

Observations on the Harveian doctrine of the circulation of the blood / by George Kerr.

Contributors

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Sir, J. M. Briger Bt

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OBSERVATIONS

ON THE

Harveian Doctrine

OF THE

CIRCULATION OF THE BLOOD.

BY GEORGE KERR.

SECOND EDITION, ENLARGED.

Ζητῶ γὰρ τὴν Ἀλήθειαν ὑφ' ἧς οὐδεὶς πώποτε ἐβλάβη.
Marcus Antoninus.

Ἡ δὲ πείρα σφαλερή.
Hippocrates.

LONDON:

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1819.

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PREFIXED TO THE FIRST EDITION.

TO

JOHN ABERNETHY, ESQ. F. R. S.

&c. &c. &c.

PROFESSOR OF ANATOMY AND SURGERY

TO THE

ROYAL COLLEGE OF SURGEONS, LONDON.

Dear Sir,

ALTHOUGH I do myself the honour of addressing the following observations to you, it becomes my first duty to state that I have no right to assume any assent, on your part, to the heretical opinions they contain. It may, however, procure them a patient perusal by some, who, firm believers in Harvey's doctrines, might otherwise deem them unworthy of notice, to mention that a part

of the manuscript was, for some time, in your hands, and that you candidly admit the *possibility* that the Harveian hypothesis may be unfounded. You say men form their own minds by repeatedly thinking in a particular manner, nor can any sudden change of their opinions be expected—that it is difficult to say who is in the right in this case, and all may be wrong—that were my observations published, some would consider them as ingenious, but they would produce no change in their opinions, while others, whose opinions were less inveterate, might be convinced by them, and think as I do. At any rate, they would excite investigation. At the same time you said, that, in your opinion, the Harveian doctrines could only be successfully combated by experiment, and that you would not publish any opinions that were not deductions from facts. Now I believe that I have stated facts for the most part acknowledged by all, and, to the best of my information, have drawn fair inferences. I have not tried experiments, because I conceive that we have

facts and experiments, reported by men of high authority, abundantly sufficient to enable us to ascertain the truth; and I can with more confidence appeal to these than I could report what I had myself ascertained, because my reports might have been suspected of partiality, or inaccuracy. In my opinion, Harvey reasoned falsely from the experiments he made, and, having persuaded himself that the pulse is occasioned by the distension of the left ventricle, by the blood exciting it to contraction, he assumed the connexion of extreme arteries with veins, and the circulation through the lungs, as necessary consequences of the first assumed fact. Some years have now elapsed since I began to entertain doubts of the truth of this hypothesis—for eighteen months past the subject has engaged much of my attention—and I can honestly declare, that the more I have studied the works of Harvey, Walæus, Pitcairn, and others, who maintain the doctrine, the more firmly am I convinced that it is altogether untenable. My objections I have stated to many of my medi-

cal friends, some of whom agree with me in thinking them insuperable—some abide by Harvey's doctrine, but say nothing pointed against the facts I have stated, contenting themselves with the repetition of assumptions and conjectures as to how the circulation *must* be carried on—and a great majority tell me that they never suspected any fallacy in the generally received opinions, and that they remain in doubt. It is, however, a question which every man of liberal education may enter upon, and decide for himself; and I cannot help thinking that our profession would have been, at the present day, more respectable, had the attention of literary men been more closely directed to our proceedings; for, as we neither have secrets, nor pretend to magic, quackery could not have prevailed so much, had it not been for the negligence and consequent ignorance of those who ought, in some measure, to have been our censors.

The medical men retired from practice, and scholars not of the profession, to whom I

have shown my observations, are nearly unanimous in assuring me that they believe them well founded ; but in many of my professional brethren there has appeared a great reluctance to enter upon the discussion, amounting to a seeming dread of conviction. Indeed, in many cases, it might be a serious affair for a practitioner to declare himself a schismatic from the Catholic physiological persuasion ; and I continue to urge the subject upon your attention, because if, after reflection and due investigation, you find reason to depart from what you call the faith in which you were educated, I shall expect your ready and full recantation. For, in the words of Celsus, I can assure you that thus you will act—" *More scilicet magnorum virorum, et fiduciam magnarum rerum habentium. Nam levia ingenia quia nihil habent, nihil sibi detrahunt; magno ingenio multaque nihilominus habituro, venit etiam simplex veri erroris confessio.*" But your remark concerning the difficulty of effecting any change in the public opinion is unquestionably just, and all I propose by

printing the observations in their present state is to call the attention of professional men to the subject. By patient and impartial investigation, the truth may be ascertained, and by fair reasoning from established facts, we certainly ought to arrive at complete and full conviction of what, in this case, the truth really is. You will admit that, since the time of Harvey, our physiology has been almost as changeable as the cut of our clothes; and Dr. William Hunter used to say that no Physiologist could leave a reputation behind him that would outlive him half a century. "When they cease from their labours," (said he,) "their labours will be buried along with them. There never was a man more followed and admired in physiology than Boerhaave. I remember the veneration in which he was held; and now, in the space of forty years, his physiology is—it shocks me to think of the light in which it appears." This instability of physiological and medical theory may, I think, be fairly traced to the doctrine of Harvey, for, although, before his time,

Physiologists differed in some points, in the great outlines they were perfectly agreed.

The ancient Physiologists believed the vital spirit of man to be something ætherial, and of a superior order to mere matter perceptible by our senses. Harvey, who reduces man to a hydraulic machine, laughs at the idea of any thing more divine than the blood in the composition of man; and very positively denies the existence of vital spirits because he had never seen them.* He argues that these vital spirits must necessarily be colder than the blood, because they are said to be derived from air and vapour—a notion which it is unnecessary seriously to refute at the present day; and, upon the whole, his ideas of the animal economy he declares to be bounded by objects of sense. From this

* Nos neque in venis, nervis, arteriis, aut partibus vivorum, dissectionibus explorando invenimus.

doctrine many, of late years, have been led to derive materialism, and the extinction of the soul by death ; while, on the other hand, the ancient Physiologists believed and taught that the soul is immortal, being in its nature indestructible, and that it must necessarily survive the body.

Hippocrates, in the beginning of one of his books, says, that “ although it is not the province of a Physician to speak of divine essences, unless in so far as they are connected with the nature of man, it is yet necessary for him to lay down some general principle from which he may reason. Therefore,” says he, “ it appears to me that what we call *animal heat* is immortal—comprehending hearing, seeing, and perceiving, all things present and to come.* In one sense it may be that every

* Δοκεῖ δέ μοι ὁ καλούμεν Θερμὸν, ἀθάνατόν τ' εἶναι καὶ νοεῖν πάντα καὶ ὁρᾷν καὶ ἀκούειν, καὶ εἰδέναι πάντα, καὶ τὰ ὄντα, καὶ τὰ μέλλοντα ἴσσεσθαι.

material form is indestructible, and that Ovid's "nihil interit, cuncta mutantur," is literally true; but the ancients believed the soul of man to be *divinæ particula auræ*, not subject to the laws of organized matter, and that its happiness or misery in another state would be influenced by the deeds done in the body. Whatever difference of opinion may exist concerning the intelligent principle in man, and its mode of existence, all appear now persuaded that an invisible, active, and powerful agent pervades all nature, not subject to our sublunary laws of motion, but passing through extended space with the rapidity of thought. That this agent is, in some manner to me unknown, the prime mover in the human body, and that from it all animal motions, voluntary as well as involuntary, proceed, is, I think, probable in the highest degree, and that the motion of the heart, and pulsation of the arteries, are to be accounted for in a manner of which Harvey had not the most distant idea.

