himself teaches! We shall have more than one occasion to revert to this.

In the 8th Chapter, he speaks, "*Of the abundance of blood passing through the heart, out of the veins into the arteries, and of the circular motion of the blood.*" And he begins with a partial admission of the claims of some who had preceded him; but of whom he mentions Galen and Columbus only, as the authority for their views.

"De quibus forsan sunt aliqui, qui antea aut Galeni auctoritate, aut Columbi, aliorumve rationibus, adducti, assentiri se dicunt mihi."

It is a negative kind of admission, we perceive, behind which his own claim may be considered as prominently exhibited; for

he immediately adds, that "those things which *remain to be spoken* of, though they be very considerable, yet, when I shall mention them, *they are so new and unheard of*, that not only I fear mischief which may arrive to me from the envy of some persons, but I likewise doubt that every man almost will be my enemy, so much does custom and doctrine once received and deeply rooted (as it were another nature) prevail with every one," &c.

"Nunc vero de copia et proventu istius pertranseuntis sanguinis, quæ restant (licet valde digna consideratu) cum dixero, adeo nova videbuntur et inaudita, ut non solum ex invidia quorundam, metuam malum mihi; sed verear, ne habeam inimicos omnes homines. Tantum consuetudo, aut semel imbibita doctrina, altisque defixa radicibus, quasi altera natura, apud omnes valet," &c.

Whatever may be thought of the first part of this sentence, relating to his apprehension of danger from the promulgation of his "new and unheard of things;" there can be but one opinion as to the truth of the latter part. The proposition, established even proverbially, is daily demonstrable, and more especially in the profession of medicine, in the admission of some favourite hypothesis, taught without diffidence, and enforced upon youth, before they are qualified to judge correctly of its merits! If the feelings thus expressed were really experienced by Harvey, on the promulgation of his views in 1628, after ten or twelve years of oral communication; and that, under the sanction of the London college of physicians; supported moreover by a host of advocates, and favourers of his doctrines: how much more ought the admonition to impress me, whilst thus opposing his long-awarded claim, as the sole discoverer of the circulation of the blood! It is possible I may stand alone in this adventure, and that it may be considered as an heretical attempt against the great *dogma fidei medicorum!* I feel constrained, however, to proceed, with a consciousness of all the impediments I may have to encounter; under the absolute conviction, that truth alone is the aim of my investigation.

But let us recur to the chapter, and inquire, what are those "new and unheard of things," the mere mention of which was to draw down mischief upon Harvey? Perhaps it may be con-

ceded that they cannot apply to any of the circumstances connected with the pulmonary circulation, since we have demonstrated, even from Harvey's own admission, the prior claim of Galen, to say nothing of Servetus and others. Should it, nevertheless, be denied, we have other authority that can with difficulty be set aside. At all events, there is nothing particularly connected with that part of the circulation, that could, by any just pretence be considered as altogether new and unheard of. We may apply the same argument to the *venous valves*, which, by his own admission, were known to several of his predecessors; and, although he claims the explanation of their use, (wherein, indeed, he was perhaps more clear, and demonstrated the circumstance more completely;) yet, it will be shown, that even here, he is not entirely without a competitor; who, if not fully establishing the object of their formation, has yet so far unfolded it, as to leave no doubt, that it was one of the chief stepping-stones, by which Harvey reached his ulterior improvements. What, then, we repeat, are those "new and unheard of things," which he alone detected? It cannot be the more accurate connexion which he gave to those separate and independent links of prior discoveries! and for myself, I can perceive nothing in the chapter, that is truly entitled to the declaration of its being, either new or unheard of. His very arguments relative to *the abundance of the blood*, which led him to conclude "that the veins should be quite emptied, and the arteries on the other side, be burst, with too much intrusion of blood, unless the blood did pass back again *by some way*, out of the veins into the arteries, and return into the right ventricle of the heart," are evidently based on the pulmonary circulation. They consist in his experiments of "opening of arteries, and many ways of searching [none of which has he related], and from the symmetrie, and magnitude of the ventricles of the heart, and of the vessels which go out from it, (noticed, as will be seen, by Servetus long before him,) as likewise from the continued and careful artifice of the doors and fibres, and the rest of the fabric, and *from many other things*," which he has not deigned to mention. Now, if all these things are mentioned by writers anterior to him, they cannot be claimed by him as new and unheard of, and so far he must

assuredly be stripped of the title that has been so liberally awarded to him.

But let us hear how greatly his physiology was improved by this grand discovery. I advert only at present to the idea he holds out, respecting the mode of nourishment by means of the blood, at p. 51, in explaining its "circular motion."

"So, in *all likelihood*, it comes to pass, in the body, that all the parts are nourished, cherished, and quickened with blood, which is warm, perfect, vaporous, full of spirit (spirituoso: quere, if the spirit here and elsewhere attributed to the blood by Harvey, is not at least, adequate to balance the same, in Galen, for which Harvey has taken him so unceremoniously to task?) and, that I may so say, alimentative: in the parts, the *blood is refrigerated, coagulated*, and made as it were, barren; from thence it returns to the heart, as to *the fountain or dwelling house of the body*, to *recover its perfection*, and there again, by *natural heat*, powerful and vehement, it *is melted*, and is dispensed again through the body from thence, being *fraught with spirits* as with balsam, and that all things do depend upon the motional pulsation of the heart." If the alleged discovery of the *mere route* of the circulation, necessarily leads to such impotent conclusions, well might Pitcairn and others admit, that it had added but little to medical certainty in practice, &c.; for, wherein are they superior to those attributed to Galen, and his asserted views of a reciprocal flux and reflux in the same vessel? for my part, however *present* physiology may be presumed to have been improved, by more expanded views of the importance of the circulation, if, indeed, this be the case; I must confess, that I cannot perceive much benefit to have accrued, or as being likely to accrue, from the *mere additional* proofs of a circulation that Harvey might be supposed to afford, to the more limited, yet plausible, notion of his predecessors. I have already adverted to this particular, when stating the opinions of Pitcairn; and I shall now merely add a few additional proofs from one of his predecessors, viz., his own illustrious master, Aquapendente, who, he affirms, *knew not the use of the valves*, and consequently was unacquainted with the true nature of the circulation; yet, we shall find him laying down his precepts for blood-letting, in

a manner not to be surpassed even in the present day: thus, treating of the cure of affections of the head, (see Medicina Practica, Ed. 1634, Paris, p. 38.) he thus writes,

"Omnes probant sectionem venæ, quia abundat sanguis." "De quantitate sanguinis nihil certi statui potest, quia vel copiosiùs, vel parciùs mittandus est, *habita ratione ætatis, temperamenti, consuetudinis, temporis anni*, quæ sanguinis quantitatem indicant: Si adsit copia, ne imitemini timidos medicos, qui non audent ultra septem, aut octo uncias mittere; Mittite 12, 16, 20℥."—"Quæritur in aliis etiam doloribus capitis liceatne venam secare? Respondeo, si perseveret dolor, venas etiam frontis secandas esse: item venas post aures, aut nasi: item venas sub lingua, si commodè aperiantur, possunt capitis plenitudinem lenire: sed nunquam adhæc particularia deveniendum," etc.—"Si quis dicat nullum reperiri medicamentum quod possit sanguinem ipsum evacuare; Respondeo, hujusmodi medicamentum purgare quædam excrementa acriora quæ sunt permixta sanguini, et ita sanguinem puriorem reddere: et hac ratione *sanguinis copia etiam minuitur*, et ipsius mala qualitas remittitur."

It seems evident to me, that a man who writes thus on venæsection, and on the effect of purgation on the mass of blood; and who, moreover, so well appreciates the influence of the different agents he enumerates in blood-letting; could acquire but few additional practical ideas, from merely being enlightened by a more distinct route of circulation being pointed out to him; even if Harvey's new and unheard of things were superadded. Numerous other examples from Aquapendente and his predecessors might be given to the same effect: and all would tend to prove, that a mere knowledge of the route of the blood, alone considered, confers no extraordinary capacity on the physician in his judgment of blood-letting.

We see throughout, that Harvey considers the heart to be the grand organ of hæmatosis. If so, it can only be by a mere mechanical action, like that of a churn; by which might be supposed to ensue, a separation of serum and crassamentum, from a prior homogeneous mass of chyle, like that of buttermilk and butter, in the churning of cream! Whoever believes the heart to be the organ of hæmatosis, can explain it in no other way! and if it is apparently absurd, as thus presented; then it may be apprehended, that Galen's doctrine of the necessity of the liver, to the formation of the blood, is scarcely

more, if as objectionable! To evince still more the petty views which this asserted discovery of the route of circulation awakened in the mind of Harvey, admitting his undivided right thereto; we have but to advance in the chapter before us, and if possible, avoid to wonder at the extraordinary honours showered upon him, to the total exclusion of his less fortunate predecessors! "So the heart is the *beginning of life*, the sun of the microcosm, as proportionably the sun deserves to be called the heart of the world, by whose virtue and pulsation, the blood is moved, perfected, made vegetable, (vegetatur, qu? enlivened,) and is defended from corruption and mattering (grumefactione, qu? clotting); and this familiar household God doth his duty to the whole body by nourishing, cherishing, and enlivening, being the foundation of life, and author of all." In all this, can much be traced, as to the luminous expansion of physiology, &c., which, we shall find, Harvey considered as to flow from this discovery of the circulation? Nor does he appear to me, to be happy in his explanation of terms; as in this very chapter, wherein he speaks of the veins as,

"Certain ways or vessels carrying blood; there are two sorts of them, the cava and aorta." That both are "not undeservedly called veins by the ancients, as Galen has observed, *because* that this, viz., the arterie, is a way carrying the blood from the heart into the habit of the body, the other, a way carrying it from the habit of the body, back again into the heart," &c.

I have already remarked on the error of Dr. Rush, respecting the asserted ignorance of Hippocrates, of the distinction between arteries and veins; and attempted to show, that the distinction was sufficiently known to that great man; with other particulars not necessary to repeat. I shall only add, that I believe Galen has no where explained the difference of these vessels in the way mentioned by Harvey. If he had, however, particularized the passage, we might better judge of this; for so voluminous are Galen's writings, that a general reference is by no means sufficient. At any rate, I have searched for it in vain. We must be permitted to judge of Galen by his own words, and not by the mere assertion of others, without a parti-

cular reference. Now, since Harvey has omitted this, I think it but justice to give Galen's own definition of an artery and vein, as laid down in his "Finitiones Medicæ," one of the introductory books; and to which reference might be had with advantage on many occasions.

"*Vena*, est vas sanguinis, et sanguini contemporati spiritus nativi, nervosa, humida, et calida, sensum obtinens; habet tamen sanguinis plus, nativi spiritus minus.

*Arteria* est vas sanguinis paucioris puriorisque; et contemporati spiritus genuini copiosioris, ac tenuioris, calidior et siccior, ac sententior quam vena, pulsatili motu prædita."

Another definition is given of an artery in the same place; but neither in it, nor in the above, do we find any thing like the explanation assigned to it by Harvey.

And now, let me again ask what is to be found in this chapter, that can be claimed by Harvey as absolutely new and unheard of, and from which he may be entitled to the enviable appellation of the Discoverer of the Circulation? What are the particular points which fully and satisfactorily establish his claim? Surely, some few judicious observations, and reasonings founded on facts established long before him; together with some few additional experiments, and which indeed are few, if only those he mentions in his work; are not sufficient to establish him as the author of the discovery! If he is admitted to have traced more luminously, and thereby rendered more probable, his great outline of that general circulation, which no one doubted of; must we therefore invest him with the full and perfect mantle? Would he have been led to that train of observation and experiment, which gave a high degree of perfection to a plausible, but problematical hypothesis, had it not been for the close approximation of his predecessors? Could he (as Dr. Z. Wood affirms in his preface), like Archimedes, boldly exclaim ευρηκα? Surely, if what he has advanced can give him this exclusive right, and make us forgetful of that of others; it may be concluded that there is some imperfection in our language demanding supervision! At the same time, it is scarcely too much to affirm that the physiology unfolded by him, and resulting from his reasoning on the facts adduced, can scarcely be admitted in a single instance! Are

his advocates to uphold his claim at all hazards, and to the complete prostration of the rights of others? Cannot a few be found, who will investigate his claims in the minutest particulars, so that the profession may hereafter, either fully acquiesce in them; or bring down the idol, so long extravagantly worshipped, to the level he should hold in the Republic of Medicine!

I now proceed to notice the 9th Chapter of his treatise, which is thus headed. "*That there is a circulation of the blood, from the confirmation of the first supposition.*"

In order to comprehend this, it is necessary to extract a small part of the beginning of the chapter. "But lest any one should think, says Harvey, that we put a cheat upon them, and bring only fair assertions, without any ground, and *innovate without a cause;* there comes three things to be confirmed, which being set down, I think this truth must needs follow, and be apparent to all men.

"*First,* that the blood is continually, and without any intermission, transmitted *out of the vena cava into the arteries,* in so great abundance, that it cannot be recruited by those things we take in, and in so much that the whole mass of blood would quickly pass through."

Precision, (although strictly demanded for Harvey on the part of his opponents, by all his admirers) appears not to have been his fort! and had he not himself so frequently called his predecessors to account for trifling peccadilloes, I should have regarded my present remarks as hypercritical. Thus, when we find him, ch. 5. charging Galen with basely (turpiter) denying, what he elsewhere affirms; we may be allowed to think, that he ought to have been peculiarly careful of tripping, especially in anatomical accuracy. Here, however, we find him, in the above quotation, asserting the continual transmission of blood from the vena cava *into the arteries,* thus passing by entirely the heart, both as respects its auricles and ventricles! And in a page or two in advance, he likewise makes use of a similar erroneous expression, viz. that the blood is transfused "out of the veins into the arteries." Unquestionably, he did not mean precisely what his words nevertheless express; but carelessness and inat-

tention are no excuse for Harvey; especially as it continued in all the editions of his work: whilst, at the same time, this very chapter amply demonstrates that he knew full well, "that the arteries receive blood *no where else* but from the veins, *by transmission* through the heart," and I should have passed this in silence, if the subject had not been so highly important, that in its consideration, Harvey's meaning, like Cæsar's wife, should not admit of even the slightest suspicion. His sentence, here, is in some measure, preparatory to the subsequent celebrated estimate of the amount of blood that may be supposed to pass from the heart at each pulsation: now this, of course can be merely an approximation, not founded on any absolute or definite data. He has consequently made the estimate, on a presumption that the left ventricle contains, when fully dilated, *either* one and a half ounces, 2 oz. or 3 ounces*; and that on every contraction "there is sent forth in every pulse of the heart, an ounce and a half, or

* Such diversity exists in different editions, as to render it difficult to know which to select. Thus to mention only the part under notice. The English translation is thus given: "Let us suppose how much blood the left ventricle contains in its dilatation when it is full, either by our thought or experiment, either ℥ii. or ℥iii. or ℥iss."

My Glasgow edition of 1751, has it as follows.—Supponamus (vel cogitatione vel experimento) sinistrum ventriculum in dilatatione, quum repletus est, continere sanguinis uncias duas, tres, quatuor;"—whilst the great 4to. edit. of the Lond. College of 1776, has thus printed it.—"Supponamus (vel cogitatione, vel experimento) quantum sanguinis sinister ventriculus in dilatatione (quum repletus sit) contineat; sive uncias duas, sive uncias tres, sive sescunciam:" The Latin text of the translation given above, is as follows in the Glasgow edition: "Supponamus unciam semis, vel drachmas tres, aut unam tantum sanguinis, quæ propter impedimentum valvularum, in cor remeare non possit:" and in the College edit. thus, "supponamus unciam semis, vel drachmas tres, vel drachmam unam sanguinis."

An idea of the difference of different editions of Harvey's treatise may perhaps be estimated by the statement of the Lond. Col. collected; by which it appears, *that* ed. de motu Cordis, differs from the Frankfort edit. of 1628, in not less than two hundred and fifty instances, and in the two Exercitations to Riolan to one hundred and fifty more. It is true they are chiefly typographical, yet four hundred errors pointed out by the College in a treatise of only about two hundred and seventy 12mo. pages, very widely printed, must be a source of great surprise. Now Harvey should be estimated by his first impressions, and not by those subsequently attained, in part from the animadversions of opponents, or as afterwards given by his friends long after his death. It is called Variantes Lectiones, edit. Francofurtensis, 1628, et edit. 1766, nostra.

three drachms, or one drachm of blood, which by reason of the hindrance of the portals cannot return to the heart."

Harvey's second proposition is "That continually, duely and without cease, the blood is driven into every member and part, and enters by *the pulse of the arteries;* and that in a far greater abundance than is necessary for nourishment, or than the whole mass is able to furnish." Now, since Harvey contends elsewhere, that the *heart alone*, drives the blood through the arteries; and this being his belief, of course there is some further evidence of discrepancy here.

His third proposition is, "That the veins themselves do perpetually bring back this blood into the mansion of the heart," though by what means, he leaves us in the dark. And after running through his estimates, he comes to the conclusion, "that the whole mass of the blood does pass out of the veins into the arteries through the heart, and likewise through the lungs." Excepting this mere calculation, Galen, Servetus and Columbus, appear to have had the same impression: nothing absolutely new seems to be adduced; especially of such a nature as to lead him to apprehend, and "fear mischief" from some persons. Of what kind of mischief he was apprehensive, I have no idea; surely, not personal violence! As to mere difference of opinion, this could, or should, merely have instigated him to a further developement of his views; which are not unfrequently difficult of comprehension, from their extreme brevity;—and by which the incorrect ideas of his opponents might have been set at rest, or fully repelled; without leaving to others to decide between him and the Momes and Detractors, *whom he never read!*

It might well be supposed from Harvey's words, soon after, that, not content with assuming the discovery of the circulation in full; he is also the only one who had noticed the influence of the so called non-naturals upon the pulse and circulation. His words admit, I think, of no other construction; and yet a quotation from his master Aquapendente's writing, given a few pages back, as well as another, from Villa Nova, will sufficiently prove that others before him had fully observed all these particulars of practical importance, if we even admit them to have been ignorant of the true or perfect route of the blood in the system. "In

the mean time, says he, this I know and declare to all men, that sometimes the blood passes in less, sometimes in more abundant quantity, and the circuit of the blood is performed sometimes sooner, sometimes slower, according to the age, temperature, external and internal causes, and accidents natural or unnatural, sleep, rest, food, exercise, passions of the mind, and the like." Now it is certain that none of the old writers appear ignorant of the vast influence of the above mentioned causes over the pulse: they lay great stress in all their writings, on these non-naturals, and none more than Galen. Nor indeed, can we suppose, that they could, practically, have directed blood-letting, with any chance of success, had they not been fully able to appreciate the importance of those causes, which they so sedulously studied in their effects; and perhaps it may be thought that Harvey admits the truth of this opinion, in the next sentence but one, wherein he adverts *to a fact stated by Galen*, and by which he thinks, strength is afforded to his opinion, that more blood is conveyed "into the arteries, and the whole body, than it is possible that it could be supplied by juice of nourishment which we receive, unless there were a regress made by its circuition." The fact he refers to is, "that if any, yea, the least artery be cut, all the mass of blood will be drained out of the whole body, as well *out of the veins, as out of the arteries*, in the space of half an hour." It is surprising how Harvey could narrate this of Galen, and doubt a conviction of a circulation between the arteries and veins, by that extraordinary man; and not less so, that he and others should have ever credited him with the absurd notion of a flux and reflux of the blood in the same vessel!

It is in this chapter that Harvey first makes mention of the valves, or "stoppages of portals;" and so far as I can perceive, his demonstrations respecting these portals, (which, aided as they are by engravings, are excellent and conclusive in the 13th chapter,) are really the only "new and unheard of things," in the whole of his writings. As for this discovery, he admits that "the most famous Hieron. Fabricius ab Aquapendente, a most learned anatomist, and a venerable old man, or, as the most learned Riolan would have it, Jac. Sylvius, did *first of any* delineate the membranal portals in the veins, being in the figure

of a Σ, or semilunarie, the most eminent and thinnest parts of the inward tunicles, of the veins, &c." Acknowledging this prior discovery of his master, he nevertheless in the succeeding page, says, "the finder out of these portals, *did not understand the use of them*, nor others (whom he names not) who have said lest the blood by its weight should fall downwards, &c." We must then perceive, that giving to Harvey the utmost latitude that he himself demands, his whole discovery of the new and unheard of things which he claims in the 8th chapter, resolves itself into that *of the use of these valves:* and whether, after what has already been shown, even by his own admission, that Galen knew the use of the valves of the heart; this, alone, is competent to invest him with the honour of the full and perfect discovery of the circulation, must be decided by the judgment of those who will reflect carefully on the subject, after duly investigating it in all its bearings. My own judgment is undoubtedly in direct opposition to this broad acknowledgment; conceiving that there is ample honour in dividing the discovery with his predecessors, and persuaded that I do him no injustice in coming to such conclusion. It remains however to see, whether even this is not too large a grant; and if his claim is not narrowed down to the mere exposition, in a clearer and more accurately demonstrated light, of those uses, which others had already assigned them. His words sufficiently imply, that some persons had already busied themselves in conjectures on the subject. Not having the works of Sylvius, I cannot precisely state what his opinion was, as it respects their use. Jac. Sylvius, mentioned by Riolan, was born in 1492, or nearly eighty years before Harvey. He was a warm advocate of Galen and Hippocrates, and probably hints at the subject under consideration, in some of his numerous writings. He died in 1555, and from the outline of his works, as given by Vanderlinden, must have been a very extraordinary man. I put him however out of the question, in order to present to the medical reader, an extract from the writings of a man illustrious in his day; wherein we may perhaps see a faint, if not a perfect and full idea, held forth of the valves in question; and *likewise of their use;* if he tacitly admits them to have been known to

others before him, of which I am by no means satisfied; but rather believe he claims this discovery for himself; at least, he speculates on their high importance in the system, and appears to have been *nearly beside himself*, on first seeing them. *And why has this writer never been noticed* by Harvey, or any one of his advocates, in connexion with any thing pertaining to the subject of the circulation? I think I shall make it apparent to every candid reader, who will carefully weigh what is stated, that Harvey was by no means ignorant of him and his writings; and that even without speaking of his views, he indirectly attacks them, and yet gives the reader not the most distant idea of whom he is speaking. Circumstantial evidence of what I thus affirm, against the integrity or the ingenuousness of Harvey is all that can be expected; but I believe it to be adequate, before any jury of our profession, to cause them to bring in a verdict against him. If such should appear to be the real state of the case; I must request again, every medical man, seriously to ask himself, where one individual "new and unheard of thing," is presented by Harvey to the profession?

The author to whom I have reference, is Archangelus Piccolhomini, a celebrated professor of anatomy at Rome, of which he was a citizen, though a native of Ferrara; he was born in 1526, and from Jenty's account, (Histor. Compend. cxiii.) must have been very thoroughly master of the subject; even had not his writings reached us. He was physician to Pope Sextus V., to whom he dedicated his "Anatomicæ Prælectiones," which were printed at Rome, in fol. an. 1586, consequently in his 60th year, and when Harvey was only eight years old. It will be recollected in the Biography of Harvey, that he was born in 1578, and we may just add, that he was admitted into Caius College in 1593, that is, at fifteen years of age; and after six years continuance there, which would bring it up to 1599, and his age to twenty-one; he proceeded to Padua, to study medicine, under Fabricius ab Aquapendente, J. T. Minadous, G. Raguséus, and Jul. Casserius; who signed his diploma on the 25th April, 1602. A full copy of this diploma is given at the end of the quarto edition of his works, printed by the College of Physicians of London, in 1766. It is clear, therefore,

that when Harvey began the study of medicine, and pursued anatomy under Aquapendente, in 1599, that *Piccolhomini's* Anatomy had been printed no less than thirteen years; and must certainly have been familiarly known to, if not a text-book of Aquapendente, and therefore, most probably, equally well known to Harvey: can the reverse be imagined, as at all likely? Padua is scarcely two hundred miles from Rome, and both schools, at that time, were amongst the foremost of the age. Books were not then made on mere speculation, as now, but for actual perusal and investigation; such a one as that alluded to, must have been well established, and familiar, wherever anatomy or medicine was taught: and yet, as before observed, *although* speaking clearly and distinctly of the valves and their use, *neither Harvey, nor any one* of his commentators or admirers have, in a single instance, referred to him! How is this, and wherefore? Let his best friends explain so great and wonderful an omission! and especially, in doing so, let them bear in mind the following indirect proof of Harvey's unquestionable acquaintance with his writings, and which I premise, before I give the extract I have in view.

It is a curious coincidence, yet one that fully assures me, that although Harvey's work was first printed in 1628, or more than forty years after Piccolhomini's, that *certain terms of reproach*, given by Harvey to his opponents, and which have already been adverted to; are also employed by Piccolhomini, under nearly the like circumstances! amongst these, it will be recollected, is to be found the contemptuous appellation of "*Momos*," that is, one who envies another, and which is translated *momes*. Now, this is too remarkable a word, and the circumstances are too nearly alike, to authorize an opinion, that it has accidentally only found a place in the writings of Harvey! It is obvious that Piccolhomini could not have copied it from Harvey, who published forty years after him. Without regarding it then, as plagiarism; I must absolutely presume that it was nevertheless extracted from Piccolhomini, during Harvey's *perusal* of his writings; and the reader will judge of the circumstances, better, after adducing the extract from P. in which it appears. He is addressing the reader in a preface, the

whole of which deserves attention; but I give only the part in which he thus vents his complaint.

"Sicuti scribente Terentio Comico, natura comparatum est, studiosissime Lector, ut qui minus habent, semper aliquid addere velint ditioribus: ita malè natura comparatum est, ut ii, qui minus sciunt, semper aliquid addere velint doctioribus, horum scripta accusantes, atque mordentes, quòd temeraria existimatione judicent, vel aliquid deesse, vel superesse, vel aliquid tale: atque ita prava natura et consuetudine ducti, in doctissimorum virorum scriptis, tamquam in scirpo nodum quærentes, à maledicendo, nunquam desistant, tamquàm homines solùm ad maledicendum nati. Hinc factum esse reor, ut qui suos infinitos præclaros diu noctuque susceptos labores, in publicam omnium utilitatem, tamquam homines ad benedicendum, et benefaciendum nati, proferre decreverint; cogantur initio scriptorum suorum, æquissimo lectori, instituti consiliique sui rationem reddere, *ut hos momos*, qui hominem ex homine exuisse videntur, à prava eorum natura revocentur, et ad humanam civilemque benignitatem sensim deducantur."

Whether this mild philippic of Piccolhomini is the basis of the more bitter one of Harvey, remains, as above stated, for the judgment of the reader to determine. Such is my belief, and for the reasons already assigned. Continuing in the same strain, all of which, however, it is unnecessary to repeat, he adds:

"Nam quum publicè hæc pronunciare sim solitus, Hippocrates anatomen invenit, Aristoteles amplificavit, Galenus perficit, mox *quis ex Momorum numero* dicet, si anatome a Galeno est perfecta, profectò hæ tuæ anatomicæ prælectiones irritæ et supervacaneæ videbuntur," &c.

We see from hence, that Harvey was not the first who had been thus virulently attacked by momes and detractors: but whoever compares the two, will find Harvey, I think, infinitely the most severe. Referring to the English translation already quoted, I would again ask, if there is not some probability of Harvey having looked into Piccolhomini's writings, although he no where mentions him? He was no opponent, since his work was printed before Harvey began the study of medicine; and it is therefore the more surprising that he has so entirely passed him by! Still further, in confirmation of our opinion that Harvey well knew the writings of Piccolhomini, I may remark, that if we look to some of his *expressions*, as in his 13th Chapter, we find them very strong, at times, in apparently

opposing the uses that had been ascribed by "*others*" to the valves; but who those "others" were, is left to conjecture altogether! so that we can draw no conclusions of our own, by investigating for ourselves, but must rest satisfied with the meagre statement of Harvey. Is this correct, especially in the investigation of a subject advanced as new; and accompanied with such "new and unheard of things" as even to make him tremble for his safety! Thus, affirming that the "finder out of the portals, did not understand the use of them," he adds, "*nor others*, who have said, lest the blood by its weight should fall downward: for there are in the jugular vein those that look downwards, and do hinder the blood to be carried upwards. I (as likewise others) have found in the emulgent veins and branches of the mesenterie, those which did look towards the vena cava, and vena porta;" &c.—"Nor are their portals in the jugulars, *as others say*, for fear of apoplexy, because the matter is apt in sleep to flow into the head through the sopral arteries." Now, I would request the reader to compare the *above affirmed uses by others*, with what we shall find in the extract following, from Piccolhomini; and judge if they are not too identical, to doubt for an instant that Harvey had him in view, at least as one of the *indefinite others* (alii, et quidam) he alludes to, although a mystery is made of his name? the reason of which must be left to the candid interpretation of the reader. Realdus (Columbus) and Piccolhomini, are likewise, certainly, of those thus loosely referred to by Harvey, as having found the valves in the mesenteric branches; for Piccolhomini, p. 95, expressly says,

"Cum sint innumerabiles venæ mesaraicæ quæ à jecore extensæ in intestina, suis extremis infingantur; existimavit Realdus, huic harum venarum infinitati, hoc est, infinitis harum extremis, suam cuique valvam esse addictam, ut sicut innumerabiles sunt venæ mesaraicæ, ita innumerabiles quoque sint valvæ, veluti ostiola eis apposita, quæ spectarent foris intrò ita, ut sinant *chylum ab intestinis*, *intra venas* fluere, non autem sinant intus foras refluere," &c.

If I am correct in my idea, I think this chapter of Piccolhomini a very interesting one, and especially in his opposition to some of Realdus' views. I should judge too, that he is speaking, as well as Harvey, of the lacteals, (not long before discovered

by Asellius; and to whom, as to so many others, Harvey has been unjust in his silence,) rather than of the veins, properly so called; the lacteals being indeed, at that period, known by the name of *lacteous veins*. Let us now, then, having thus rendered it possible that Harvey was actually acquainted with this author's work, see what he knew, and has said respecting the valves; and how far it is probable, that the uses assigned to them by him, were, in the highest degree, calculated to lead Harvey to conclusions rather more perfect than had been previously the case, especially as strengthened by a few, perhaps, newly devised experiments.

In treating of the veins, p. 412, which is very concisely done, he terminates the subject in the following manner: and I must again entreat the reader to bear in mind, that this author is not referred to, (so far as I have been able to pursue the research,) by a single person, from Harvey downwards, in relation to this valvular apparatus of the veins, and its uses! although he is unquestionably one of the nameless "others" of the Harveian illustration:

"Restaret itaque post omnium partium explicationem, ut de dispersione et distributione omnium venarum, et arteriarum, quæ illarum sunt comites, disputarem. Verùm quoniam rem hanc celeberrimi anatomici præ cæteris rebus accuratissimè tractarunt, idcirco omittendam putavimus, ab illis petendam. ☞ Unum solum eis addere volo, *magni momenti, ab omnibus prætermissum, quod mihi summam admirationem, quum illud comperi ita excitavit, ut ferè in ecstasim ageret. Quod est, in mediis venis reconditas esse innumerabiles penè valvas, quæmadmodum in orificiis vasorum cordis.* Hæ venarum valvæ maximè conspicuæ sunt in divisione ramorum venæ cavæ. Quarum aliæ supernè deorsum, aliæ infernè sursum, spectant. Ex. gr. ubi vena cava diducitur in jugularem externam et internam, ibi collocatæ jacent valvæ supernè deorsum spectantes. *Quem in usum et finem?* Aliquando demonstravi cerebrum et partium superiorum præpotentem esse vim attractricem, quoniam sanguis quum gravis sit, valida vi sursum trahi et rapi debebat. Illæ igitur valvæ venas claudentes, tantum spatii relinquunt, quantum satis sit sanguini in superas partes attracto; quæ valvæ si non adessent dum homo, ut dormiat; vel ut quiescat, decumbit, universus sanguis fluidus existens irrueret in cerebrum, hujus ventriculos inferciens, et apoplexiam committens. Hunc igitur in finem in superioribus venis, fabræfactæ sunt valvæ supernè deorsum spectantes, ne in decubitu, confertim sanguis in cerebrum impetat, mille cerebri affectus præternaturam procreaturus. Similiter in inferioribus venis, ex. gr. ubi vena cava bipartitò scinditur in tibias progressura, sunt collocatæ valvæ infernè sursum spec-

tantes. *Quem in usum?* Ne sanguis quum sit gravis et fluidus, totus repentè procumbat in pedes, inferioresque partes. In venis itaque à natura constitutæ sunt valvæ, idque ex parvis venarum intervallis, alias sursum, alias deorsum spectantes, in eos præclarissimos usus, quos modo exposui paucis."

If right in my conjecture, as I think I am, that Harvey had perused the writings of Piccolhomini, I cannot but think, likewise, that the uses thus ascribed to the valves, first gave to him the idea of the more perfect intentions of their use, which he more fully developed. That he has not named this very intelligent and perspicuous writer redounds not to his credit! and all things considered, we surely find still further reason for demanding of his admirers, what "new and unheard of things" Harvey actually has propounded? What is it he really and *justly* claims as his own, in the establishment of the circulation promulgated by him? It, apparently, must be narrowed down to a very small compass, divided thus amongst so many of his predecessors.

Continuing the subject in connexion with the valves, he (Harvey) notices the impetuous spouting of the blood from arteries when cut; together with the emptying of the vessels completely, both arteries and veins, conformably to an assertion of Galen quoted by him, that, "not only in the apertion of the great arterie, but if any, yea, the least arterie be cut, *all the mass* of blood will be drained out of the whole body, as well out of the veins as out of the arteries, in the space of half an hour," which, if Galen knew as a fact, as Harvey thus admits, it would seem to convey the impression, of a conviction on his part, of a perfect and complete union of these distinct classes of vessels; and, of course, that a circulation existed between them, without which such an event as he describes could by no means ensue. What mattered it, that he knew not the precise mode of communication, which he supposed to be that of anastomosis. Has Harvey's porosities been better established? But Harvey further notices, in the like connexion, the effect of tying the aorta at the root of the heart, and opening any other artery: the arteries will then remain empty, and the veins full, as explained by his views of the circulation of the blood, from the veins to the arteries, through the heart; adding, that perchance the fulness

of the veins, and the emptiness of the arteries after death, "gave occasion of doubt to the ancients, and of believing, that *spirits* alone were contained in those concavities, whilst the animal was alive." If words have any meaning, and more especially, if their meaning be the same, when used alike by Harvey and by Galen; the former and his advocates can scarcely convict the latter of a fault or error, into which Harvey himself has so repeatedly fallen! If he meant not what his words import, it might be equally asserted in behalf of Galen, had he not, even by the statements of Harvey himself, as formerly pointed out, positively denied that the arteries contained ought but blood alone! Without maintaining, however, that Harvey really believed that spirits existed in the blood, we can only affirm that his words do repeatedly express it; and it surely would be unfair to measure Galen by a rule that would not equally apply to Harvey. It was not, after all, a general opinion of the ancients, putting Galen out of the question, any more than in the time of Harvey; who can with difficulty be exonerated from such a belief. Harvey concludes this chapter by an assertion, in my opinion, unproved; but which will, I apprehend, apply more fully and correctly to himself, individually, than to almost any other writer I have consulted on the subject. "Last of all," says he, "from hence we may imagine, that no man hitherto, has said any thing aright concerning the anastomosis, where it is, how it is, and for what cause," adding, "I am now in that search." A search he never brought to a conclusion.

In resuming this particular, already so largely dwelt on; but which I stated as requiring reiterated notice, from its frequent enunciation by Harvey, I shall here quote the words of his enthusiastic biographer, from the 28th page of his life, in the 4to edit. of his works, 1766, published by the College of London.

"In nulla re magis Harveius elaboraverat, quam ut ostenderet experimentis sanguinem a venis in arterias ea solum lege duci posse, (that is, as just before stated, that 'sanguinem denique ex arteriarum extremis vi cordis *propulsum in carnium meatus tradi*, et ex his, a venarum principiis in cor deducendum excipi affirmasset') ut ab illis cordis ventriculis exceptus et horum motu propulsus in arterias progrederetur; et, quod ad Riolanum præcipue spectat, nullam esse sanguinis a venis in arterias reciproca-

tionem. Hoc consilio in *prima sua excercitatione* valvularum in venis usum summo studio perquisivit et exposuit. (Remember that this was forty years after Piccolhomini's exposition.) Ea res siquidem, valvularum *nempe forma atque fabrica, demonstrante Fabricio*, primum Harveii animum *ita percusserat, ut quasi fulgure coruscante* (☞ by Piccolhomini's premonitory scintillations !) *veras sanguinis vias subito* (!) illustratas perspicere sibi visus esset. Idcirco non *nisi dubie* atque *hæsitanter de viis, quibus ab extremis arteriis in venarum principia* tradatur sanguis, loquitur: et *quodam modo definere fugit, utrum per carnium meatus*, porositates vocat, sanguis propulsus in venarum radices detur, *an* ductu arteriarum continuo in eas deferatur.* *Anastomoses tandem*, quales nimirum veteres voluerunt, *omnino pernegat:* et demum re diligentius pensitata, non experimentis victus, (nam Harveii temporibus nemo, ne *microscopii* quidem ope, venarum arteriarumque copulationes mutuas unquam viderat,† concedit *arteriarum propagines minimas inter venarum tunicas ita posse perrepere, ut sanguis in venas obliqua tradatur via, quali scilicet ureteres in vesicam, et ductus choledochus in intestinum progrediuntur.*"‡ But (continues his biographer,) "Quod rem totam spectat, *necesse est, ut confiteamur* (!) *recentiores, oculis nostris judicibus incorruptis*, ostendisse sanguinem plerumque in venas ex arteriis *ductu continuo, nullo parenchymate interjecto*, deferri; nec tamen Harveii *conjecturam* omnino rejiciendam esse: (good! this is calling things by their proper name; and Harvey's discovery, in the eyes of his warm advocates, thus diminishes to a mere conjecture!) arteriæ enim pleræque, eæ, quæ venarum tunicis vitam et alimentum ferunt, nulla minore vena interposita, in venam cui alendæ dicantur, eodem prorsus modo quo Harveius vult, sanguinem suum effundunt."

Such are the accredited views of a so called discovery! which has immortalized one man, whilst the (nearly or) equal merit of others, is thrown completely into neglect or oblivion! These precious confessions by the College, of Harvey's uncertainties as to the only link apparently defective, must help to determine the real character and proportion of his claim. As he died in the year 1658, or thirty years after the first edition

* And yet, with this acknowledged doubt and difficulty, not less conspicuous *now* than in the time of Harvey, if not indeed of Galen himself; with this proof, that the knowledge of the circulation was then, and is yet imperfect in its most important link of communication, we confidently affirm the discovery to be complete, and give the entire merit of it to Dr. Harvey!

† Yet he employed them, as he says in ch. 4, "*ope perspicilli ad res minimas discernendas!*"

‡ And yet, the discovery of the circulation was considered complete, and the full award given to Harvey!

of his work, he had every facility of perfecting his system, which the co-operation of friends, or the animadversion of opponents, might present for his consideration; and what did it amount to? To the same obscurity, the same uncertainty; but with no relaxation of his exclusive claim: nor a doubt of that being a discovery, which is even yet imperfect and unsettled!*

If the outline of the *general* circulation may be admitted as being more correct, and rendered more clear through his arguments and facts; such acknowledgment must be withheld, when we descend to particulars: and in no part, more than in that which has thus again obtruded on the patience of the reader! It cannot, however, be prætermitted, considering the importance he appears himself to have attached to it; and I can only entreat forbearance, whilst I enlarge on the subject. It has already been shown, that many of his advocates, in speaking of an anastomosis, have viewed it as existing between the *large branches* of veins and arteries; what Galen exactly meant by it, might perhaps be difficult to establish, except indeed to satisfy most readers, that it was not the anastomosis of Harvey's conception, mentioned above, as being of an oblique nature, resembling the opening of the ureter, or ductus cholidochus! Although Galen mentions anastomosis in several places; it is one of them alone, to which Harvey confines himself, viz., that in which Galen's words run thus:

"In toto est mutua anastomosis, atque oscillorum apertio arteriis simul cum venis," etc.

Had Harvey written this himself, no doubt his advocates

* The reader is requested to turn back, (after reading this wonderful supposition of the college, as to the causes leading Harvey to speculate on the circulation to the statement *he himself* gives; chap. I. p. 54; we shall find none of that lightning-like energy, which is so poetically assumed to have siezed upon the mind of Harvey, actually to have existed, so as to have led him instantly (*subito*) to perceive the true route of the blood. No! Harvey tells us, *At length he did believe he had hit the nail on the head.* But how tame is this, to the vivid statement of Piccolhomini, who, on the discovery of the valves of the veins was so excited, as nearly to fall into an ecstacy! Piccolhomini was really in earnest, Harvey is but his simple follower! why the award was so readily granted by the college, without an apparent dissenting voice, if such was the case, it is impossible to determine.

would find every thing in it that the words express, and more; but proceeding from a man so little qualified to think, as it would seem they consider Galen; the plain meaning must be set aside, and a new version of anastomotic conjunction be thus ascribed to Harvey; who, in his first edition at least, gives no explanation of his ideas on the subject. I have before adverted to this, when noticing the 7th chapter, wherein, speaking of the valves of the heart, he refers to Galen's 6th book, ch. 10. de usu partium, and gives a long quotation, of which the few words above, constitute a part. I may here moreover remark, that in Dr. James de Back's (of Rotterdam) "Discourse of the Heart," and containing a warm "defence of Harvey's circulation," Lond. 1673, at p. 87, when impugning the opponents of his system; we find *some extremely curious confessions*, which throw some shade upon his candour; and in part, set at naught the explanation above quoted from the College in his behalf, as to anastomosis. In the part alluded to, de Back thus upholds Harvey's opinion against that of Descartes. "He (that is, Descartes) says, that the commendation of this invention, (anastomoses) is to be ascribed to an English physician, which broke that ice, to wit, resolved that doubt, why the veins are not emptied, and the arteries not burst, since all the blood which passes the heart, flows out of these into them."—"It is true indeed, (continues de Back,) that venerable Dr. Harvey, endeavouring to render the tenent of the circulation of the blood more possible and plain to the minds of those that were averse from it, (because some, as he says, believe nothing but what they have an authority for,) brings that place of Galen, (de usu partium, 6. cap. 10,) where he says, that there is a mutual anastomosis in all, and an interchangeable opening betwixt the veins and arteries, where they touch." I have more than once referred to this quotation from Galen by Harvey, and need not repeat what I have before said. But the reader must not be deprived of the confession of de Back (I will not say) in *favour* of Harvey; because it places him in rather an unenviable position, which assuredly was not intended by his panegyrist. "But the venerable man (Harvey) *cites that place only as it may further his purpose*, though it be his intention, that the

blood passes through the habit of the body; and not without reason, since nutrition is performed in manner aforesaid." We cannot hesitate, then, to admit with his warm adherent de Back, that Harvey *merely* "*cites the place, only as it may further his purpose*;" and hence it would seem to follow, that he makes a convenient stepping-stone of Galen, when he deems him useful; but brings none of those numerous references forward, which might illustrate and support his claim to a knowledge of the circulation, altogether or in part.

In drawing the comparison between the opinions of Harvey and Descartes, relating to the anastomosis of the vessels, some may possibly allow the advantage to the latter. After giving some of his own ideas of these passages of the blood through the most hidden recesses, &c., by means of pores, de Back proceeds to say, "The most famous man, Descartes, makes these anastomoses so necessary, that by them he thinks the way is only open to the circulation of the blood, yea, so manifest and patent will he have them to be, that that which out of the arteries through their extremities does flow into the veins, suffers, as he says, no change;" &c. It may be wondered, possibly, that as so few medical men have been mentioned by Harvey and his advocates, how it happened that Descartes, who was not a physician, should be so greatly noticed, on a subject in which, apparently, he had no concern! I can answer this only by a surmise. It has, in an early part of this treatise been already noticed, that V. F. Plempius is stated by Dr. Z. Wood, to have changed his opinions from the "persuasive and forcible reasons" of Harvey. Now, it is not unlikely, that such a remark might induce a reader to look into Plempius; in doing which, he would soon discover that Plempius holds a long correspondence with Descartes, relative to many of these very particulars; but as Descartes differs considerably from Plempius, and consequently from Harvey, de Back takes a politic step in prepossessing thus his *readers* in favour of Harvey, before he should take up the other. See Plemp., Lib. 2. cap. 5. p. 170, et seq. In these letters, dated 1638; of course, ten years posterior to Harvey's work, is to be found a rude sketch of the attempt of Galen to introduce a hollow tube into the artery; as has been before noticed, on account of

the strange and unaccountable tergiversation of Harvey, as I think was clearly demonstrated.

I proceed now to the next, or 10*th Chapter*, headed as follows: "*The first supposition concerning the quantity of the blood which passes through from the veins into the arteries, and that there is a circulation of the blood, is vindicated from objections, and further confirmed by experiments.*"

This chapter is pretty generally satisfactory; some very interesting experiments are referred to, which are, perhaps, exclusively his own; and are of a nature to force conviction of the truth of the general proposition, and the outline of the general circulation. They are, in fact, worthy of all praise; they lead Harvey to the conclusion, that "there are two sorts of death, extinction by reason of defect; and suffocation by too great quantity.

The 11*th Chapter* is taken up with the consideration and proof of his second supposition, viz., "*That the blood is driven into every member and part, and enters by the pulse of the arteries; and that in far greater abundance than is necessary for nourishment, or than the whole mass is able to furnish.*"

Harvey here attempts to show, by sundry experiments, made with ligatures of different degrees of force, "that the arteries are vessels carrying the blood from the heart, and the veins the vessels and ways by which it is returned to the heart itself."

These experiments are equally interesting and satisfactory, as well as his general remarks, except as regards the pulsation of the arteries, wherein he seems to differ somewhat from his former declaration, "that it depends solely on the impulsion given to the blood by the heart." But when he adds to the preceding quotation, "that the blood in the members and extremities does pass from the arteries into the veins (*either mediately by an anastomosis, or immediately through the porosities of the flesh, or both ways,*) as before it did in the heart and thorax out of the veins into the arteries," then it would appear, that the same difficulties exist, as have already been noticed; the idea of porosities intervening between the arteries and veins, has never

been substantiated, although it has been continued at intervals, from Harvey to the present day; and has been opposed by the microscopic observations of Swammerdam, Lewenhoeck, Ruysch, and others. Harvey, at any rate, seems here entirely unsettled in his belief, whether the one or the other, or both, were the intermedia of communication; and I must refer the reader back to the extract from the college on this subject, to aid him in coming to a right conclusion, as to the imperfect notions he had conceived on the subject. We see, beyond a doubt, that, left as he has done it, the demonstration is altogether imperfect; and merely circumstantial, whichever opinion may be advocated; that is, if nothing is to be admitted that is wanting in proof. And we must again repeat, that the doctrine of anastomosis, as advanced by Galen, was at least fourteen centuries old. If Galen is wrong as to this doctrine, Harvey cannot be right; nor has he in any manner improved it! and if he sustains the doctrine of porosities, which Galen opposes, he has not proved it. It may be remarked, that Galen had no doubt of the truth of the position he maintains; whilst Harvey seems entirely at a loss, in one place, to which side he should attach himself, or if he had not better clinch the matter, by adopting both; whilst in other places he firmly sustains porosities, and as firmly decries the anastomoses! If thus inconsistent with himself, why should such inconsistency be passed by, and an exclusive claim be made for a discovery which he obviously left imperfect? The rest of this chapter is taken up, as stated, with the character and effects of ligatures; together with an explanation of those effects, conformably to the views of the circulation. They are interesting, and assist greatly in substantiating the doctrine; but in connexion with my more particular object, scarcely require to be noticed. I might perhaps object to some of the explanations given; as at p. 68, when he assigns as the cause of fainting, on untying the bandage in blood-letting, "the return of *cold blood* to the heart;" and I might again recur to the repetition of his ideas, "that the blood does pass out of the arteries into the veins, and not on the contrary; and that there is an anastomosis of the vessels, *or* that the pores of the flesh and solid parts are pervious to the blood;"! but it would be only an equal repetition of my

former remarks; and I shall merely observe, that the unfortunate horns of this dilemma, pores and anastomosis, seem to have entangled and perplexed Harvey during his own life-time, and his professional posterity ever since; as if to enforce a belief, that, as the doctrine of the vitality of the blood is a matter of revelation; so that mysterious union, by which it is possessed of this wonderful accompaniment, is intended still to remain a mystery; by our inability to detect the real character of that connexion, which must necessarily exist between the arteries and veins!

The 12th chapter is headed, "*That there is a circulation of the blood, from the confirmation of the second supposition.*" This, it will be remembered, is, that "continually, duely, and without cease, the blood is driven into every member and part, and enters *by the pulse of the arteries:* and that in far greater abundance than is necessary for nourishment, or than the whole mass is able to furnish." All which, in like manner, is chiefly dependent on the same proof for its elucidation, which is derived from ligatures. It is therefore scarcely necessary to dwell on this chapter. I shall only remark, that here, p. 72. we find, again, Harvey's opinion laid down, that "the force and impulsion of the blood *is only derived from the heart.*" This was Galen's opinion, and if so, cannot be considered as one of *the new and unheard of things*, which the former adverts to. What he exactly means to convey to the reader, in the very next sentence, a part of which reads thus, "and the arteries at no time receive blood *out of the veins*, unless it be out of the left ventricle of the heart," I do not exactly comprehend, seeing that he thus passes by the right ventricle of the heart, and the pulmonary passage. It is at least, obscure. A recurrence to his calculation of the amount of blood passing in a definite time, assists him in the further consideration of this chapter. It is probable that all may not coincide with him in ascribing to *fear*, ("by which the heart do beat more faintly,") the diminished flow of blood; or that "after the same manner does it come to pass, that women's flowers and all other fluxes of blood are stopped." Not being absolutely connected with the object of my pursuit, I shall quit the subject, and proceed to the next chapter.

*The 13th Chapter* informs us that "*The third supposition is confirmed, and that there is a circulation of the blood from the third supposition.*" Which is, "that the veins themselves do perpetually bring back this blood into the mansion of the heart;" and hence that a *circular motion* is made by it. This proposition appears to be of the greatest importance to him: this idea of a *circular* motion (if not a misnomer) seems, indeed, the very gist of all his remarks, to which they all tend; and which would almost seem to be the pith and marrow of the "new and unheard of things," to which he had before referred: and, as no one before him had actually employed the term of circular motion to the circulation, however they might have understood its route; unquestionably, Harvey (so far as the term extends, and may be considered either correct or judicious) must be entitled to all the advantage it can possibly afford him! As I observed, it appears to supersede both the preceding propositions; as it enables him to explain his ideas, beyond what he hitherto had done, relative to "the quantity of blood that passes through the lungs and heart in the centre of the body, and likewise from the arteries into the veins and habit of the body." In this chapter he points out the way in which the circulation is completed, by the "blood flowing back from the extremities, through the veins, into the heart, and how the veins are the vessels that carry it from the extremities to the centre." All which, he thinks sufficiently credible, and considers them much strengthened by "the portals which are found in the concavities of the veins, *their use*, and from ocular experiments."

It has already been shown that Harvey does not pretend to claim the discovery of the valves; those of the heart having been known not only to Galen, but even to Aristotle and Hippocrates; whilst those of the veins, he here immediately ascribes to his preceptor Fab. ab Aquapendente, or to Sylvius, if Riolan was right. This part of the appendage to his "circular motion" of the blood, constitutes, therefore, no portion of the "new and unheard of things" which, even at the printing of his book in 1628, or twelve years after their first *public* promulgation, led him to apprehend mischief to himself, and that they would set every "man almost," like an Ishmael, against him! Can the reader form a judgment, *which of his new and unheard of things* were calculated

in the remotest degree to produce such a catastrophe in the pursuit of science? Cannot a reason be surmised for this apprehension on his part, in the utter contempt with which he treats some of his opponents; and the slight merit ascribed by him to any; as well as in his total omission of many of his predecessors, whose writings ought to have been duly noticed! The candid and unbiassed reader must determine how far I have succeeded in proving that Piccolhomini was among (if not) the first who discovered the valves and pointed out their use. If the reader should decide that, as he, Harvey, admits he did not discover the valves; and yet, that he considered the passage of the blood back, through the veins, to be rendered "*plain enough* from the portals found in the veins," let me ask him, whether a man who was so excited at seeing them, as nearly to fall into an ecstacy, (ita excitavit, ut ferè in exstasin ageret) was less likely to have a suspicion of their use, than one, who, like Harvey, has told all he has said of them in the most phlegmatic manner? Could that which was so plain to Harvey, make no impression on his excitable precursor? If we admit that Harvey has more definitely and better demonstrated their use, (which we may have rendered doubtful;) surely, this cannot give him a claim to either a perfect explanation of the circulation, whether particularly, or generally considered; and still less to that of the sole discoverer! Is the pioneer not a discoverer, or at least deserving of some merit, if any there be, because he is deficient in the opportunity or means of a more successful follower? or, is his very name to be forgotten, although clearing away the rubbish of the wilderness for his successor? But who, of all these pioneers, has he mentioned or omitted, and to what extent? And now, let us follow him, and see what use he ascribes to the valves; and then balance between it, and that which he admits had been hinted at before him! If *their* deficiencies are to exclude *them* from a participation in the honour of the discovery; surely, a deficiency or imperfection on the part of Harvey, ought to be equally fatal to his claim.—Now, he says, (p. 77,) that,

"The *portals were made*, (*omnino*) *lest* the blood should move from the greater veins into the lesser, and tear or swell them; and that it should not go from the centre of the body to the extremities, but rather from the

extremities to the centre. *Therefore* by this motion the small portals are easily shut; and hinder *any thing* which is contrary to them; for they are so placed and ordained, that if *any thing should not be sufficiently hindered in the passage by the horns of the foremost*, but should escape as it were through a chinck, the convexity or vault *of the next* might receive it, and so hinder it from passing any further."

From this quotation, it would seem to me, that Harvey's ideas of the use of the valves are infinitely less expanded than those of Piccolhomini! In fact, they resolve themselves principally into that of presenting an obstacle to *the forward passage towards* the heart, *of "any thing which is contrary to them."* What edition Dr. Wood employed in his translation, as above given, I know not: he must have been, at any rate, very ignorant, or extremely inattentive, as I have already pointed out in several instances; and I am therefore disposed to think he has here given a wrong translation, on which, nevertheless, my observations are founded; and the reason I continue them, even under the conviction I have, is for the purpose of again enforcing the necessity of referring to the originals, in all cases when possible. Now, the part above translated by Wood, "therefore by this motion the small portals are easily shut; and *hinder any thing* which is contrary to them," stands thus in my Glasgow edition, and in that of the College, respectively: "Ita *enim* huic motui valvulæ tenues facile occluduntur, contrarium *motum* omnino supprimentes." *Gl.*—"Ita huic motui valvulæ tenues facile *recluduntur*, contrarium omnino supprimunt." *Lond.*—Now, whether Wood's translation is from Harvey's first, or any subsequent edition, and will bear the construction he gives, I know not: but the above Latin extracts differ, as we perceive, and one of them may, though not probably intended, afford some ground for Dr. Wood. The reader will therefore recollect, that the remarks I here make are altogether dependent on the possibility of Wood being correct; and are to be received for merely what they are worth in such a connexion. I repeat, then, that Harvey's notions of the use of the valves are limited and scarcely probable, when he refers, *principally*, to their presenting an obstacle to "any thing that is contrary to them." Contrary to what? Is it to the valves? What harm would they receive? And if

we suppose, with Harvey, that this "thing," whatever it might be, (ut si *quid* per cornua. L. ut *quidquid.* Glasg.) had actually escaped detention by the horns of the foremost, why should it not as likely escape the others, which are growing progressively larger? This is surely a rude conception of their importance to the system; for what, except blood, do we ever find in the veins, to be thus obstructed by this valvular appendage? And how could the blood itself, as he suggests, even if no valves existed, move in a retrograde manner, from the greater into the smaller veins, whilst these last were kept continually filled, by a vis a tergo? Nay, even admitting it to be the case, on what principle could Harvey suppose this ideal movement of the blood, from the greater into the lesser veins, should *tear or swell* them, considering the numerous intercommunications by anastomosis of the vessels themselves? The whole, to me, seems at best a mere gratuitous assumption on his part, which will not bear a closer examination than some of those to which he makes an imperfect reference, from not mentioning the names of the individuals, whose opinions he very unceremoniously attempts to set down! Why has Harvey not afforded us the opportunity fairly, of estimating the force of his opposition, by directly referring to the individuals he has in view, and particularly pointing out the part of their writings? Who can tell from his own words whom he means? or, if perchance we may hazard a conjecture, to what part of the works of an uncertain author shall we have recourse? Why, I repeat, is this illiberal plan pursued by Harvey? Surely, all his opponents were not Momes and Detractors! and, in a proposition of such importance, by which, from his own statement, "new and unheard of things" were to be announced to the medical republic, could he justly imagine that all his assertions were to be adopted without the slightest investigation or objection on the part of others? Was this "dogma fidei medicorum," as Pitcairn expresses it, to be unresistingly enforced; and its verity and novelty, or its exclusive claim by an individual, not to be inquired into, save under the penalty of obloquy and reproach? If, from any cause, the Profession of that period thought it unnecessary to analyze those claims in every point of view; whether convinced, without further inquiry, of what was pre-

viously known, of the perfect and undoubted claim of Harvey; or deterred, from a dread of being classed among the Momes and Detractors, by his caustic pen; assuredly the object of inquiry is of that importance, that the mere lapse of two hundred years cannot be considered as sufficient to preclude a renewal of the controversy. Galen maintained a supremacy for more than one thousand years, in every department of our science; yet a judicious investigation into his claims, has (perhaps) pruned him down, and his pretensions, even beyond what is strictly correct. If it may be thought that Harvey has been wanting, in some respects, in candour to his contemporaries, as we believe the fact to be; there can be no great ceremony required to open and renew the subject of investigation on this side of the Atlantic. If deserving of the full honour of this great physiological discovery, his numerous adherents every where, will quickly detect the fallacy of these pages; and that honour will continue to descend undisputed and unclouded to the latest posterity! But, if what I claim for others is not entirely unfounded, let a just verdict be awarded in their behalf. I know no case recorded, in law or medicine, in which a close and uncompromising scrutiny and cross-examination is so requisite, in order to elicit truth! All will agree, that if his claims are truly founded, they cannot suffer from such a rigid touchstone; and that the claims of others cannot be improved, if error or deception forms their basis! This scrutiny, it is probable, may be more appropriately and certainly pursued in Europe, from the greater facility of access to all those writings of that period, in which we are for the most part so defective in this section of the Globe.

Here, on the subject of the valves, which led to these remarks, I must refer the reader once more to the writings of Piccolhomini, and especially to that part already noticed. Printing his work on anatomy when Harvey was only eight or ten years old, we must reasonably conclude, that the valves, if discovered by him, must have been the source of wonder and astonishment, so as, in his vivid description, to have "ita excitavit, ut ferè in ecstasin ageret:" and equally, that they must have been familiarly explained and described by him in his lectures. Compare the lukewarm account which Harvey gives of them, and let this

very circumstance decide between them. The difference is as great, as the statement and action of the two women in the judgment of Solomon. Piccolhomini, we have shown, could be no jealous rival of Harvey, or opponent of his popularity and fame, since he so long preceded him; and was certainly of such high standing as a teacher, that a reference to him and to his writings, bearing, as they do, so closely on the subject, could never have discredited Harvey; whilst such *total omission* of him, strongly leads to a suspicion of the neglect being more than merely accidental; and cannot be overturned even by supposing (a thing incredible), that Harvey was absolutely ignorant of the man, and of his writings, although studying at Padua, so near to Rome; the seat of his professorial labours! Admitting that the explanation of the use of the valves is imperfect in the hands of Piccolhomini; the reader is requested seriously to consider whether a part, at least, of Harvey's explanation is not equally inconclusive; and likewise to reflect, whether even this imperfect exposition of Piccolhomini, was not the probable precursor of Harvey's improved elucidation? Such superior elucidation is indeed conspicuous in this chapter, in Harvey's exposition of an arm tied up for bleeding; and his explanatory remarks in the various steps of the process. As these, however, are particularly referred to, by letters connected with corresponding marks in the accompanying engravings, it would be impossible to notice them, unless the figures were themselves introduced. Nor indeed is this by any means essential to the object in view.

I shall, therefore, proceed to the next, or 14*th Chapter*, headed, "*The conclusion of the demonstration of the circulation of the blood.*"

I have again to express my regret at the continual reference I am compelled to make to the same subjects: but Harvey has so often renewed them, that I am compelled, in obedience to the plan I have adopted, to follow in the course he has himself marked out. In this chapter Harvey propounds, in the last place, his opinion concerning the circulation of the blood, and says that,

"Seeing it is confirmed by reasons and ocular experiments, that the blood does pass through the lungs and heart by the pulse of the ventricles, and

is driven in and sent into the whole body, and does *creep into the veins and porosities of the flesh*, and through them returns from the little veins into the greater, from the circumference to the centre, from whence it comes at last into the vena cava, and into the ear of the heart in so great abundance, with so great *flux and reflux*, from hence through the arteries thither, from thence through the veins, hither back again, so that it cannot be furnished by those things which we do take in, and in a far greater abundance than is competent for nourishment; it must be of necessity concluded, that the blood is driven into a round by a circular motion in creatures, and that it moves perpetually; and hence does arise the action and function of the heart, which by pulsation it performs; and lastly, that the motion and pulsation of the heart is the only cause."

Here, then, in a few words, the whole business is laid down, in plain and explicit terms. But the singularity of the explanation thus given, consists in his paradox, that the circulation and perpetual motion of the blood, gives rise to "the action and function of the heart," whilst the "motion and pulsation of the heart is the only cause"—of what? why, of the circular and perpetual motion "of the blood!" Both are alike cause and effect, reciprocally of each other! will his words admit of any other construction? If they cannot, how stands the position, philosophically considered? The reader must determine for himself. I may further observe, that if disposed to view the terms he employs, unduly, that is, without considering their appropriate and definite connexion with other parts, we might readily do him (as he has done with Galen), the injustice of ascribing to him the idea of a mere flux and reflux of the blood, analogous to the tides of Euripus. Had Galen been fairly judged of by himself, such an opinion would never have been urged against him; and I only make this observation, to point out how very readily errors may be heaped on individuals who probably in no way merited the aspersion.

Here again, the renewed assertion of the flow of blood through the whole body, by "creeping into the veins and porosities of the flesh," requires a further attention. What Harvey intrinsically means by the "porositates carnis," he has not explained in his treatise; but in one of his exercitations to Riolan, I have on a former occasion pointed out his conception of it. We must estimate it accordingly, either by that, or by the statement of his

advocates; some of whom have already been pressed into the service, with the view of showing that, even confining our ideas of the vascular connexion, with Harvey, to porosities alone, no uniformity exists amongst its adherents! It might not, perhaps, be improper to consider, what would be the probable result of blood, thus passing *from* its arterial channels into the porosities, or parenchymatous structure of the lungs! Wherein may it be viewed as differing from simple extravasation, by which an engorged state of the lungs would ensue, and peripneumonia notha, or something like it, inevitably follow, before it could reach the asserted patent orifices of the veins? But could they remain patent, with the blood, thus extra limites, pressing on every adjoining part; and by what mechanism or structure could the veins effect it? The evil is the same in character, if we carry this proposition to the porosities or parenchyma of any other part or organ. It would indeed appear, that in striving to evade the Galenical anastomosis between the arteries and veins, he has completely closed the door to any explanation of their junction, by an intermediate class of vessels, now known as the capillary link, and by most of its adherents, I believe, considered as nearly, if not entirely, without the range of impulse from the arteries, and acting by some unintelligible inherent power, by which the blood is received by, or penetrates the veins. A capillary link was indeed always maintained, but it was not considered as a separate one, but merely the minute, or most attenuated branches of the arteries, joining with the veins; the latter beginning, according to some writers, where first appeared a portal, stop or valve; and modified in character, moreover, by the difference of its coats. This spontaneous continuation by united tubes, seems scarcely to have been dreamed of by Harvey; his anastomosis seems rather a junction of the sides of vessels, by an opening somewhat like the ureter into the bladder, or the gall-duct into the intestine. If neither porosities nor anastomosis be fully adopted, in other words, if we know nothing of the mode of union; and if Harvey knew as little, or less than Galen, on the subject; wherein has *he* demonstrated the true route of circulation; or by what false logic can his probable, or even circumstantial evidence, be speciously denominated a discovery?

With these, and other circumstances that will probably be recollected and considered by the reader, we must leave this chapter; requesting him at the same time, to reply ingenuously to the question so often proposed, of what "new and unheard of things" he has been informed by Harvey; and why has he so completely failed to speak of the great hepatic or portal circulation? A circulation almost isolated, and of a character peculiarly its own; and bearing apparently the same affinity to the general circulation, that the vast and important ganglionic system of nerves bears to that of the cerebral organs: both separately independent of, and yet mutually essential to, the welfare of the other.

The 15th chapter, headed thus, "*The circulation of the blood is confirmed by probable reasons*," is not exempt from those repetitions and erroneous data of which I have so frequently complained; but which I must nevertheless follow, as he leads the way. Referring to Aristotle, de Respiratione, we come to some luminous traits of the physiology, which the *discovery* of the circulation had unfolded in the mind of Harvey: perhaps they are quite as correct, nevertheless, as any now promulgated; and I advert to them to show, that the mere knowledge of the *true route* (if we absolutely know it) of the circulation, no more mended the physiology, than it did the practice, of the day. But what says Harvey?

"Seeing death is a corruption which befalls by reason of the defect of heat, and all things which are hot being alive, are cold when they dye, there must needs be a *place and beginning of heat*, (as it were, a fire and dwelling house) by which the nursery of nature, and the first beginnings of inbred fire may be continued and preserved; from whence heat and life may flow, as from their beginnings into all parts; whither the aliment of it should come, and on which all nutrition and vegetation should depend. And *that this place is the heart*, from whence is the beginning of life, I would have nobody to doubt."

I must, after duly weighing the premises, express my doubt whether Galen, in any part of his voluminous writings, has comprised so much absurdity, as Harvey has here done in so small a space. Will this good man's interdiction prevent us doubting

the truth of his position, that the heart is the beginning of inbred fire, or animal heat? Has he not, himself, already shown, that the heart is not "the beginning of life," in the proof he has afforded of the formation of blood, prior even to that of the heart itself? He furnishes no proof of his affirmations, and they can scarcely be tolerated in the present day. His chain of proofs of a circulation, so far as this is concerned, is therefore defective. Nay, if he had confirmed its truth, he ascribes it to Aristotle, and can therefore claim no merit from it, or locate it amongst his "new and unheard of things." I scarcely think that Aristotle himself, his predecessor by two thousand years and more, has reasoned so erroneously, so ridiculously, on the subject of animal heat, and its evidences, as Harvey does, in this chapter: thus, he almost immediately subjoins to the preceding extract, that a motion was required to the blood, that it might return again to the heart, lest,

"Being *sent far away* into the outward parts of the body, from its own fountain, *it would congeal and be immoveable*,"

And which, indeed, would probably be the fact, if his affirmation was true, of its escaping from the arteries into the porosities of the flesh: and,

"Seeing therefore, that the blood, *staying in the outward parts is congealed* by the cold of the extremities, and of the ambient air, *and is destitute of spirits*, as it is in dead things, it was needful it should resume and redintegrate, by its return again, *as well heats, as spirits*, and indeed its own preservation, from its own fountain and beginning."

What claim has Harvey to throw a stone at Galen, respecting his asserted ideas of a spirit in the blood, when we thus see him perpetually enforcing his belief of the same, so far as words have meaning? Can that be right in Harvey, which he himself reproves in another? or, having thus reproved him, is he himself not ten-fold more reprehensible? I do not believe Galen has, any where, so egregiously committed himself (although he believed the blood to be the vehicle by which animal heat was conveyed to every part) as to talk of the blood, "staying in the outward parts, and congealing there by the cold of the extremities, and of the ambient air!" Will any of Harvey's warmest

advocates maintain such views as are above given, in all their details? or how would that venerable man, himself, be now received, if standing on the spot of his former eminence, and promulgating to the present College of Physicians his singular propositions?—His views respecting the circulation, then, if correct in every particular, and if admitted fully to be his own; have not elucidated those functions, for which that singular and perpetual process of the animal economy seems to have been intended. To throw those views on Aristotle, since he refers them to him, will scarcely answer: if they are Aristotle's, he completely adopts them; and since he has been shown to claim and retain many particulars that were known previously to others; there can be no difficulty in allowing him all the honour which these physiological opinions can confer upon him. He tries to strengthen these views, by observations or comments not more worthy of regard; but all evincing, how little physiological benefit the merely pointing out the route of circulation with more precision, actually conferred upon him! thus, he talks of that exterior cold that chills the extremities, causing them to look blue,

"Like those of dead men, *because the blood stands still* in them, (as in carkesses in those parts which are down tending) whence it comes that the members are numbed, and hardly moveable," &c.

Is such the physiology of the present day? are these dogmas to be received, because they bear the impress of Harvey's pen? But let us hear the luminous proofs and illustrations that immediately follow!

"They could, says he, certainly by no means, (especially so soon,) recover heat, and colour, and life, unless they were by a new original, a flux, and appulsion of heat, again cherished. For how can they attract, in whom heat and life are almost extinct? or those that have their *passages condensed and stopped with congealed blood*, how could they receive the coming nourishment and blood, unless they did dismiss that, which they before contained, and unless the *heart were really* that beginning from whence heat and life, (as Arist. Resp. 2.) and from whence *new blood* being passed through the arteries *imbued with spirit*, that which is enfeebled and chilled, might be driven out, and all the parts might redintegrate their languishing heat and vital nourishment almost extinct."

It is surely unnecessary to add more, with the intent of showing that, however Harvey might justly boast (dedicatory epistle) that he "did not profess to learn and teach anatomy, from the axioms of philosophers, but from dissections, and from the fabric of nature;" it would not have been amiss, if he had benefited even by the physiology of Galen, to improve his own. If more proof is, however, required, it presents itself in the next page, in relation to the concoction and distribution of nourishment; which, as

"All creatures live by nourishment, inwardly concocted, it is necessary that the concoction and distribution be perfect, and for that cause, the place and receptacle *where the nourishment is perfected*, and from whence it is derived to every member. But *this place is the heart*, since it alone of all the parts, (though it has for its private use the coronal vein and artery,) does contain in its concavities, as in cisterns, or a cellar, (to wit, ears or ventricles,) blood for the public use of the body; but the rest of the parts have it only in vessels for their own behoof, and for private use."

Here we must again ask, if present physiology teaches the heart to be the place and receptacle where the nourishment is perfected? and whether, with all his knowledge of the circulation, Harvey possessed any correct views of the nature and function of respiration, of animal heat, or of the concoction of nourishment? Has he not also been guilty of some ambiguity, to say the least of it, in affirming the blood to be squeezed out of the capillary veins into the little branches, and from thence into the greater, by *the motion of the members and muscles;* since the motion of the blood had before been solely ascribed to the impulse of the heart alone? Admitting even that the power of the heart was capable of driving the blood out of the arteries into the porosities of the flesh; he has no where explained by what subsequent power, it becomes afterwards enabled to reach the veins!

But we proceed to the consideration of the 16*th Chapter*, which is thus headed: "*The circulation of the blood proved by consequence.*"

To much of this chapter, there can, perhaps be but little objection. Indeed, the *consequences* are but the natural result of

the premises adopted. It may, however, be remarked, that this chapter places Harvey in the light of a powerful friend and advocate of the doctrines of Humoralism. This pathology, so strongly adopted by Hippocrates and Galen, who are denied to have had a knowledge of the circulation of the blood; is now opposed by those who think they are fully masters of it, in every particular. Their partial estimate of this important fluid would almost seem to imply, that they regarded it as of but little actual importance in the animal economy; and it might be just as well to regard it as dead matter merely, since they generally deny it to be acted on by morbid or therapæial agencies, which, according to them, can never reach the blood, unchanged or undigested! Be this, however, as it may, it must be admitted that in this chapter, Harvey has evinced much physiological and pathological absurdity, showing still more clearly that the mere route of the blood, simply considered in itself; that is, the circulation in its simplest aspect; had not much enlightened him, as to the important connexions it maintained with every part of the body; nor unfolded to his view any great superiority in his physiology or pathology, beyond that of antecedent ages. Few would now, it is presumable, give the priority of research to Harvey; or regard him as the founder of a new and improved doctrine in those branches of medical science; let his other writings determine, not this only, but likewise his practical attainments, in order to judge whether, in any of them, he rose above the common ranks of the profession. Even in his justly celebrated treatise on Generation, &c., it will be found that much of it was really established before he commenced his pursuit, not only by his master, Aquapendente, but even by Hippocrates and Aristotle.