The experiments made by Sir Humphry Davy, who has analysed many substances, long, but erroneously, termed simple, convince me that no really simple substance can be comprehended by our senses. If all terrestrial substances have not yet been analysed, and reduced to that state in which they elude our view, and escape into the atmosphere, that affords no convincing proof that there are bodies in nature unsusceptible of such analysis; as it is no proof that undiscovered countries are inaccessible, because no navigator or traveller has yet approached them. The essence of all matter philosophers hold to be one and the same, and that what is susceptible of decomposition may again, by synthesis, acquire new form; and this is exactly what was taught by Pythagoras, and is, in fact, the philosophy of Moses.—Such also was the opinion of Sir Isaac Newton, who says, “Are not gross bodies, and *light*, (or æther,) convertible into one another, and may not bodies receive much of their activity from the particles of light, which

enter into their composition? The changing of bodies into light, and light into bodies, is very agreeable to the course of nature, which seems delighted with permutations. Water, which is a very fluid tasteless salt, she changes, by heat, into vapour, a sort of air—and by cold into ice, which is a hard, pellucid, brittle, fusible stone; and this stone returns into water by heat, and vapour returns into water by cold. Earth, by heat, becomes fire, and by cold returns into earth.” He proceeds to give many other instances of transmutations, and, upon the whole, appears to adopt the Pythagorean doctrine, so beautifully expressed by Ovid—

“ *Resolutaque Tellus*
In liquidas rorescit aquas: tenuatus in auras
Aëraque humor abit: dempto quoque pondere rursus
In superos aër tenuissimus emicat ignes.
Inde retro redeunt: idemque retexitur ordo.
Ignis enim densum spissatus in aëra transit;
Hic in aquas: tellus glomerata cogitur unda.
Nec species sua cuique manet: rerumque novatrix
Ex aliis alias reparat Natura figuras.”

According to this philosophy, the Mosaic account of the creation presents nothing incompatible with the principles of just reasoning; and the expressions of Scripture, that *we are his offspring*,* and that in our Creator we live, move, and have our being, convey nearly the same meaning as the following beautiful lines from the “Interpres Sacer Deorum”—Orpheus.

Zeὺς πρῶτος γένετο, Zeὺς ὕστατος ἀρχικέραυνος·
 Zeὺς κεφαλὴ—Zeὺς μέσσα—Διὸς δ' ἐκ πάντα τέτυκται·
 Zeὺς πυθμὴν γαίης τε καὶ οὐρανοῦ ἀστερόεντος.
 Zeὺς ἄρσην γένετο, Zeὺς ἄμβροτος ἔπλετο νύμφη,
 Zeὺς πνοὴ πάντων, Zeὺς ἀκαμάτου πυρὸς ὁρμή.
 Zeὺς πόντου ῥίζα—Zeὺς ἥλιος ἠδὲ σελήνη·
 Zeὺς βασιλεὺς, Zeὺς ἀρχὸς ἀπάντων ἀρχικέραυνος.
 Πάντας γὰρ κρύψας, αὐτὸς φάος ἂν πολυγηθὺς
 Ἐξ ἰερῆς κραδίας ἀνενέγκατο μέγμερα ῥέζων.

* Τοῦ γὰρ γένος ἑσμέν.—The words of the poet Aratus, quoted, with approbation, by St. Paul, in his Address to the Athenians.

The same sentiment occurs frequently in the writings of the ancient Poets and Philosophers; and among the Romans, such expressions as “Jupiter est quodcunque vides”—“Jovis omnia plena”—show that they entertained the same philosophical principles with the ancient Greeks.

With regard to man, they entertained very elevated notions of his origin, susceptibility of improvement, and the future state of the good after death. They did not believe, as some materialists affect to believe, in a “*principle of intelligence*,” that may be purged, vomited, and sweated—dispersed by evacuations, and renewed by beef and pudding, turtle, port, and porter;* nor did they believe, because the soul of man is not, *per se*, comprehensible by our senses, that it has, therefore, no existence. “Bury me” (says Socrates to a

* Monthly Review of M. Tralle’s Work concerning the immaterial Soul.

friend) “as you please, provided you can catch me; but it seems I cannot convince you, that, when my dead body lies before you, the Socrates, who now addresses you, and reasons with you, will be no longer present.” The address of Cyrus to his friends, as given by Xenophon, expresses very beautifully the same sentiment, the same conviction of the immortality of the soul, and that all the parts of the body are merely its instruments. The Physicians of antiquity attended particularly, in every case, to the state of the patient’s mind; and some very striking instances of their sagacity are upon record, in discovering that disease had originated in affections of the mind. On the other hand, they knew that a diseased body may disorder, impede, or altogether suspend, the operations of the mind—that intemperance debases man, and prevents the free exercise of the reasoning faculty,*

* Quin corpus onustum

Hesternis vitiis, *animum* quoque prægravat unà,
Atque affigit humo *divinæ particulam auræ*.

presenting an insurmountable obstacle to that improvement which ought to prepare the soul for a more perfect and dignified state of existence. This physiology Harvey ridicules as the conjectures of ignorance, telling us that the blood is the true vital principle of the human body, as he had never discovered any thing more divine in his dissections; and inferring the non-existence of whatever is not perceptible by the human eyes. Let it not be said that *time* has confirmed the Harveian doctrine. It has prevailed for nearly two hundred years; but the erroneous doctrines of Ptolemy, concerning the system of the universe, prevailed for a much longer time; and the antient doctrine of Pythagoras, so long ridiculed by the ignorant as repugnant to the common sense of mankind, was received as a new discovery. The human mind is not always progressive in knowledge: we have dark ages, in which error prevails, and the most absurd speculations are received as founded in truth. In our profession it has become fashionable to speak with contempt

of the *trammels* of antiquity, as imposing fetters on original genius; and the consequence is, that hypotheses, wild and extravagant, spring up and perish, with inconceivable rapidity, and in endless succession. An extension of the same principle would rid us of the trammels of education, that we might witness the sublime efforts of the human mind, unbiassed and unprejudiced. Is it not singular that medical men only should have to complain of restraints imposed upon their exuberant genius by the precepts of antiquity, (from which, indeed, they have very effectually disengaged themselves;) and, after innumerable failures, should still persist in believing that truth is to be ascertained by conjecture? The first orators of our times formed themselves upon the models of antiquity—I give, as instances, Chatham, Mansfield, and Fox. Robertson was accustomed to animate the zeal of the scholars of the Edinburgh High School, by assuring them, that whatever credit he had acquired as an historian, he owed to a careful study of the

antients—Euclid maintains his superiority as a mathematician—Archimedes as eminently skilled in the mechanic powers—we find Canova and Flaxman eagerly expressing their admiration of the unrivalled excellence of the works of Phidias. In poetry, sculpture, architecture—in every thing, we admit their superiority, (as I have elsewhere observed,) with the exception of medical science, including Physiology, and *Scotch Metaphysics*; for my countrymen declare that they have, of late, very much improved upon Aristotle.*