Although erroneous in his explanation, we find him attempting his proof of the heading of this chapter, by means of the endermic application of remedies; which, he says, when outwardly applied, use their force within, as colocynth, aloes, garlic, cantharides, adding, that,

"From hence it is constantly averred, perchance not without cause, *that the veins*, through their orifices, draw a little of those things which are outwardly applied, and carry it in with the blood, after the same manner as

those in the mesenterie do suck the chylus out of the intestines, and carry it to the liver, together with the blood."

How much of this speculative explanation will be acceded to, by Harvey's advocates at the present period, it would be interesting to know; as well as many other particulars, equally well illustrated in his writings.

A little further in this chapter, we reach the part I formerly adverted to, wherein he falls completely into the very same error he so sedulously points to in Galen. I allude to that in which he considers Galen as maintaining the motion of the blood up and down in the same channel, like the tides of "Euripus reciprocating its motion again and again, hither and thither." If words have any meaning, I think the following will sufficiently prove this: now, since Harvey quotes the passage from Galen, in order to prove his want of knowledge of the circulation; we might equally quote the one in question, for a like purpose, if he had not, elsewhere, so fully proved its existence; and I believe, if Galen should ever be invested in the fair dress of an English translation, very many parts would go far to demonstrate, that this isolated passage of the Euripean tide cannot disprove it, any more than the following disproves that of Harvey. In fact, the ideas conveyed by Harvey's express language, would seem to be precisely what he before so abundantly criticises in the immortal Galen: viz., that in the same stem or branch of the capillary veins, *there are two opposite motions, one of the chyle upwards, and another of the blood downwards;* and that this is done by a main providence of nature. But how the chyle gets into the capillary veins, he no where points out. If he could only have satisfied us as to this *first step* of this very singular assertion, in a physiological point of connexion with his details of the circulation, I should not think much of then adopting the residuary portion, viz., that of the *two opposing currents* in the same vessel; even although more absurd than that of the tides of Euripus; which, though running backwards and forwards, yet, they did this at *different times*, and not at one and the same moment. If then I am correct in the meaning of the quotation, in which I think it will strictly bear me out; then, I must truly think, that the asserted Galenical or Aristotelian tides of Euripus, ought for

ever, to yield the palm of absurdity to the superior and more glorious one of Harveian discovery! We may in addition remark, that in what is precedingly stated by Harvey, of the "chylus sucked out of the intestines" by the veins, we are warranted in believing, that he imagined this very first step of sanguification and circulation was effected by veins, and not by lacteals; for he adds, that they "*carry it to the liver with the blood.*"! If he knew no better than this, although the lacteals were then, unquestionably, familiar to anatomists; how deficient does this great discoverer of the circulation appear, on points of quite as much importance! Worse and worse, in fact, the further we advance, and which leads us to the full quotation I have kept in view. Whoever adopts his opinions, therein expressed, no doubt will fully ascribe to him the whole and undivided discovery of the circulation! yet it may possibly cause some, at least, to hesitate, who may never have seen or read the writings of Harvey.

"In the mesenterie likewise, the blood entering into the cœliac arterie, the upper and nether mesenteries, goes forward to the intestines; by which, *together with the chylus attracted by the veins*, it returns through the many branches of them into the porta of the liver, and through it into the vena cava; (qu? do not the extremities of the portal veins empty into the hepatic veins, before reaching the cava?) so it comes to pass, that the blood in these veins is imbued with the same colour and consistence, as in the rest, otherwise than many believe: for we *must needs believe, that it very fitly and probably comes to pass*, in the stem or branch of the capular (capillary) veins, that *there are two motions, one of the chylus upwards, another of the blood downwards;*"

It well became Harvey to criticise Galen on a somewhat supposed similar occasion, viz., for his idea of blood, and vapour or spirit, being contained in the same vessel: he seems of the metaphysical school of the Hudibrastic philosophers, that

> "One way they free will disavow,
> Another, nothing else allow."

"But, continues Harvey, is not this done by a main providence of nature? For if the raw chylus should be mixed with the concocted blood in equal proportions, no concoction, transmutation, or sanguification should from thence arise," &c. "So in the meseraic veins, being dissected, there is found a chylus, not the chylus and blood apart, but mixed, and the same

both in colour and consistence to the sense, as appears in the rest of the veins; in which notwithstanding, because there is something of the chylus unconcocted, although insensible, nature hath placed the liver; in the meanders or crooks of which, it is delayed, and receives a fuller transmutation, lest, *coming too soon raw to the heart*, it should overwhelm the beginning of life."

From this, it would appear, that Harvey was unacquainted with the mode of entry of the chyle to the blood; and that he was even an adherent to the Galenical doctrines of hepatic sanguification; although we shall shortly show him to be, as indeed has already been done, altogether opposed to it; so that, which he really accredited must, in a great measure, be left to conjecture.

That Harvey has justly appreciated the immense importance of the blood, is no where more conspicuous than in this chapter, when adverting to observations to be given, and inquiries to be made "concerning the forming of births," and the reason of one part being the cause of another;

"And many things likewise concerning the heart, as why (Aristotle, lib. 3. de part. Animal.) it was made the first consistent, and seems to have in it, life, motion and sense, before any thing of the rest of the body be perfected: and likewise of the blood, why *before all things*, and how it has in it the beginning of life, and of the creature; why it requires to *be moved* and driven up and down; and then for what cause the heart seems to have been made."

In this quotation, we must perceive, *that if the blood is created* before all things, and moves, even before vascularity is regularly completed, by which its circulation could be accomplished; this motion, at that early period of fœtal existence can be no more than a mere flux and reflux, and consequently resembling the tides of the Euripus. Imperfect as this is, it must still be adequate, by some means, inappreciable to us, to promote the growth of the different parts, and thus advance them to their higher destiny, with a full perfection of organic developement; and in all this, it would be difficult to establish, that either porosities or anastomoses bore a part. Not far from the above quotation, we find Harvey thus condensing all his expectations as to the influence of this, his assumed discovery of the circulation,

"in all parts of physick, physiological, pathological, semeiotick, therapeutick, when I do consider with myself *how many questions* may be determined, this truth and light being given; *how many doubts* may be solved, *how many obscure* things made clear, I find a most large field, where I might run out so far, and enlarge myself so much, that it would not only swell into a great volume, which is not my intention, but even my life time would be too short to make an end of it."

If we may judge of what we have thus lost, by what appears in his pages, perhaps many may think, that as regret is unavailing, so it may be also unnecessary: yet it would have been gratifying to peruse a complete treatise of Harvey on these "parts of physic" respectively, even if the loss may be no ways overwhelming. And let every medical man, here, answer truly, on his professional integrity, what physiological, pathological, semeiotic or therapeutic *truth*, has Harvey elicited by his grand discovery, so far as *he* has entered on any of these topics? How many doubts has *he* actually solved? How many obscure things, made clear? And let him also accurately determine, which are the "new and unheard of things" that he has really presented to our view. Having done this, then perhaps he may be esteemed a fair and impartial judge of Harvey's real claim to the discovery of the circulation, either partially or completely; and how far he is entitled to that full blaze of glory, which for more than two centuries has enveloped him; whilst scarcely the slightest ray has been reflected from it to any other individual! If a fair and a judicious examination of the works of Galen has divested him of a supremacy in every branch of medical science, which he held during nearly, if not more, than ten centuries; surely there can be no impropriety in scrutinizing as severely, but truly, the claims of Harvey; and in divesting him of that portion of his honours, to which he may be found unentitled; and which a partial examination of his claims could alone have given him. His anatomy, physiology, and pathology, so far as we can judge by his writings, are imperfect, and unproved in many particulars; and, as before observed, all alike, tend to prove that a knowledge of the mere route of circulation, if fully shown to belong to him alone, was of comparatively little advantage in his hands. The correctness of these remarks, are

however, to be estimated by others. I proceed to consider the 17th or last chapter of his treatise.

In this, he attempts to show, that the "*motion and circulation of the blood is confirmed by those things which appear in the heart, and from those things that appear in anatomical dissection.*"

Contradiction and absurdity are apparent in many of the statements of this great man. Thus, he says,

"I do not find the heart in all creatures to be a distinct and separate part; for some, as you would say plant-animals (zoophyta sive plant-animalia) have no heart; colder creatures of a softer make, and of a kind of similary constitution; such as are palmer worms and (*lumbricorum*, translated *snails*, by Wood) earth-worms, and very many things which *are ingendered of putrefaction* (quæ ex putredine oriuntur!) and keep not a species, have no heart, as *needing no impulsor to drive* the nutriment into the extremities:"

Is this so? Have the animals mentioned no heart? And do they require no circulation? for if *they* have none, it might be legitimately regarded as of minor importance in the higher orders; and its boasted discovery might sink into insignificance. Are many things (plurima) ingendered of putrefaction? But to proceed from bad to worse!

"For they have a body (connatum et unum, absque membris, indistinctum habent) connate and of one piece, and indistinct without members; so that by the contraction and returning of their whole body, they take in, expel, move and remove the nourishment, being called plant-animals; such as are oysters, mussles, sponges, and all sorts of zoophyts, *have no heart;* for instead thereof they use their whole body, and this whole creature, is as a heart."

Surely Harvey depended on others on some occasions, when his own eyes might have better served him. Is it possible that comparative anatomy was at so low an ebb in his time, that he knew not that the oyster has a heart, and that a very conspicuous one, which was so ably demonstrated and delineated by Willis, a few years subsequently? Are the animals he mentions indistinct, and without members? and is it by mere "contraction and returning (relaxatione) of their whole body," that they take in and remove their nourishment? or do they, from want of a heart, use their whole body as such? It might be well, for Harvey's honour, to explain, if possible, these and

other difficulties in his writings; which indeed ought never to have appeared, and never would have appeared, had he comprehended that discovery, the merit of which he exclusively claims! Wherein, in the above extracts, are the evidences of his learning anatomy from dissections and from the fabric of nature? as he assures his fellow-members of the college, in his dedicatory epistle! where the evidence of a profound physiologist? In maintaining the doctrine of putrefactive generation, (spontaneous we presume,) who will now uphold him? I do not at this moment recollect, whether this doctrine found an advocate in Galen; but rather believe not; because, whenever Harvey could, he seems to have had no scruple in going directly in opposition to him; of which a few instances are pointed out. And yet, with the conjunction of such error and ignorance, a few well contrived experiments, although not all new, have raised his name to the summit of the Temple of Medicine; whilst numbers, equal, or superior to him, are scarcely known even at its portals! In a verdict of such importance, the professional dignity and its justice seems to have been usurped by a very limited number of individuals, compared to the vast number, who then, and since, have yielded to their clamorous award, with scarcely the slightest knowledge of Harvey's writings, or of those who preceded him. Even now, I ask each reader, in perusing these lines, truly to reply, Have you, during your medical career, ever looked into the treatise that has immortalized his name? If not, your approval of his claim depends on tradition alone, in which your judgment has no part. It is incumbent on all to read him, as well as those who preceded, or were contemporaries, and who have advocated or opposed his claim; before they can deliberately and conscientiously declare themselves to be free from that prejudice, which an early bias had given in his behalf. Then, and then only, do I believe a just verdict may be looked for, in which all might *reasonably* confide: for I cannot think, that the elucidations of Harvey, if now first presented, and accompanied by the *preparatory* steps of so many of his learned predecessors; would produce that enthusiastic deference to his claim, and to which he is only partially entitled.

In this chapter we have further evidence of his having employed the "perspective glass," or microscope; an advantage unknown to his predecessors: and if, by its employment, he supposed he saw the *pores*, of which he has made such use, it is obvious, that it is not sustained either by Lewenhoeck or Ruysch, who certainly were not less expert than himself, and that with instruments of far improved structure. If the discovery of the circulation in Harvey's hands, led to no greater improvement in physiology than his writings imply, we must be constrained to believe, that simply considered, it was of but trifling importance. Will any one, who has perused the writings of each, propose Harvey, in connexion with any branch of medical science, as in any respect equal to, not to say superior, to Galen? we doubt it. And if Galen knew not the correct route of the blood, yet in practice, physiology, or pathology, has seldom or ever been surpassed; have we not a right to conclude, in opposition to all that is so commonly affirmed, that a mere knowledge of the route of the circulation has added but little to medical perfection?

In p. 94 he says, that

"In those *that have no blood* and are colder, as in snails, shell-fish, crusted-shrimps, and the like (sed in exanguibus et frigidioribus quibusdam, ut cochleis, conchis, squillis crustatis, et similibus omnibus inest pulsans particula, quasi vesicula quædam vel auricula sine corde; &c. Glasg. Ed. 1751. p. 147.—Sed in exsanguibus et frigidioribus quibusdam, ut cochleis, conchis, squillis, crustatis, his omnibus inest pulsans particula, quasi, etc. Lond. Ed. 4to. 1766, p. 77.) there is a little part which beats, (like a little bladder, or an ear,) without a heart, making its contraction and pulse seldomer," &c. (☞ Note above, the difference of the two Latin editions.)

Here we see that several animals are called exsanguineous, which undoubtedly have blood, although not red; but he has not thus limited his expressions. And soon after, speaking of other "creatures who have blood, ('ut ranis, testudinibus, serpentibus, hirundinibus,') as frogs, *snails*, serpents, swallows," we find the translation infinitely removed from the original, in which the snail is not mentioned! Stating here, however, that "in creatures which are a little bigger, and hotter, as having blood in them, there is an impulsion of the nutriment required, and such a one

perchance as is endowed with more force; therefore in fishes, serpents, *snakes*, *snails* (not in the original!), frogs, and others of the like nature, there is both one ear, and one ventricle of the heart allotted, whence rises that most true axiom of Aristotle (de part. Animal. 3.) that no creature having blood, does want a heart, by the impulsion of which it is made stronger, and more robust; and the nutriment is not only stirred up and down by the ear, but likewise is thrust out further, and more swiftly." What was his absolute idea of the value and nature of the circulation, beyond the proof of its actual existence, may admit of doubt, from the above office given to the ear of the heart, as well as from other passages; and of which the following is evidence, from the same page, viz.;

"Moreover, *because* that more perfect creatures need more perfect aliment, and a more abundant native heat, that the nutriment of them may be concocted, and acquire a further perfection, *it was fit* that these creatures should have lungs, and another ventricle, which should drive *the nutriment* through them!"

If ridicule were always the test of truth, here is ample room for its proof; but it is too serious, when we regard the subject, as one by which an idol in medicine has been elevated and worshipped for more than two hundred years. Had Harvey written in the English language at first, by which his countrymen might have been led to read him generally, and not merely receive all their knowledge of him by tradition, since few will look into the original Latin; I feel persuaded, that such homage never would have been paid to him: it is not very different from that which the Grand Lama receives from his devoted followers; and, probably will be hereafter acknowledged to be nearly as unfounded as the other. In ascribing, as he immediately does, to the right side of the heart, a greater magnitude, p. 96; he gives as a reason, that it "administers not only matter to the left, but gives nourishment likewise to the lungs." This doctrine, which he attributes to Aristotle, is again repeated at p. 107, that "the vena arteriosa hath such a wide orifice, because it carries a *great deal more blood than is necessary for nourishing the lungs.*" Now would it not appear from this, that he knew nothing of the bronchial artery, and that he really had not the remotest idea of

the value and importance, nay the absolute necessity of the pulmonary passage, beyond that of its being the direct route from the one to the other ventricle; a fact already proved to have been as well known to many of his predecessors!

It has been customary, I believe, to ascribe to Harvey the axiom, that the blood is the "primum vivens, et ultimum moriens." I have already demonstrated, I think, that if the remark is well founded, it is due to Aristotle, even by Harvey's own confession. At p. 100, of this chapter it would seem, however, that he rather attributes this extreme property of life to the *right auricle*, where, speaking of the ears, as being the first movers of the blood, he adds, "especially the right, being *the first thing that lives, and the last that dies.*" So also we find in his treatise, *de Generatione*, (Exerc. 57. p. 244. *ed.* 1662, *Amst.*) when speaking of the ventricle and auricle, as to which is superior, he says, "Quippe has (auriculæ) pulsare primo, et vivere, ultimoque emori, compertum est." Plain as is this statement, in two different treatises, he actually contradicts himself in the 2d exercitation to Riolan, p. 147, or at least seems to be in absolute doubt on the subject altogether.

"*Whether* the blood be *moved* or *driven*, or *move* itself by its own intrinsical nature, we have spoken sufficiently in our book of the motion of the heart and blood."

The particular place is not mentioned; it is enough, however, to demonstrate the uncertainty of his mind, and to render doubtful the previous assertion respecting the auricle. This is more complete, if we take into view what he affirms in his 52d. Exercit. p. 195, (*de Generatione*,) of the innate powers of the blood—

"Ideoque concludimus, sanguinem *per se* vivere et nutriri; nulloque modo ab alia aliqua corporis parte, vel priore, vel præstantiore dependere."

How is it possible to draw permanent conclusions respecting the writings and meaning of an author, who has so many strings to his bow, that if one is cracked, another immediately presents itself, of an opposite character? I could enlarge quotations on this head; but probably the reader may deem the above fully sufficient.

I would like to know his meaning in this chapter, when he

says, "the left ventricle possesses *the middle* of the heart." There is unquestionably some error that calls for redress, although the Latin text expresses the same thing. A few sentences after, the word *hirudo* or leech, in both Latin editions, is by Dr. Wood translated *swallow;* an additional example of the necessity of continual reference to the original! Is it conformable to the observations of anatomists, what he affirms at p. 101, that he had found the right auricle in some men so strong, as to appear equal in strength to the ventricles in other men? I ask for information, and not with the idea of denying the fact: yet some of the assertions he proceeds to state, appear to me to require revision and confirmation: so likewise do some of the explanations he affords of different parts, as of the arteries, veins, &c. Thus he tells us, p. 104, that as

"Nature, which is perfect, makes nothing in vain, and is sufficient in all things, the nearer the arteries are to the heart, the more they differ from the veins in their constitution, and are more robust and full of ligaments, but in the furthest dispersions of them, in the hand, foot, brain, mesenterie, and spermatick vessels, they are so like in their constitution, that earnestly viewing their tunicles, it is a hard business to know one from the other."

How correct all, or at least a part of this may be, I leave to the verdict of the anatomist. It is the more requisite to consider the subject, since he proceeds to remark, that *this* (similitude of arteries and veins), is so, for just cause; and he enters into an elaborate discussion, in which will be found, I apprehend, more sound than substance. Many other parts of this chapter would perhaps admit of doubts being raised respecting them, but being not so immediately connected with my object, I deem it useless to dwell upon them, and shall, therefore, here conclude my remarks. I shall, however, insert a few extracts from Harvey's Treatise on Generation; as these have a connexion with it. They are taken from the 18mo. edition of Amsterdam, printed in 1662. Leaving them for the reflection of the reader, I insert them as I find them in the order of the pages.

I have already adverted to Harvey's belief in spontaneous generation; and we here find him, p. 13, accrediting, bona fide, men, or at least women, having tails in Borneo, as a defence to modesty! He assures us, on the authority of a certain surgeon,

"Vir probus, mihique familiaris, ex India orientali redux, bona fide mihi narravit, in insulæ Borneæ locis à mari remotioribus et montosis, nasci hodie *genus quoddam hominum caudatum*, (uti olim alibi accidisse, apud Pausaniam legimus) è quibus ægre captam virginem (sunt enim sylvicolæ) ipse vidit, *cum cauda carnosa, crassa, spithamæ longitudine intra clunes reflexa, quæ anum et pudenda operiebat.*"

And why so? because the tail, in animals, is "tale pudicitiæ tutamen," &c., that, "usque adeo velari ea loca natura voluit."! Lord Monboddo was ridiculed for some similar notion, I believe; but here we have it under the authority of Harvey! So much for credulity! He affords us, however, no reason for the excess of modesty in the Bornese females, which should require this appendage; whilst the women of every other part of the world, are entirely devoid of it!

In the 51st Exercitation, p. 190, "de particula genetali prima," we find him affirming that it is the blood "qui primus in generatione conspicitur," not in the egg only, but in the conceptions of animals. He believes it even prior to its receptacle; and that it is the "particulam corporis principalem"—as the heart "Cor esse ipsius organum, circumlationis ejus destinatum, quippe functio cordis, est sanguinis propulsatio."

At p. 192, he maintains the life of the blood, from Levit. xvii. 11 and 14.

"Vita igitur in sanguine consistit, quippe in ipso vita atque anima primum elucet, ultimoque deficit. Crebra enim vivorum dissectione expertus sum, moriente jam animali, nec amplius spirante, cor tamen aliquandiu pulsare, vitamque in se retinere. Quiescente autem corde, *motum* vidias in auriculis superstitem, ac postremo in auricula dextra; *ibique tandem cessante omni pulsatione, in ipso sanguine undulationem quandam*, et obscuram trepidationem, sive palpitationem reperias."

From whence, together with some other points, he concludes, p. 193:

"Quibus clarè constat, sanguinem esse partem genitalem, fontem vitæ,

primum vivens et ultimo moriens, sedemque animæ primariam; in quo [tanquam in fonte] calor primo, et præcipue abundat, vigetque; et à quo reliquæ omnes totius corporis partes calore influente foventur, et vitam obtinent. Quippe calor sanguinem comitatus, totum corpus irrigat, fovet, et conservat: quemadmodum jampridem, libello de motu sanguinis, demonstravimus."

Much more is superadded to prove the priority and superiority of the blood;

"Ut anima primo et principaliter in ipso residens, illius gratia, *tota in toto, et tota in qualibet parte inesse*, merito censeatur."

Denying that Aristotle and all physicians are correct, in regarding the liver or the heart to be the "autor et opifex sanguinis," he affirms that the blood itself, is "potius autorem cordis et hepatis;" and that the heart was solely constructed "ut perpetua pulsatione, (venarum arteriarumque ministerio) sanguinem hunc accipiat, eundemque quoquoversum per totum corpus propellat," p. 195. This chapter conclusively proves the very high and deserved estimation in which Harvey held the blood; nor is the next (Exercit. 52.) less tenacious in its claims in its behalf. Referring to, and quoting Aristotle, [de hist. Anim. lib. 3. ch. 19.] in words that seem clearly to express a perception of a circulation, and its necessity, without, however, entering into any abstract consideration of its particular route; we must be blinded by prejudice, I think, if we cannot be persuaded to grant to Aristotle a tithe of that honour, that has been so lavishly bestowed on Harvey!

"Priusquam corporis quippiam visu discernitur, sanguis jam genitus et auctus est, palpitatque (ut Aristoteles ait) intra venas, pulsuque simul quoquoversum movetur; solusque omnium humorum sparsus per totum corpus animalium est. Et semper quamdiu vita servatur, sanguis unus animatur et fervet."

This, and more I might quote, is strong language, yet probably in a great degree correct, and I may as well state here, an extract of a similar character (from Exercit. 55. p. 228.) showing moreover, that, however exalted his opinion of the heart and blood, it was scarcely superior to that of Aristotle.

"Cor itaque (sive, nostro arbitratu, sanguis) est prima animæ sedes, fons vitæ et focus perennis, calor genitalis, ipsumque adeo calidum innatum; primum partium suarum omnium instrumentalium efficiens, animamque pro fine sortitum, quæ illis omnibus, ceu instrumentis, utatur. Cor, inquam, est, (ex sententia Aristotelis,) cujus caussa partes cunctæ in animalibus fabricantur; idemque earum omnium principium simul, et opifex existit."

It would be difficult to say which of these great men estimated, most correctly and highly, the heart and blood; but I have no doubt, that neither of them could think as they did, respecting them; without a perfect conviction, though only a partial comprehension of a circulation; and if Harvey has really more truly traced its route, that he may thank the abundant facts that had accumulated from the time of his wonderful predecessor. But what thanks have these elicited? scarcely are they noticed, whilst arrogating to himself the full discovery at p. 196,

"Circuitum sanguinis admirabilem, *à me jampridem inventum*, video propemodum omnibus placuisse: nec ab aliquo quippiam hactenus objectum esse, quod responsum magnopere mereatur."

The whole of the 51st, 52d, and 53d Exercitations, are so much and closely interwoven with the doctrines of the circulation, that I think they ought to find a place in Harvey's exposition. They are, moreover, amongst the most interesting parts of his treatise; and I could find much to consider in them, in the investigation I have been pursuing. He speaks of the want of feeling or sensation in the blood; a fact announced by Aristotle, but *not referred to him*, by Harvey; nor another, of an analogous character, the want of sensibility in the brain and spinal marrow, which is also mentioned by him; (see hist. Animal. lib. 3. c. 19.) wherein, after noticing the first above mentioned fact, he says, "Quinetiam cerebrum, et medulla tactum non sentit." Harvey's words are, p. 198, "Neque enim cerebrum, medulla spinalis, aut crystallinus, vitreusque oculi humor quicquam sentiunt," &c. Here, moreover, in proof of the great insensibility of the heart itself, he mentions the case to which I have in p. 103, 4, referred; but in which he was forestalled by one somewhat like it, yet of infinitely greater interest in the hands of Galen! I need not repeat them here; nor refer to them, further than to renew my astonishment,

that a case so similar to his own, and so particularly detailed, was not even hinted at, as to be found in Galen!

I have noticed in a preceding page, that some writers, as Diemerbroeck, had doubts whether the blood ought to be considered as a part of the body. Of its being so, Harvey is perfectly satisfied; and he follows out, pretty closely and extensively some of Aristotle's views; which I notice, only to state his accordance with Aristotle, (p. 205,) that the blood is to be regarded, "non ut simpliciter intelligitur, et cruor dicitur; sed, ut corporis animalis pars vivens est." And he then proceeds, in true scholastic style, to consider it *materialiter*, et *formaliter*, no doubt, to the perfect satisfaction of his advocates and adherents. At p. 243, Exercit. 57. we again find him dwelling on his favourite doctrines of the prior formation and motion of the blood, and of its being imbued with a vital spirit, before any sanguifying or motive organ existed.

His ideas relative to the nourishment of the fœtus, are to be found at p. 252. As might be expected, denying, as he does, anastomoses; he considers the dictum of Hippocrates as superior to that of Fabricius and other anatomists, who regarded the amniotic liquor as sweat, and as injurious to the fœtus; whilst with Hippocrates, Harvey esteems it nutritive,

"Partemque ejus tenuiorem et sinceriorem, *intra venas umbilicales haustam*, primogenitas fœtus partes constituere, et augere; ex reliquo autem, ceu lacte, *per suctionem* in ventriculum deglutito, ibidemque cocto, sive chylificato," &c.

All which curious assertions he very learnedly attempts to prove, by affirming, that should the fœtus, swimming in that fluid, open its mouth, the fluid would enter into it, and should other muscles move, the liquor would be swallowed! Prodigious deductions! And he yet goes still further in favour of Hippocrates, by asking why we should hesitate to affirm that the "fœtum in utero sugere," and he proves it to be so, as he supposes, by many facts and affirmations!

His 60th Exercitation is chiefly on the subject of the utility of the yolk and the white of the egg. In this, he reviews the opinions of his master Fabricius. At p. 265, adverting to the

blood, which appears during the progress of the incubation of the chick, and how it is formed,

"Sive, à quo opifice uterque liquor in sanguinem mutetur, nondum existente jecore? Non potuit enim dicere illum in ovo à materno sanguine profluxisse," &c.

Proceeding in this manner, referring to Fabricius, he says,

"Silentio autem præteriit difficultatem maximam, et medicorum animos non leviter torquentem; nimirum, quomodo jecur sit origo et opifex sanguinis: cum hic non solum in ovo reperiatur, ante natum aliquod viscus; sed et ipsi medici doceant, viscerum omnium parenchymata, esse sanguinis duntaxat affusiones? Estne opus, autor sui opificis? Si hepatis parenchyma fit ex sanguine, quomodo illud hujus caussa fuerit?"

In Exercit. 71, p. 314, de calido innato, he affords further proof of his high estimate of the blood.

"Solus nempe sanguis est calidum innatum." "Nihil sane in corpore animalium, sanguine prius aut præstantius reperitur; *neque spiritus*, quos à sanguine distinguunt, *uspiam ab illo separati* inveniuntur." Compare this with Galen's ideas of spirits in the blood, if he really entertained them. "Est igitur sanguis sufficiens et idoneus, qui sit immediatum animæ instrumentum; quoniam et ubique præsens est, et huc illuc ocyssime permeat." p. 317. Et multa alia similia.

At p. 322, after a long consideration of the blood, through several pages, Harvey notices thus its existence, either *in* or *out* of the veins.

"Sanguis nempe *extra venas absolutè*, et per se consideratus quatenus est elementaris, atque ex diversis partibus (tenuibus scil. serosis, crassis, et concretis) componitur, *cruor* dicitur, paucasque admodum et obscuras virtutes possidet."

How, with the above impression, he could so tenaciously maintain the intermedium of arterial and venous communication to be that of the porosities of the parts, I cannot well imagine; nor is that difficulty diminished, by the succeeding sentence.

"In venis autem existens, *quatenus est pars corporis*, (this is the source, probably, of the opinion that the blood was not a part of the body, as I have stated from Diemerbroeck) eademque animata et genitalis, atque immediatum animæ instrumentum, sedesque ejus primaria," &c.

Proceeding to the pinnacle of hyperbole, he calls it the Sol microcosmi, et ignis Platonis, and declares it deserving of the name of spirit (spiritus etiam nomen meretur,) and awards it a superiority, which indeed it well deserves, over every other part of the body, and terminating his eulogy, at p. 323, with the following emphatic terms.

"Eodem ergo res redit, si quis dicat, animam et sanguinem, aut sanguinem cum anima, vel animam cum sanguine, omnia in animali perficere."

I cannot omit here to state, in proof of every superiority that can be attributed to the blood, (by its warmest partisan, and no one is probably more lavish in its behalf, than Harvey), that whereas *every other part* of the body, has been noticed as deficient, in different monstrosities recorded; *this alone*, this fons et origo of the miscrocosm of nature, has *never* been found wanting. Even the stomach, the great centre of sympathy, has been wanting, not merely till the period of birth, but to that of forty years; whatever may be said to the contrary, by those who boast of having searched deeply and extensively into the subject.

An excellent remark of Harvey follows the last quotation; which, although principally referring to the subject of the blood, will be admitted as very applicable to many other cases—

"Solemus, rerum negligentes *speciosa nomina* venerari. Sanguis, qui nobis præ manibus atque oculis, nil grande sonat: ad spiritum vero, et calidi innati, *magna nomina* obstupescimus."

Here he gives us a curious story of a certain stone from the East Indies, commemorated by Mizaldus and others for its wonderful corruscations; which, whether he accredited or not, as in the case of the caudated females of Borneo, he does not say; but he terminates the story and the chapter in these words, assimilating as it were, the wonders of the blood, with those of the extraordinary stone in question.

"Tam stupendum lapidem quis non admiretur, credatque eundem supra vires elementorum agere, et corpus aliud participare, spiritumque æthereum possidere? præsertim, cum eundem elemento solis proportione respondentem videat. At vero Fernelio Œdipo, parva flammula totum ænigma solvit. Ad eundem pariter modum, si sub fabulæ involucro sanguinem alicui depingerem, lapidisque philosophici titulo insignirem, atque omnes ejus

singulares dotes, operationes, ac facultates ænigmaticè proponerem, illum procul dubio pluris æstimaret; supra vires elementorum agere facile crederet, corpusque illi aliud ac divinius non illibenter attribueret."

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It is however high time to arrest my extracts; which I could willingly and readily multiply, in order to point out the exalted character that Harvey entertained of this most wonderful of the wonders of creation; and in which, perhaps, no one excels him, either of ancient or modern times. His estimation of this fluid, indeed, redeems him greatly in my mind, for the neglect evinced throughout his writings, of the debt that he owed to his predecessors, by his exclusive claim to the discovery of the circulation. In his high estimate of the blood, Harvey is really at home; he omits nothing that can give it the highest claim to our notice and regard; although much of it is due to those who preceded him: and I have only to regret, that by grasping at too much, like the dog in the fable, he deserves to be shorn of much of those honours that have so long been heaped upon him.

And now, in bringing these remarks to a conclusion, I deem it proper to renew my inquiry, what is it that we absolutely owe to Harvey? I mean as it respects his asserted claim to the full discovery of the circulation of the blood. A circulation of that fluid, abstractedly considered, I think, has been substantially evinced, to have been held by Aristotle and by others, even down to his days. However incorrectly they understood the mere route of circulation through the vessels; they appear to have comprehended the utility and the necessity of it to nutrition, to animal heat, and secretion, equally as well as we now do: whilst, in the discharge of blood by venæsection, arteriotomy, cups, scarification, and even leeches, they were not less bold, nor less successful than ourselves. Experience taught them all that it now teaches us, in the various modes of its evacuation; and the danger and benefits of its employment, were just as well comprehended by the mind of Hippocrates, two thousand years ago, as by Harvey, the affirmed discoverer of its route. But, what is it that he actually did discover? Was it the lesser or pulmonary circuit? We answer, No! for independently of Cæsalpinus and others, the near contemporaries of Harvey, we find, that rather than ascribe it to them, by whom it is so well

described, he admits it as nearly known to the illustrious Galen; and even affirms this fully, in words not to be overturned by sophistry. "It does therefore clearly appear from the words and places of Galen, a divine man, father of physicians, both that the blood doth pass from the vena arteriosa into the little branches of the arteria venosa, both by reason of the pulse of the heart, and also because of the motion of the lungs and thorax," &c. Admitting then, that Harvey actually was unacquainted with the prior right of Servetus and others, to the discovery of the pulmonary circulation, we here perceive a full admission of right in Galen; and therefore this cannot be the part he claims. What is it? we repeat. Is it the knowledge of the valves of the heart, and of their uses, as leading to a knowledge of the route? By no means; for here again he affirms that "Galen explains the use and necessity of those shuts," de Usu part. 6. ch. 10. All claim on behalf of Harvey is, by his own statement, for ever cut off, so far as respects the pulmonary circle. What then does his claim consist in, that is peculiarly his own, and in no wise derivative from others? Is it the discovery of the venous valves? Assuredly not; for he ascribes this to his master Aquapendente, or to Sylvius, although he says they knew not their use. This then forms no part of his exclusive claim: but, if Galen could, from their form and location, so well appreciate the use of the valves of the heart; is it not reasonable, that a man like his great master Aquapendente, one of the most enlightened anatomists of his age; should, on discovering the venous valves, reason on the subject of their use in the animal economy, and be led to the same conclusions which Harvey adopted, especially as he thinks it was so easy to conceive of them. But putting him aside, how can he overpower the claim of Piccolhomini, who must be regarded, by every candid and generous mind, as having clearly led the way in this particular? for we cannot, for one instant, doubt that his writings were fully known to Harvey. What then is left for him to claim? Is it simply the passage of the blood from the heart by the aorta, and its return by the vena cava? We may admit it as possible—nay, even as probable—but is it undeniably the case? When we perceive the mode of explanation given by him, of its passage from the arteries into

the veins; and find him absolutely unsettled in the attempt; ("*either* mediately by an anastomosis, *or* immediately through the porosities of the flesh, *or* both ways,") surely we may judge that he knew nothing about it; and that his conclusions, if even just, were, at best, conjectural, and not proved. What then remains for him? The whole tenor of these remarks, and the numerous extracts given, either from Harvey himself, his adherents, or opponents, appear to me to prove the injustice of ascribing *to any one* individual, the extraordinary honour of singly discovering the circulation of the blood! What exact proportion belongs to each individual, will be differently estimated, as peculiar circumstances may modify our impressions; but, that a small proportion only will be found to belong to Harvey, I doubt not, will hereafter be conceded, if the present generation cannot surmount long established opinions: and in the interim, it will be well to establish his undoubted rights, by authority admitting of no appeal: and in so doing, let not those of Galen be overlooked and unheeded. In admitting Galen's knowledge of the pulmonary circulation, the steps to the general circulation may be said to be comparatively easy; perceiving the obstruction from the valves of the heart, to the retrogression of the blood, after passing those portals, he must have observed, that still no accumulation ensued, and consequently a free passage was *somehow* accomplished; he saw and acknowledged its discharge from the left ventricle, through the aorta to all parts of the body; and was equally aware of its constant flow into the right side of the heart, through the vena cava! What then was wanting to his understanding of the general circulation? He every where announces the anastomoses of the vessels; and is as positive in this respect, as Harvey was with regard to the passage by the pores of the flesh,* a doctrine he appears to have known, but by no means to have adopted: nor even now, is the dispute termi-

* When I say Harvey was positive as to the porosities adverted to, I am, perhaps, going too far; the whole tenor of his writings, proves his utter want of comprehension on this point; his vacillation and uncertainty as to the subject of vascular communication, the only point in fact remaining in his time unsettled, and not now a jot better comprehended than in the time of Galen. If my readers are not as yet satisfied on these particulars, I must request them to accompany me

nated, as to which is correct in this particular. Under every view of the subject, it seems to me, that the utmost Harvey effected, was by means of some well-devised experiments, and by an accumulation of facts from preceding writers, to have smoothed down some difficulties which still existed. That he, and he alone, discovered the circulation, would not now be conceded; supposing he could, in the present day, advance his ideas in precisely the same manner he did, two centuries ago. His demands have been agreed to, without due examination into their merits; and have been transmitted as an heir-loom to the profession, who have taken them altogether on trust, and as any other long continued tradition may be presumed to impress itself upon us. Could many of those to whom I have referred, arise

once more, to the "Life of Harvey," as given in the College edition of his writings, of 1766, p. xiii. Here we read as follows:

"*Duo sunt* quidem, ut nequid dissimulemus, quibus in ratione sanguinis circumferendi explicanda Harveium *defecisse dolemus*. Vim enim arteriarum in humoribus propellendis minime sensisse videtur. *Arteriarum etiam minutarum cum venis conjunctionem primum pernegavit; eandem postea* invitus agnovisse videtur, *nec tamen rem penitus intellexisse.*"! And yet, forsooth, the award is granted him of having fully completed and perfected the discovery of the circulation! but let us hear a little further on, (p. xxviii.) when noticing his correspondence with Marquatus Slegelius of Hamburgh, respecting some of Riolan's opinions, his biographer thus proceeds. "Præterea, cum in Epistola ad Riolanum prima* *omnino pernegasse videretur* arteriarum fines cum venarum principiis committi, *nec anastomosin ullam mutuam* vasorum horum, quæ sensu percipi posset, exsistere dixisset; sanguinem denique ex arteriarum extremis *vi cordis propulsum in carnium meatus tradi, et ex his a venarum principiis* in cor deducendum excipi affirmasset; paulo uberius sententiam suam de arteriarum conjunctione cum venis in hac Epistola exponit," &c. Yet, with all this in favour of porosities, and in opposition to anastomoses, even more than twenty years after his work, he scruples not to *make use of anastomoses*, by the affirmation of his great admirer and advocate, Dr. De Back, "only as it may further his purpose," which, if it means any thing, convicts Harvey, like the traveller in the fable, of blowing hot and cold with the same breath!