The medical authors of antiquity we hold convicted of neglect, nay even of folly, upon the authority of a man whose writings bear

* In his Preface to a Dissertation on the Gout, Sir Richard Blackmore, one of the Heroes of the Dunciad and Scriblerus, very coolly gives his reader this reference:—" *But if the reader is desirous to see Aristotle's Philosophy fully displayed and confuted, I refer him to the 5th Book of my Poem, entitled Creation.*"

evidence that he was unacquainted with theirs, who, from appearances obvious to the meanest capacity, infers gross ignorance in the most learned men of the most learned ages.

Before I conclude I must notice an observation in one of your letters on this subject, viz. :—"That you are satisfied that I, who have so ingeniously discovered such strong objections to the Harveian doctrine, could, you are persuaded, have found many stronger ones to that of Erasistratus." As yet, the physiology of the ancient Physicians is but a new study with me; but, in as far as I have proceeded, I do assure you that I think it much more rational than our own; and, in particular, I have seen nothing in the doctrines of Erasistratus, as they are preserved by Galen, which, to me, appears contradictory. As to genius, or discovery, I have no claim; the objections to the Harveian hypothesis arose spontaneously, without effort on my part; and the present rude sketch is evidently not the production of de-

liberate study. My object in putting the notes, in manuscript, into the hands of professional men, was to know whether I had mis-stated any fact, or drawn unfair conclusions from the facts adduced ; and that object I did not attain. I believe myself pretty well fortified by respectable authorities, but I shall gladly receive corrections as to facts and reasoning. I shall only observe, that, as mere assumptions prove nothing, and may be multiplied *ad infinitum*, I consider them unworthy of serious discussion.

I make no apology for numerous quotations, for I think them applicable ; and the original words of an author convey his sense in the clearest manner. I wish to have the opinions of those to whom the authors I have quoted are well known, and their language familiar ; and it can do students no harm to send them to consult their dictionaries. Quintilian says, that, in his times, there were some who considered “ *id pulchrum quod interpre-*

tandum sit"—because they then enjoyed a pleasure "non uti legerint vel audiverint, sed quasi *invenerint*."

I have the honor always to remain,

With great esteem,

DEAR SIR,

Your's, very sincerely,

GEORGE KERR.

Aberdeen, June 8, 1816.

TO

JOHN ABERNETHY, ESQ.

&c. &c. &c.

DEAR SIR,

THE doctrine of Harvey, concerning the circulation of the blood, has so much influenced medical practice, and led to a change so complete, both in Physiology and Pathology, that, believing as I do in the truth of the ancient doctrines, and that sooner or later they will fully recover their pristine reputation, I conceive that I am most usefully employed in calling the attention of medical men to the subject, and stating, more fully than before, my reasons for believing Harvey's hypothesis unfounded.—In my first public address to you

upon this subject, I said, "that in many of my professional brethren there had appeared a reluctance to enter upon the discussion, amounting to a seeming dread of conviction." This was true at the time I wrote, and it remains so at the present day; and I cannot acquit you yourself of the charge of *shying* the subject in your last Physiological Essays. From an expression in one of your Letters to me in 1816, viz. "*that you would be provoked to write upon the subject,*" I was led to expect, with some impatience, the appearance of your Essays, trusting that I should find satisfactory answers to my objections, or an admission of their validity.

The following passages in your 5th Lecture, convinced me that my expectations had not been well founded; and while some Physiologists admit that my objections appear to them unanswerable, you think it unnecessary to enter upon their discussion.

At page 221, entering upon the subject of

the circulation, you say, "Though I now engage in a subject, which more especially I have a right to discuss, as it relates to human anatomy; yet I must not weary you by detailing facts familiarly known, or proving propositions which are generally admitted. Were I also to advert to the doctrines of others, I should have to collate asserted facts and opinions irreconcilable with one another, and to engage in controversial discussion; yet why should I bandy about the ball of controversy, wasting both my own labor and your time, if I know I must let it fall at the conclusion, just where I took it up at the beginning? It seems best for a Lecturer simply to say what he thinks, and why he thinks it; because it is the only way in which he can feel, and consequently create an interest in that which he delivers." In the conclusion of this Lecture you say, "This theory of the circulation has been assaulted, and yet, as far as I can perceive, without being injured or invalidated; therefore I am disposed to maintain it for the same reasons that

I am his (Mr. Hunter's) theory of life, because it is probable, cautiously, and philosophically deduced, and adequate to explain all the phænomena of health, disorder, and disease."

These extracts contain, no doubt, a declaration of your Physiological creed ; but you will admit, that they leave me as much as ever at a loss to know, whether you are prepared to obviate my objections to Harvey's hypothesis adopted by Mr. Hunter, or wish merely to evade them, as unworthy of serious notice. When I first thought of agitating this very important question, I expected to be favored with particular answers to the objections I had stated, which to me, appeared to demand attention ; but, as I have before stated, many of my professional brethren, appear to have very little inclination to enter upon the subject ; and those who will not admit the possibility of Harvey being in error, nor take time to consider the question attentively, I cannot expect to convince. I know, however,

that some who rank high in our profession, admit the strength of the objections urged against Harvey's hypothesis ; and upon the whole, the heretical publication has been received with more tolerance than I expected. I have read over with attention, whatever has been published, in answer to my objections, but I do not find that the writers in favour of Harvey's hypothesis conduct the argument in such a way that I can properly reply. They assume the *fact* of the circulation, and then elaborately state how that circulation *must be carried on*. Indeed, this *petitio principii* began with Harvey himself, and must continue as long as his hypothesis shall be defended. Every one of his experiments is to be accounted for without supposing a circulation of red blood ; but he appears to have no idea that a fluid of a gaseous nature will distend containing vessels, or that the escape of the sanguineous gas (if I may use the expression) from the arteries, must instantly be followed by blood from the venous system. That blood is readily derived from all

parts of the body to a small superficial wound, and that, under certain circumstances, such a wound may produce a fatal hæmorrhage, is true; but that fact does not appear to me to prove any natural communication of vessels, in the healthy state, beyond mere contiguity; and the disturbed state of the system, and violent attraction of blood into the arteries, appear to be produced by the direct communication opened between the contents of the vessels and the atmosphere.

I have already said, that on this point it is certainly possible to ascertain the truth, and it is not a question that ought to be disposed of by medical men, by merely observing with Sir Roger de Coverley, that "*much is to be said on both sides.*" If the doctrine of Harvey be unfounded, we have fatally erred, and for the good of mankind ought to retrace our steps without delay; and it is, in my opinion, the most important question that can occupy the attention of medical men, to ascertain the fact. When I say, that after attending to the

subject very particularly for several years, my conviction is more and more strengthened, of the fallacy of this hypothesis, I shall perhaps be told, that having conceived improper prejudices against it, they acquire strength by my partiality to one side of the question : but I uniformly find, that those who agree in the possibility that Harvey is wrong, soon come to believe it highly probable. Upon the publication of the first edition of these observations, I sent a copy to a gentleman in Yorkshire, and requested him to favour me with any remarks that his medical friends might offer upon the subject. Some time after he wrote me, that he had mentioned the publication to their first practitioner in Doncaster, who laughed at the idea of any fallacy in the hypothesis of Harvey. He, however, agreed to read the pamphlet, and returned it, saying, that he did not see ready answers to the objections, but yet could not believe them well founded. I wished to have this gentleman's *adversaria*, but upon a second reading, he gave a more favour-

able opinion of the objections; and I understood that some time after, he had expressed himself to my friend, as convinced of their validity.

In no one case have I found the arguments against my objections strengthened by discussion; and although I have heard of some experiments tried in support of the Harveian system, in as far as I have heard them described, they merely go to prove that blood will flow from the vein of a limb, when heat is transmitted by the artery, and will not flow when that heat is intercepted.

Time and discussion appear to be in favour of my objections, and they are powerful auxiliaries; but they can produce no effect upon those who believe Harvey infallible; and a false hypothesis could not possibly have prevailed for two hundred years. Enquiry, however, is excited, and I am well persuaded that some, and those well qualified to do justice to the subject, will conduct the investigation

with all the attention it merits, and with strict impartiality to a fair conclusion.

In the mean time, permit me to notice one subject upon which you and I differ very widely. You seem to think that it was an advantage to Mr. Hunter that he had read very little, and had scarcely any books in his library, unless such as had been presented to him by their authors. I always highly respected the talents of Mr. Hunter, but that he did not know what had been done in the cultivation of physiology and pathology, and advancement of those sciences before his time, I cannot admit to have been any advantage to him, nor that it ought to add weight to his authority, that he was unacquainted with the language from which the technical terms of his profession are derived. Without miraculous interposition, education is absolutely necessary to form the physician and surgeon; for we may as reasonably expect to meet with new instances of the gift of tongues, conferred by supernatural aid, as the manifold

qualifications of our profession conveyed without instruction, as well as long and patient study. How much you are attached to his doctrines I know, but I am not singular in believing, that according to his brother's prediction, they will not survive him half a century ; nor indeed is it possible that they can, if his first physiological principles be false.

I cannot help again recurring to your declaration, that it is unavailing to enter upon the discussion of this subject, "to collate asserted facts and opinions irreconcilable with one another ; knowing, as you say, that you must lay down the ball of controversy where you first took it up." You do not mean to say, that it is impossible to ascertain the truth, whether blood naturally circulates through our arteries or not ; and if it be possible, is it not of the highest importance, that the fact should be entertained ? How should the hypothesis of Harvey have obtained credit, had this argument been really valid ? or, how shall we dispose of it, if really

false, without fair discussion of its merits? I complain, however, that the discussion is not conducted according to the common laws of evidence, when the Harveians require us to believe in the astonishing contractile power of arteries, which is never seen, and no experiment tends to show that it exists—and that when the aorta is tied, anastomosing branches are immediately ready to convey red blood to the lower extremities, to be returned by the veins. In these instances, I am required to believe in what cannot be demonstrated, but is merely supposed to exist, because, without such suppositions, the Harveian hypothesis cannot be maintained. When, on the other hand, I show that a little blood thrown into the carotids, under circumstances favourable for its easy transmission, renders the countenance livid, and say, that I believe the exactly similar appearance of apoplectic patients is also produced by blood thrown into the carotids—the conclusion is denied, because it is necessary to the circular hypothesis, that the arteries should be always full of blood. I

can easily believe that my arguments will have no weight with those, who on the one hand extend their faith to what evades or contradicts our senses, and on the other, refuse their assent to conclusions drawn from phænomena evident to all: but to act thus, is to do violence to the cause of truth, which sooner or later must prevail. From what I have already seen, I am confident that the subject will excite enquiry, experiments will be tried, and conclusions will be formed, of which the public will judge, whether they be the fair deductions of sound argument. The rising generation having few prejudices to overcome, will enter upon the enquiry with less reluctance than those, who must, upon the supposition that the Harveian doctrine is unfounded, abandon in their advanced age, what they were taught in their youth.

Quæ

Imberbes didicere, senes perdenda fateri.

Thus I augur of the progress of discussion, from the manner in which the first sketch

upon this subject has been received, and the assurances of some who rank high in our profession, that they believe with me that the circulation of red blood is a mere fiction. Whether you are to be prevailed upon to alter your opinions, I shall not pretend to foretell; but I continue to address this second edition to you, convinced that you will allow me the credit of having stated my objections *bona fide*, and believing them true. When about six years ago I pointed out to you a passage in Paulus Ægineta, particularly describing your improved operation for aneurism, you received the information with all the candour that distinguishes your character, observing that, although the mode of performing the operation was a discovery to you, you should, in future, believe the words of the wise man, that *there is no new thing under the sun*. It will be no *new* thing if again we recur to ancient physiology, finding the inconsistencies of the modern doctrines irreconcilable and endless, and that, bewildered in their mazes, we wander on still more and

more astray. That truth may be lost sight of, and that for a long time, is very true ; but it remains unchanged in its nature ; and while false hypotheses are at all points vulnerable, truth must gain something from every discussion fairly conducted, and must necessarily, at length, prove the πανδαμάτωρ, subjecting all things to itself.

Confident that time and truth will fairly determine the question I have agitated, I content myself with having brought forward some objections that to me seem insurmountable to the doctrines of Harvey, having good reason to believe that what I have suggested will be followed up by others in a manner better suited to the importance of the subject.

I have the honor always to remain,

With great esteem,

MY DEAR SIR,

Yours very sincerely,

GEORGE KERR.

Aberdeen, April 5, 1819.

OBSERVATIONS,

&c.

MORE than three years have now elapsed since I offered, for the consideration of some gentlemen of the profession, what appeared to me to be serious and insuperable objections to Harvey's hypothesis of the circulation of the blood ; and having received no satisfactory answers to these objections, I soon after submitted my *observations* to the public, just as they were first written out for the inspection of my friends. My object was to call the attention of the profession to the subject—to profit by correction of any fact I might have

mis-stated, or of any error I might have fallen into in drawing conclusions otherwise than according to the rules of sound reasoning. Publication I found was absolutely necessary ; for my correspondents who defended Harvey's hypothesis, seemed to think it sufficient to repeat, from Fleming's physiology, or the text books generally used in our medical schools, the commonly received doctrine of the circulation, without considering that many of the objections I stated had never occurred to Harvey himself, nor, in as far as I know, to any of the supporters of his doctrines. By printing my observations, the ground was so far cleared ; and as all that I have seen urged against my objections, does not, as far as I can judge, invalidate them, while many facts have come to my knowledge tending to prove them well founded, and these facts supported by the most unexceptionable authorities, I am induced again to bring the question before the profession and the public.