Had I not prepared for the press the Biography of Harvey, herein given, from the British edition, as stated, before I got into my hand the College edition of his Life, I should certainly have given it in its place; and even now, I am much disposed to present it to the reader, at the hazard of some repetitions. I certainly would urge its perusal in connexion with this Inquiry.

* It must be remembered that these letters to Riolan were first printed in 1649, at Rotterdam—that is, twenty-one years after his treatise *de Motu Cordis*, which in his dedication to the College, he had affirmed was "perfect some years ago."!

from their slumbers, and peruse the writings by which his claims are considered to be substantiated; would they not indignantly exclaim, with the Mantuan Bard,

"Hos ego—scripsi, tulit alter honores."

How far I have been enabled to redeem my pledge, I now leave to the candid judgment of the profession; at least, of those members of it, who consider truth as of more importance than a name, and who will seriously devote themselves to an unbiassed investigation of the subject in all its bearings.

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☞ As considerable matter yet remains for notice, I have placed it in form of an Appendix, in the succeeding pages; hoping that it may be found not altogether devoid of interest, and earnestly requesting the reader to believe, that had I not considered it a duty, I would never have entered on a subject so entirely disagreeable, as that of questioning the claims of a man who has so long obtained the suffrages of the medical community.

# APPENDIX.

As I imagine it may be useful for any individuals who may think proper to pursue this inquiry, to have some notice of where they may look for its investigation, I have subjoined in this appendix, the names of several whose writings bear upon the subject, in a greater or less degree. Many of them are taken from the Biography of the Diction. des Sciences Medic. I have chiefly followed the alphabetical order, simply for the sake of reference, instead of attending to the æra of the individual. Few of these works are to be found in America; but will probably reward, in their perusal, some European writer. Under the general head of "Anatomistes," we find in the Biography above adverted to, some short hints, &c. connected with individuals and the circulation, with which I shall begin, merely stating what is to be expected in that particular.

Besides these, Vanderlinden mentions the names of Bravo, Citadinus, Conringius, Regius, Beverovicius, Tozzus, Highmore, Schlegelius, Gassendus, Lowerus, Drake, Spigelius, Deusingius, Leichnerus, Cause, Ulmus, &c.—And

In the 5th vol. of the Dict. des Sciences Medic. p. 228, we have a long and interesting article of above twenty-five pages, on the subject of the circulation, by M. Lerminier, who gives references to various authors.

EXTRACTS FROM THE MED. BIOGRAPHY, OF DICT. DES SCIENCES MEDICALES. ARTICLE "ANATOMISTES."

"John Baptiste Cannani found the valves of the V. azygos." "An anatomical theatre was established at Pisa in 1552; and in that year, M. Servetus, who was subsequently burned by Protestant fanatics, after having escaped the flames of the Inquisition, discovered the pulmonary circulation. In 1556, a theatre was opened at Montpellier. Andrew Cæsalpinus had a glimpse of the larger circulation in 1571."—"Jerome Fabricius, (this was Harvey's master,) *confirmed the existence* of the venous valves, to the discovery of which he erroneously laid claim."

"Perhaps it was wrong to neglect (in anatomy) the plan employed by Riolan, viz. that of inflating, to enable him to demonstrate the connection of vessels."

"In 1662 Caspar Aselli perceived the chyliferous vessels. This discovery is perhaps more important than that of the circulation of the blood,

and yet Aselli is scarcely known amongst us, whilst William Harvey, an Englishman, has made his name resound throughout Europe."*

"William Harvey—The pupil of Fabricius, who made known to him the valves of the veins; Harvey applied himself to discover their use, and was thus led to a knowledge of the circulation, foreseen, as we have said, by Servetus and Cæsalpinus. He demonstrated this great discovery in 1619,† and after many researches in its confirmation, he made it the subject of an immortal work, in which facts and reasoning mutually support each other. He triumphed over all his antagonists, had the happiness, refused to so many philosophers, of seeing his opinions generally adopted in his life-time, and furnished a remarkable example of the mode that should be adopted in the demonstration of an important discovery. Descartes, in spite of his taste for hypothesis, embraced his defence."—"About the period of Harvey thus immortalizing himself, M. A. Severinus made some interesting remarks on anatomy; James Primrose, Caspar Hofmann, and Em. Parisanus, attacked violently the author of the discovery of the circulation." "Paul Marcard, Slegel, and Henry Leroy, most zealously defended the great Harvey." "John Walæus, Roger Drake, George Ent, and Germain Conring, defended and *perfected* the doctrine of the circulation of the blood." "Isbrand Diemerbroeck, showed himself one of the most ardent defenders of the circulation of the blood." "Charles Drelincourt successfully repeated Harvey's experiments on the circulation."

"F. Ruysch discovered the bronchial artery,‡ and the capillary circulation, and demonstrated the true route of the lymph." "Anthony Leuwenhoeck, by means of improved microscopes, clearly saw the circulation of the blood in the smallest vessels, and the direct passage of this fluid from the arteries into the veins." "Stephen Blancard showed, by injection, the direct communication of arteries and veins."

What we have further to say on the subject of Harvey, may as well be connected with the preceding notice of him under the head of Anatomistes.

Harvey, Wm.—Extracts from and remarks on his biography, by A. J. L. Jourdan, from vol. v. Biog. of Dict. des Sci. Med.

Whatever writers may precisely mean, may be accurately known to themselves; but they must be judged by others, from the *commonly received*

* ☞ Under the head of Asellius, p. 387, when adverting to *his* discovery; which, however, the modest Aselli rather ascribed to Hippocrates, Plato, Aristotle, Herophilus, Erasistratus, and Galen; the editor remarks, "that the great Harvey is deserving of censure, for having shown a degree of enmity to Asellius; and for having maintained that these vessels did not convey chyle."

† I doubt if the editor had read his work, as he thus errs in the date by three years! It is a loose mode at all events.

‡ A century after Harvey.

ideas of the words they employ, in connection with the subject treated of. Now when Harvey is called the Discoverer of the Circulation, I ask every candid man, whether, unaccompanied by any restrictive or explanatory clause, the *exclusive* claim has not been considered as his alone; and whether the whole subject is not usually merged altogether in him! In the biography as given by Jourdan, there is however such a limitation; and had it always, either by Harvey himself, or his advocates, been freely admitted, I never should have objected to his participating in the honour, so far as he is justly entitled thereto: even to the admission of its full correctness, which, in various parts, his warmest friends cannot maintain to be the case.

"No one, says Jourdan, is ignorant that Harvey *discovered* the circulation of the blood." Now follows that judicious restriction, which most certainly neither enters into any part of Harvey's writings, nor scarcely in any of those of his friends and partisans. "But we should greatly deceive ourselves, if we should here take the word discovery in its rigorous meaning;" (☞ and why not? in fact, the word, both in French and English, and we may add, in Latin, is absolutely *rigorous in its* acceptation;) for—il s'en faut de beaucoup que tous les points de la théorie du mouvement du fluide nourricier fussent également inconnus avant les researches de cet illustre anatomiste.—We have seen, proceeds the writer, that Harvey had Fab. ab Aquapendente for master, at Padua. Now amongst his numerous anatomical pursuits, Fabricius *was much occupied* with the fœtus and the venous valves. These were likewise the objects of Harvey's especial attention. It is very probable, therefore, that the knowledge he thus acquired at the lectures of Fabricius, gave him the idea of the circulation, without supposing, with Vanderlinden, that the suggestion was given to him by a London apothecary; although, even in such case, we might say with reason, that it was a happy suggestion improved by the aid of genius. But, since the valves are directed *towards the heart, it was impossible not to conclude* (☞ and why will not this conclusion equally fit the master as the student!) *from this circumstance*, that they serve to direct the blood towards that organ. This theorem once admitted, the sight alone of the valves of the arteries at their origin from the heart, should cause a similar conclusion, that the blood is carried from the heart into the arteries. This idea of a circulation, *had not entirely escaped* his predecessors, (he here refers to Servetus,) in whose writings we find, at least, the pulmonary circulation described, although obscurely, and without the developements and proofs, which were so *requisite in the age of Bacon*, in which Harvey lived. (Why more then, than at any other period!) Columbus had indicated more clearly the lesser circulation. Cæsalpinus left still less to be desired in this respect, and speaks even in terms quite precise (assez précis) of the return of the blood by the veins; but except the proof derived from the ligature over the vein, (d'une ligature qu'on applique sur la veine) his writings contain no ulterior detail of this important doctrine.

"Thus, continues the writer, the mind (les esprits) was in the tract of the

discovery of the circulation, and some parts of this great organic phenomenon had already been perceived, when Harvey appeared, to vivify, as it were, an idea vaguely floating in the mind, to collect all the facts observed up to his time, to augment the mass, and add thereto all the essential developements, and finally to deduce those general conclusions, that are the natural result."

☞ This is a beautiful picture of Harvey, but which the perusal of his works will, I think, not be found to justify. Even the extreme silence, as from whence most of the facts he adduces were derived;—his tacit appropriation of them to himself; his apparent dread of vindicating his claims, by answering his opponents, and strengthening his views, so as to subdue all opposition; leave some reason to think that he feared the result would be unfriendly to him. It is perhaps not of much importance, but the learned biographer has stated, that "Harvey decided, in 1619, as may be judged from his dedicatory epistle, to *teach publicly* the circulation of the blood, but did not print his book on the subject until 1628." It appears, however, from the Biography given from the English B. Dict. and elsewhere, that he was appointed lecturer of anatomy and surgery in the College of Physicians of London in 1615, "and the year after (1616) read a course of lectures there, in which he opened his discovery relating to the circulation of the blood." It is *this want of accuracy* as to dates, that has given rise to so many disputes as to rights of discovery, &c.; and I think it proper therefore to notice this, even at the risk of being considered too minute.

We are told in continuation, that Harvey begins by combating the errors of the *ancients*, and particularly to prove that the arteries are not intended, as they maintained, to convey an aërial spirit or pneuma (l'esprit aérien ou le pneuma) through the body, but the blood. And if my readers will attend throughout, they will see that on this point, and they will find it proved, from Harvey himself, that the arteries carried *only blood, according to Galen*, (the *only ancient* quoted by him on this head,) whilst *he himself*, if words have meaning, in various places advances this very untenable position. Even the arbitrary evaluation of the *amount* of blood, (the idea of which is, perhaps, exclusively his own,) which passes from the heart at each contraction, is entirely hypothetical, even by his own admission; and, as M. Jourdan has well observed, "he went too far, and fell into a mistake, which has reigned despotically in the schools ever since, since, at present, scarcely can three or four physiologists be found, to oppose it, with this, and some of the deductions drawn from it. In truth," he adds, "Harvey has not explained himself clearly—nor perhaps did he ever think of asking himself, if it was really the same blood that flowed in the arteries and veins." I perfectly agree with Mr. J. in considering H. as having by no means clearly explained himself, either here, or in many other parts of his writings: but whether accidental or intentional, I cannot fully satisfy myself.

I must quote a little further, here, since it will be seen to be connected

with that unknown region of the circulation, that terra incognita, which, like the shores of Greenland, have disappeared from observation. Adverting, then, to the proposition above, of the character of the blood in the vessels, whether it was the same that flowed in both arteries and veins—he proceeds to say, "The *successors* of Harvey admitted this identity, or to speak more accurately, they supposed a direct communication between the arteries and the veins, although such hypothesis rendered inexplicable, on the one part, the phenomena of nutrition, and on the other, the actual difference of arterial and venous blood; and in this respect it may be boldly affirmed that Harvey went too far. His theory of the circulation of the blood has consecrated (consacré) a great error, attested by the word circulation itself; for the blood does not circulate *in the rigorous acceptation* of the term." I advert to this, more particularly to show, that, as Harvey denies this *immediate intercommunication* of veins and arteries, as we have so repeatedly shown; the term circulation is still less correct, than if the doctrine of anastomoses were absolutely true. We perceive, however, the great difficulty in thus adopting that view of the subject, in relation to nutrition, &c. and probably others, besides myself, may come to the conclusion, that it is all a mystery, and likely to remain so, and consequently, that the circulation is not yet discovered! But I cannot avoid another remark, with the most perfect respect to the learned biographer; viz., how quickly he has found it necessary, (only two pages apart,) to divest the *word discovery* of its *rigorous acceptation* in one place; whilst he has as strongly rested on, or enforced that *rigorous acceptation* of the same word, (at least it is implied,) when the occasion required it, to disprove anastomoses. As to the mode in which the function of nutrition is performed, it is wrapt in secrecy from its obvious minuteness of deposition in whatever way it may take place.

Undertaking, as I have, to demonstrate the unwarrantable character of that claim, by which Harvey has been greeted as the great *discoverer* of the circulation; but which Mr. Jourdan declares is not to be taken in its rigorous acceptation; it would be ridiculous in me to appear to suppose, with that gentleman, that *envy alone* operated in the breasts *of all the opponents* of Harvey. Whatever faults were committed, they were not restricted to one side only. Who began the abuse that became so virulent, I know not! If Harvey had been content to *say nothing on the subject of these momes and detractors*, &c.—if he had afforded no hostile attitude in his treatise, perhaps the plea might pass; but as it is, I cannot feel it correct, to advocate the moderation, or animadvert on the abuses of either. It is taking, I think, too much upon himself, when Mr. Jourdan thus assumes the absolute right of *discovering in its rigorous acceptation*, that, "Envy raised itself on every side against him: but he replied *only by contempt* to these theoricians, to those austere (farouches, qu?) admirers of antiquity, who are always ready to combat facts by reasoning, or to elevate the ancients, in the sole intention (dans la seule vue) of undervaluing the moderns." May we not affirm, perhaps with more truth, that the little

intercourse now maintained with the ancients, has really led us to undervalue them, from the ignorance of what they have done to science generally, or to medicine in particular?

If Harvey, as Mr. J. says, amidst these attacks upon his self-love had had the wisdom to keep quiet, and wait for time to do him justice, we might have sympathized with him, on the libels with which he was assailed, although I have not been able to find out wherein they exactly consisted, or how Harvey supposed himself entitled to an exemption from that criticism, which his relation, Gideon Harvey, so unsparingly bestowed, at a later period, on the whole college and profession of physic. But since, if not the first aggressor, he did take up the cudgel, and call names; it is perhaps best, that now, at the distance of two hundred years, we should calmly survey the scene; and at least, not blame severely, without reading accurately, all that has been bandied backwards and forwards. If Harvey has not done justice to his own master Aquapendente, and many others, his predecessors and contemporaries; by what sophistry shall we be led to credit, that the same treatment towards himself was unjust and improper?

But I must forbear; having yet to notice what is said by Jenty as to Harvey; in order more completely to estimate him: especially as it embraces much interest in its connexion with his writings.

"Wm. H.," says he, p. cxxv. Compend. Vol. I., "a celebrated physician, was born at Folkstone in Kent, in the year 1577. He studied five years at Padua, where he took a doctor's degree; afterwards took the same degree at Cambridge; and having been physician to King James I. and Charles I. and President of the College of Physicians; he died in 1657, in the 80th year of his age. His *discovery* of the circulation of the blood, was of the utmost importance in physic, of any that was ever made, and immortalized his name: but as it has been frivolously disputed whether the honour of it belongs to him, I shall transcribe a passage from Wotton's Reflections on Ancient and Modern Learning, which sets this affair in a true light: 'This discovery, *first made perfectly* intelligible (☞ Is it so, even at this day!) by Dr. Harvey, is of so very great importance to show the communication of all the humours of the body with each other, that, as soon as men were perfectly satisfied that it was not to be contested, which they were in a few years; a great many put in for the prize, unwilling that Harvey should go away with all the glory.'" This is scarcely its *true light*, as, perhaps, most persons will admit, at the present day, when the subject may be taken up, without those personal feelings that then existed! "At last," continues Jenty, "Harvey printed a discourse, on purpose, upon this subject, at Frankfort, in 1628. This gave him a just title to the honour of so noble a discovery; since what his predecessors have said before him was not enough understood, to form just notions from their words. One may also observe, *how gradually this discovery*, as well as all abstruse truths of human disquisition, was explained to the world." The proposition of this last sentence every one must acquiesce in: but in doing so, how can that be

called a *discovery*, which was but the merely perfecting of what his predecessors knew! But let us hear further, what Jenty urges, in order to prove Harvey's *full claim* to the honour that has so long been awarded to him. It may be worth while, too, to recollect that of *all those* who have made the full award, from the time of Harvey to the present day, not one has been himself a party to a *single link* in this extensive claim; their right to make such award, by which, all but Harvey alone, are cut off from any participation of the honour, may well be called in question! Would they have so freely done so, had *they* given birth to the suggestions and explanations of Servetus and others? We may undertake, I think, to answer for them, No.

"Hippocrates first talked of the usual motion of the blood; Plato said, that the heart was the origin of the veins and blood that was carried about every member of the body; Aristotle, also, somewhere speaks of a recurrent motion of the blood: still all this was only opinion and belief. It was rational, and became men of their genius; but not having, as yet, been *made evident* by experiments, it might as easily be denied as affirmed."

This reasoning will apply to Harvey equally, so far as the mode of union between arteries and veins is explained by him!

"Servetus first discovered, that the blood passes through the lungs; (☞ Harvey seems to admit it as known to Galen!) Columbus went farther, and showed the uses of the valves of the heart, (☞so did Galen!) which let the blood in and out of their respective vessels, but not in the self-same road. Thus the *way was just open* when Harvey came, who built upon the first foundations." (☞But gave no credit to the previous architects; unlike the building of St. Peter's in the Eternal City, where the first architect received his full proportion of honour, with those who followed, and "built upon the first foundations.")

"To make his work still the easier, the valves of the veins, which were discovered by Father Paul, the Venetian, (even the discoverer of the valves is not fully acquiesced in!) had been not long before *explained* by F. ab Aquapendente, when the circulation was yet more clearly demonstrated."

"There was one thing still wanting, to *complete* this theory, and that was, *the knowledge how the veins received* that blood which the arteries discharged. (☞Aye, *every thing but this*, was adequately known before Harvey; and it is remarkable, but not less true, that it is *the only part* of the circulation that remains undiscovered even at the present day! What then *has he actually* discovered of the whole chain? Now, hear how this *connexion* was explained, and judge if we know any thing about it, more than was known to, or imagined by Harvey, Galen, and even Hippocrates! the mystery remains unelicited, and the honours decreed to Harvey have been too precipitately awarded!)

"It was believed that the mouths of each sort of vessels joined into one another. That *opinion* was soon laid aside; because it was found, that the capillary vessels were so extremely small, that it was impossible, with

the naked eye, to trace them." (Rather, perhaps, as being Galen's decided doctrine of anastomosis; it would, by forestalling Harvey, tend greatly to diminish his claim to be considered the *sole discoverer* of the circulation of the blood.) * * "This put them *upon imagining*, (it is well to call things by their right name!) that the blood *ouzes out of the arteries*, and is absorbed by the veins, whose small orifices receive it, as it lies *in the fibres of the muscles*, or in the *parenchyma* of the bowels; which *opinion* (here's a discovery ending in opinion!!) has been generally received by most anatomists since Dr. Harvey's time. But Leuwenhoeck has found in several sorts of fishes, which were more manageable by his glasses, than other animals, that arteries and veins are really continued siphons, variously wound round each other towards their extremities, in numberless mazes, all over the body; and others have found what he says to be very true, in a water newt. So that this discovery has passed uncontested."

☞ Which; anastomoses, porosities, or siphonic termini! The question, however, is not whether contested or uncontested;—has it been settled, so as to have at present one common creed? or is it not just as obscure as when Harvey or Galen wrote their *speculations* on the subject?

"And since it has been constantly found that nature follows like methods in all sorts of animals, when she uses the same sorts of instruments; it will always be *believed*, that the blood circulates in man after the same manner (demonstratio ad absurdum) as it does in eels, perches, carps, bats, and some other creatures, in which Leuwenhoeck tried it: though the ways *how it may be visible to the eye, in human bodies*, have not, that I know of, been yet discovered!" ☞ And yet we are told, the *discovery* of the circulation is complete! Surely, we must not accept of these terms in their *most rigorous acceptation!*

"But T. Bartholine, and Consentine, have raised up a modern rival to Harvey, for the honour of the discovery of the circulation; which is the celebrated Father Paul. What they relate, amounts only to this; that in a manuscript of Father Paul, that was left in the hands of Father Fulgentius at Venice, the particulars of the *true circulation* of the blood, as published by Harvey, are contained: and *hence they conclude*, that he communicated it to Fabricius ab Aquapendente, who told it to Harvey whilst he was at Padua, (☞ who claimed it as his own; at least, the story is as likely as the opposite one, viz.,) but the truth of this affair appeared to be, (☞ where is the proof of this?) that after Harvey's return to England, he made *a present of his book*, just then published, (☞ in 1628—compare dates, as far as possible, with respect to Father Paul, in this business) to the Venetian ambassador; who, immediately after going home, *lent it to* Father Paul, whose curiosity (☞ ah! fatal curiosity, which led the good Father to pluck an apple from a tree of knowledge in the illustrious Harvey's garden!) led him to make some extracts from it, which are con-

tained in the MSS above mentioned. What made this story *the more likely* to be true, was Father Paul's sagacity in anatomical researches, who *first observed* the contraction and dilatation of the pupil of the eye (why does not Jenty call this a discovery!) and is said to have communicated to Aquapendente his knowledge of the valves of the veins."

☞ This ridiculous story is at once set down, by the simple fact, that Harvey's book was "just published" in 1628, and poor Father Paul died in his 72d year, on the 14th January, 1622! See Biog. Dict. vol. x. p. 209, Lond. Edit. of 1784. The whole of this very extraordinary man's biography might be interesting: and would induce the reader to believe in the probability that he really made the discovery ascribed to him. I shall, however, so far entreat the reader's patience, as to give him, from the above source, what is there related on the subject; the biography being from Fulgentio's life of Father Paul. He was born at Venice, in 1552, that is, 25 years before Harvey.

"He, Father Paul, studied likewise anatomy, especially that part of it, which relates to the eye; on which he made so many curious observations, that the celebrated Fabricius ab Aquapendente did not scruple to employ, in terms of the highest applause, the authority of Paul on that subject, both in his lectures and writings. Fulgentio expresses his surprise at Aquapendente, for not acknowledging, in his 'Treatise of the Eye,' the singular obligations he had to Paul, whom he declares to have merited all the honour of it. (If so, Fabricius received a just return for his ingratitude, at the hands of Harvey; but poor Father Paul *between them both*, seems to have fallen to the ground.) He asserts likewise, that Paul discovered the valves which serve for the circulation of the blood, and *this seems to be allowed;* but *not that he found* it, as Walæus, Morhoff, and others, have contended in *prejudice to our countryman Harvey*, to whom that discovery has usually, and indeed justly, been ascribed. A book was published at Amsterdam, 1684, in 8vo, with this title, "Inventa Novantiqua; id est, brevis enarratio ortûs et progressûs artis medicæ, ac præcipuè de inventis vulgo novis aut nuperrimè in ea repertis:" in which the author, Theodore Jansonius ab Almeloveen, far from allowing Harvey to have discovered the circulation of the blood, affirms it to have been known to several others, and even to Hippocrates himself. But as to what concerns Paul, he has the following remarkable passage: 'Joannes Leonicenus says, that Father Paul discovered the circulation of the blood, and the valves of the veins; but durst not make the discovery public, for fear of exposing himself to trouble; since he was already but too much suspected, and there wanted nothing but this new paradox to transform him into an heretic, in a country where the Inquisition prevails. For this reason, he entrusted the secret to Aquapendente alone, who, fearful also of becoming obnoxious, communicated it but to a few, and waited till his death, before he would suffer his treatise concerning the valves of the veins to be presented to the Republic of Venice:

and as the slightest novelties in that country are apt to create alarm among the people, the book was reposited privately in the Library of St. Mark. But as Aquapendente had discovered the secret to a curious young English gentleman, named Harvey, who studied under him at Padua, and as Father Paul at the same time made the same discovery to the English ambassador, these two Englishmen upon their return home, being in a country of freedom, published it; and having confirmed it by a variety of experiments, claimed the whole honour to themselves.' Dr. George Ent, in his letter to Harvey, prefixed to his *Apologia pro circulatione Sanguinis*, attempts to refute this account, by observing, that the Venetian ambassador, having been presented by Harvey with his book, lent it to Paul, who transcribed many things from it, and this among the rest: but there is *a very great difficulty* (insuperable!) in this passage of Ent; for *it is certain*, that Harvey's book was not printed till 1628, whereas Paul died in 1623. However, Dr. Friend has *very well ascertained the sole discovery* (yes, so far as mere assertion goes; a reed in his hands, of no more force than in others!) of the circulation to Harvey, by showing, that none of those, to whom it has been ascribed, understood the nature and manner of it; and that, though Aquapendente could discover and describe the valves of the veins, yet he was at the same time *ignorant of the true use of them*, (☞ so Harvey tells us long prior to Friend!) as appears from his own description of them." (And what beyond assertion has Friend given?) The mystery has, assuredly, never been completely unfolded; and if so, the claim has been prematurely awarded! —We return to the Biog. Dictionary.

Thomas Bartholine, second son of Caspar, born in Oct. 1616, at Copenhagen, an admirable anatomist and most learned man, who, we are told here, ought to be reckoned "amongst those who contributed most to the progress of physiology, by defending warmly, the doctrine of Lymphatic vessels, against the repeated and violent attacks of *Harvey*, Riolan, Horst, and Hoffman." He likewise was one of the first who adopted and defended the circulation of the blood, discovered by Harvey; and he forcibly opposes the ridiculous theory of the flux and reflux of this fluid, *which Fort. Licetus had imagined!** He acknowledged the heart to be insufficient to propel the blood to all parts, and to aid its action, he admitted irritability in the arterial coats. He thought the air penetrated the blood, and he had noticed that the column of air introduced into the bronchiæ, is not altogether expelled during exspiration."

Blankaard.—"Tractatus novus de circulatione sanguinis per fibras, nec non de valvulis in iis repertis." Amst. 12mo. 1676—1688.—The author, the editor subjoins, admits the continuity between the arteries and the veins,

* Here we perceive, the Euripus-like movement of the blood, is awarded to another individual, who was posterior to Harvey! The more we examine the subject, the confusion is more confounded!

or rather the junction of these two orders of vessels, by the intermedium of a hollow fibre (d'une fibre creuse) furnished with a great number of valves, which permit the blood to flow from the artery into the vein, but prevent the reverse taking place.

Francis de le Boe—Sylvius, born in 1614, died Nov. 1672.

"De le Boe, says the Biographer, would merit a place in the history of medicine, of but little honour, had he not been the first professor on the continent who dared to embrace and maintain Harvey's opinion of the circulation of the blood. In 1658, (thirty years after H.) when occupying the chair of practical medicine, he contributed all in his power, and all the ascendency of his talent, to spread and confirm that splendid discovery. He appears to have been a pretty extensive writer; amongst his works enumerated, are "Disputationum medicarum," &c. of which the third is, "De chyli mutatione in sanguinem, circulari sanguinis motu, et cordis, arteriarumque pulsu," which probably embraces the particular objects of our present inquiry.

Boerhaave.—"If we desire to have an idea of the enlarged manner (manière large) in which he marks out the great revolutions of science, it is sufficient to cite expressions, by which he cut up those long discussions, raised as to the circulation of the blood, and claimed alternately, with warmth, by many enlightened nations. Immortalis Harveius demonstrationibus suis omni priorum theoriâ eversa, novum omnino, et certum, jecit huic basin scientiæ. No anatomist, claiming the discovery, loses his rights, but Harvey has the happiness and merit of demonstrating it."

Bohn, John, born 1640.—He admitted, between the arterial and venous extremities, an intermediate parenchyma, without which, he could not conceive that nutrition could take place. Too judicious not to seize with avidity upon a truth so important, he propagated with all his power, the discovery of the circulation of the blood, and demonstrated it at Pavia, with the machine of Boyle. At length, his pupil J. C. Lange stated that he injected the bronchial vesicles from the pulmonary artery, and the placenta from the uterine arteries, and if he opposed the erroneous ideas of F. de le Boe, he imitated his zeal in extending the discovery of Harvey.

Colombo, (Mat. Realdus) of Cremona, seems to have studied anatomy under Vesalius, whose absence he supplied in 1542, and *succeeded him*, two years afterwards. He died at Rome in 1577. "So greatly had the pursuit of anatomy been extended, that he dissected forty bodies annually, and made several discoveries, *one of the most important of which, is that of the pulmonary circulation*, which he has described more accurately and clearly than Servetus; but he has ascribed to himself many others, of which he has been justly divested, and restoration made to their true authors."

☞ This is just as it should be; and an equitable verdict would equally divest Harvey of some of his undue pretensions. One, we perceive here, in the claim for Colombo himself: but, says Jenty,

"*Servetus first discovered*, that the blood passes through the lungs; Colombus went farther, and showed the uses of the valves of the heart,

which let the blood in and out of their respective vessels, but not in the self-same road. Thus the way was just open when Harvey came, who built upon the first foundations." Anatomy, vol. 1. Hist. Comp. p. cxxvi. At p. cii. Realdus Columbus is said to have flourished about 1544, and that he "was intimate with Vesalius, whose public lectures he had frequently an opportunity of hearing. He is charged by some, with want of gratitude to Vesalius, from whom he is said to have stolen every thing that is valuable in his own works: But others maintain, that he had a clearer idea of the parts than Vesalius, and described more accurately; and it is certain that his Latin is very pure."

Jenty notices also the use Columbus has ascribed to the *lungs*, viz.:— that the blood and vital spirit might be prepared and generated in them, for "the blood being attenuated by elaboration in the right sinus of the heart, is carried through the vena arteriosa to the lungs; where, by their continual motion, it is agitated, still further attenuated, and mixed with that air which is drawn in through the nostrils and mouth, and carried through the rami of the aspera arteria to the whole of the lungs; which air is itself prepared by this collision: so that the blood and air, being thus mixed, are received into the rami of the arteria vena, and at last carried through the trunk itself, to the left ventricle of the heart; from which they are carried through the aorta, in every direction, to all parts of the body."

"Since this opinion, continues Jenty, is largely insisted on by M. Servetus, we have reason to suspect, that Columbus borrowed it from him. (☞ And why not Harvey also ?) This also Galen had advanced long before Servetus, when he says, that when the thorax is contracted, the venous arteries, which are in the lungs, being on all hands pent up and compressed, quickly throw out the spirit contained in them; but that they receive some portion of blood from the vena arteriosa, by minute and invisible orifices."

The title of his (Columbus') book was "Realdi Columbi in almo Gymnosia, Patavino anatomici celeberrimi, de re anatomica libri quindecem." Venice 1559, Fol. Paris 1572, 8vo. Leyd. 1667, 8vo.

Carrere, Jos.—Born in 1680, died in his 55th year. Rector of the academy of Perpignan. He maintained a thesis *against* the circulation of the blood, entitled, Animadversiones in Circulatores.

Carrere, Jos. Barthelemy Francois.—Born also at Perpignan, 1740, and obtaining the title of Emeritus Professor in that University; he died in 1802. In 1764, he wrote a "Dissertatio physiologica de Sanguinis circulatione," and another in 1772, "De retrogrado Sanguinis motu."

Cæsalpinus, Andrew.—Born in 1519, at Arezzo in Tuscany. He was professor for many years at Pisa, but went to Rome, as first physician to Clement VIII., and was made professor in the College de la Sapience. He died in 1603.

His chief title to glory, is that of having known and well described the lesser, or pulmonary circulation. He knew that the blood passed from the

right ventricle into the pulmonary artery, and from it, into the veins of the same name, which conveyed it to the left ventricle. His information went, in fact, much farther, if we judge from the following passage: "In animalibus videmus alimentum per venas duci ad cor, tanquam ad officinam caloris insiti, et, adeptâ inibi ultima perfectione, per arterias in universum corpus distribui, agente spirtu, qui ex eodem alimento in corde gignitur." If we add to this, that Cæsalpinus had noticed the swelling of the veins *below* the ligatures, and the return of the blood by these veins, it cannot be doubted, but *that he also knew the great* circulation; all that was requisite to have given him the *exclusive honour* of this great discovery, was to have described it separately (de la decrire à part), and especially to have been always consistent with himself; but overcome by his love for Scholastics, he always sacrificed the observation of nature, to his endless disputes on the most obscure points of philosophy," &c.

Among his writings, noticed in the biography, are "Quæstionum peripateticarum, libri V." 1569, &c. in which appear, apparently, the passages which incline us to believe, that he at least suspected the circulation of the blood; but it is added, they are all ambiguous, and their obscurity justifies the obstinacy of writers, who, unacquainted with any other, refuse to despoil Harvey of a part of his glory, in favour of Cæsalpinus; the above extract is taken, we are told, from his treatise, *de Plantis*, and that it *should remove all doubts.*

☞ We must be permitted to add, that if the above extract is sufficient to remove all doubts as to Cæsalpinus' prior claim, fifty may probably be found of equal force in Galen. However, it may be as well to know what others have said respecting this great man; and I extract from Jenty's Compendium in the 1st. vol. of his Anatomy, p. cvii. et seq.

He was, says Jenty, a strong champion for the peripatetic doctrine, in opposition to Galen, who was at that time reverenced as an oracle. Hence it was, that the writings of Cæsalpinus, though very valuable in themselves, were neglected; and those passages which he casually wrote, concerning the circulation of the blood, *either not adverted to, or not understood, by any*, till Harvey published his treatise on the subject. (And here, let it be kept in mind, whilst reading the extracts made by Jenty, from his writings, that Harvey has not in the remotest manner alluded to him, or them; yet his work was printed before Harvey had begun the study of his profession.

It would appear that, with Aristotle, he supposed the heart to be the source of the arteries, veins, and nerves. In Quest. 4. he proves, that in respiration, no external air can have access to the heart; and he has these words, "for the membranes are so fitted and adapted to the mouths of the vessels, that when the heart is dilated they are opened; but when it is contracted, they are shut."

"Some of the vessels, continues he, which terminate in the heart, *send their contents into it;* such as the vena cava into the right ventricle, and the venous artery into the left. Some of them, on the other hand, *draw their*

*contents from it;* as the arteria aorta from the left ventricle, and the arterious vein from the right; but they all have membranes so fitted and adapted to them, that the mouths of the intromitting vessels will not admit of a return, and the eliminating vessels will not admit of an intromission. It happens, that when the heart is contracting, the arteries are dilated; and when it is dilating itself they are contracted."

☞ Here we have in a few lines, the sum and substance of several chapters of Harvey; he has, however, entered more fully into the proof of things; but ☞ did he not know of this in Cæsalpinus?

He tells us also, that "the several phenomena appearing upon the dissection of a subject, correspond excellently with this circulation of the blood, from the right ventricle of the heart, through the lungs to the left ventricle." With much to the like effect relating to the cause of respiration, &c.

The following is not less interesting, as assuredly forestalling much of Harvey.

"The veins become turgid *beyond* the ligature, and not betwixt it and the heart; but it ought to have been otherwise, if the motion of the blood and spirits had been from the viscera to the several parts of the body. For the passage being obstructed, the progressive motion of the blood is stopped, so that the veins should have become turgid between the ligature and the heart." He here appears to call in the aid of Aristotle, and tide of Euripus; but, as I apprehend, to no satisfactory result. Euripus seems to have been a kind of watch-word; as the reader will perceive that it is of frequent occurrence in the writings of the day. He goes on, however, more to the purpose, thus, "for the understanding of which passage, we must know that the passages of the heart are so contrived by nature, that there is an entry from the vena cava to the right ventricle of the heart, from which there is a passage into the lungs: and that from the lungs there is another passage into the left ventricle of the heart, from which, at last, there is a passage into the arteria aorta; certain membranes being fitted to the mouths of the vessels, to hinder the return of the fluids: for thus there is a *perpetual motion* from the vena cava through the heart and lungs into the arteria aorta."

As for some of the speculations or hypotheses, &c. of Cæsalpinus, they are scarcely more absurd than those of Harvey in most respects; and cannot militate against the above plain exhibit of the pulmonary circulation. He writes, however, (says Jenty,) "as one would think, very explicitly upon this matter; *yet* we will not take upon us to determine, positively, that he knew this affair distinctly. We rather think with Wotton, that this *notion* had only been occasionally and slightly treated of by Columbus and Cæsalpinus, who themselves, *in all probability*, did not know the consequences of what they asserted; and therefore it was never applied to other purposes, either to show the uses of the other viscera, or to explain the nature of diseases: neither, for any thing that appears at this day, had they made such numbers of experiments as were necessary to explain their doctrine, and to clear it from opposition. All this Dr. Harvey undertook to do, and with

indefatigable pains traced the *visible* veins and arteries throughout the body, in *their whole* progress from and to the heart, so as to demonstrate, even to the most incredulous, not only that blood circulates through the lungs and heart; *but the very manner how*, and the time, in which that great work is performed."

☞ Compare also what is said of Cæsalpinus by Senac, on the heart; and Sprengel in his Histoire de la Medicine. What Friend has stated, is to be seen in one of the papers preceding this essay! and with this and more, let the reader anxiously determine what part, if any, of the pulmonary circulation is due to Harvey!

Chaillou, James, a French physician of the seventeenth century. In the following named work, he admits the reality of the circulation, but endeavours to prove that it was known to Hippocrates.

"Recherches sur l'origine du movement du sang, du cœur et de ses vaisseaux, &c. Paris 1664, &c.

Charlton, Walter, born 1619—like Harvey, he was physician to Charles I., and member of the college, having graduated in 1642. In 1678 the University of Padua offered him the chair of Practical Medicine, which he accepted at first, but subsequently refused it: in 1680 he was chosen by the college to give the lectures on anatomy, and in 1689, he was elected the President of that body. His greatest merit, says his Biographer, consisted in his showing himself one of the warmest partisans of the circulation of the blood: and strove by every means to deprive the liver of the important rank assigned to it in the doctrine of hematosis. His works connected with the circulation, are apparently, Oratio Anniversaria, &c. in 1680, in praise of Harvey; and three anatomical lectures on the motion of the blood through the heart and arteries, &c. 1683.

Conring, Herman born 1606, graduated in 1636, and died in 1681. He appears to have been held in the highest estimation; amongst one hundred treatises at least, we find one entitled Diss. de sanguinis generatione et motu naturali, 1643. It is remarked, that he is the first who taught the circulation of the blood, at Helmstaedt, where he was professor.

Ent, George, born in 1603. "A zealous partisan of Harvey, he defended with much skill and constancy, the circulation of the blood, without however being able to avoid mistakes and paradoxes." The title of his work is "Apologia pro circulatione sanguinis, quâ respondetur Æmilio Parisano." Lond. 1641. and 1685. 8vo.

Fabri, Honore, a Jesuit, born in 1606. He had, says his biographer, the audacity to appropriate to himself the discovery of the circulation of the blood, and his confrere, le P. Regnault, delighted to add a fresh wreath to the honour of his company, did not hesitate to adjudge it to him. The author of the *Ancient origin of the new doctrine of natural philosophy*, depends on Fabri's having taught the doctrine in 1638; but Harvey preceded him by ten years: adding, moreover, that the treatise is a tissue of plagiarisms.

Folli, Cecilius, born in 1615, and a professor of anatomy; he published a treatise at Venice in 1639, 4to. entitled,

"Sanguinis à dextro in sinistrum cordis ventriculum defluentis facilis reperta via; cui non vulgaris in lacteas nuper patefactas venas animadversio præponitur."

Folli, Francis, a contemporary, born in 1624, and a physician at the court of the Medici—he died in 1685—and was one of the most ardent propagators of the discovery of the circulation, and the first to try transfusion. His treatise is,

"Recreatio physica, in qua de sanguinis et omnium viventium universali analogicâ circulatione disseritur." Flor. 1665.

Fouquet, Henry, born in 1727—died, 1806. Though highly noticed in the Biographical Dictionary, is introduced here, merely to mention a work of his, "Prælectiones medicæ decem, habitæ in Ludovicæ medico Monspeliensi," &c. Montpel. 1777, 12mo. The first and second are connected with our present object, viz.,

De certis et dubiis in systemate Harveano de circulatione Sanguinis.

De veterum doctrinâ circa sanguificationem. Neither of which have I seen.

Fabrizio, Jerome. *Fabricius ab Aquapendente*, from the place of his birth, in 1537; appears to have been one of the most learned men of his age: he was educated at the university of Padua, in which he was subsequently the successor to Fallopius, in the anatomical chair, in 1565, for fifty years; he died in 1619, at the age of eighty-two. He discovered, says his biographer, the valves in the veins; although thirty years previously, *Etienne*, *Cannani*, and *de la Boe*, had spoken of them; their existence was contested by the anatomists of the period, especially by Vesalius, Eustachius and Fallopius. But for the researches of Fabricius on the valves, perhaps his pupil Harvey would not have confirmed the circulation, suspected by Cæsalpinus and Servetus.

The works of this great man were numerous—those which are principally connected with the subject before us, are,

"De formato Fœtu." Padua, 1600, fol. reprinted several times.

"De Venarum ostiolis." Padua, 1603—1605, fol. In this, his labours on the valves appear; and it is of course, one of the most remarkable that has been published on anatomy.

"De Respiratione et ejus instrumentis libri duo." Padua, 1615.

"De formatione Ovi et Pulli." Padua, 1621.

Jenty has said respecting him, at p. cxxii. of his Compend, what is, indeed, a confirmation of the above: we are there informed, "that *he first observed the valves* of the veins in 1574, of which, it is said, he was informed by Father Paul; but he was not acquainted with their structure or uses."

Compare also, Friend, Senac, and Sprengel.

GALEN.—"Speaks very distinctly of the movements of the systole and diastole of the heart. A passage in one of his books, (Introd. ad Medic.

p. 373. edition not mentioned,) seems to show that he had some idea of the circulation; Aristotle, persuaded of the flow of blood from the heart to the extremities, regarded its return as probable. Galen admitted that the blood was carried by the pulmonary artery into the lungs to nourish them, and that a part of it returned to the heart."

All this, like most of the references to Galen in any way, is far too meagre an account of what that great man knew on the subject. Indeed, the more I look into his writings, the more am I persuaded, that by far the greater part of the information attributed to him, is not from personal investigation of his works, but derived from some previous author, who often quotes at random, or at least in a way so general, as to preclude the finding the passage in question. I propose here, to introduce a very few references to his writings, merely to enable any one, who chooses to look into them, to find a clue for further investigation as to his real knowledge of the circulation. The edition referred to, is that of Basil, 1549, by Frobenius.

*Comment. in lib. Hippoc. de Natura Hominis*—1. p. 139 B. "Eodem modo habet, *to* proficisci," p. 140. Nay, even to p. 144, the reader's time would not be lost.

*De atrabile.—Frob.* p. 154. Of the blood—its colour—consistence, &c., in arteries and veins—coagulation out of and in the vessels, and other parts. Its tar-like appearance—arterial and venous blood the same.

*P.* 163. Some remarks relative to the atrabilis being, (as one of the humours of the body,) contained in the blood, which go to accredit his belief in a circulation. "*Forte igitur*, &c., *to commeet.*"

*De bona habitudine.—Frob.* p. 174. In speaking of the Athletæ, and their training, &c., he notices that sometimes, from their augmented diet, or increased plethora, (sanguine nimium aucto,) they are suffocated; or break a blood-vessel of the lungs or liver, &c.; and he gives from Hippocrates the case of sudden loss of speech from vascular repletion.

*An sanguis in arteriis natura contineatur.—Frob.* p. 213. The proposition is fully sustained by Galen, in opposition to Erasistratus and others, by reasons and experiments altogether unanswerable. The dispute at that period seems to have been, as to whether air, or blood, or both, were naturally contained in the arteries. And his opponents, by no means decided amongst themselves; are regularly pursued in their explanations, and their difficulties and absurdities are pointed out. A question is proposed for Erasistratus himself to resolve, viz., what would result from the wound of an artery in the arm? and which he presumes could be replied to only in the way he points out—and which it would seem nearly impossible for him to explain so clearly, if he had not, not only a belief in a circulation, but also, a tolerably perfect idea of its route—p. 218. "*Cogita quæso*, &c., to *erumpere.* It would appear from his words, that, connected with other parts of his writings, we cannot well deny him such ideas. Whilst in a further part, he mentions other difficulties which the doctrines of Erasistratus

abound with; and he explains how the arteries are filled; and maintains the power of the heart in distending the arteries; referring to a further consideration of this, in the work "*de Decretis Hippoc. et Platonis.*" Adverting also to the experiment which Harvey has noticed, of introducing a hollow tube into an artery, &c., and which I have more than once referred to, in my remarks on Harvey's claim to be considered the discoverer of the circulation.

In his treatise "*de Causis Pulsuum,*" lib. 3., he enters into an explanation of the operation of the so called *non-naturals*, in promoting the action and changes of the pulse—of the influence of age, &c., on it—of artificial habits, and other causes, &c.; and in the fourth book, he treats of its modification by various preternatural causes, as emotions and passions of the mind, and sundry diseases—and in a manner which ably maintains the hand of a master.

In the fifth book of his treatise, "*de Anatomicis administrationibus,*" which is itself imperfect from the fifth chapter of the ninth book; we find cause to regret, that, besides this, the tenth, eleventh, twelfth, thirteenth, fourteenth and fifteenth books, are also lost. And he moreover notices a "*decimus sextus illius operis liber, agit de arteriis, venis et nervis,*" in which, says he, I have explained what is commonly and generally known respecting them. The loss of this is especially to be regretted, since it would probably have better enabled us exactly to appreciate the full extent of his knowledge and views respecting the circulation!

In the seventh book, he gives an account of the pleura and pericardium, and draws a comparison of the former with the peritonæum—then speaks of the heart and arteries; of the different opinions respecting the vessels of the lungs, and of the pulse—wherein much is to be learned as to the views of a circulation—and may go far in the consideration of the question as to its discovery. In one part of it we find the following words: (Froben. p. 353 A.)

"Quales igitur toto corpore existunt arteriæ, tale vas ex dextro cordis sinu procedens, in totum pulmonem ramorum serie diffunditur. Quales autem venæ, tale ex sinistro; ut ex tribus vasis pulmonem intertexentibus, quod à sinistro cordis ventriculo proficiscitur, arteria venosa nuncupetur, quod à dextro, arteriosa vena," &c.

In ch. 14, 15, of this book, he proceeds to state what is to be seen in the thorax on dissecting a living animal. It is an interesting statement, and in more than one place, seems to bear considerably on the general circulation. The pulsations of both sides of the heart are particularly adverted to; and even the ultimate motion of the auricles, at long intervals, after that of the ventricles has altogether ceased. Here, too, we find him again opposing the opinion of the arteries containing air.

His highly interesting book "*de utilitate Respirationis*" is not wanting in remarks, that seem more or less applicable to an idea of a circulation; and

in one part, it seems probable, that even Erasistratus had an imperfect conception of the pulmonary circuit.

In his book "*de Pulsuum usu*," the doctrine of a circulation appears by no means obscurely upheld, although the language may not exactly conform to present views; and yet, in technicality, it is probably nearly as correct as that of the present day; and few would write as he has done in this book, who had not a strong conception of the necessity and use of a circulation; and of the close connexion of the arteries and veins. Thus, after sundry considerations, he comes to the following conclusion: "*Et cum semper vacuatas cum arteriis venas deprehendissemus, veram esse sententiam de communibus arteriarum et venarum osculis, et communi de una in alteram per ea transitu, nobis persuasimus*," &c.

It will no doubt be objected, that Galen here implies a mutual transmission from arteries to veins, and from veins to arteries; nevertheless, although this seems prima facie his meaning, I am disposed, from other parts of his writings, to doubt if such was the case; and here especially, the loss of the sixteenth book of his anatomy, treating of the blood-vessels, is much to be lamented. I shall advert to one fact alone from him, as it is made use of by Plempius, see p. 35; who there quotes it in proof of Galen's acquaintance with the anastomosis of the vessels. "Si multis amplisque *arteriis præcisis* jugulare per eas animal velis, invenies *ejus venæ* æquè *atque arterias* vacuatas; quod sanè nunquam fieret, nisi inter se haberent altera in alteram ora reclusa." De Nat. facult. lib. 3. cap. 15.

In the sixth book of his treatise "de usu partium," we find the passage adverted to by Harvey, wherein the connexion of arteries and veins is explicitly sustained. "In toto corpore mutua est anastomosis, atque oscillorum apertio arteriis simul et venis," &c. And here we likewise find his explanation of the use of the valves of the heart, and a description of them; (11th chap.) and his language shows, moreover, that he comprehended the nature and influence of the right side of the heart. In the 17th ch. of this book, again maintaining, in opposition to Erasistratus, that the arteries contain blood, he also renews his opinion of the anastomosis of arteries and veins; "orificiorum arteriarum ad venas apertiones non sine causa neque frustra paravit natura, sed ut respirationis ac pulsuum utilitas non cordi soli atque arteriis, sed cum eis, venis etiam distribueretur," &c.

The sixteenth book "de usu partium," seems one that goes very far, both by implication and directly, in support of the opinion, that a circulation was known to, and taught by Galen. So continual are those implications, as to forbid their adscription to accident alone; but rather to place them to the result of well-founded opinions, arising from facts insulated in themselves, but strongly supporting each other, and the common doctrines to which they may give rise. The book in question considers the distribution of the vessels throughout the body. The artery, vein, and nerve, he calls, here and elsewhere, the common instruments of the body, entering into the composition of every part; and the great equality of the

distribution of the veins by nature, and the community of use of arteries and veins, is pretty explicitly affirmed. To every reasonable mind, it is believed, that enough may here be found to satisfy it, that the views and researches of Galen, into the mysteries of the circulation, were pretty extended; and that from the hitherto *very limited references* to his writings, on this particular, especially by Harvey himself, the medical public has really been kept altogether ignorant of what he has so largely considered in so many of his works.

In several of the books of his treatise, entitled, "de Hippocratis et Platonis Decretis" we find many allusions to a circulation also laid down, adequate to stagger the usual adscription of its *discovery* to Harvey. In the 7th ch. of the first book, he says, "E sinistro enim cordis sinu arteria maxima exoritur, quasi quidam arteriarum omnium truncus, quæ per totum animalis corpus distribuuntur," and opposing an opinion held by Erasistratus, that arteries terminated in nerves. In the second of these books, he states the difference of cutting the three species of vessels; (the nerves being considered as tubular, were so regarded:) immediate death from the immoderate effusion of blood, by dividing the jugular veins or carotid arteries, unless prevented by tying them up; whilst if the nerve only be tied, or cut, or compressed, the animal merely loses his voice. The sixth book is no less important in numerous particulars, tending to elucidate Galen's ideas of a circulation, and even showing that an idea of it was not unknown to Plato himself.

What can be conceived of by others, as to the peculiar views of Galen respecting the circulation, from the following sentence from his treatise "de formatione Fœtuum," I know not; but it seems to me to admit of but one plausible construction, that of an extensive and well grounded apprehension of a general flow of blood throughout the system. "Tantum igitur hoc habeo, quod de causa animalium formatrice asserere posse existimem, quod summa in ea ars, summaque sapientia inest, quodque postea, quod formatum corpus fuerit universum, id in toto vitæ curriculo tribus motuum principiis, ex cerebro per nervos et musculos; ex corde per arterias, et jecore per venas gubernatur." Other quotations might not unaptly be made.

Time and space are, however, wanting to go through the other divisions of his writings, to sustain and strengthen what I have above presented to the reader. I will, therefore, notice but one more from his treatise "de Tremore, Palpitatione, Convulsione, et Rigore," 3. 195. In the fifth chapter, considering the locality and causes of palpitation, and adverting to the propriety of bleeding in some of those cases, although opposed by many, he proceeds to note the statement of Hippocrates; and mentions, from whence blood should be drawn in certain cases, in a manner that strengthens, I think, greatly what we have urged as to his views of a circulation. Indeed, the whole of this book indicates that practical experience in bloodletting; that, if he is denied *a knowledge of its route*, sufficiently evidences,

that such knowledge, *considered simply in itself*, can be of no important advantage. And if Hippocrates and Galen, without a knowledge of the circulation, have probably never been surpassed as practitioners, let the adherents of the Harveian claim, fairly point out the real advantage that medicine and its branches have attained by his asserted discovery.

Gericke, Peter.—Born 1693, and was a Professor of Anatomy at Helmstaedt in 1730, and died in 1750. He published a tract in 4to, in 1733, at Helmstaedt, entitled "Programma de venarum valvulis, earumque usu." The biographer adds, that he attributed the discovery of the valves of the veins to Michael Servetus: and proposes the whimsical notion, that these folds (replis) are less intended to prevent the blood from retrograding, than to prevent the too great distention of the sides of the veins.

Vidus Viduus,—(Guido Gui,) of Florence, more known under the name of V. Viduus. We have already referred to this excellent writer in the preceding part of our inquiry.

Hannemann, John Louis.—In 1675 appointed to a professorship at Kiel, which he filled for fifty years. A copious writer, but, says his biographer, he would not have been remembered, unless he had shown himself to be one of the most declared *adversaries to the circulation*, and if his animosity to the *beautiful discovery* of Harvey, had not brought on him a most severe censure by Thom. Bartholin. The following would seem to be his writings connected with the subject: it may be remarked, he first studied divinity.

"Ovum Harveianum generationis animantium curiosum. Quo demonstratur adversus materialistas, quod generatio animalium fiat ex nihilo." Kiel, 1675. 4to.

"Exercitatio de vero et genuino sanguificandi organo ad Thom. Bartholinum." Kiel, 1675. 4to.

"De Motu Cordis." Kiel, 1706. 4to.

Harder, John Jacques, German anatomist, born 1656—died 1711. He appears to have taken up the subject in the following treatises.

"De naturalis et præternaturalis sanguificationis in humano corpore historia." Bale, 1690. 4to.

"De Sanguinis motu vitali." Bale, 1694. 4to.

"De Chyli secretione et distributione." Bale, 1698. 4to.

Hofmannus, Caspar, born 1572—died 1648. He appears to have been an extensive writer. A few of his productions seem connected with our subject, and he is probably the Hoffmann whose name appears in Harvey's treatise, ch. . It may be remarked that he was five years older than Harvey, who survived him about ten years. The subject of the circulation, and the disputes respecting it, must necessarily have been familiar to him. I have none of his works connected therewith.

"Dissert. de usu venarum et arteriarum mesaraicarum." Altdorf, 1616.

"Dissert. de usu venæ arteriosæ et arteriæ venosæ." Altdorf, 1618.

"Diss. de pulmone, ejusque usu secundum Aristotelem." Altdorf, 1622.

"Dissert. de Sanguine." Altdorf, 1622.

"De Thorace, ejusque partibus," &c. Frankf., 1627.

"Problema, cur natura fecerit duo vasa sanguiflua, venas et arterias." Altdorf, 1627.

HOFFMANN, MAURICE, born 1622, and professor of anatomy in Altdorf in 1648, after the death of the preceding Caspar. He filled several chairs successively during fifty years.

"Dissert de Motu Cordis et Cerebri, sanguinisque ac spirituum animalium perpetuo, pro vitæ continuatione, per corpus commeatur." Alt., 1653.

"Dissert. de transitu sanguinis per septum cordis impossibili contra Galenum et Riolanum, anatomicum Paris. ejus defensorem." Alt., 1659.

"Dissert. de transitu sanguinis per medios pulmones facili, contra Riolanum ejus osorem." Altdorf, 1659.

HUMEAU, FRANCIS, M. D., born 1628, at Poitiers. Wrote against the Harveian discovery,

"In circulationem sanguinis Harveianum exercitatio anatomica." Poitiers, 1659.

KYPER, ALBERT, professor at Breda in 1646.

"Institutiones medicæ ad hypothesin de circulari sanguinis motu compositæ." Amsterdam, 1654.

"Anthropologia, corporis humani contentarum et animæ naturam et virtutis secundùm circularem sanguinis motum explicans." Leyd., 1647.

LEICHNER, ECCARD, born 1612, made professor at Erfurt, in 1646.

"De motu sanguinis exercitatio anti-Harveiana." Arnstadt, 1645.

HECQUET, PHILIP, born 1661. This most excellent man and physician, is the one so unjustly satirized by Le Sage, under the denomination of Dr. Sangrado. He is mentioned here, merely because I find in his biography, a statement that has a slight bearing on the subject of the circulation. "Avec Stahl et Keill, il admettait à l'extrémité des vaisseaux une substance spongieuse et vésiculaire, servant de réservoir aux reliquats des sues superflus pour la nutrition."

HEISTER, LAURENCE, born 1683, one of the most celebrated anatomists of Germany, and an extensive writer; amongst his works are,

"Programma quo inquiretur: an sanguinis circulus veteribus fuit incognitus." Altdorf, 1714. 4to.

"Programma," apparently a new edition of the preceding. Helmstaedt. 1721. 4to.

LINDEN, J. ANT. VANDER, born 1609, died in 1664. Professor at Leyden. Among his writings, are,

"Hippocratis de circuitu sanguinis." Leyd., 1661.

LISCHWITZ, J. CHRISTOPHER, German professor at Leipsic and Kiel, born 1693, died 1743.

"Dissert. an aer ex pulmonibus substantialiter transeat ad sanguinem." Kiel, 1735.

"Dissert. de principio venarum." Kiel, 1736.

Lower, Richard, born 1631, died 1691.

"Tractatus de corde; item de motu et colore sanguinis, et chyli in eum transitu." Lond., 1665.

Maurocordato, Alex., of Scio, born 1636, died in 1711. Only one production connected with medicine, viz., his Thesis,

"Pneumaticum instrumentum circulandi sanguinis, sive de motu et usu pulmonum dissertatio philosophico-medica." Bologne, 1664.

Parisano, Emile, of Rome, studied at Padua under Aquapendente, probably therefore contemporaneous with Harvey, as a pupil, since he was born the same year, 1577. He was, apparently, one of the "momes and detractors," with whom Harvey would have nothing to do. Among his writings are,

"Nobilium exercitationum," part 1, Venice, 1623—part 2, 1635—part 3, 1638. In the second part we have his treatise, entitled

"De Cordis et Sanguinis motu ad Guil. Harveum."

Pecquet, John, born in , died in 1674. His biographer says, he contributed greatly by his reasoning and his discoveries, to prove the circulation of the blood, which still had some opponents. One of his treatises is entitled,

"De circulatione Sanguinis et chyli motu dissertatio."

Pietre, Simon, son-in-law to Riolan, who concealed him during the bloody massacre of St. Bartholomew; had a son of the same name, surnamed *le Grand*—of whose writings, the following seem to appertain to our subject:

"Disputatio de vero usu anastomoseon vasorum cordis in embryo." Tours, 1593.

"Nova demonstratio et vera historia anastomoseon vasorum cordis in embryo cum corollario de vitali facultate cordis in eodem embryo non otiosa." Same place and year.

Pitcairn, Arch., born 1652, and held a professor's chair at Leyden. Among his writings, are

"De sanguinis circulatione in animalibus genitis et non genitis." Leyden, 1693.

"De causis diversæ molis qua fluit sanguis per pulmonem, in natis et non natis." Same year and date.

"De motu sanguinis per vasa minima." Idem.

We have amply extracted from his writings, in the course of our Inquiry.

Primrose, James—Born at Bordeaux, graduated at Montpellier in 1617, and went to England, with a high reputation, and was soon known by his success in practice. He died in 1660. His works, which appear to have brought down so many anathemas on his head, are the following,

"Exercitationes et animadversiones in librum de motu cordis et circulatione sanguinis, adversus Guil. Harveum." Lond. 1630.

"Animadversiones in J. Walæi disputationem quam pro circulatione sanguinis proposuit." Amsterd. 1639.

"Animadversiones in theses quas pro circulatione sanguinis in Academia Ultrajectensi Henricus Leroy proposuit." Leyd. 1640.

"Destructio fundamentorum medicinæ Vopisci Fortunati Plempii." Rotterd. 1657.

Something further respecting Primrose will be found in other parts of this inquiry.

Riolan, John, father and son. The elder Riolan appears to have graduated about the period of Harvey's birth. He died in 1606; of consequence prior to the enunciation of the circulation. It was Riolan, Jr. who was the opponent of Harvey; and, like Primrose, was born in the same year with him, 1577. He graduated at Paris in 1604, and was named Prof. of anatomy and botany in 1613. He was first physician to Mary of Medicis. He died at the age of eighty, having twice been cut for the stone (subi deux fois la cystotomie.) His writings connected with the subject of our consideration, were

"Opuscula anatomica nova." Lond. 1649.—In which, says his biographer, the person who fully proved the circulation, was worried. Is it possible, adds he, that this could have been printed in London? The very stones would rise in England against those *who depreciate the national glory*. Another edition, in Paris, in 1652, is principally directed against the circulation of the blood.

"Responsio prima edita, anno 1652, ad experimenta nova anatomica, Joannis Pecqueti adversus hæmatosim in corde, ut chylus hepati restituatur, et nova Riolani de circulatione sanguinis doctrina sarta tecta conservetur." Paris 1655.

Rolfink, Werner—Born 1599.

"Dissert. de chylificatione et circulatione sanguinis," Jena, 1632.

"——— de circulatione," Jena, 1642.

Rudbeck, Olaus—Born 1630.

"Dissertatio de circulatione sanguinis," Westeras, 1652.

SERVETUS, MICHAEL—Born 1509, *burnt* Oct. 27, 1553. "In his Christianismi restitutio (1553. Vienne in Dauphiny), he has, in the 5th book, positively asserted, that the whole mass of blood passes through the lungs, by means of the pulmonary artery and veins. It is this which has given him a distinguished place in the history of anatomy."

☞ This is too concise an account, by far, of the statement of Servetus; and we therefore think no apology necessary for introducing the whole extract from the work in question; as we find a *part* thereof, ready translated to our hands, in the Compend of Jenty, we shall make use of it, and in its proper place introduce that portion which is omitted, though why, we cannot say.

It may be premised that the works of Servetus possessed by me, are in 4to, entitled "Historia Michaelis Serveti," printed at Helmstadt, by H. A.

Allwoerden, 1727, &c. And in the proemium, the author, § 9, thus expresses himself. "Animus nobis erat in hanc inprimis quæstionem accurate inquirere: an Servetus dudum ante Harveium circulationem sanguinis invenisset? Affirmant id doctissimi homines, rerumque medicarum experientissimi: ex quibus nunc Godofr. Guil. Leibnitzium,* Henr. Wottonum,† Sam. Massonum,‡ Josephum Morlandum,§ Jacob. Douglasium,‖ tantum nomino. Atque fateor, verba ejus, quæ Sam. Crellius,¶ preter alios publice legenda dedit, in hanc pene sententiam Lectores inducere. Nos auxilio inprimis viri in his rebus magni celeberrimique Laurentii Heisteri negotium hoc conficere cogitabimus." The above may prove useful to future inquirers on the subject, and I proceed now to the extract referred to, and which is to be found at p. 231 of the above history of Servetus, and translated in part by Jenty, at p. 100 of his Historical Compend.

"There are, says he, in the human body spirits of three different kinds; the natural, animal, and vital; which are really not three, but two, distinct spirits. The vital is that which is communicated, by anastomoses from the arteries to the veins; in which it is called natural: the blood therefore is first; whose seat is in the liver and veins. The vital spirit is second, whose seat is in the heart and arteries. The animal spirit is third; which is like a ray of light, and has its seat in the brain and nerves." Here, Jenty has omitted more than half a page of matter, which I give in the original of Servetus, p. 231. "In his omnibus est unius spiritus et lucis Dei energia. Quod a corde communicetur hepati spiritus ille naturalis, docet hominis formatio ab utero. Nam arteria mittitur juncta venæ per ipsius fœtus umbilicum, itidemque in nobis postea semper junguntur arteria et vena. In cor est priusquam in hepar a Deo inspirata Adae anima, et ab eo hepati communicata. Per inspirationem in os et nares est vere inducta anima. Inspiratio autem ad cor tendit. *Cor est primum vivens, fons caloris* in medio corpore. Ab hepate sumit liquorem vitæ, quasi materiam et eum vice versa vivificat. Sicut aquæ liquor superioribus elementis materiam suppeditat, et ab eis juncta luce ad vegetandum vivificatur. Ex hepatis sanguine est animæ materia, per elaborationem mirabilem, quam nunc audies. Hinc dicitur *anima esse in sanguine*, et *anima ipsa est sanguis*, sive sanguineus spiritus. Non dicitur anima principaliter esse in parietibus cordis, aut in corpore ipso cerebri, aut hepatis, sed in sanguine, ut docet ipse Deus, Gen. 9. Levit. 17. et Deuter. 12."

I should be much pleased if any person could suggest a reason, why

* Discours de la conformité de la foy avec la raison § xi. p. 17. et in litteris ad summe ven. nostrum præsidem a. 1717. d. 24. Sept. exaratis.

† Reflections upon Learning, p. 42.

‡ Histoire critique de la repub. des Lettr. Tom. vi. p. 350.

§ Disquisitions concerning the force of the heart. Lond. 1714. 8. p. 79.

‖ In specimine Bibliographiæ anatomicæ, p. 189. Lond. 1715. 8.

¶ In Bibl. Bremensi, class. I. Fasc. v. p. 757.

Jenty should have left out of his translation this, not unimportant, part of an extract, wherein we find Harvey forestalled in more than one particular! We go on now to Jenty's continuation.

"Now to understand how the blood is the life, he (that is, Servetus) says, we must first understand the substantial generation of the vital spirit, which is compounded of, and nourished by, inspired air and the subtilest part of the blood. The vital spirit has its original in the left ventricle of the heart, by the assistance of the lungs, which chiefly contribute to its generation. It is a subtil spirit wrought by the force of heat, of a florid (qu? *flavo* colore) colour, having the power of fire; so that it is a sort of shining vapour, made of the purer part of the blood, containing within, in itself, the substance of water, air and fire. It is made in the lungs by the mixture of inspired air with that elaborated subtil blood which the right ventricle of the heart communicates to the left. *Now that this communication is not made through the septum of the heart, as is commonly believed;* but the subtil blood is very artificially agitated *by a long passage through the lungs from the right ventricle of the heart, and is prepared, made florid by the lungs, and transfused out of the arterious vein into the venous artery;* and, at last, in the venous artery itself, it is mixed with the inspired air, *and by expiration purged from its dregs;* and thus, at length, the whole mixture is attracted, by the diastole of the heart, into the left ventricle, being now a fit substance out of which to form the vital spirit.

"Now that this communication and preparation is made by the lungs, is evident, from the various conjunction and communication of the arterious vein with the venous artery in the lungs: The *remarkable largeness*, of the arterious vein likewise *confirms it*, since it would never have been made of that form and bulk; nor would it have emitted so great a quantity of very pure blood out of the heart into the lungs, *if it had been only for their nourishment;* nor would the heart have been this way serviceable to the lungs, since the fœtus in the womb is otherwise nourished, by reason of the closeness of the membranes (ob membranulas illas, seu valvulas cordis) of the heart, which are never opened till the birth of the child, as Galen teaches."

Here again Jenty omits an extract of some importance, which is as follows: "Ergo ad alium usum effunditur sanguis a corde in pulmones hora ipsa nativitatis et tam copiosus. Item a pulmonibus ad cor non simplex aer, sed mixtus sanguine mittitur per arteriam venosam. Ergo in pulmonibus fit mixtio. Flavus ille color a pulmonibus datur sanguini spirituoso, non a corde. In sinistro cordis ventriculo non est locus capax tantæ et tam copiosæ mixtionis, nec ad flavum elaboratio illa sufficiens. Demum paries ille medicus, cum sit vasorum et facultatum expers, non est aptus ad communicationem et elaborationem illam, licet aliquid resudare possit. Eodem artificio, quo in hepate fit transfusio a vena porta ad venam cavam propter sanguinem, fit etiam in pulmone transfusio a vena arteriosa ad arteriam venosam propter spiritum. Si quis hæc conferat cum iis, quæ scribit Galenus, lib. 6 et 7, de usu partium, veritatem penitus intelliget, ab ipso

Galeno non animadversum." In place of this very interesting extract which immediately follows the part above of his translation, Jenty has added, what I cannot find in Servetus, "So that the whole mixture of fire and blood is made in the lungs, where there is a transfusion out of the arterious vein into the venous artery, which Galen took no notice of." If he meant this concise statement as the translation of the above, or as conveying its just meaning, I think he has acted unfairly toward Servetus. He proceeds thus in his translation of what follows the above. "This vital spirit is transmitted, from the left ventricle of the heart, into the arteries of the whole body; so that the more subtil parts get upwards, where they are yet more refined, especially in the plexus retiformis, which lies in the base of the brain; where, from vital, it begins to become animal, and approaches the proper nature of the animal soul."

He here terminates his quotation, and we find enough in it to perceive, how much is actually stated, of which, without the slightest acknowledgement, Harvey made use! We must, however, conclude with his own remarks, following directly on his translation. "The circulation, says he, of the blood, is a discovery of such importance, that every one who gives the remotest hints of it, has some party to take him by the hand, and canonize him as the first discoverer. Thus Hippocrates, Galen, and a great many more, have had their respective champions, in this particular, who have pronounced boldly, either one way or the other, just as whim and caprice directed them. But as such a turn of mind is a disgrace to philosophy, and a reproach to human nature, whose glory and dignity consist in shaking off prejudice, and adhering inviolably to truth, wherever it can be found; so we will not absolutely pronounce, that Servetus knew the doctrine of the blood's circulation: But it is certain, that the first step made to this noble and useful discovery, was the finding that the whole mass of blood passes through the lungs by the pulmonary artery and vein. Now that Servetus had a pretty distinct idea of this matter, is sufficiently plain, from the foregoing passages: but he talked in too vague and indetermined a manner, to be esteemed a full and uncontested discoverer." In these remarks I entirely coincide, and especially the last part, which, mutatis mutandis, will nearly as well apply to Harvey, who has most certainly in many parts, talked quite as vaguely and indeterminedly as Servetus.