That the physicians of ancient Greece

were educated with great care, that many of them were men of great talents, and these talents improved by close and unwearied attention to professional studies, is universally acknowledged. A physician amongst the ancient Greeks, was a man of general knowledge; and Galen has written a book to show that a good physician ought also to be a philosopher. He also describes particularly the natural and acquired qualifications that may enable a student to commence his professional studies and duties with good hopes of success, and the description well deserves our attention. It is given in the words of Victor Trincavellius, and the original is subjoined in a note.¹

¹ Πρῶτον μὲν ὁξεῖα φύσις ὥσπερ ὅπερ ἂν ἐκδιδάσκηται μάθημα λογικὸν ἐτοίμως ἔπεσθαι.—δεύτερον δὲ ἢ ἐκ τῆς παιδικῆς ἡλικίας ἀγωγή τε καὶ ἀσκησις, ὡς ἐν τοῖς πρῶτοις γενέσθαι μαθήμασι, μάλιστα δ' αὐτὸν ἐν ἀριθμητικῇ τε καὶ γεωμετρίᾳ γυμνάσασθαι δεῖ, καθάπερ καὶ Πλάτων συμβουλεύει—τρίτον ἐπὶ τούτοις ἅπασιν ὑποσχεῖν τὰ ὅσα τοῖς κατὰ τὸν ἑαυτοῦ χρόνον ἀρίστοις εἶναι δοκοῦσιν.—Εἴτα τέταρτον, αὐτὸν εἶναι φιλοπονότατον, ὡς μηδὲν, μηδὲ ἡμέρας μὴτε νυκτὸς ἐκμελετᾶν ἄλλο πλὴν τῶν μαθημάτων.—Εἴτα πέμπτον ὅπερ ὀλιγίστοις ὑπῆρξεν ἀληθείας ὀρεχθῆναι, καὶ τοῦτο

The student must be "*primum* natura perspicax, ut edoctus facile quamcunque rationalem disciplinam consequatur. *Alterum*, educatio exercitatioque, a teneris annis, in primis disciplinis, in primis vero, operæ pretium fecerit si juxta Platonis preceptum, se in Geometria et Arithmetica, exercuerit. Ad hæc *tertium* quibusque optimis sua tempestate habitis preceptoribus se auditorem præstitisse. *Quartum*, laboris eo esse patientissimum, ut diu nocteque nihil aliud commentetur, quam quæ ad disciplinas spectent. *Quintum*, quod paucissimis datum

σπουδάσαι μόνον ἐν ᾧ παντὶ τῷ βίῳ καταφρονήσαντα τῶν ἄλλων ἀπάντων ἢ τοῖς ἄλλοις διεσπούδασται.—Πρὸς τούτοις ἐκτὸν ἐκμαθεῖν τινα μέθοδον ἣ διακρίνεται τὸ ἀληθές τε καὶ τὸ ψεῦδος. Οὐ γὰρ δὴ ἀποχρήσει γε μόνον εἰς τὴν εὕρεσιν ὧν ζητοῦμεν ἐπιθυμήσαι τῆς ἀληθείας, ἀλλὰ χρὴ καὶ δυνάμιν τινα τῆς εὕρεσεως πορίσασθαι. Ἐβδομον ἐπὶ τούτοις ἅπασιν ἀσκήσαι τὴν μέθοδον, ὥς μὴ γιγνώσκειν μόνον ἀλλὰ κεχεῖσθαι δύνασθαι. Εἰ γὰρ δὴ τοῖς ῥήτορσιν ἐλάττονα τέχνην μετερχομένοις οὐχ ἱκανὸν εἶναι δοκεῖ τὸ γινῶναι τὴν μέθοδον, ἀλλ' ἐν ᾧ παντὶ τῷ βίῳ τὴν ἀσκήσιν ἑαυτοῖς μεταχειρίζονται, πολὺ μᾶλλον τοῖς δ' οὕτω μέγαλα ζητοῦσιν οὐκ ἀποχρήσει μόνως ἐκμαθεῖν τὴν μέθοδον; Εἰ μὲν οὖν ἐν τι τῶν εἰρημένων ἐνδέοι τῷ καθηγουμένῳ τῆς ἐπὶ τὴν ἀληθειάν ὁδοῦ, δίκαιόν ἐστι μὴ πάνυ τι τυχεῖν ἐλπίζειν ὧν ἐφίεται. Εἰ δὲ ἅπαντα ὑπάρχει, τί κωλύει ζητεῖν τὸ ἀληθές ἐπ' ἐλπίσιν ἀγαθαῖς;

est, veritatis amore esse captum; soloque illius studio toto vitæ curriculo teneri, cæteris omnibus neglectis quæ à quampluribus aliorum multifieri solent. *Sextum*, aliquam præterea methodum, qua verum a falso discernatur didicisse. Neque ad eorum inventionem quæ inquirenda nobis proposita sunt, satis fuerit veritatis desiderio affici, nisi etiam viam aliquam eam inveniendi expeditam habeat. *Septimum*, insuper eam methodum ita jugi exercitio tritam habere, ut non modo norit, sed etiam valeat. Porro si oratores, qui in arte longe minori versantur, non sibi satis esse methodum novisse putant, nisi illam assidua exercitatione, per totam coluerint ætatem; quanto minus iis, qui magna adeo sectantur, methodum *didicisse* sufficiet? Itaque si vel unum horum quæ recensuimus in illo desideretur, qui ad viam veritatis introducendus est, non erit admodum sperandum illum posse sui voti fieri competentem. Si vero omnia cumulata adsint, quid deinde vetat quo minus *veritatem*, etiam bona spe exquiramus?"

We find Hippocrates enjoining his son

Thessalus to study geometry and arithmetic, as the exercise would not only be useful and creditable in the concerns of life, but as it would increase the activity of the mind in the prosecution of medical studies: but it must be admitted, that at the present day we are not over nice in the selection of youth naturally well qualified to become physicians, nor is the course of study severe. It is notorious, that in many instances physicians are *created* without any previous study; and in our most fashionable school of medicine, whoever can contrive to get through a mock examination, and get a pamphlet published in Latin on any medical subject, may, whatever his talents or acquirements be, obtain the highest testimony the learned body have it in their power to bestow, of his being amply qualified to practise, and even *teach* the healing art.

It is not a little extraordinary, that while we admit the superiority of all the works of art that have descended to us, through a long succession of years; and notwithstanding the

decay of learning, and neglect of the Greek language, are still sensible of the excellence of the poets, historians, orators, and mathematicians, who flourished two thousand years ago, we should yet consider the works of the physicians of antiquity as unworthy of our careful study. It appears to be a prevailing opinion, that the medical men of the present day, are possessed of all the knowledge of antiquity, to which they are daily superadding great and valuable improvements, so that the profession is now entitled to rank higher than at any former period. The ancient writers are very nearly forgotten, or mentioned with a sort of pity bordering upon contempt, whenever a Lecturer is about to apprise his pupils of their good fortune, in coming into the world during the present enlightened age. In the mean time, one very strong circumstance passes unnoticed, that with all our supposed advantages, the ancient physicians and surgeons cured diseases, and remedied accidents, much as we do at present, or by approved means which we have allowed to fall into *desuetude*. The invention of the ligature,

for the purpose of repressing hæmorrhage from arteries, has been attributed by Lecturers of the present day to Ambrose Paré ; but its use was particularly described by Archigenes and Heliodorus, two of the most ancient writers on Surgery ; and the cure of Aneurism by double ligature, is exactly described by Paulus Ægineta.

It is commonly repeated, that the ancients were ignorant of the distinction between arteries and veins ; but the assertion is altogether unfounded : they practised Arteriotomy in many cases, and tied the Vena Saphena for the cure of varices of the leg, as has been done in modern times ; but regarded the operation as too painful and dangerous to be generally practised. Galen informs us, that he was relieved from severe indisposition, by opening the artery between the thumb and forefinger, and taking away two pounds of blood, which he might easily have taken from the superficial veins ; and he very minutely describes the anatomy of veins, their coats and structure. In the veins, the ancient phy-

sicians believed the blood of the body to be contained, with the exception of a very small quantity diffused in a subtile vapor, which, in the healthy body, fills the arteries. Such was the opinion of Galen ; but an older physician of great name, *Erasistratus*, maintained that blood is not naturally present in the arteries ; but by diseases, wounds, or violently disordered respiration, may be derived into them ; and although his writings are lost, the account of his tenets given by Galen, while he attempts to refute them, is so complete, that we can form a very clear idea of the dispute at the present day.

The opinion of Erasistratus was, that the vital spirits, collected by means of the lungs from the atmosphere, were delivered to the heart, and by it transmitted through the whole arterial system. He believed that when arteries are wounded, the vital spirits escape, and the blood passes into the left side of the heart to supply the place ; that the extreme branches of arteries and veins do not generally inosculate ; and that fever or partial inflammation, ac-

according to circumstances, are the consequences of the presence of blood in the arterial system.

To this doctrine Galen objected—first, that there was no proof of the escape of vital spirits from a wounded artery, because we never see them so escaping ; and secondly, that according to the idea of Erasistratus,* the

* It has been asserted, that Erasistratus generally condemned blood-letting, because he believed that abstinence, fresh air, and cold water, would in every case render it unnecessary to men living temperately. In other words, he is said to have been a Pythagorean as to medical practice, trusting every thing, as Lewis Cornaro did, to temperance, and a well regulated life.

La preferenza poi che la medicina de Pitagorici dava al regolamento del vitto sopra tutti gli altri rimedi, fa molto stimare la loro sagacità, a chiunque sa con quante tediose esperienze s' arriva al fine a QUELLA NOBILE INCREDULITA SULLA VIRTU DELLE DROGHE, CHE SUOL DISTINGUERE ALCUNI POCHI MEDICI DA MOLTI E VULGARI.—Such is the praise bestowed upon the Pythagorean practice by a very learned physician of the last century.—*Cocchi, del Vitto Pittagorico.*

The same author informs us, that Pythagoras reckoned *three* fluids in the human body ; that he distinguished them “ secondo la differenza della loro densità, sangue, acqua, o siero, o linfa, e vapore.”

whole vital spirits must be exhausted, before the blood can come to supply their place, and this we know to be inconsistent with the fact. As Erasistratus was long dead before Galen wrote, and his followers, accused of some errors in practice, had fallen into disrepute, we can only conjecture what reply he would have made had he been alive to the objections of his antagonist. Although Erasistratus denied the existence of blood in the arteries in the healthy state, he would probably have said, that he never denied the existence in them of an elastic fluid, of which the blood forms a component, although small part; and that therefore the dispute was merely verbal. Indeed, with many great qualities, it is not to be denied, that Galen, who professed so much regard for truth, and enjoined his pupils to prize it above all things during the whole course of their lives, was vainly ambitious, wishing to be considered as the founder of a sect; and often uses language against his antagonists unwarrantably gross, calling them "asses," with other expressions of contempt,

altogether unbecoming a philosopher. Yet, he never believed, as our modern Physiologists profess to do, that the arteries are naturally full of blood;—his opinions very nearly coincided with those entertained at a later period by Servetus,* who, although he has been said to have made a near approach to the discovery of the circulation, yet plainly shows in the quotation below, that he never would have given his assent to the hypothesis, which is altogether inconsistent with his physiology.

The question agitated by Galen, whether blood be naturally present in the arteries, has long ceased to occupy the attention of medical men, as the experiments and writings of Harvey were supposed to decide the question,

* At sane videtur Galenus ambitiose et animose, libros de Venæ sectione (the book *an sanguis natura sit in arteriis* may be added) scripsisse, annum agens trigesimum quartum, cum videret Erasistratum in omnibus medicinæ partibus tanquam virum admirabilem prædicari. Eo modo et Archigenem insectatus est, ut famam ejus deprimendo suam magis extolleret.

Riolanus.

and prove that the arteries in the healthy state are always full of blood ; but many facts appear irreconcilable with this hypothesis, which for about two hundred years has obtained credit, confounding all medical theory, and rendering physiology utterly incomprehensible.

Harvey's hypothesis rests chiefly upon these facts :

1. That wounded arteries pour out blood, which is evidently derived from the heart.

2. That when a ligature is applied to an artery, the vessel swells on the side of the ligature next to the heart, and the pulse ceases in the part of the vessel between the ligature and the extremity.

3. That the valves of the veins seem so disposed as to facilitate the passage of venous blood towards the heart, and prevent its reflux towards the extremities ;—and upon the

application of a ligature, the vein swells between that ligature and the extremity.

The systole of the left ventricle he attributes to the contractile power of the left ventricle, exerted when it is distended with blood; and the diastole of the arteries, he says, is occasioned by a suddenly transmitted *unda* of blood, pervading the arterial system. He admits, that the pulse of arteries is not progressive, and that the beat of the heart and extreme arteries, exactly coincide *quasi trajecto fulgure*; at least, this, he says, is the case in many animals; and he owns that for a long time, he believed the cause of the motion of the heart and arteries was fully comprehended by God alone. After many experiments however, and long study, he says, that he believed himself master of the subject, and published his opinions, that the world might judge of them, and that he might openly reply to some who had received these opinions *iniquo animo*, and had attempted to injure his reputation.