In justice, however, to Servetus, I must repeat the question, why Jenty *has omitted* a part of Servetus' extract, which is absolutely of the highest importance, in supporting his claim. There is a want of candour in this, which is deserving of reproach: but in 1766, or nine years *after* Jenty, appeared the 4to edition of the college to which I have referred; in the life of Harvey thereto attached, p. 15, we find him defended from the prior claims of others, and of Servetus amongst them. Equally with Jenty, the highest injustice is done to Servetus, by omitting parts, that actually tend to illustrate either what follows, or what has preceded: parts again are found at the conclusion, or rather located where its connexion is less im-

portant, than where Servetus had placed it. Some words differ from Servetus, as ejicitur for efficitur, and a few are omitted; the stops differ in several places; and, as I think, alter the meaning. Upon the whole, either accidentally or intentionally, I think Servetus might well apply the old proverb to himself, in his relation to the college, and to Jenty: between two stools he fell to the ground.

In the very admirable *Introduction* to Senac's treatise on the structure of the heart, wherein he gives us a detail of the individuals to whom the discovery of the circulation has been ascribed, we find likewise, p. 77, this notice of Servetus adverted to, and the same imperfect kind of extract is made from his writings. The whole ought to have been given, or none; since one part becomes a strong exemplification of the other, and I am constrained to repeat, that I think that poor Servetus has been most unjustly mutilated by all those who have referred to his writings with the sole view of disparaging him, and sustaining the claims of Harvey.

Stahl, George Ernest.—Born in 1660; among his numerous works are one or two, apparently connected with our subject, but which I have not seen.

"Positiones de mechanisimo motûs progressivi sanguinis, quibus motus tonicus partium porosarum necessitas ad motum sanguinis, lymphæ, seri dirigendum admittendum vel excludendum demonstratur." Halle, 1695.

"Positiones de æstu maris microcosmici s. fluxu et refluxu sanguinis præcipuè in paroxysmo febrili tertianario in sensus incurrente." Halle 1696.

Valla, George, among his writings has a work entitled:

"Nemesii de natura hominis liber è Græco Latinus factus." Lyon, 1538.

As Nemesius is one of those, to whom the discovery of the circulation has been ascribed, it may be proper to notice this edition of his treatise here.

Wale, John de, or Walæus.—Born 1604; his biographer says, that he was one of the first who taught the circulation of the blood; but that he wished to take the honour of the discovery from Harvey, and give it to the ancients. Quere, if this is the fact.

"Epistolæ duæ de motu chyli et sanguinis ad Th. Bartholinum." Leyden, 1641. See these letters in Bartholine's Anatomy.

Verheyen, P.—Professor of anatomy and surgery in the U. of Lovain, in his Supplementum Anatomicum, printed at Brussels, 1710, 4to; that is, about a century after Harvey's promulgation of his views and opinions, thus speaks of Harvey, in reference to the circulation, whilst treating on the subject of generation.

"De ejusmodi ovorum productione singularem fovit opinionem G. Harvæus Medicus Regis Angliæ, homo *ob inventam*, aut saltem (ut quidam volunt) *divulgatam* sanguinis circulationem, toti posteritati colendus." p. 308.

At p. 281, the same anatomist gives us the following ideas on the sub-

ject of nutrition, wherein he advantageously employs both anastomoses, and some sort of pores; yet it is but supposition!

"Ut verò intelligatur qua ratione diversæ sanguinis particulæ cedant inter partes sibi conformes, *supponendum est*, præter communem viam circulationis, quosdam esse poros in arteriis (et vero similiter etiam in quibusdam venis) maximè in illis, quas ob tenuitatem capillares vocant, tantum aliqui quibus sanguinis, particulis transitum concedentes; per quos dum reliquus sanguis ad alia vasa transmigrat, particulæ magis conformes ob transitum faciliorem minusque impeditum divertantur, ac deinde ab aliis subsequentibus inter substantiam partis propellantur." The subject is no doubt, now, perfectly plain, and fully illustrated!

Widelius, Wedel—George Wolfgang—Born in 1645, died in 1721. Among his numerous writings, we find "Dissert. de circulatione sanguinis." Jena, 1696.

# ADDENDA.

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Since bringing to a conclusion the foregoing extracts, additional matter has presented for investigation, which I must not withhold from the reader: yet there must be some limitation, for I believe that scarcely can an author, anterior to the early part of the last century, be perused, without finding food for reflection on the subject of the circulation.

We shall commence with some remarks and observations from the writings of Fallopius. My edition of his work in two vols. fol., was printed at Frankfort. in 1600, that is, twenty-eight years before Harvey's. He was born, according to Vanderlinden, in 1490, and died in 1563, when seventy-three years of age, that is, prior to the birth of Harvey. In chap. xii. p. 128, with a consideration of the veins, (including the arteries,) wherein he points to the different views that had been advanced as to the origin of the proper veins—and agreeing with none of them, he adds, "Quare de principio venarum est instituenda quæstio, scil. de parte aliqua corporis, quæ mereatur dici principium venarum." He considers the idea of the hepatic origin of the veins as altogether invalid, inasmuch as *they*, the veins, are formed *before* the liver. In this, then, he has preceded the observations of Harvey, who cannot claim this as one of his new and unheard of things, although he no where mentions, I believe, that the position is maintained by any who preceded him. Fallopius' words are, p. 129, "Quare quo ad principium generationis dico, epar non esse venarum principium, *quoniam venæ factæ sunt ante jecur*, imo, *ab omnibus concessum est nullum esse viscus* quod sit ante venas, et possit esse generationis principium venarum." If, then, Galen was wrong in this particular, as to the origin of the veins, Harvey, at least, was not the first to notice it.

That the heart was the first part to appear among the solids, seems to have been asserted by Aristotle, (cap. 3 and 4, lib. 3, de Animal., &c.) "Cor est principium caloris et sanguinis; venæ sunt instrumenta deferendi huis utriusque; ergo cor est principium venarum."—"Consentit igitur Galenus cum Aristotele, quod cor sit prima pars quæ apparet inter carnosas," p. 130; and we have already seen that Aristotle, by Harvey's own

admission, *preceded him* in the notice of the primary formation, and intestine movement of the blood itself, if the fact is really as stated.

In the 13th Chapter, Fallopius reviews the reasons, &c., of physicians in opposition to Aristotle on the origin of the veins from the heart, from which something may be gleaned, as to the points connected with a circulation; thus, p. 132, "Si vena esset cordis instrumentum, frustra ageret natura: quoniam fecisset aliquot partes ad usum inutiles: sunt enim tres membranæ quæ trisulcæ vocantur, quot sunt in quolibet animali, et sunt in dextro ventriculo cordis, et factæ sunt, ne sanguis egrediatur ad venas; ergo sanguis, si transfunditur ad venas à corde, istæ tres membranæ erunt inutiles; quia, si cor mittit sanguinem, ipsæ non claudunt cordis orificium." He adds, a little further on, some facts relating to fishes, from which he deduces, "Quod cor non potest esse principium venarum, *nec sanguinis officina*," which most physicians accredited, and which Harvey's superior lights did not overthrow, in his consideration of the subject!

How much better Fallopius conceived of the use of the right ventricle, than Harvey, may be estimated from the following quotation, p. 133: "Ad quartum, dico, quod *si in corde per se* adesset dexter ventriculus, *et non per accidens*, fortasse valeret argumentum: sed cum adsit per accidens, non valet argumentum: sed adest ventriculus dexter *non ratione sui, sed ratione pulmonis:* patet hoc, nam animalia carentia pulmonibus, carent dextro ventriculo; at, habentia pulmones habent dextrum ventriculum etiam;" compare Harvey's ideas, with the above.

Although, perhaps, accompanied by a mistaken explanation, in the following passage, we clearly trace a complete idea of the pulmonary circulation. Yet Harvey gives no notice of it to us! Was Fallopius and his writings unknown to him? He, Fallopius, is speaking, p. 138, of the uses of the diastole of the heart: the second use enumerated, is thus stated (from Galen, lib. 3, de Facult. Natur. cap. 14), as depending on the then prevalent ideas of a horror Vacuæ:

"Ista autem dilatatio alium habet usum, ut scil. etiam sanguinem attrahunt arteriæ, nam cor dilatato ventriculo sinistro *attrahit ex dextro ventriculo sanguinem tenuem;* idem fit in arteriis, *quæ attrahunt ratione vacui* sanguinem spirituosum ab ipsis venis conjunctis."

If Harvey had stated this, the medical reader would have been enraptured; but it is disregarded because proclaimed by Galen so many centuries ago.

In Chapter 16th, when treating on the subject of the pulse, to show that it is not inherent in the vessels, in a manner highly interesting, he refers, p. 143, to Galen's experiment of the tube inserted into an artery, (of which we have already remarked the uncandid account, &c., of Harvey,) which continued to pulsate below, if the artery was not tied upon it—but ceased its pulsation, when compression was made by ligature on the vessel. It seems a favourite record in the writings of those who preceded Harvey—and may deserve a strict repetition in the present day, to determine its real value in settling the opinions on this point. I cannot omit to mention, that

Fallopius has advanced an argument hereon, that I do not recollect elsewhere to have seen, but which is certainly deserving of consideration. It is in p. 145, in the following words. "Tertio, si facultas hæc in arteriis esset insita et ingenita, et non aliud emanans, sequeretur, quod in arteriis inflammatis deberet variare pulsus in frequentia etiam, non solum in magnitudine, quia hujusmodi facultas, si est insita, insita est in forma arteriæ, quæ est ipsa temperatura: at in inflammationibus temperatura patitur, ergo et facultas, unde minor fiet pulsus et rarior, sed hoc non fit in inflammatione; ergo arteria habet facultatem hanc a corde." It seems to me, that scarcely an idea has been advanced by Harvey, that cannot be traced in the writings of his predecessors!

I must not deprive the reader of the pleasure he may derive, from seeing the opinions of Mayow, a man, to whom Beddoes and others have awarded the claim of the discovery of Pneumatic Chemistry; a discovery as great in the physical world, as that of the circulation in the animal. Mayow published his "Tractatus quinque Medico-physici," in 1669, or about forty years after Harvey's work, and twelve after his death. In the Oxford edition of 1674, he speaks in warm terms of Harvey, in his treatises, De Respiratione; so that his testimony is of value in every respect. I shall merely extract, however, one passage from p. 19, of his fourth treatise, "De Motu musculari," as being immediately connected with our subject; in which the inquiry is "Quomodo sanguis per musculos transit." "Quo autem carnis musculosæ structura, et usus magis innotescant, inquiramus breviter, quo *ritu sanguis iter suum per carnem capessit.* Neque enim iis assentiendum esse arbitror, qui *sanguinis extravasationem* (Mayow calls things by their right name!) statuunt; cujus sententiæ præcipua ratio est, quia nulla esset, uti aiunt, partium nutritio, si sanguis intra vasa sua jugiter contineretur; neque enim fluvius pratis adjacentibus quicquam fertilitatis impertit, nisi superatis rivis aquæ fœcundantes iis superfundantur. At *vero sanguinis extravasatio res adeo confusa esse videtur*, ut eandem in accuratissimâ animalium structurâ, ubi singula arte ordineque nunquam satis admirandis componuntur, locum habere vix putandum sit. *Præterea concipere plane nequeo, qui fieri possit, ut sanguis extravasatus minutissima venarum oscula subriet:* Etenim si sanguis per musculi molem diffluerent; videtur quod arteriarum, venarumque ultimæ propagines à sanguine easdem ambiente comprimerentur, ita ut sanguis venarum oscula per compressionem illam occlusa, introire non posset. Ad hæc, sanguinem in musculis extravasatum non esse inde liquet, quod sanguis ad musculum appellens, si musculo infligatur, totus non erumpit; quod tamen contingeret, si sanguis extravasatus per musculi molem diffluerent."

"Circa sanguinis ergo transitum per musculos arbitrari fas sit, *venas, arteriasque capillares per vasa quædam diversi generis conjungi*, ita ut continuus sit inter easdem aliqualis ductus. Nempe existimo arteriarum extremitates in vasa peculiaria desinere, quæ mox ab ortu suo in canales, seu potius vesiculas membranaceas penè infinitas, *hic illic anastomosibus*

*variis conjunctas propagantur ;* vesicularum autem earum propagines varias, tandem in canalem unum coëuntes, in venarum oscula inhiantia terminari. Planè ut sanguinis massa dum per ambages illas tortuosas huc illuc variè pervagatur, extravasationem mentiatur," &c.

From all that is here collected, it is obvious that the circulation was not completed, or understood by Harvey or his successors ; how then can he with any show of reason, be hailed unanimously as its discoverer? Even at the present day, the same uncertainty exists as to the real nature of the case, as is evident in the speculations of Bichat and his followers, who candidly and repeatedly confess, that this intervening communication of arteries and veins is uncertain and obscure. It is not any part of my object, to enter into a consideration of the subject; being only desirous of showing that Harvey has not accomplished it; and that, excepting a few confirmatory experiments, he is really in no wise its discoverer. If riveting together a series of links more or less disunited, but which had long been known before Harvey was born, can entitle him to the proud and enviable distinction; even although it must be admitted that several of those links are yet incomplete; surely science is a bubble, and undeserving of serious attention. Time is too short to admit of our being tickled by hypothetic assertions, which, if plausible or probable, yet want the stamp of perfection, in spite of the learned lucubrations of every physiologist, from the time of Harvey to the present day.

A few lines from the writings of J. Langius, "Epistolarum medicinalium Libri," 12mo. Hanoviæ, 1605; will show the state of things *about the period* of Harvey's investigations. We extract them from p. 33 ; the eighth chapter is thus headed: "Chirurgi quare phlebotomatos sanguinem sorbere cogunt." He thus, after some remarks, apostrophizes the ignorance of surgeons. "O audax Chirurgicorum ignorantia, qui nesciunt, aerem, vitalis et animalis spiritus fomentum, non modo per arterias cerebri, pulmonis et cordis cavernas, sed per occultam quoque inspirationem et poros, universos corporis artus perreptare, qui ob arteriarum cum venis coadstomosim, sanguini quoque permisceretur," etc. By which we may judge, perhaps, that the doctrine of pores was by no means uncommon before Harvey ; and which we have in various places demonstrated.

In 1697, the Opera Medica of Ettmuller, in 3 vols. fol. were published at Frankfort; from which much might be extracted, but I limit myself to a few short quotations, to prove, that even after the days of personal controversy had passed away, and men were fully persuaded of a circulation ; all were not even then disposed to acquiesce in the unqualified award that had been made in Harvey's behalf. We may suppose that Ettmuller, living so long after Harvey, could have no reason for denying or objecting to his claim, save that arising from a conviction of its being unfounded beyond certain limits.

Vol. I. p. 9. "*Harvæus et Conringius* vulgò habentur *primi Inventores* circulationis sanguinis: sed reverà non sunt, et notitiam hujus demum acci-

perunt à *Paulo Sarpa*, Veneto monacho ordinis senitæ, qui reverà *primus et verus inventor* est hujus circulationis."

Id. p. 106. cap. x. "*De sanguinis ex corde ad quasvis partes corporis distributione, distributique usu.*" In this extract we shall discover the little advantage that had been derived to physiology, &c., by the asserted discovery of the Circulation; as well as that "shadows, clouds, and darkness," still shrouded and obscured it, even three-fourths of a century after Harvey had declared it perfect!

"Sanguis et chylus in pulmonibus fermentatus et rarefactus, cor in specie sic dictum, hoc est, sinistrum ejus ventriculum, distendit, qui se contrahit liquorem contentum impetuosè extra se propellit, qui ex corde in arteriam magnam irrumpit, et per hujus ramos adscendentes et descendentes, tanquam canales, in totum corpus, vel nutriendum vel vivificandum, distribuitur usque in minutas arteriolas, seu capillaria vasa, per partium solidarum substantiam dispersa idque solâ quidem cordis vibratione; ita ut omnis, quem medicus in carpo pulsum deprehendit, à sanguine per cordis systolen huc derivato, ac arteriæ latera feriente et distendente, fiat, ex arteriolis iisdem sanguis, qui vel à nutritione vel ab aliorum humorum præparatione et depuratione restitat, intrat capillaria venarum, *partim immediatè per minutas anastomoses* seu inosculationes, *partim mediatè per substantiam seu porulos* partium, per quæ ad majores truncos, hincque per venam cavam, in cor refunditur; qui motus, cùm fiat in circulum, autoribus Harvæo Anglo, et Conringio Germano inventus, dicitur Circularis, cujus centrum est Cor, peripheriam verò constituunt extimæ corporis partes, lineas ex centro ad peripheriam constituunt arteriæ et venæ," etc. Proceeding further in his explanatory remarks, he comes to sundry references, as follows, on the subject of the Circulation.

"Confer. de hoc circulari motu *Walæus* in Epist. ad Bartholin. de chyli et sanguinis motu, qui cum aliis Aristoteli et pluribus antiquioribus cognitum fuisse, p. 773, scribit; item *Charleton* in Œconomia animal. exercit. 6. item *Harvæus* qui tanquam alter Columbus sectionibus et experimentis microcosmum pererrans, hujus circulationem nostro seculo primitus propalavit, Exercit. de motu sanguinis; item *Conring.* de generat. et motu sanguinis, qui etiam per exactum scrutinum invenit motum circularem: revera tamen primus hujus inventi Autor fuit *Paulus Servita*, Religiosus Venetus, vid. Bartholinus in Epistol. cent. 1. Epist. 26. *Highmor.* Disquisit. Anatom. de arteriis cerebri: quidam etiam attribuunt *Columbo Cæsalpino*, utpote qui circulum sanguinis ex dextro cordis ventriculo per pulmones in sinistrum describit l. 5. Quæst. peripat. 5. p. 126.—Alii, quos inter etiam *Walæus* l. a. Hippocrati sanguinis circulationem jam dum notam voluerunt l. de Venis—de Natura humana, et de Alimento," etc.

The parts on which Walæus appears to rest his belief in behalf of Hippocrates, are as follows:

"Crassæ Venæ sibi mutuo alimentum subministrant, internæ externis, vicissimque externæ internis."

"Omnium quæ nutriunt, unum est principium, unusque omnium finis, idemque finis et principium."

"Alimentum in pilos, in ungues, et in extimam superficiem ab internis partibus pervenit: ab externis partibus alimentum, ab externa superficie ad intima pervenit: Confluxio una, conspiratio una, consentientia omnia."

"Veteres unanimi consensu, adds Ettmuller, omnes partes sanguine nutriri et augeri asseruerunt, &c."

On all this we can but remark, every circumstance proves that a credence in a circulation was general amongst the ancients; they perceived its necessity to life, in perhaps every particular that could now be urged, either of respiration, animal heat, or nutrition, in terms as strong, although differing from ours, as founded on philosophical principles no longer tolerated. That the circulation in the exact route, as now delivered by anatomists, differs in some particulars from that advocated by the ancients, cannot be denied; but is it just therefore to abstract even what cannot be disavowed that they knew? Had Hippocrates ever written *expressly* on the circulation, he would have explicitly described it; but in merely mentioning it in the cursory manner that is found in his writings, it is doing him an injustice to deny him any claim to its acquaintance, when we perceive in the few extracts above, such language as no man of sense would employ, who had no definite meaning to attach to them. The whole business is confined to a short argument; all writers nearly agree, that the ancients had a knowledge of, or belief in, some kind of circulation or progressive motion of the blood from the heart, throughout the body; on such belief, they practised blood-letting, and other evacuating modes of treatment, on principles that can be explained in no way but by their credence in a circulation; a knowledge of which is yet denied them. But what say the different writers opposed to their claims, in attributing a discovery of the circulation to Harvey? why, they almost all differ. One tells us that he discovered the valves; he himself, ascribes this to others. He claims, himself, the explanation of their use; I have proved that Piccolhomini preceded him in this respect. Take any point, that is either tangible or intangible, and we find Harvey anticipated by some one, either of ancient or more modern times. Whilst the only real point of controversy might be regarded as that of the mode of intercommunication of arteries and veins, which he attributes to himself; we find that he is not only as defective, in this respect, as those who preceded him; but that he was altogether undecided at times, what plan he should adhere to, and that he died without coming to any thing like proof respecting his vacillating opinions. Let any one recur to the extracts given from the College, and also from Pitcairn, and we shall find that he had not enlightened the obscurity of the subject! Under all these impressions, spread throughout so many volumes, and continued during more than two centuries, it is probable, that some of my readers may feel a little astonished, that so much has been granted to Harvey, on a ground-work so im-

perfect; so little admitted for his predecessors, with a superstructure so imposing!

We have in the 3d Vol. of Ettmuller, p. 1617, a tract entitled "De Chirurgia infusoria." Wherein, long after Harvey's works were printed, and no opposition made to the general doctrines of the circulation; we have the following remarks as to the origin of the doctrine, which will, of course, be tested by the other authorities here assembled for the purpose.

"Notum est, inter nova hujus seculi inventa ferè primaria recensendam esse circulationem sanguinis, cui occasionem dedit inventio valvularum seu ostiolorum in venis, præsertim artuum majoribus. Primus valvularum illarum inventor fuit ineunte anno 1579 G. Fabricius ab Aquapendente, Professor Patavinus, post quem brevi primus Salomo Albertus Wittebergæ per αυτοψίαν confirmavit. Horum consideratione factum dein est, ut postmodum ratiocinando de sanguinis motu mirabili Paulus Sarpa, frater seu monachus Veneta, Aquapendentis familiarissimus, conjecturare primus incipit. E contrà G. Harveius, Anglus, primitus sibi inventionem sanguinis circulationis tribuit, asserens, P. Sarpam omnia ab ipso hausisse, (where has Harvey done this?) et omninò fatendum, Harvæum primum fuisse, qui experimentando negotium hoc manifestavit; Sarpam verò, quod ratiocinando ejusdem vestigia nobis exhibuit; quos insecutus Walæus."

☞ Such then is the uncertainty of the business; such the unsettled nature of every part of Harvey's claim; such the quicksand foundation on which the award has been granted to him! Can it well be doubted, that even admitting his claim to be fully established, yet, that in a variety of its parts confirmation is defective; and that, in fine, if *we are as yet, ignorant* of the real character of the connection between the arteries and veins, whatever may be the probability of our circumstantial evidence, it is but circumstantial, and the circulation remains yet to be established! Harvey therefore, two hundred years ago, could *not have discovered it*, or the shadows, clouds and darkness that still invest it, could not have existence.

But I must quit Ettmuller, and introduce to my readers, the "Opera Omnia" of *Christ. J. Langius*, fol. Leips. 1704. Here, in thesis 17, headed *Circulatio Sanguinis*, p. 110, he thus adverts to its discovery,

"Motum hunc sanguinis descriptum Gulielmo debemus Harvæo, Doctori, Professori et medico regio anglico, qui æterna nominis sui fama in Exercit. Anatom. de motu cordis et sanguinis primus *omnium antiquorum* dogmata de hac materia tradita, non solum falsissima demonstravit, sed quoque, *quo modo sanguis verè in gyrum perpetuo a corde agatur*, docuit." His full acknowledgment of Harvey's claim as "Inventor," cannot then be doubted, and, consequently, what he says further must be regarded as the established principles of the Harveian school. At p. 111, after stating the general outline of the route of circulation of the blood, impelled chiefly by the force of the heart, he thus proceeds, "Transcurrit proinde sanguis hic ex majoribus truncis arteriosis ad minores, *tandemque vascula capillaria, quorum finis in partium poris est.* Sed brevis hic est ejus mora, *quoniam ex his a*

*venis minutissimis promptè rursum absorbetur*, et ad vasa majora ipsosque truncos amplissimos venæ cavæ revehitur, e quibus denuo eadem, uti modo dictum, ratione auriculis infunditur. Accidit autem hic sanguinis refluxus a superioribus partibus ex gravitate naturali, cujus ratione unumquodque fluidum deorsum movetur. Sed ex inferioribus, e quibus perpendiculariter ascendere cogitur, *partim a sanguinis arteriosi affluxu ac pressione* (how this was to happen, with blood thus previously thrown out of the arteries into the porosities of the parts, Langius affords no information,) continua, *partim vero a venarum textura renitente* deducendus est, quibus simul succurrunt valvulæ, cavitates venarum occupantes, quæ obicem ponunt regressum molienti huic fluido."

It would seem that Langius then fully upholds the doctrine of pores, as held forth by Harvey, and opposes anastomoses; and this we shall see still further upheld in p. 114 and 116. In the former page he thus states the *objections* that had been made to this doctrine of pores: "Negatur, says he, ex arteriis per poros partium ad venas abire sanguinem.

1. Quoniam Anastomoses dantur.

2. Quoniam per fibrarum carnearum tubulos ex arteriis movetur ad venarum ostiola, quam hypothesin defendens Steph. Blancardus, Tr. de circulat. sanguinis per tubulos, probat, *a.* ex hæmorrhagiæ defectu, si secundum longitudinem secetur musculus—*b.* ex autopsia per atramentum in arterias injectum, cujus ope non solum connexionem tubulorum horum cum venis, verum quoque valvularum præsentiam in illis posse cognosci docet."

To these, he replies at p. 116.

"1. Anastomoses illas Antiquorum h. e. inosculationes arteriarum et venarum *huc usque nemo anatomicorum vidit aut demonstrare potuit*, ad non-entia ergo confugiunt adversarii.

"2. Admitto hanc doctrinam, quamdiu sanguis circulatur in musculis, (It is not a non-entity in the muscles, then, even if equally invisible or undemonstrable!) ast quoniam etiam transcurrit viscera, glandulas, aliasque partes, in quibus ejusmodi fibræ carneæ non occurrunt, *malui cum aliis* generali pororum partium termino designare *locum illum extravasationis* sanguinis arteriosi quam ante occupat, quàm a venarum ostiolis excipitur."

And again, referring to some objection relative to the venous valves, he says (same page) in order to do it away, "a valvulis, quas intra venas reperire licet, illud adjutorium, quos sibi pollicetur, (J. A. Borelli de motu animal.) ad hanc rem expectari non posse, *cum tantum in ramis, non æque in truncis amplioribus* illæ appareant."

At this part, or Thesis on the circulation, we have some further interesting appurtenances, which I cannot forbear communicating, inasmuch as they have a strong association and connexion with Harvey's works; and elicit somewhat of that, which *he* (H.) has left uncommunicated to the reader. I have already adverted to Harvey's denunciation, in no very measured terms, of the *Momes and Detractors* of his doctrines, whose works, however, *he never read!* Credat Judæus! It no doubt would have been satisfactory to others, not personally engaged in this anatomical and physiological con-

flict, to have known who they were, that so unhandsomely opposed the novelties (new and unheard of) of Harvey; and what they could possibly urge against the doctrine; but all this, so far as Harvey is concerned, is left to mere conjecture; and, but for others, these momes and detractors would have remained in oblivion, which, no doubt, was Harvey's earnest desire. His good friend Langius, however, gives us some little insight, when speaking of the objections that had been made to the doctrine of the circulation, p. iii. obj. 1.

"Negatur," says he, "motus sanguinis circularis *in totum*."

"1. A cunctis Harvæo antiquioribus medicis, utpote qui unanimiter statuunt, ex hepate per venam cavam ad partes vehi sanguinem nutritionis gratia, et per arterias spiritus vitales ad conservationem vitæ.

"2. Post manifestatam jam circulationem negarunt ex professo *quatuor viri sequentes*. (☞ The momes and detractors!)

"*a*. Jacobus Primerosius in animadversionibus ad librum de motu cordis et circul. sang. adversus Harvæum. *Item* in animadvers. in Joh. Walæi disputationem, quam pro circulatione sanguinis Harvejana proposuit. *Item* in Animad. in Theses Heinrici Regii pro circul. sanguinis. *Item* in vindiciis animadversionum contra Regium.

"*b*. Æmilius Parisanus, Nobilium Exercitat. de subtilitate, part. altera. cap. de Cordis et Sanguinis motu adversus Harvæum.

"*c*. Eckardus Leichnerus, Exercit. anti-Harvæana de motu sanguinis.

"*d*. Homobonus Piso, Tr. Ultio Antiquitatis in sanguinis circulatione."

I cannot omit here to mention the *manner* in which the objections to the doctrine of the circulation by Harvey, as above referred to, by Primrose and others, are met by Langius in his response to them, obj. i. 113.

Resp. 1. "Damus lubentissimè veniam priscis medicis, quibus ex seculi infelicitate hic tantum motus sanguinis cognitus fuit.

2. "Quandoquidem quæ Primerosius adversus hunc circularem protulit motum, ex professo examinis incudi subjecit jam olim Heinr. Regius Tr. cui Titulus: ☞*Spongia pro eluendis sordibus* animadversionum Jacob. Primeros. in theses ipsius pro Circul. Sanguin. et Georgius Entius in Apologia pro Circulatione, qua respondetur Æmilio Parisano *cunctaque Doctoris hujus argumenta nullius momenti esse, dudum ostendit;* Leichneri vero et Pisonis rationes, cum, vel omnino repugnent autopsiæ, vel merè sint sophisticæ, minimè videntur prolixam confutationem postulare." Such are the arguments by which an opposition to the new doctrines of a circulation were met and sustained. If the objections by Primrose and others were deemed untenable; surely they required a different treatment to overturn them!

Amongst the objections made, we find one thus headed, p. 112, 7th. "Negatur à sanguinis impulsu arteriarum motum dependere:" and I refer to it merely from its involving the experiment of Galen, before adverted to, of introducing a tube into an artery. "Si arteriæ per medium dissectæ fis-

tulam immittas, et in utroque latere laqueo circumjecto connectas, ultra laqueum non amplius palpitare videbis arteriam, utut sanguis injiciatur." In p. 115, we have the following response,—"Fundamentum dubii hujus est allegatum Galeni experimentum, ad quod solvendum non opus habemus *cum Harvæo Tr. de motu cordis dubitare, num illud in corpore vivo possit adornari; potius negamus illud, utpote falsissimum,* cum contrarium omnino in quovis arteriæ trunco sub ligatura et tubulo h. e. motus satis conspicuus appareat vid. *Raymundi Vieussens* Neurograph. Univers. c. 4. p. m. 23. sequ."

If Langius means hereby to deny the experiment of Galen; he merely does what Harvey did before him; but he appears to have not known, or to have forgotten, that Harvey, after an equally positive denial; in his Exercit. ad Riolan, actually affirms that he himself did perform it! As to the deductions therefrom, I have nothing to remark, as my object is only to show, that some inconsistency is at least apparent, in the subject!

If we take up *Juncker's* Institutiones Physiologiæ et Pathologiæ Medicæ, 12mo. Halæ, 1745, we shall learn something of the sentiments at a later date. At p. 97, we find the following question and reply. "209. An vero sanguis ex *arteriis immediate transit in venas?*

"Sunt qui a *particularibus* venarum et arteriarum *anastomosibus*, in pulmonibus, plexu choroideo, vasis emulgentibus, utero gravido, membro genitali et alibi forte obviis, argumentantur ad *anastomoses universales;* sed *probabilior* (no certainty yet, we perceive, that is, nearly 120 years *after* Harvey's book appeared,) esse videtur *illorum sententia*, qui sanguinem in ipsam *partium porositatem infundi, et ope motus tonici in venas* transprimi contendunt; (!) ita enim non solum corporis nutritio, sed et partium amputationibus discissarum, aut suppuratione absumtarum coalescentia eo melius explicari poterit."

Here we may remark, that Juncker makes the circulation of a *triple* character, in reply to the question, p. 97. "Numne sanguinis circulus per omnia est equalis?" By no means, he replies, "Nam *longissimum* circulum describit sanguis, qui artuum extremitates perfluit; *breviorem*, qui per pulmones pellitur; *brevissimum* vero, qui cordis substantiam ejusque transit auriculas." It will be remembered that Harvey, nor Juncker, nor any other physiologist, have noticed, that there are as many *minor* or particular circulations, as there are organs, to which the blood is merely conveyed by its general and incessant flow. The lungs and heart are precisely on a footing with every other organ in this respect; their functions are distinct; like those of all the rest, each is peculiar in its character, and cannot fully be supplied vicariously. The heart impels the whole circulating mass, like a powerful forcing pump; the other organs are required to separate what is no longer wanted, or effete; hence the various emunctories of urine, sweat, &c.: while some prepare it partially for its renewed arterialization in the lungs, by removing from the *venous mass* itself, a large proportion of materials, (as bile) that would probably frustrate or impede the pulmonary func-

tions; and so far as this is the case, with the ancients, we may well believe the liver to be an important organ in the process of sanguification. *No organ alone* can be so characterized, for *all*, by the removals made by them from the general mass, co-operate in this perpetual renovation of the vital stream! Each organ, therefore, has its respective circle of distribution from the parent stream; like the minor satellites and planets of the greater system of nature, they all contribute to the general harmony of the microcosm of man. And, although of high import, as the source of every stream, thus irrigating as it were, the peculiar organs, on whose perfect functions health and life depend; yet, the *general* circulation may almost be considered, when compared with that of each individual part or organ, as sinking into insignificance! nor will *its sole consideration* elucidate a single point of physiology! In a limited degree, Juncker, indeed, seems to have adopted this view of the subject. "Unde patet, quod quædam sanguinis pars pauciora, alia vero longe plura vasa una quidem vice percurrat. Prius in partibus cordi vicinis fieri apertum est, posterius autem contingit, dum sanguis per tot aortæ ductus in cerebrum penetrat, et duræ meningis sinus ramulosque perreptat, itemque per venæ portæ ramificationes, ventriculum, pancreas, aliaque viscera propellitur," &c. The uses of the circulation, he imperfectly notices in reply to the next question of "Quænam vero usum præbet sanguinis circulatio?" They are, to preserve the fluidity and heat of the blood; to absterge the solids of effete sordes, and to soften the fibres; to cherish every part by its heat; to remove useless matters at all times by the various emunctories of secretion and excretion; to deposite in due amount the requisite nourishment to parts; and restore it when lost by disease. It is conspicuous, however, that the office of the *general* circulation is here promiscuously intermingled with that of each part of its particular destination: and which requires sedulously to be kept in view, if we would desire to comprehend (though imperfectly), the importance of the blood in the animal economy. In this respect, Harvey is greatly deficient, at least in his treatise on the circulation itself; for he does more justice to it, in that on generation. Perhaps I cannot do better than give the next question and reply of Juncker, after more than a century from Harvey—his advocate in the fullest extent, since he thus terminates a reply to a question, of whether the circulation was known to others before him. "Hinc gloria inventionis merito relinquenda erit Guil. Harvejo, Anglo, Jacobi ac Caroli I., Angliæ regum, archiatro quondam atque Collegii medici Londinensis anatomes professore ac præsidi, qui anno 1628, in Exercitationibus anatomicis, de motu cordis et sanguinis, suam hac de re sententiam cum orbe erudito communicavit."

None will, I trust, hesitate in giving its *due weight* to the reply I allude to, to the following question, as containing the *extreme extent* of Harvey's claim: if more can be found, I have not yet, with every care and desire to do him justice, been able to attain it.

"Unde autem probas, dari sanguinis circulationem? p. 99.

"Ex variis quidem Harveji *aliorumque recentiorum* medicorum observationibus et experimentis; præsertim vero ex phænomeno illo satis manifesto, dum in corporibus vivis, vegeto sanguinis motu, et cordis pulsu instructis, *ligata arteria inter cor et sui ligaturam intumescere* cernitur, *detumescere* autem *inter illam ligaturam et extremitates* ramificationum suarum, et per consequens ipsarum partium; unde in aprico est, sanguinis massam per arterias omnino moveri a corde extrorsum, versus partes periphericas. Ex adverso, *venis ita ligatis*, comparuit, quod eædem *inter cor et ligaturam detumescant*, *inter ligaturam autem et extremitates turgefiant*, luculento iterum indicio, quod sanguis per venas a partibus versus cor refluat."

But we have said enough from Juncker; and if we proceed from him, down to the present time, we shall find, that we are no more enlightened now, than by Harvey or Galen, *how the blood actually passes from the arteries to the veins*, whether by pores or anastomoses, or both, or by what other means:—so as, in fact, to compel us to declare, that a circulation, absolutely true and certain, is not more *completely and fully* demonstrated in *all* and *every particular*, than by Galen and others, anterior to Harvey; and we demand not mere assertion or speculative notions, but absolute proof and unequivocal demonstration of the nature of this important link of the circulation; without which, it is utterly inconsistent with truth to say, that its discovery is complete; and still more so, to invest Harvey with honours, to which, I think, we have proved him so little entitled.