It has been said that no physician, upwards of forty years of age, when these opinions were promulgated, was convinced by them; but however that may be, they met with very little opposition, if we may judge from the writings of the medical practitioners of his time; and from the fact, that before the death of Harvey his Hypothesis was generally received as true. Riolan, Primerose, and some others, wrote against it; but Bacon, about the same time, having pretended to discover a new mode of philosophizing, and the wretched pedant James, affecting to believe that he had a chancellor superior in philosophy to Plato and Aristotle, and a physician of better talents than Hippocrâtes or Galen, the belief in the circulation became a court fashion. It was never suspected, that the experiments of Harvey might be inconclusive, and his reasoning upon them fallacious; and from his time we may date the general disregard of the writings of the antient physicians; for no student would take up his time in reading the works of men ignorant of the circulation, and

the consequence has been, a succession of fugitive theories in physiology, and disease that baffles all description.

It will be allowed that such a discovery as that claimed by Harvey, if really true, ought to have simplified medical practice, and afforded more distinct and correct views of the animal œconomy, and better means of curing disease. We ought to have been able to point out the certainty derived from the discovery of a most important fact; and if we really had so far exceeded the antients in accuracy of physiological research, a corresponding improvement in pathology was to be expected as a necessary consequence. A careful inspection of the antient authors will, however, teach us, that the cure of diseases was as well understood in their times as now, the necessary allowance being made for the different habits of men, which in some instances have introduced diseases supposed to have been unknown to the antients, or have so altered the symptoms of others that very differ-

ent treatment has become necessary. In many cases it can be shown, that after the time of Harvey, we entirely lost sight of approved methods of cure in daily use among the antients; to some of which, we are now recurring as modern improvements—as, the cure of insanity, by hellebore; of gout, by colchicum; the application of cold water, in certain cases of fever; the use of the ligature, to secure the principal vessels of a limb before amputation; and the gradual division of the soft parts, to allow them to retract during the operation.'

¹ Ἀποβροχιστίον οὖν ἢ διαρρήπτιον τὰ φέροντα τῶν ἀγγείων ἐπὶ τὴν κοπὴν, καὶ διαδετίον ἐπὶ τινῶν πᾶν μέρος, ψυχρῶ τε προσαιοητίον, ἐπίουσι δὲ καὶ φλεβοτομητίον.

Archigenes, de Amputatione, apud Nicetam.

Ἐμοὶ μὲν οὖν κειόσπουδον τάχος ἐπιτηδεύοντες κατὰ μίαν ἐπιβολὴν ὅλα τὰ σώματα ἐπιχαράσσουσιν, ἔπειτα ἐκπρίζουσιν τὰ ὅστια. Οὐ γίνεται δὲ ἀκίνδυνος ἡ τοιαύτη ἀφαίρεσις. . . . Διὸ ΜΟΙ δοκεῖ τὰ ἀσαρκότερα μέρη τοῦ κάλου τρότερον διελεῖν, ὥς κατὰ τὸ ἀντικινήμιον ἔπειτα πρίζειν, καὶ κατὰ τὴν τῶν ὀστέων πρίσιν, τὰ λοιπὰ σώματα διακόπτειν.—κ. τ. λ.

Heliodorus, apud eundem.

In short, the practice of the Greek physicians appears to have proceeded upon full information and sound reasoning; and they appear to have been fully as successful, while they believed the veins to be the only proper blood vessels, as we now are, while we pity their ignorance of the circulation.

That Harvey instituted many experiments, in order satisfactorily to prove his Hypothesis, is true; for he opened a great number of living animals, and was upon the whole a pains-taking anatomist. But from these experiments and demonstrations, he establishes no one fact of which the ancients were ignorant; but founds his claim to discovery upon conclusions drawn from the experiments which do not appear to be deductions of fair reasoning. That upon the application of a ligature to a limb, the veins swell between that ligature and the extremity; that the course of the blood poured out from a wounded artery is from the heart; and of the position and structure of the valves of

the great vessels :—of these the antients had perfect knowledge. And yet, upon these circumstances, Harvey rests his system; having recourse, for the most part, to reasoning from probabilities; and overlooking entirely, as far as we can judge from his writings, the most formidable objections to his doctrine.

Thus he accounts for the empty state of the arteries after death, by the assumption, that at the approach of death, when the extreme vessels are losing their action, and the limbs turning cold; yet, at the very last moment, the *nisus* of the heart and vessels is sufficient to throw the whole contents of the arterial system into the veins.

But the veins are not found generally distended after death; on the contrary, they shrink in many parts of the body, an accumulation taking place about the heart: and in capacity they are not equal to receive, in addition to their usual contents, a quantity of

blood sufficient to fill the arterial system; which, in a full-grown man, is by some physiologists calculated at four pounds and a half.

Harvey supposes that the antient physicians were deceived into the belief that arteries do not naturally contain blood, by observing their empty state after death; which he thus attempts to account for, concluding with an assertion which shews that he was not quite so much enamoured of *truth*, as Galen wished his pupils to be :

“*Vacuitas arteriarum in corporibus mortuis* (quæ forsitan imposuit Erasistrato ut arterias spiritus tantum aërios continere existimaret), inde evenit, quod quando subsident meatibus occlusis pulmones non ulterius respirent, et per ipsos sanguis libere non potest transire : perseverat tamen per temporis spatium cor in expellendo ; unde et sinistra cordis auricula contractior et ventriculus ; pariterque arteriæ inanitæ et (non sanguinis successione repletæ), vacuæ apparent. Sin simul

cor pulsare cessaverit, et pulmones respirando transitum præbere, ut in iis qui aqua frigida submersi suffocantur, aut syncope et morte subitanea extinguuntur, pariter repletas venas et arterias reperies." In this short passage much is assumed that is not well founded, and that is asserted which is really false. Harvey had no right to suppose for a moment that Erasistratus, whose dissections of the living bodies of criminals amounted to some hundreds, and who expressly denied the presence of blood in the arteries, unless forced into them by wounds or disease, was imposed upon by the common appearances after death. And his reasoning, by which he attempts to account for the empty state of the arteries after death, is altogether inconclusive : for he first assumes that the pulse is really "*impulsus sanguinis per arterias*," and the presence of the blood returned from the lungs, the real cause of the distension and consequent contraction of the left ventricle ; and then, after the lungs, according to him, afford no supply of blood to the left side of the heart, "*perseverat tamen*

per temporis spatium cor in expellendo." By his own statement, blood is not supplied to the heart; and what can possibly *expel* it from the arteries, which he admits are found empty? He admits no other agent than the blood, and yet he supposes an effect produced in the evacuation of the arteries when that blood is no longer present, and can neither stimulate the heart nor arteries to action.