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☞ It may not be amiss to advert to a quotation from Fallopius, at p. 208, beginning thus—"Ista autem dilatatio," &c.—as, in referring to it, it is stated that Fallopius has quoted Galen for what is there given. It would appear, therefore, that the whole paragraph was Galen's; whereas it was intended merely to refer to the attractive power with which he invests the arteries. The idea of a vacuum belongs to Fallopius. *Galen's* 14th chapter, adverted to, is headed, "De Vi attractrice Cordis et Arteriarum." His views are, however, connected in his explanation with the difference in the specific gravity, (levity, &c.) of different bodies.

# AN INTRODUCTORY LECTURE,

IN

## VINDICATION OF HIPPOCRATES.

DELIVERED IN THE UNIVERSITY OF PENNSYLVANIA, NOVEMBER 3, 1829,

BY JOHN REDMAN COXE, M. D.

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GENTLEMEN,

NEARLY forty years have elapsed since, under the guidance of my much respected friend and preceptor, the late Professor Rush, I imbibed the highest veneration for the character and writings of the illustrious Hippocrates; the earliest of those medical authorities, whose works have reached us. Participating in the enthusiasm which he bore for that venerable man; you may judge how much I have felt, in preparing a lecture for your consideration; which is intended as a vindication of that great father of medicine, from aspersions thrown upon his fame, by Dr. Rush himself;—and that, at the very moment when, it is evident, he was striving to do honour to his memory!

Although it may unquestionably be deemed hazardous to attempt the refutation of an individual, who so long, and so deservedly, was esteemed the pride and ornament of this University; yet, a sense of duty to him, to Hippocrates, whom he so fondly cherished, to you, and to myself; compels me, in the confidence of truth, to strive to counteract impressions given to the world, which I deem to be unfounded; and therefore derogatory to the standing which Hippocrates has always maintained, wherever medical literature has found a footing.

Had Dr. Rush confined his assertions to his Class alone, I should probably not have ventured on opposing his opinions; but, in giving them publicity, through the medium of the press; they seem to call for some remarks in *this place*; from whence, more than twenty years ago they were promulgated: nor can I doubt, that were he still alive, he would with kindness regard my feeble attempt to rescue the character of one, whom he delighted to honour, from the undeserved, *and undesigned* aspersions, to which his pen has given origin.

Although, for myself, I consider Galen as infinitely superior to his great predecessor; whose writings have been so ably commented on by him; yet, I am not the less desirous of doing justice to the medical character of a man, who has commanded the suffrages of every age! In reference therefore to the point in question, all who have read the interesting series of INTRODUCTORY LECTURES published by Dr. Rush in 1811, must well remember, that one of them is "*On the opinions and mode of practice of Hippocrates.*" The panegyric of the Professor, on the illustrious subject of that Discourse, must meet the decided approbation of every one, who is at all conversant with his writings! And, perhaps, no stronger evidence is requisite, in order to establish the high opinion entertained of Hippocrates, by Dr. Rush himself; than the declaration made by him in the lecture alluded to; that he had translated the Aphorisms of Hippocrates into English, before he was twenty years of age!

Now, it is this very high respect, so deservedly entertained, and so publicly avowed to his class, in 1806; that renders every error or misrepresentation of importance, that has proceeded from his pen. The opinions of those, who may deem the writings of Hippocrates to be useless, or even unworthy of perusal, whilst yet they join in senseless acclamations to his worth, and in tributes of respect to his memory; these, I repeat, may justly be regarded as of little consequence: not so, as it respects the judgment awarded by men like Rush; learned alike, in present and in ancient science; *their* opinions require to be most sedulously weighed; and any misconceptions of an author should be severely scrutinized, by an impartial appeal to the only certain mode of deciding on his merits; viz. by a reference to the writings of Hippocrates himself, or of such, as have reached us, under the imposing sanction of his name! If, by such a measure, we shall have reasonable grounds to believe, that from inadvertence, our great and respected preceptor has asserted what is unfounded, or even doubtful; how important must it be, to guard against the chances of injustice to the ancients, *by those*, who quote them only from second-hand authorities, and deem a reference to the original as of no account! If every medical question connected with the *Ancients* was thus to be tested by the touchstone of investigation; more justice would be awarded to them, than is usual in the present day; and a salutary scepticism towards contemporary writers, might probably be less requisite, than, in my opinion, is now absolutely proper! Although, indeed, few will take the trouble to pore over the pages of the ancient authors; I am not the less persuaded, that much is lost, *both of pleasure and instruction*, in so completely relinquishing such original merit, for the meretricious trappings that have been, too obviously, on many occasions, derived from *their* extensive but neglected wardrobe.

The following quotation will, at once, unfold the assertions from Dr. Rush's lecture, which I desire to controvert; and I can only regret, that they are not of so tangible a form, as to have at once enabled me to place my finger on the spot:—in other words, that Dr. Rush has altogether

omitted, either by text or note, to specify the particular parts of the writings of Hippocrates, from which his unqualified assertions are deduced!

"I shall begin this part of our lecture, (says the Doctor) by taking notice of his *ignorance* in anatomy. *He confounds the offices of the arteries and veins; and afterwards, the offices of both, with the nerves and ureters. He ascribes the same uses to nerves, tendons, and ligaments. He mentions but one muscle, and that is the Psoas, in the human body. He discovers no knowledge of the circulation of the blood; his account of the heart, the brain, the senses, the intestines and organs of generation, is so replete with absurdities, that it would be disgusting to mention them.*"

Dealing thus, altogether in generals; you will readily perceive that it becomes a task of infinite difficulty to disprove assertions, that have no reference to any particular part of the writings of Hippocrates, but which yet comprehend them all, in one fell swoop! What edition of his works, Dr. Rush may have consulted, I know not! I think however it could neither have been those of Foesius nor Duretus; both, amongst his most accurate translators and commentators! And, it is from them chiefly I shall attempt to disprove, though in the same general manner, those assertions; thereby vindicating, I trust, Hippocrates from the charge of undue ignorance: *and ourselves*, from an undue appreciation of his merits, (which is justly chargeable upon us,) if Dr. Rush's assertions could be fully substantiated.

To pursue Dr. Rush's order, we are to notice,

1. The charge of his ignorance in Anatomy!

Although, it is true, that Hippocrates has not written *any express* work on anatomy, by which we might form an estimate of his extent of knowledge in this especial branch; yet he has not left us ignorant of much important matter, spread throughout his numerous treatises; more especially in those *on the heart; on the glands; on the nature of man, and his parts; on bandages; fractures and luxations*, and a few more. How inadequate a test this may be of his proficiency may, perhaps, be appreciated; by considering how far, even Dr. Rush's attainments in that branch, could be fairly estimated, by what is incidentally spread throughout his own multifarious medical treatises! The writings of Hippocrates can, however, I think, scarcely be consulted, without a tacit acquiescence in the opinion, that whoever wrote them, must have had a competent acquaintance with practical anatomy!

Had Dr. Rush pointed out any *particular* deficiency, and noted the part of Hippocrates, where it was to be found; it is not unlikely that all would perceive a sufficient reason for acquiescence in his opinion! But, may not the same argument, or plan of proceeding, be equally valid against every writer, and even against Dr. Rush himself! In short, are we justified in questioning the general acquaintance of an author, with the science on which he treats; from finding some partial, perhaps unimportant, instances to the contrary! Fair and equitable criticism, would undoubtedly forbid it: and surely, what a sense of justice would condemn as improper, in relation to

our contemporaries; ought at least to prevent its unduly being urged against a writer of more than two thousand years celebrity!

It has been contended with great pertinacity, that Hippocrates and his contemporaries, and so down to Galen; and even those, we may say, for centuries posterior to him, possessed but little acquaintance with practical anatomy! and yet we hesitate not to award to Hippocrates, the distinguished appellation of the Father of Medicine! involving ourselves thereby in a dilemma, from which it may be difficult to escape! For if, as is at present generally admitted, anatomy is the great and legitimate basis of true medical science; how can we reconcile it to truth, to endow a man with an epithet so imposing, if we truly accredit his ignorance of the very foundation of his science! If, on the contrary, the title is appropriate; whilst we still persist in denying his anatomical acquirements; then, the necessary deduction, undoubtedly, must be, that anatomy *is not essential* to the knowledge of medicine!

The vast attainments of Hippocrates have, indeed, been in every age conceded; and I believe it would be a work of infinite difficulty to prove a superiority of therapeutic skill, even in the present age, over that which he possessed! It is true, indeed, that of the cases Hippocrates has recorded in his writings, the greater part proved fatal! What then; are we to deduce from thence, from about one hundred and fifty or two hundred cases of disease transmitted to us by him, that they were all he ever had? Unquestionably he had some particular reason for transmitting these to posterity, rather than others; and he has shown a degree of candour, not often exemplified by his successors, who have chiefly recorded their successful cases, but have left their unsuccessful ones in the tombs of their patients! Hippocrates was not a boaster; had he been so, the practice of nearly three-fourths of a century would have doubtless afforded him ample scope for thousands of successful and remarkable cases! But would they have been more instructive than the few he has left us? if indeed, it may not fairly be questioned, whether they were intended for ought but private reminiscences; or how far they are to be considered as fully developing his practice, or even of proceeding from his pen.

The facility which printing, and the rapidity of travelling by sea or land, *now gives* to the immediate dissemination of the works of the present day; is in the deepest contrast with former times; and places almost instantly on an equality, in every part of the globe, the man who has, perhaps, never expended a serious thought on the subjects of his professional pursuits; and him who has experimentally inquired into, and diligently searched after them! In those distant periods, on the contrary, few probably, whatever might be their personal attainments, from a practice the most extensive, and a life the most prolonged; few possessed the means of extending their information beyond their immediate vicinity; or of transmitting their knowledge to succeeding generations! No periodic journals then existed, which, in the space of two or three months, could convey the local information of

Egypt or Greece, to the utmost bounds of the Hyperborean regions; or reach, in far less time, the *Ultima Thule* of the Poets. Is it probable, however, that the same ardent desire of information; the same appetency of research; did not, *then*, arrest the sedulous inquirer after Truth, in every department of scientific investigation, that is conspicuous in similar characters of the present day? Let none, however, venture to reply to this, who have feared to look into the writings of Hippocrates, of Aristotle, of Galen, and other illustrious ancients; which, fortunately, have been preserved amidst the ravages of time; and stand, like the scattered *Oases* of the desert, inviting still, the weary traveller to revel in the feast they are capable of affording!

Amidst the destruction of ancient libraries, by accident or design, the ignorance of the art of printing, may have for ever obliterated the single manuscript of some philosophic Physician, and thus have left him unknown to us, even by name; and thereby silenced all his pretensions to the esteem of his successors. If we may be permitted to judge of writers now lost to us, (yet possibly the contemporaries of Aristotle, of Hippocrates, of Plato, and of others,) by the pages which *their* more fortunate destiny has preserved from destruction; we may venture to affirm, that Science was as sedulously cultivated as at present, with *means inferior* to those we now enjoy! And this must be considered as giving them a strong claim to our respectful attention; instead of that too common disposition to disparage all their contributions, and to consign them to perpetual obliviscence!

Resting my vindication of the anatomical knowledge of the ancients, *in part*, on the preceding general observations; I shall, I think, render it still more conclusive, by referring you to the pages of the immortal Aristotle, in his instructive and interesting treatises, *de generatione animalium; de historia animalium; et de partibus animalium.* Aristotle lived during the period of Hippocrates, and was possibly acquainted with him. The latter was born about the 80th, the former about the 99th olympiad; so that, although Aristotle mentions Hippocrates, he must have been too young to have been his associate and personal friend. In reading the works alluded to, there is infinitely too much practical information given, to permit a doubt of its being derived from immediate dissections, either by himself or by his predecessors and contemporaries; and that, not merely of animals, as asserted, but of man himself. Permit me to ask the Comparative Anatomist of the present day; whether, with the feeling awakened by an investigation of an inferior animal; he *could stop short* in his inquiries of the nature of man! He could not, I am persuaded, if really anxious in his pursuit of truth: and what he will thus feel to be true, of himself, *individually;* he may readily conclude to be so of hundreds in every age, who have probably never left a vestige of their information behind them. If the few that have reached us, have less perfection in their researches, as recorded, than at present is *the case;* let us remember that the art of injecting the vessels, and microscopic observations being then unknown; a comparatively imperfect

glimpse of the circulation could alone be perceived; and dissection itself could not be so minutely pursued, as it has been since the period of Harvey, of Ruysch, and others; a period scarcely reaching two hundred and fifty years back; but from which, indeed, we may trace the *full* developement of anatomical investigation. To all, however, that has been said, I must add the authority of both Galen and Celsus, men whose names are of *some standing* in medical literature; and whose veracity has, I believe never been questioned! a due attention to what they have said will, I think, amply satisfy you, that anatomy was well understood and pursued by the ancients; and that it was not *comparative* alone. I must refer you to the original for the *full detail* of what I cursorily give from Galen's treatise, "*De anatomicis administrationibus*," (Lib. 2. cap I.) entitled "*Cur ab antiquis anatomica scriptis non tractarentur.*"

His meaning, in a few words, is as follows: That the ancients have written nothing on anatomy, is by no means to be objected to them as a fault; inasmuch, as the subject *was so common and frequent amongst them;* that even children pursued it under their parents' roofs, both by lecture and *practically;* not only physicians, but philosophers studied it; and hence, there was but little apprehension of its being lost to the world. In process of time, however, this freedom of opinion and of inquiry being destroyed, and the exercise of anatomy being restrained; it became gradually less accurately detailed by tradition; and it was requisite to preserve it in commentaries, rather than trust to speculation alone. He adds, nevertheless, that treatises on anatomy were not wanting; and he especially refers to one, written by Diocles, who lived *before* Aristotle; as well as also to others, by both elder and younger members of the profession! At length, says he, the knowledge of anatomy was principally intermingled in books, with the diagnosis, prognosis, and therapæia of diseases, as is the case with Hippocrates; but from the danger that these speculations should perish; both on account of the negligence of the men of his day, as well as because it was no longer taught to youth; he deemed it proper to write commentaries thereon himself.—I might add much more to this, but what is thus cursorily mentioned, is worth a volume of objections to the anatomical knowledge of the ancients; and might well establish the propriety of a translation of the works of Galen! I shall barely notice further incidentally, that Galen unquestionably suspected, if he did not absolutely know, that *sensation and motion* were dependant on *different* nerves! *Idem.* Lib. 3. His words are "Hæc itaque tum in interna manus regione, et pedis parte inferiore, cognovisse convenit, aliaque non pauca circa arterias, venas, nervosque: ac primum, *non ex iisdem nervis*, et sensum, et motum singulis digitis dispensari:" etc. This is in connexion with his relation of a case of considerable interest; at the conclusion of which he adds (and they are not the words of a merely comparative anatomist) "Dies me deficiret, si omnia percensere vellem, quæ id genus juxta pedes manusque; tum in militibus in bello convulneratis, tum hisce gladiatoribus, tum aliis multis

privatis, conspexi accidisse *medicis anatomes imperitis*, per omnia se turpiter in ipsis gerentibus." At the same time, from a knowledge of the accidents from such ignorance in anatomy; he states particularly, that he was accustomed to exhort young men, who pursued dissection (qui ad dissectionem studium incumbunt,) to acquaint themselves in the first place with the anatomy of the most important parts; since they see daily, physicians, who indeed know the number and nature of the membranes of the heart; or the muscles of the tongue, and other things of a like description; yet ignorant of the structure of the external parts, and of the prognosis or cure of their affections;" &c.

Celsus, when speaking of the different sects in medicine, in the excellent preface to his work, and of the importance of experience in addition to reasoning, &c., thus goes on (p. 7, Grieves' translation) "Besides, as pains, and various other disorders, attack the internal parts, they (viz. those who declare for the necessity of a theory in medicine) believe no person can apply proper remedies to those parts which he is ignorant of; and therefore *that it is necessary to dissect dead bodies, and examine their viscera and intestines;* and that Herophilus and Erasistratus had taken far the best method for attaining that knowledge, who procured criminals out of prison, by royal permission, and dissecting them alive, contemplated, even whilst they were breathing, the parts which nature had before concealed; considering their position, colour, figure, size, order, hardness, softness, smoothness, and asperity; also the processes and depressions of each, or what is inserted into, or received by another part; for, say they, when there happens any inward pain, a person cannot discover the seat of that pain, if *he have not learned where every viscus or intestine is situated;* nor can the part which suffers, be cured by one, who does not know what part it is; and that when the viscera happen to be exposed by a wound, if *one is ignorant of the natural colour of each part*, he cannot know what is sound, and what is corrupted; and for that reason is not qualified to cure the corrupted parts; besides, they maintain, that external remedies are applied with much more judgment, when we are acquainted with the situation, figure, and size of the internal parts; and that the same reasoning holds in all the other instances above mentioned." If this be really the fact, certainly dissection has never since been carried to the same extent; and we reasonably suppose, therefore, that the healthy appearance of the different parts examined, was never so well exemplified by the dissections of dead bodies, by other anatomists: this being the case, we may well judge, from Celsus' observations, that the absolute and accurate knowledge of anatomy, (exclusive merely of what injections have elicited) may even have been superior to our own! Proceeding in his observations, to the opposite sect of Empirics strictly so called, or those who denied the utility of reasoning in medicine, and therefore, as it might seem, opponents to anatomical investigation; we find the *practice of dissection* absolutely confirmed by that very opposition! This would be, however, far too long for quotation, although amply deserving of your candid

attention: the opinion of these opponents may, however, be learned, by the concluding observation of Celsus, viz. that for these reasons, "it is not necessary to *lacerate even dead bodies;* which, though not cruel, yet may be shocking to the sight," &c. Celsus, at the termination of the whole preface, (p. 20.) thus speaks his own sentiments; after having illustrated those of the opposing sects, and given a variety of useful and important remarks. "Again, to dissect, (says he,) the bodies of *living men*, is both cruel and superfluous. *But, the dissection of dead subjects, is necessary for learners; for they ought* to know the position and order of the parts, which dead bodies will show better than a living and wounded man. But as for the other things which can only be observed in the living bodies, practice itself will discover them in the cure of the wounded, somewhat more slowly, but with more tenderness."*

I beg you, now, to consider whether the observations I have thus introduced to your notice, (and many of a similar character might be added,) convey the words of persons unacquainted, practically, with *human* anatomy, themselves; or, in the least doubting the acquaintance of their predecessors therewith! If these statements will not satisfy the greatest sceptics, as to the anatomical investigations of antiquity; it is my firm belief that they would not accredit the fact, even from the lips of one of the persons thus dissected, if he were to rise up before them! I may be here permitted to remark, that half a century has scarcely elapsed, since the first School of anatomy was opened in America! If none of the thousands of our medical men ever pursued the subject before that period, how were students to gain the slightest knowledge in this important part of their professional inquiries! for but a very small proportion of them visited the European shores! And yet, no vestige of their inquiries have been left for our benefit. Twenty schools now present themselves in all directions; and facilities of instruction, unknown of old. *Now*, however, *as then*, and as from the beginning of time, unquestionably, the majority of mankind has opposed the practice of anatomy; although, all are so intimately interested in the skill of the surgeon. The conviction of this truth; and the consequent expectation of his having rendered himself fitted for practice; has not removed in this day of philosophic apathy; nor will it probably, in any future period, remove the difficulties that environ its pursuit.

Judge, then, how much greater difficulty must have formerly been superadded, in the superstitious conviction of the ancients, that the *unburied* corpse, consigned its tenant spirit to perpetual wanderings on the Stygian shores! Prejudices, *differing in kind*, but not less powerful than those of old, encircle the breast of the far larger portion of mankind! Yet, all admit

* This, and more that might be added, will demonstrate, I apprehend, that although Galen speaks of dissecting *Apes;* yet he by no means wishes it to be understood, that he limited himself to them; if even, we can suppose a sufficient supply of them for that purpose.

the necessity of dissection; and, that without it, medicine as a science could have no existence! What is true then of us; must be equally so of our medical progenitors, of the days of Hippocrates; and equally so of *his predecessors*, to whom he awards all due credit, in various parts of his works, especially wherein he treats "*de prisca medicina.*" Printing has long rendered facilities to science; and information can no longer be insulated in a small district of the globe. The time required formerly, to convey a solitary truth to some adjoining territory, can now promulgate it throughout the civilized world; discoveries now made, are, by the press, rendered permanent and general in their extension; in former ages, few, and far between, were the promulgations of scientific research! Difficult even, the task of preservation, from the paucity of requisite means; so that tradition was almost the only medium of conveyance; a medium, whose inadequacy, and whose uncertainty, we may well appreciate, by those ages of darkness, from whence tradition has transmitted the idle tales of Demigods, and chimeras, and every species of fantastic folly, that might arise in the fancy of individuals, and be preserved in the annals of the nursery; whilst the more important intellectual banquet, or true historical narration on which they were founded, having no superior source of security than tradition; by degrees became the property of the poet, and dwindled down at last into the legends of fanaticism and credulity! Let us not, whilst thus admitting numerous sources by which the human mind was sunk below its level; let us not suppose that the mighty master-spirits of the age were at all deficient, or, that the powers of their minds were not as active and efficient as in the present day! In all ages, and in every region of the globe; and under every variety of political government; the majority of mankind will ever be the hewers of wood and drawers of water: and, although from the facilities of science, her votaries may now be more numerous; it would be the height of presumption to arrogate to ourselves, that exclusive power of research, &c. whilst the works of Homer, of Aristotle, Plato, Hippocrates, Galen, and many others, hold forth their claims to the highest ranks of literature and science!

I proceed now to consider, in the second place, how far Dr. Rush is correct in his assertion, that Hippocrates has confounded the offices of the *Arteries* and *Veins!*

And here, it might be sufficient, simply to state, that Dr. Rush has not adverted to the difference of meaning formerly applied to these two sets of vessels, if he indeed actually knew it. Before* mentioning this in particular,

* The *arteries and veins*, if confounded by the ancients, yet their distinction was understood by others, as well as physicians. *Aulus Gellius*, in Book 18. chapter 10. shows this. Vide Beloe's transl. vol. 3. p. 352. chap. 10.

"Those persons are mistaken, who imagine, when inquiring into the state of fever, that it is the pulse of the vein, and not of the artery, that they feel.

"When the learned men who were with Taurus had heard the physician speak in so illiterate and improper a manner, calling the *artery the vein*, attributing his error to ignorance, they began to whisper to each other, and to signify disappro-

it may be proper to show, generally, that numerous instances occur in early writers, as Hippocrates, Aristotle, Galen, Celsus and others, not excepting the poets, both of Greece and Rome; proving that great latitude of expression was permitted, and much difference of signification in terms, which now we have restricted to peculiar and definite meanings. The difficulty arising from this source will be readily appreciated, if you will take any word in our own language, and consider its various significations; all of which may be fully and correctly understood by us; but which would become the source of a thousand errors, in the hands of an inadequate foreigner, in translating it into another language: and this, more especially, if the word was spelt alike in every instance; and was similar only in sound. Apply this, now, to the imperfect translator from the Greek into the Latin, French, or English, &c. and see what a fertile source of error and confusion; rendered probably still more so, from the imperfection of a manuscript, or the interpolation of the text, by some commentator according to his own peculiar and sectarian views!

*a.* The word *hemorrhois*, now, specifically applied to an enlargement of, or discharge of blood from, the hæmorrhoidal vessels; was formerly em-

bation by their looks; which, when Taurus observed, turning with great mildness, as his custom was, to the physician, 'We have no doubt, worthy Sir,' he said, 'that you are not ignorant of the distinction between arteries and veins; you know that the veins have no power of moving themselves, and that we only examine them for the purpose of drawing away blood; but that the arteries, by their motion and pulsation, show the state of the health, and the degree of intenseness of fever; but it is easy to see that you spoke rather with a view to accommodate yourself to the common mode of discoursing, than through ignorance of the nature of the vessels, and you are not the only person I have heard speaking so incorrectly, calling the artery the vein;'" &c.

This leads Gellius to investigate the matter; and he tells us, he "remembered to have read on the subject of the veins and arteries nearly to the following purport. A *vein*, called by physicians αγγειον, is a receptacle for the blood, mixed and blended with the vital spirit, in which the blood is in a much greater proportion than the spirit; an *artery*, on the contrary, is a receptacle for the vital spirit blended and mixed with the blood, but in which the spirit predominates. Σφυγμος, pulsation, or the pulse, is the natural and involuntary motion or contraction and dilatation of the heart and arteries; by the ancient Greek writers it is called the systole and diastole of the heart and arteries."

The ancients called all the vessels of the body by the name αγγειον. Machaon applies it to the bag containing the fœtus in utero. Angiologia is that part of anatomy that describes the vessels, veins, arteries, lymphatics, lacteals, &c. Beloe, 354. Blood-vessels were originally called by one name (veins, φλεβι); Artery signified the Aspera arteria or windpipe; observing at length that some vessels had a motion or pulsation, and others not, they supposed those endowed with motion to be filled with spirit or air, which it was thought they received from the lungs, and them they called arteries. Those without motion, and carrying blood, were called veins.

ployed, generically, for *any* species of hæmorrhage. (Duretus, in Coacas. et De morb. mulier. p. 463, 464, § 19.) "Mulieribus notæ graviditatis fluxiones aphthiferæ dolorificæ. Has pessimè habet hæmorrhois." On which, Duretus clearly shows, that the word hæmorrhois is, by Aristotle, (de generatione Animal. cap. 19, lib. 1.) used generically, of *any* hæmorrhage (αιμα & ῥεω). It seems to have been applied by some writers (αιμοῤῥοος) to even signify a female in the period of menstruation: and Hippocrates, on more than one occasion, has applied it, particularly, to an hæmorrhage from the lungs; whilst the word hæmorrhagia (*general* with us) seems to have been often used in an absolute manner, to designate our Epistaxis, or bleeding from the nose. Duretus, Coac. p. 217, 561.

*b. Stomach*, with us, implies anatomically the *whole* of the viscus that receives the food for digestion; but it was formerly, strictly appropriated to the cardia, or upper orifice of the stomach; or even for the gullet, or œsophagus itself, although occasionally used as we employ it. Hippocrates says, (de carnibus, see Fœsius. p. 249, l. 27.) και η Φαρυνξ, και ο στομαχος, και η γαστηρ, και τα εντερα; ad eandem omnino rationem, fauces, *gula*, venter et intestina, &c. idem (Fœs. 274, de Oss. natura) "per fauces et gulam (στομα Os & αγχω coarcto) ☞ Φαρυγγος και στομαχου. See also, De corporis resectione, 916, where œsophagus and stomach are used as we mean them. Hippocrates seems to have used the word στομαχος in his treatise de superfœtatione, to imply the os uteri.* The word gaster in Greek, in Latin ventriculus, is chiefly employed to mean, the stomach itself. (Duretus in Coac. p. 487, Tract. 4, de excrementis. "Sed aliunde profectam, ubi regnum est bilis, quæ illinc prorepit ad fundum Ventriculi, et inde ad Stomachum, quinetiam omnes ideæ proritati ventriculi atque stomachi, &c. Celsus employs the word stomachus pro Gutture, Lib. 4, c. 1. p. 182. Amst. ed. of 1713.

*c.* Cardia, as stated above, implies the upper orifice of the stomach; it

* Gellius, book 17. cap. xi. p. 293, Beloe's translation. "Plutarch in his Symposiacs defends the opinion of Plato, relative to the structure of the œsophagus and windpipe and against Erasistratus, on the authority of Hippocrates." ☞ Erasistratus was right, for Hippocrates and others supposed a part of the drink descends into the lungs, to moisten and support them.

"*Œsophagus*; the word στομαχος, whence the Latin stomachus, is used by old Greek writers for any *narrow passage or channel* leading to a cavity. Hippocrates calls the *neck of the bladder and of the uterus*, stomachos, though now confined to the œsophagus or gullet which leads from the mouth to the *ventriculus* or stomach properly so called."

Book 19. ch. 6. 378. On shame producing blushing, whilst fear blanches the cheeks, from the problems of Aristotle. Macrobius, book vii. ch. 11. Gellius repeats from Aristotle, "Is it because, in people ashamed, the blood *flows from the heart to all parts* of the body, so as to stop upon the surface; but in people afraid, it rushes *from all quarters* toward the heart.

☞ Some ideas of a circulation here seem apparent.

means the *heart* in Greek, and yet from it we have many compound medical words, all connected with the stomach, and having no reference to the heart itself, as Cardialgia, Cardiagmos.

*d.* The word *Uterus*, now specifically appropriated to an organ peculiar to the female; was formerly, not uncommonly applied to signify the cavity of the abdomen,* which indeed its etymology will justify (☞ uter vel uterus, uteri, probably derived from uter, utris, meaning pellis, or a skin or bottle, in which wine or oil were kept.† In short, so numerous are the instances of a variation in ancient and modern nomenclature, whether arising from wrong translations, imperfect copies of original manuscripts, or total misconception of the determinate meaning of the word in the place employed; that, certainly, if this be unattended to, we shall be liable to the grossest mistakes, as to the real state of ancient medical writings, and probably, as in the present case, do great injustice to a man, whom we otherwise wish to honour.

With these preliminary observations, we are probably now better prepared to estimate the terms of Artery and Vein, whose offices are said by

* Uterus, pro abdomine, Celsus, p. 183. Amst. 1713. "At sub corde atque pulmone, transversum ex valida membrana septum est, quod à præcordiis *uterum* diducit," &c.

† The variation of meaning, in words, may be well exemplified in a few references to the word Uterus. The Greek terms for the uterus, or womb, appear to be, Μητρα, hence matrix.—Νηδις, venter, vel receptaculum, (Old Lexicon,) "apud Hippoc., significat *omnem cavitatem* atque conceptaculum quo humor alendis partibus idoneus continetur. Item uterus, ut et Lat. alvus pro utero sumitur." Δελφυς, vulva, uterus. Dictus uterus, quod in eo, tanquam in utre quodam fœtus contineatur, (Plaut. in Aulul.) hinc uterum ferre dicuntur, quæ gravidæ sunt, εν γαστρι εχειν. It is also called υστερα, from υστερος, υστρος, venter, posterior; hence, ustera vulva vel uterus, quod extremum locum inter viscera obtineat. *Crispinus Lexic.* That uterus and venter are reciprocally taken for each other, may be shown from Virgil and from Ovid. Thus, the former, mentioning the *belly* of the wooden horse, as filled with the Greeks, says:—

—"*Uterumque armato milite complent.*"—Æn. 2, l. 20.

"Inclusos *utero* Danaos, &c."—Id. l. 558. See also l. 258.

Whilst the latter, (Met. lib. 10, l. 505,) says:—

"*Media gravidus tumet arbore venter.*"

And in Met. 11, l. 311:—

"*Ut sua maturus complevit tempora venter.*"

Vulva, whose present meaning seems rather to apply to the pudendum or foramen majus, or even perhaps the labia externa, seems formerly to have implied the womb itself; as in Aristotle, de Generat. Animal. lib. 1, cap. 12: and as we learn from the line of the poet, commemorating the delicacy of the gravid uterus as a bon bouche, where he exclaims,

"Nil melius turdo, nil *vulva* pulchrius ampla." Hor. Ep. lib. 1. Ep. 15. l. 41.

And Celsus employs the word in various places, synonymously with uterus. (Lib. 5, l. 26, p. 286. Grieve, p. 274.)

Dr. Rush to have been confounded by Hippocrates. The Greek word, signifying a vein, is φλεψ or φλεβη, from whence our term of phlebotomy. Now it is indisputable, that this same word implied, not only the veins of the body, but also the veins of metal in the earth; and the strong fibres or ribs of leaves; so that it might be thus viewed generically. But what is more to our purpose, the word φλεψ implied a *vessel* of any kind, (Fœsius, p. 86, note F.) or generally, vessels, as will presently appear. Now, although in the writings of Hippocrates, the word arteria is not uncommon, yet, if used *without an adjunct*, it more particularly seems to mean the trachea arteria, or windpipe. This adjunct renders the case specific; as arteria aorta. In like manner, when phlebs is used for artery, it is discriminated by the adjunct of *pulsating;* and in the writings of Hippocrates and Aristotle, and almost every ancient author, the generic term of vein or vessel was subdivided into *venæ pulsantes*, or arteries; and the *venæ non-pulsantes*, or veins proper. This distinction was so universal, that it reached to the time of Galen; and we might even say to the assumed period of the great discovery of the real character of the circulation of the blood, by Harvey. Galen, (de Caus. Morb. c. 3. et de Anatomicis;) makes the artery to differ from a vein, "quod vena sit vas sanguinem continens, *non pulsans;* sed arteria *est vas pulsans.*" Again, in his book, (de Corporis Temperatura, Bas. Ed. 1536, fol. p. 12, l. 25,) in explaining Hippoc. in 2d or 6th Epidemics, when he speaks of the vein (φλεψ) in the cubit, *pulsating;* he says, that here, Hippocrates means the artery; adding, "Venas etenim et arterias veteres vocabant, ut sæpius annotavimus."*

* ARTERIA.—Vide *Bas. Fabri.* Thesaurus Erudit. Scholast. Ed. Lips. 1696.

Αρτερια. *Spiritus Semita.—Pliny*, lib. 2, cap. 37, sub fin.

*Prudentius*, in Hymno ante Cibum, calls it, *Vena* abdita corde.

*References* (*Seneca*, lib. 3, Nat. Quæstiones, cap. 15; *Pliny*, lib. 2, cap. 36; *Gellius*, lib. 18, cap. 10.) to show that a distinction is made between the artery and vein; which distinction not being strictly attended to by authors, vein is often put for artery, of which numerous examples are shown by Marsilius Cognatus, a physician of Verona. Observat. lib. 1, cap. 6.

By the term *artery*, the two passages to the stomach and lungs from the fauces, have been called; (Gula, Pliny:) also, fistula cibalis, and f. spiritualis; (Lactantius, de Opific. Dei, cap. 2:) principally confined, however, to the windpipe, as, arteria, or asp. art. or τραχεια αρτερια; (*Cicero*, lib. 2, de Natura Deorum, c. 54; *Plin.* lib. 2, c. 37; *Gellius*, lib. 17, cap. 11:) asperiora arteria, (*Lucretius*, lib. 4, v. 532:) arteria, vena vitalis, *vet. Gloss.*

Calepini. Ang. An arteriee or vayne, wherein vitale spirite miyed with bloode doth runne in te bodie.

VENA. *Faber.* Thes. Erud., &c. p. 2439-40, Leips. 1696.

Rivum sanguinis.—Plin.

Sanguis per venas in omne corpus diffunditur, et spiritus per arterias.—Cicero de Nat. Deor. cap. 55.

Difference between art. and veins, Gellius, lib. 18, c. 10, from medical books is

Celsus uses the word *generically* in many places; and hence *Grieves*, in his translation of that author, takes an early opportunity, (p. 5, Preface, Lond. Ed. 8vo. 1756,) to point out the fact. "*Vessels*, (says he in a note,) in the original, vena, which is used by our author as a general term for *arteries and veins*. In this place (he adds) it is evident he means arteries."— "And he often speaks of the motion of the veins, where, it is plain, he intends the pulsation of the arteries. Arteria, he uses to signify the windpipe, and also the sanguiferous arteries, as in ch. 1, of B. 4."

The term arteria, taken alone, implied as I have said, for the most part, the trachea arteria, or windpipe; strictly and correctly meaning, a receptacle or passage for the air or spirits. Now the arteries being generally found empty after death; they were imagined, principally to convey the animal spirits, commingled with a portion of the blood. According to Duretus, (p. 427, l. 39,) the focus, or centre of heat was placed in the heart by the ancients; and the air was supposed to be drawn into the lungs, in order to moderate it; hence, arteria, originally and truly, was applied to the windpipe alone; and as the arteries, or *venæ pulsantes*, were presumed to convey the vital spirit, or air; they likewise, ultimately obtained the name of arteries. The etymology of the word, is from Παρα το αερα τηρειν, that is, aerem ducere, or attollere. In referring to Aristotle, we shall find him (lib. 3, cap. 3, de partibus animalium,) using the word arteria for windpipe; and scarcely in reference to any other part. In his treatise "de Corde," he tells us, that two *veins* (meaning the aorta and vena cava,) arise from the heart, because of the double nature of the blood; and he elsewhere adds, that blood is no where found *out of a vein*. Whilst then, we find numerous instances of a distinction drawn by the ancients between the veins and arte-

thus given. Ut *venas* dicat "Conceptacula sanguinis mixti confusique cum spiritu naturali, in quibus plus sanguinis, minus spiritus sit: *arterias* conceptacula spiritus misti confusique cum sanguine, in quibus plus spiritus, minus sanguinis sit. *Venas* itaque suâpte vi immobiles esse et sanguinis tantum demittendi gratia explorari: *arterias* autem motu atque pulsu suo habitum et modum febrium demonstrare.

It was not uncommon in exploring the pulse, to name the vein instead of the artery, both among the Greeks and Romans—Vide *Persius*, Sat. 3. v. 107; (also *v.* 91, of Arterial Pulsation ☞ currere venas.) *Seneca*, Epist. 22. So also Pliny, Pulsus Venarum, lib. 2, c. 97: and percussum Venarum, lib. 7, c. 51. *Val. Max.* lib. 5. c. 7, v. 91.

*Aristotle*, Hist. Animal. 3, cap. 4, calls the aorta, vena *minor;* and the cava, v. major. P. 140, principium venarum (φλεβων) cor est. P. 134, pulsantes venæ et non pulsantes, de part. animal., 2, c. 9, &c.

Κοιλη φλεβες (*hollow* veins), Hippoc. de Carnibus Fœs. 250. So called by him and others, to distinguish them from the veins of metal, or the ribs of leaves, which are solid, yet called phlebes.

See even Harvey as to the ancient nomenclature of veins for arteries. P. 23, Anat. Exercit. on the motion of the heart, ch. 3, refers to Aristotle, de Animal., c. 9, de Respirat., and also to Galen.

ries; we also find sufficient to satisfy us, that they looked upon them as not very dissimilar. And accordingly Aristotle, in one place, says, that the *vein going to the lungs*, is arterial; and the artery is veinal; an expression strongly implying some suspicion of the lesser, or pulmonary circulation. It would appear clearly, I think, that the term vein, or canal, was applied to the vascular system generally, before the introduction of that of artery. The aorta was usually called aorta arteria, and probably was derived from the Greek word, αος, signifying spirit, and in so far, analogous to arteria; for according to Fœsius, (*de locis in homine*, p. 415, l. 19,) the word aorta is employed by Hippocrates for the aspera arteria; and from what Duretus says, (præn. Coac. Gen. Ed. fol. 1665, p. 272, and note, p. 275,) aorta (αορται), appears also to have meant *the substance of the lungs*, or rather the bronchia pulmonum or air-vessels, "the substance of the lungs, says he, besides its fleshy, spongy part, consists of a triple union of the arteria venosa, the vena arteriosa, and the aspera arteria." In his chap. 17, de Corde, Aristotle points out that the blood is contained in the veins, and not in the lungs; by which he appears to draw the line between the parenchyma and the vascular portion. "Et cum pulmo *non intra se, sed in venas* contineat sanguinem," &c. On this subject we may likewise consult Galen as to αορτη, in his books, de Dissectione Arteriarum, and de Usu Partium.—Homer uses the term; and Duretus says, that according to some, it is by no means improbable, that aorta was, in fact, a common name for *all* arteries; that is, *pulsating veins*, or vessels conveying the vital spirits. We may add, that Aristotle even pretends to point out a difference between the veinous and arterial vessels; viz., that the pulsating veins (arteries) have *two coats*, and the non-pulsating, *only one*; (Corol. de Anat. Ven. non-puls. et puls. 390.) Cicero, in his treatise, de Natura Deorum, conforms to the received opinion of the different functions of the arteries and veins, when he says, "Sanguis per venas, et spiritus per arterias." And the poets, as Ovid and others, assuredly use the word vein generically for blood-vessels.*

But although Hippocrates, in common with his contemporaries and successors, employed these terms indiscriminately, (or rather, I should say, that of vein for artery, but never the reverse;) yet it appears equally certain that he did not confound them, as is evident from several of his writings: thus, (de Carnibus, Fœs. 250, l. 9 to 30,) in speaking of the vessels arising from the heart, "There go," says he, "from the heart, two hollow veins; one of which is called the artery, the other, the vena cava. The artery is hotter than the vena cava, and distributes the spirits; besides those veins, there are others," &c. "In a word, from the V. C. and from the artery, originate all the vessels (φλεβες) of the body." And in another treatise, (de Alimento, Fœs. 382,) he expressly derives the veins from the liver, the arteries from the heart: "Venarum origo tanquam radix, hepar est; et

* Metamorph. lib. 2, l. 824; 6, l. 307; 7, l. 291; 10, l. 289.

arteriarum, Cor. Ex his, per omnia sanguis et spiritus pervagatur, calorque per hæc permeat. Facultas una et non una, ex qua hæc omnia, et ab his diversa administrantur." Now here, the words φλεβων and αρτεριων, are respectively employed; and it would seem adequate to settle the question.

If we look into Aristotle, (de Hist. Anim. lib. 1, cap. 16,) we find he speaks of the aorta as a vein. "Atque ea singulari arteria partes in utrumque pulmonis latus duæ dependent. Venæ quoque majori, ac alteri, cui nomen est aortæ, pulmo connectitur. Spiritus vero, quoties inflatur arteria, cava subit pulmonis."

Again, (in his *lib.* 3, *idem*, cap. 2 & 3,) he takes up the consideration of the different opinions, "*de sanguine et venis*," of *Syennesis*, Med. Cret.; and *Diogenes* Appolloniati. From which it would appear, that even then, the nature of the blood and the origin of the veins had already become a subject of dispute among authors; and that Aristotle was desirous of rectifying their errors. The nature of the principal *veins* being obscured by their collapse by death, Aristotle opposes the opinions of the above named physicians, and then takes up those of Polybius, (probably the son-in-law of Hippocrates,) and endeavours to refute them. Polybius appears to have maintained (chap. 3,) the existence of four pair of veins, the *first of which* (*a Sincipite ortum*,) pass down, &c., whilst other writers seem to have derived *the whole* from the head and brain; a position asserted by Aristotle to be erroneous; and he then proceeds to give his own views of the subject.

"Two *veins*," says he, "are found in the thorax, opposite the spine; a larger and a smaller. The latter, called the *aorta* by some. Both arise from the heart. The aorta, although smaller, being much the strongest. They end in branches that are lost to the sight, from their minuteness." And he then states their subdivisions, in this and the 4th chapter: in the which, (de Partibus Animal. lib. 2, ch. 4,) it is said, that in animals respiring, *two kinds of veins* appear, viz., pulsating and non-pulsating. His commentator adds, that Galen thinks the pulsating veins appropriated to convey the spirit, which carries the heat with it, though he admits they also convey blood; yet that the non-pulsating veins are more adapted to convey this last; still the former carry the spirit to the members; and inasmuch as the blood is the aliment of the members, it was essential that this fluid should be devoid of sensibility. On this point, in ch. 10, *he* says, "the power of feeling is not given to any part devoid of blood; nor is it appropriate to the blood itself; but it afforded it to the parts proceeding from it: hence, no part devoid of blood, in those who have blood, can feel, neither the blood itself; because! it is no part of the animal! Those parts only are sensible that have blood."

Inattention to what I have thus largely dwelt upon, has I think, unquestionably, misled Dr. Rush, and probably many others; who could not, with such ample testimony before them, have on this point aspersed the credit of Hippocrates. Independently of the word phlebs being used for both artery and vein, I have stated that it was also employed to designate *every*

*different species of vessel;* and hence we find Hippocrates, (*de Nat. Ossium*, Fœs. 274,) using φλεβες to characterize the canals leading from the vesiculæ seminales to the termination in the urethra;* and which may possibly serve to explain another part of Dr. Rush's assertions, viz., that Hippocrates confounded the offices of the arteries and veins with the ureters; or perhaps, where speaking of the kidneys, he says, "They have some resemblance to the heart, and like it, have cavities, &c., from which proceed a *vein* to the bladder." The word phlebs, here, must necessarily imply the hollow duct, or ureter; for Celsus, (lib. 4, c. 1,) without circumlocution, calls the ureters, veins; no doubt, meaning thereby, canals or ducts or passages or vessels. "A renibus singulæ venæ, colore albæ, ad vesicam feruntur: αρτηρας Græci vocant, quod per eas inde descendentem urinam in vesicam distillare concipiunt." And hence we again infer, that this term was by no means so restricted by the ancients, when applied to the animal economy, as it is at present. If further proof is wanting, even of its application to the ureter, it may be found, I think, in the following, from Aristotle, (Hist. Animal. cap. 18,) when speaking of the kidneys, "Pertinent ad renes meatus, tam *ex vena majori, quam ex aorta*," &c. "Habent igitur, sinum exiguum, à quo *meatus duo insignes ad vesicam* deveniunt. Alii et ex aorta frequentes, ac validi eadem pertendunt. Ex medio autem renum singulorum *venæ singulæ* cavæ, nervosæ dependent, spinam prætereuntes ipsam angusto itinere," &c. Indeed, even so late as Blancard, (vide Med. Lex.) we find it stated, as if not then obsolete, that "arteriæ aliquando pro venis sumunter:" whilst under the word *nervus*, he says, "Nervus, tendo, et ligamentum malè a chirurgis confunduntur." Surely, if this loose nomenclature was common so lately as in the days of Blancard, almost, we may say, a contemporary; it does not follow, that the anatomists of his time were unacquainted with their distinction of uses, &c.! and if we cannot venture to charge them with ignorance, although guilty of calling parts by other names than are now familiar; why should Hippocrates and his contemporaries be

* As some persons appear to have thought that the word φλεβες, here used, means rather the venæ spermaticæ; I give, both the original, and the Latin of Fœsius.

"Εχ δε αυτοις φλεβες εχατερωθεν τη ουρητηρος ες τον αιδοιον τεινουσι."—Hipp.

"Ex his autem locis venæ ab utraque meatus urinarii parte in pudendum feruntur."—Fœs.

*Galen*, (de Anatomicis Administrationibus, lib. 6, ch. 13,) tells us, that an useless dispute had been maintained by the anatomists, as to the name of the ureter, and whether it were more appropriate to call it an artery or vein.

This subject appears to be renewed in Galen's treatise "de Naturalibus facultatibus." Thus, when relating an experiment of tying up the ureters, intended to disprove the opinion of Asclepiades, relative to the passage of drinks to the bladder; the explanation he gives of the ureters, throws considerable light upon the ancient views of different tubes in the body; which, although intended for different purposes, had yet the common appellation, of (φλεβες,) phlebs or vein, given to them.

more hardly dealt with? Such inaccuracies, if they may be so deemed, may require elucidation; but it assuredly is beyond the boundary of just and defensible criticism, to make them the foundation of so strong a charge against so deservedly eminent a writer; and more so, when we consider this charge, as not even pointing to any one solitary passage of his works, by which we might ascertain the correctness of so sweeping a denunciation!

After all that has been urged, let me still be permitted to ask, what it is that Dr. Rush intrinsically means, when he asserts that Hippocrates "*confounds the offices of the arteries and veins?*" Is there, in point of fact, any distinction between them? are they not both, at least as to their larger branches, to be considered as merely channels, formed to serve a purpose, that in no other way could be so well accomplished? viz., that of conveying a mobile living fluid to and from every part, where nourishment and secretion are required; If it be objected that the arteries and arterial blood, alone, are intended for this purpose; I ask, is not the *vena porta* as important in its *secretory functions* as any artery in the body? or, may I not rather ask, that inasmuch as the functions of life are principally connected with the minute extremities of the vascular and nervous systems; extremities, so exquisitely small, as to baffle even microscopic aid, and leave us in the uncertain fields of speculation! whether, we have any absolute and well-grounded knowledge of the real operation, the ultimate functions of those delicate parts? if this be so, how can we in any certainty declare their offices different! or why has Dr. Rush left this unexplained? As mere vessels of conveyance, I know of none that possibly can exist, except, perhaps, the valves in the veins;—as organs of secretion in their ultimate extremities, both subserve the intention, for especial purposes! Is indeed our knowledge so accurately sustained, as to enable us to affirm that we are more fully masters of the *process* of sanguification, or nutrition than Hippocrates or Galen? Are we prepared definitively to assert, that those great men were wrong in considering the liver to possess a prominent part in this extraordinary process of sanguification? or that the great and principal vessels of the body (such as do not require the aid of injection to demonstrate) were not fully known and appreciated? as we know was the fact with the anastomosis of the vessels.*

It is here, you will perceive, that the ignorance ascribed to Hippocrates, as to the Circulation, would appropriately be considered; but I have already anticipated it in a great measure, in connexion with the preceding part. I shall therefore only request you *to remember*, that, admitting our present views of that important function to be perfect and complete; it is scarcely two hundred years since the sagacity of Harvey, assisted by the collateral

* Hippoc. de locis in Homine. (Fœs. p. 409.) "Hæ autem omnes venæ inter se communicant, et mutuo confluunt." And indeed, we may add, that the sympathy of the breast and genitals, is ascribed, in a great degree, to the anastomosis of the mammary and epigastric arteries.

aid of microscopes and injections; perfected, what was suspected long before him; and although correct in some parts, his explanation of other parts, (Exerc. ch. 3. p. 24.) would not be more satisfactory to the numerous physiological inquirers of the present day, than that of Hippocrates or Galen. Surely, if our medical ancestors of only two centuries past are not blameable for their ignorance in respect to the circulation; it can scarcely be deemed just to asperse the character of a man who lived twenty centuries ago, for a deficiency in the same particular. If he did not comprehend the circulation *as now* taught, he yet sufficiently appreciated the high importance of the blood; perhaps he even considered it more highly, than at least, the *exclusive* Solidists can be supposed to do, conformably to their contracted opinions; for he regards it as one of the four important humours of the body, on whose changes, the operations of disease and health do in a great degree depend. That he admitted of its motion, however, there cannot be a reasonable doubt, although its regular and systematic line of march may not have been distinctly understood. He acknowledges its passage, and that a very free one, to all and every part of the body! Nor is its importance, as I have remarked, more highly estimated by any writer of any age! In speaking of the heart, (Fœs. p. 269.) he says, "the two ventricles are the sources of the life of man; from them issue forth those streams that irrigate, and carry life with them to every part." As a medium of therapeia, few writers have given better and more forcible instructions as to venæsection than Hippocrates. He bled as freely, when occasion required, as we do now; and with a judgment not surpassed in the present day. And why indeed should this not be the case? Venæsection is a remedy to be fully appreciated, *only by experience*; and not depending for its propriety on vague and hypothetical notions! Surely then, if now, each practitioner, just emerging from our benches, deems himself a master of the language of the vessels, from merely hearing some remarks on the subject; and boldly prescribes venæsection according to his *as yet unpractised* judgment; surely, I repeat, this important measure cannot be presumed defective, when issuing from the mandate of an experience of more than half a century; even though we may admit an ignorance of the real route of the circulating fluid.

Having, I trust, not unsuccessfully, vindicated Hippocrates as to his asserted ignorance of anatomy, and confounding the uses of the veins and arteries, &c.; I proceed, *thirdly*, to consider how far he has confounded *the uses of the nerves, tendons, and ligaments*; or rather, how far he has ascribed the same uses to them.*

Now here I cannot doubt, that a little care bestowed by Dr. Rush, in investigating the meaning of the words, as formerly understood; or, as by metonymy, employed occasionally, indiscriminately; yet without de-

* *Geoffroy and Savary.* Dict. de Sc. Med. V. 2. Anatomie, p. 38, assert that Aristotle and Hippocrates confounded the nerves, ligaments, and tendons.

stroying the intrinsic original meaning of each; would have satisfied him, that although, both by poets and philosophers, they have thus been frequently placed for each other; yet, that their distinctive use and application was more perfectly understood by Hippocrates, than by them. And this being demonstrated, will necessarily satisfy you of the incorrectness of this part of the aspersion thrown on Hippocrates.

The Latin word *Nervus* (which in the English, implies simply the organ anatomically denominated nerve;) has yet, even in the English, the adjective, *nervous*, implying strength and vigour; and that, both physically and metaphorically. Now this Latin word nervus is derived from the Greek νευρον, as is admitted by most etymologists. As giving a term of strength, therefore, it was applied to other parts and things, and where tone and vigour were pre-requisites; or, to imply somewhat which directly or remotely might be considered as connected with power. Thus, metaphorically, Demosthenes, Cicero, and most of the ancient orators and poets have figured *money*, as being the "Nervi belli." And Quintillian, equally uses it in reference to the mind, when he speaks (Education. lib. 1. cap. 2.) of the "nervi sapientiæ nil temeri credere," a metaphor, to which I earnestly request your attention, in the consideration of the subject now before us. "Frangere nervos et mentis et corporis;" is another metaphor of equal beauty in the hands of Quintillian. And I may here remark, that what the ancients implied under the term of *nervus belli*, viz. money; we equally designate, by a metaphor, wherein we drop the word nerve, and employ sinew in its place; thus we say, that money is the *sinew* of war; but we speak of nervous language or speech, in common implying vigour or strength, alike, in Greek, Latin, or in English. This being the fact, can it be considered as surprising, that the same figure, should give the term of νευρον to the sinew, or tendon; a part in which strength is peculiarly called for; as is the case in various parts of the writings of Hippocrates? (Fœs. p. 277. l. 16. *De oss. Natura*, on flexion et extension;) or that it should in like manner be employed to designate the string of a bow or catapulta:—(Ovid and others,—Terence, in Phormio act. 2. sc. 2. v. 11.) Neither can we wonder at its occasional employment to mean the stocks, or wooden vinculum for the feet. (Tertul. leg. 12. Tab.) That it was intended to imply vigour in the highest degree, may be also inferred, from finding the word νευρον employed by Aristophanes (in Avibus) to signify the penis, or membrum virile. Tibullus, an amatory poet, has used it with the same intention, in his poem "ad Priapum;" and Juvenal, in his ninth and tenth satires, seems to equally employ it thus.

That it was intended to imply strength, is also obvious, from finding that even the nerve, (*as now distinguished*) is at times called τονος, as in Hippoc. de Corde (Fœsius p. 269. l. 46.) et de ossium natura, (id. 277. l. 17.) and that he distinguished them succinctly from the tendons properly so called, is obvious, when speaking of the *origin* of the nerves (νευρων de Nat. Oss. Fœs. 274. l. 27.) he tells us, that it is from the occiput, and

along the spine; whilst elsewhere, they are metaphorically used for the tendons of the muscles and the ligaments of the joints (de Nat. Pueri. Fœs. 281. l. 19.) That by τονοι Hippocrates comprehends the nerves, is rendered more obvious, (περι αρθρων, vel de articulis,) by a note F. p. 1002 of Fœs. (☞ references) as well as from Galen de Articulis; and hence, says Fœsius, "since τονοι comprehends in its signification, the nerves, it was unnecessary to express them particularly. Hippocrates sometimes simply says τονοι, at others he adds the generic distinction, as τονος νευρωδεας. The remainder of the genus he calls τενοντα, and hence we have νευρωδεας τενοντας; and hence perhaps the ancients called by the common name of nerve, (νευρων) the three genera of nerves, tendons, and ligaments. (Galen. 6. Epidem. Com. 1. It nevertheless is the fact, that from Hippocrates downwards, (although, occasionally, thus metaphorically or synonymously used, yet,) the intrinsic and real meaning of nerves, was the chords proceeding from the brain and spinal marrow, which are the instruments of feeling, &c. Thus, Cicero (2. Nat. Deorum. c. 23,) says:—

"Nervis enim vis inest sentiendi, oriuntur ex cerebro."

What is a little remarkable; and arising probably from the idea that the nerves were hollow; and hence, as vascular, coming under the name of vein or phlebs; we are told by Erotian, that Hippocrates calls the veins themselves νουρον εναιμον, (sanguine præditus. I cannot find the place in Hippocrates, it not being noted.)

From what has been stated from various sources, it would then appear, I think, that although Hippocrates under the common name of nerve, (νευρον) with Aristotle and others, occasionally implies both nerve and tendon; yet that, from various parts of his writings, we gather distinctly the respective origin of the nerves, of the tendons, and of the ligaments, as separate and independent parts. If, however, in this illogical nomenclature, he has but followed the order of the day; he must *at least be freed from the imputation of confounding their uses in the animal economy!* Occasions indeed are not wanting, in which, by the application of various terms together, the specific character of each becomes identified. Thus, in his treatise on fractures, (Fœs. 759,) when speaking of the risk attending a fall or leap from on high, he says, "that thus roughly falling on the heel, the bone is luxated, the flesh is bruised, the fluids forced from their vessels, (φλεβια venulæ,) and much swelling and pain attends. There is (he adds) a large bone, the astragalus, placed directly below the tibia, which is connected by *vessels and nerves*, (φλεψι και νευροις) the tendo achillis (ο τενων) being inserted into the lower part of the os calcis." This quotation, aptly and sufficiently proves, that the distinction of nerve, tendon, and vessel, must have been familiar to him; or he never would have named them altogether in one place! It would be sufficient, indeed, to assert its sanction by general use, which cannot be doubted; and therefore, that no possible blame could attach to Hippocrates. Metaphoric language was more common then, than now; and we might as rationally conclude, that Dr. Rush

himself misapplied, or misunderstood his terms, when he, in common with the medical world, speaks of a *nervous* fever; or nervous temperament, &c. as, that Hippocrates was ignorant of the distinction of his terms.*

We need only further on this point, to look into the works of Homer; and here, by poetic licence, if not legitimately, we shall find the words τενοντε or tendons, νευρην or nerve, and even φλεβη or vein (rather vessel) indiscriminately used on more than occasion: and it may be added, that they are almost as indiscriminately translated by Pope within the period of the last century.*

4. I shall now, in succession, endeavour to show, that the assertion of Hippocrates mentioning *but one muscle*, (*the Psoas*,) in the human body, is equally unfounded: and that, whether we take the meaning to be, either that Hippocrates has only mentioned this one muscle by name; although he might be otherwise a proficient in myology; or that he knew not, that any other muscle actually existed. The meaning seems to be of this last character: but as the Doctor has not fully explained himself, I prefer to give his words their utmost latitude. So far, then, as it respects the first of these propositions, we may mention his expressly calling the heart, a muscle; (de Corde, Fœs. p. 268) which is all, that we at present denominate it. He moreover, when speaking of a luxation of the lower jaw, expressly calls by name, (the temporal muscle) the crotaphite and masseter muscles,

* Νουρον, NERVUS, *Duretus* p. 18. τονος l. 41. signifies tonus, vel ipsum robur, tendo, et ligamentum. Nervus, triplex, viz. Tonus, tendo, et ligamentum. Τονος or the true *nerve*, is a propagation from the brain and spinal marrow.

Νουρα, nervi, p. 423. Here the term is sufficiently explained in its various meanings by Duretus, as those "qui sunt motus et sensus authores, nati è cerebro et ejus propagine spinali medulla; *tum* qui è musculis enascuntur, et in articulos inseruntur, τενοντάι Græci vocant: *tum* qui articulos ipsos connectunt, faciuntque illum συμφυσεως &c. Ac certè illa *tria nervorum genera* continent ipsos artus, (femores:) *primum* quidem, impertiendo movendi vim et sentiendi: *alterum* ad ducendis et abducendis membrorum articulis: *tertium* connectando artuum, per arthrodia, ginglymus, et enarthrosis.

Vide also his reference to 418, on the subject. See also, Faber's Thesaurus, its various significations. ☞ Aristophanes uses it for the penis: now, suppose some ill-judging translator was to employ the term in this sense! What could be said?

Aristotle in ch. 5. hist. Animal, περι νευρων and their origin; here, under this term, he seems rather to mean the tendons and ligaments, than what we now understand by nerve.

Id. ch. 9. νευροις, for ligaments, the bones "nervis deligantur."

* Homer, lib. 5. l. 307 }
14. 465 } τενοντε ☞ nerve, Pope.
20. 478 }
8. 328 νευρην ☞ tendon. do.
13. 546 φλεβα ☞

and that in more than one place. Other examples might, I believe, be readily shown; but these are fully adequate to my purpose, which is to prove that he was neither ignorant of myology, nor of its nomenclature, so far as a regular nomenclature might at that time have existed. If, on the contrary, we suppose that Dr. Rush infers that Hippocrates knew, and considered but one muscle as having existence in the body; I reply, that had this great man been a very idiot, he must have appreciated the influence of muscular motion, and its varied actions, as dependant on *a vast variety* of these organs! It is when speaking of the structure of the spine, that he mentions the Psoas muscle, as filling the interior and lower curvature of the dorsal column, and as being the *only one there;* and this, I suspect, has been the source of the error into which Dr. Rush has fallen; without duly scrutinizing the validity of his charge, by an accurate investigation of his author.

If more is requisite to establish my position, I shall refer you to his essays "de Officina Medici," and "de Fracturis," Fœs. 740, &c., as well as to nearly all his writings; in which repeated reference is afforded to the action of different muscles, although not particularly specified by name.*

5. But what shall we say of the *sweeping condemnation* of Hippocrates, contained in the concluding paragraph of Dr. Rush's assertion? viz. "that the account of the heart, the brain, the senses, the intestines, and organs of generation, is so replete with absurdity, *that it would be disgusting* to mention them!" It is possible that had Dr. Rush pointed to some specific instance, we might have received it as correct: but here is not a solitary redeeming clause to console us for such a complete prostration of the *Divine Old Man!* Let us, however, in endeavouring to redeem our own pledge, cursorily examine if the assertion is true in itself; and we shall pursue the arrangement of Dr. Rush.

* Speaking of the natural situation of the parts, as essential to be considered in the locality, extension and flexion in fractures, &c. he adds, "When the leg, instead of remaining in its state of extension, changes its place, the *muscles*, the *vessels*, the *nerves*, the *bones* also change their position; and if free, will assume that which is *best*."

Again, "When we wish to produce flexion, the *muscles*, by contracting, will lose their position as well as the bone." "*Many muscles*, he tells us, cover the radius at its upper part."—"When the humerus is broken, if we make extension by holding the hand and fore-arm, before we apply the bandage, it happens that by afterwards flexing the fore-arm, the *muscles* will change their situation."

In fracture of the femur, he tells us, the *force of the muscles*, will separate the bone, as soon as extension is remitted; and that the *muscles*, which are strong and active, will even surpass the power of bandages.

In his treatise *de articulis*, when speaking of a luxation of the humerus, he says, that as to a luxation towards the anterior part; he had never seen it, nor did he believe it possible. Physicians, adds he, deceive themselves, in cases wherein the *muscles* surrounding the joint, and arm, have become *wasted*. In such cases, the head of the humerus *appears* to project on the anterior part.

1. The *Heart*. And here I can truly say, I find nothing on the subject, in any part of the writings of Hippocrates, deserving of a censure so severe! In the treatise, expressly "de Corde," Fœsius, 268. there is, I think, much to admire; and, if we condemn what may not be satisfactory to us at present; yet nothing will be found that is either disgusting, or that would seriously impair the memory of its great author.

He considers with sufficient distinctness, its shape, its pericardial investment, and the moisture therein; which he supposes to transude from the heart. Here, his theory maintains itself, by what may possibly be regarded as absurd in the present day; but which is admirably constructed when viewed through a long retreating vista of twenty-two centuries. Thus, Hippocrates supposes the heart to draw the pericardial fluid adverted to, from the lungs; which last are presumed to receive it in the process of drinking, during which a portion passes down by the side of the Epiglottis, through the Trachea. This position seems to have been contested by the medical contemporaries of Hippocrates, if we may judge from his anxiety to maintain it. "A proof, says he, that a part passes this, is, that if you give water tinged with blue, or with minium to an animal (altéré) thirsty; but especially to a hog, which is by no means nice or delicate; and if its throat be cut, and the Trachea is opened, we find it tinged with the colour of the drink;" but, adds he, all persons cannot perform this experiment.

He says, the heart is a very strong muscle, as well from its tendons (νευρα) as from the mass of fleshy fibres. It has two ventricles (γαστερας) not exactly alike; the right one having an opening at the base, corresponding to one of the large veins, (φλεβι) and he well discriminates, that although called the right, it is nevertheless in the left side. It is not so strong as the left one, which is placed beneath the left breast, where its beating is distinctly felt. He supposes the heat of the heart (the presumed focus of heat) to be moderated by the natural coldness of the lungs; as well as by the air inspired; and although this may now be set down as absurd; do we, in fact, know much more about animal heat, than Hippocrates did so long ago? And are not the various hypotheses in explanation thereof; *now*, quite as meagre and unsatisfactory as that of Hippocrates? His theory is, at least, quite as well sustained, as any of ours!

By cutting off the auricles and base of the heart, an opening in each ventricle is perceived; these two ventricles are the sources of the life of man! from whence flow those streams which water all the interior of the body, and convey life to every part.

The auricles, he supposed, were intended by nature as bellows, to draw in air; and in which he thinks he discovers the skill of a superior artist! in adding, as it were, such machinery for the conveyance of air, as is evident from the perpetual motion of every part of the heart: the auricles having their own peculiar motion, by which they are enabled to expand, and contract, &c. He moreover *notices the valves;* and in conformity with the opinion of the day, explains their use in restraining the flow of blood

into the left ventricle; but permitting the passage of the air or animal spirits. (See also Fœs. *de Carnibus*, p. 250.) Upon the whole, however obscure and singular the doctrine of Hippocrates may *now* appear, as to the uses of the heart and its various parts; let us recollect, that only about two centuries have elapsed, since Harvey is presumed to have rectified those ancient errors; which had scarcely been modified during a period of more than two thousand years!

With this kept in view, we may rather feel disposed to admire the sagacity of this venerable writer: and lean with toleration to the consideration of so luminous a superstructure as he had raised on an imperfect basis! In all of which, nothing appears that is, in my estimation, capable of exciting the least disgust!

2. *Of the Brain and Senses.*—Whatever we may think of the anatomical knowledge of Hippocrates in respect to the brain; I can only say, that it becomes no one, less conversant with the dissection of that organ than Gall or Spurzheim; to cast the slightest objection on Hippocrates! If we consider that those gentlemen have created a new æra, from which we may date an improved method of investigating and unfolding the brain; and that, (apparently,) our present knowledge of it is as superior to that of only half a century past; as *it then was*, in comparison with the time of Hippocrates; we shall probably come to the conclusion, that we have little right to draw invidious comparisons; I may however state, that, with the energy of a great and inquiring mind, he observes, that whatever the brain (the domicile of the senses, which he terms the ministri cerebri,) knows, is conveyed thereto, by the eyes, the ears, the tongue, and the extremities; which may be considered as forestalling the celebrated axiom, that "Nihil est in intellectu, quod non fuit prius in sensu." The brain, is according to him, the *Internuncius* of intelligence; (de Morb. sacro, Fœs. 309.) and he adds, that the septum transversum or diaphragm is improperly called φρενες by the Greeks, since the mind has nothing to do with that organ: "temerè ac fortuitò sortitum nomen videtur, et ex instituto, non re vera, neque a natura," &c.

He mentions (*de Morbo sacro*, Fœs. p. 304,) the brain as being double, "Cerebrum duplex in hominibus et omnibus animalibus;" and elsewhere, (*de Locis in Homine*, Fœs. p. 408,) that it has two membranes (μενιγγες) or coverings; with much more, that would seem to imply at least, no inconsiderable knowledge of its structure and functions. In his treatise "*de Glandulis*," p. 270. he considers the brain as a gland; nor does he want reasons in defence of his assertions.

So far as his metaphysics enable us to appreciate his knowledge of the senses, &c. it was apparently, conformable to the philosophy of the age; nor is it more visionary or absurd than most of the views that have been promulgated up to the present time, that have not had a foundation on the firm basis of Phrenology! A science, it is true, comparatively in its infancy; but which displays the vigour of Hercules! and which must, in spite of

opposition, sooner or later, be the principal pillar of support, and the key-stone, to every rational investigation of the mind and its functions!

3. *Of the Intestines.* It would appear, by reference to different parts of the writings of Hippocrates and his commentators, as if, (at least by many,) an opinion was entertained, of there being but *one intestine;* which had the general name of colon, (*de Corporis Resectione*, Fœs. 916.) Some obscurity unquestionably exists; for in other parts, a distinction is made; and the duodenum is spoken of, if not by name, yet sufficiently described as being about 12 fingers-breadth in length; smaller than, but resembling the stomach. The rectum also seems noticed, in "de Nat. Hominis," p. 229; and, in "de Carnibus," p. 249 and 252, the jejunum, under the name of νηδυος; and in various parts he mentions their diseases, and modes of cure. This word νηδυος is the same formerly mentioned, as implying "Venter, vel receptaculum cibi, et apud Hippocratem, omnem cavitatem atque conceptaculum quo humor alendis partibus idoneus continetur. Item uterus, ut et Lat. alvus pro utero sumitur. Νηδυια, τα, intestina, &c. (Vide Lexicon Græco-Lat. 1583, p. 765.) Hira, Intestinum quod jejunum dicitur, quod semper vacuum sit," &c. In the treatise, "*de Glandulis*," (Fœs. 271,) Hippocrates mentions the glands connected with the intestines.

It may be a question of curiosity, though probably of no use, when, and by whom, the intestinal tube received its present subdivisions! Nature seems to present a very decided distinction into two parts; the large and the small: but the subdivisions of each of these, into three distinct portions, appear rather the result of fancy, than of any well-grounded line of demarcation. Be this, however, as it may, all that I am at present interested in, is to show, that, even admitting what Hippocrates has said on the subject of the intestines to be ridiculous and absurd; it is at least in conformity to the opinions of the day; and has nothing disgusting, beyond what the nature of the case itself renders otherwise impossible to avoid; and certainly, this cannot be deemed legitimately liable to the animadversions thrown out by Dr. Rush.

4. *Of the Organs of Generation.* I shall readily admit, that the consideration of the subject of the organs of generation, may be, or not, according to circumstances, one of infinite interest, or of the highest indelicacy and disgust. Certainly, to the medical man, every part of the body is replete with wonder, and demands his strict attention to its structure and its functions, if he expects to be useful in a morbid deviation. Perhaps, none more so, than the organs under consideration! And if contemplated with the eye of philosophy, physiology, and pathology, as they ought to be, (at all events by medical men;) nothing more interesting can command our inquiries; whilst on the contrary, nothing can be more disgusting, when solely regarded with the eye of licentiousness and lust. As assuredly, this was not the view in which Hippocrates regarded these important appendages for the continuance of the human race; but with the strictest eye to medical utility; if even his views may be considered as absurd; yet certainly there

is nothing disgusting about them, at least so far as I have been able to discover! Nor can I believe, that in this respect, the most fastidious would find cause for complaint! More especially when it is recollected, that the theory of generation which Hippocrates advanced, even at the distant period of twenty-two centuries; this same theory, I say, with little modification, may be considered as dividing the opinions of medical men, between itself, and the doctrines of sympathy, which, in my estimation, is at least, the more absurd! That some of Hippocrates' speculations, may, in the present age, excite a smile, I will readily allow; and I will even admit, that he has made the uterus too important a personage in the female system! What then; are our present views on all these points less obscure than his; or less deserving of a smile! Are not the countless abortions that have dropt from the press within the last twenty or thirty years, on every topic of medical, physiological, or pathological inquiry, sufficient to prove the absurdity of the opinions which we refuse to tolerate. And shall a man who wrote four hundred years before the birth of Christ, be judged as harshly now as he, who, without the *tithe* of his merits, has nevertheless possessed ten thousand times his advantages in medical research! The truth is, the views of Hippocrates, even if absurd; are those of a great, a vigorous, and a master mind; unaided by any very extensive means of previous inquirers; yet unshackled by the past, or contemporary authority! and perhaps not sufficiently restrained in its luxuriance, from a defect of many facts familiar to the present generation; and which, if living, he could far better illustrate and connect together!

With such impressions on my mind, can you wonder, gentlemen, that I have attempted to shield from obloquy, the memory of this illustrious member of our profession; or that I should regret, that my respected preceptor should not rather, in addition to his actual commendations, have recalled to mind that beautiful remark, "non offendar paucis maculis," and have kindly cast a veil over those imperfections which he *supposed* he had detected in the most ancient and the most venerable of our medical authorities!

FINIS.

## ERRATA.

Preface, page xv. l. 16, for *demonstare*, read *demonstrare*.
x. 19, for *were* read *was*.
34. 28, for *cordisopinionem*, read *cordis opinionem*.
35. last line, for *æ ue*, read *æque*.
126. note, edition of the College, stated as of 1776.—It should be 1766.
208. line 32, for *attrahunt* read *attrahant*.

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