In those killed by submersion in cold water, it is true that some considerable quantity of blood, varying according to circumstances, is found in the left side of the heart and arterial system; but the arteries and veins are never, in such cases, equally full of blood; and in cases of death by syncope, lightning, or the effects of a noxious *gas*, the arteries are found empty, as usual. All anatomists of respectability agree in this, that wounds of considerable arteries, disease which enlarge their cavity, or that violent disorder of respiration, occasioned by suffocation, leave blood in the arteries after death; as also, that mal-conformation

of the heart, which occasions a livid countenance during life : but in cases of sudden death from common accidents, the arteries are found empty. In this instance it is clear that Harvey made an unfounded assertion in aid of his Hypothesis. He does not, indeed, say that he really found in common cases of sudden death, the arteries equally full of blood with the veins ; but he tells his reader that *he* will find it so ; an assertion altogether contrary to the fact : for, unless in the cases mentioned, the blood is never caught as if *in transitu* through the arteries ; and even then they are but very partially filled.

Another objection to the Hypothesis is this : that although the red globules of blood are said to have been distinctly seen by many, no anatomist has yet discovered the anastomoses of the terminating arteries with incipient veins, by which these red globules should pass.¹

¹ Mr. Abernethy says, that such anastomoses have been seen ; but adds, that before they can be exhibited, the body must be in a state of incipient putrefaction.

That veins may be injected from arteries, is true; but it is also true, that the vessels, immediately after death, become incapable of containing their proper fluids: and it is not at all wonderful that a warm injection, propelled with some considerable force, should by transudation pass into the veins. Indeed, Mr. Cruikshank particularly mentions, that he frequently injected arteries, veins, and lymphatics, at one and the same time.

It appears neither to have occurred to Harvey, nor to his zealous advocate Walæus, that, according to their new doctrine, a great, and seemingly insuperable difficulty, occurs in accounting for a regularly continued circulation in limbs, (*e. g.* the lower extremity,) when the femoral artery has been obliterated for the cure of aneurism, or the saphena, on account of varices. In cases of the amputation of the thigh, the femoral artery pulsates strongly; and if left unsecured, would, in less than two minutes, occasion death from loss of blood. According to Harvey's doctrine, with what-

ever rapidity the blood is thrown into the vessel, it must be, by means equally expeditious, returned to the heart; but no vein that possibly can return the blood which presses upon the ligature, exists, and regurgitation upon the heart is supposed impossible, on account of the valves at the origin of the aorta. We are told that anastomoses supply the functions of the returning vessels in their natural state; but if Harvey never could trace anastomoses in the more minute arteries and veins, none of his followers pretend to exhibit them in large trunks. And upon the dissection of stumps, many years after the operation, not the slightest connexion between the visible arteries and veins is to be discovered; the femoral artery being just as empty of blood, as if all the trunks of veins, which, according to the Harveian doctrine, ought to return the blood to the heart, had been in a perfect state.

Again, upon the supposition that the pulse ($\sigma\phi\upsilon\gamma\mu\omicron\varsigma$) is really occasioned by the progress

of a portion of blood propelled at certain intervals through the left ventricle of the heart ; the question occurs, why is the beat of the heart, and that of the most distant pulsating arteries, exactly synchronous ? The motion of a fluid propelled through elastic tubes, in diameter such as the arteries, and by a force much greater than that exerted by the heart, is still progressive ; as may be ascertained by the fingers applied, and also by the eye : whereas the natural pulse of warm-blooded animals is transmitted instantaneously, and altogether, as Harvey observes, “ *quasi trajecto fulgure.*”¹

Another difficulty occurs in accounting for the velocity with which the blood is supposed to pass through the arteries and veins.—According to the Harveian doctrine, the blood is

¹ Motus itaque cordis omnino ad hunc se habet modum, et una actio cordis est, ipsa sanguinis transfusio, et in extrema usque mediantibus arteriis propulsio ; ut pulsus, quem nos sentimus in arteriis, nil nisi sanguinis a corde impulsus sit.—*De Motu Cordis. Cap. V.*

in continual motion through the heart, and through the extremities ; and as the veins are considerably larger than their corresponding arteries, it follows that in them the motion must be slower ; and as the velocity of fluids in rigid tubes is, *cæteris paribus*, in the inverse ratio of the squares of their diameters, we may suppose the velocity of blood in the veins to be, to that with which it passes through the arteries, as *one* to *three*. But by the calculation of Keill and others, the velocity of blood issuing from a wounded artery, is not less than ten times as great as that with which it issues from a vein of nearly the same size ; and the truth is, that supposing the pulse to be, as Harvey says, *impulsus sanguinis in arteriis*, the velocity of the blood in the arteries is not to be measured ; for, like the electric shock, it is instantaneous, while the motion of the venous blood is so slow, that no means of ascertaining what it really is have been devised, while the vessels remain entire ; and when vessels are divided, the velocity with which their contents issue, affords no informa-

tion what that velocity really is in the natural state. Harvey supposes the blood propelled from the left ventricle of the heart and aorta, to be received from the right ventricle and cava, and the diameters of the two great vessels ought to furnish the data whereby to compute the velocity of the contained blood ; but as general and well established principles are here found inapplicable, a strong presumption immediately arises, that the blood does not circulate in the manner described by Harvey. It has been said, that but a part of the red blood propelled by the arteries, is returned by the veins ; but Harvey supposes the whole quantity returned ; and indeed it is very doubtful how far the blood is concerned in nutrition and secretion, although it is essential to life ; for the fact is admitted by the most respectable authors, that, after death has been occasioned by actual starvation, or the slow wasting of phthisis pulmonalis, the quantity of red blood contained in the veins is nearly the same as when a healthy man suffers death by a sudden accident.

Then what becomes of the arterial blood transmitted to a limb, when the principal vein of that limb is tied? This objection had been proposed to Walæus, and it was asked why, upon tying the arm, in order to perform blood-letting, when the veins are compressed, and the arteries continue to perform their office, a great congestion of blood does not take place below the ligature; whereas, in fact, the veins rise as completely within a few seconds, as after so many minutes; and if the ligature be kept on for a considerable time, they actually subside, the pulse still going on? —To this he replies, that *it may be* the veins have not been completely closed by the ligature, and that by the small openings still left, the blood may escape from the extremity: but if the vein, say the saphena, has been taken up with two ligatures, and divided, as in varicose cases, what happens then? To this, too, it will be answered, that the circulation would be carried on by anastomosing branches. But although this is merely beg-

ging the question, because the principal channel by which blood is said to be conveyed from the limb being cut off, there is no provision made by nature discoverable by our senses, to remedy the defect immediately—let the assumption be supposed true, and another case of even greater difficulty comes to be accounted for. Suppose the axillary or subclavian artery obliterated, where no branch has yet been given off to supply the arm; how happens it that, even in this case, when, if we believe in the evidence of our senses, not a drop of arterial blood can reach the hand, the veins on the back of it, and up the arm, remain turgid? According to the Harveian hypothesis, the heart is every instant exhausting the blood from the veins, and in an arm in this condition, the venous blood ought to have disappeared as soon as no further supply could be obtained from the arteries: yet, that this does not happen, is proved by experience, for the veins of the arm have remained turgid for months after the artery was obliterated.

ated at the axilla, and till death, although the limb remained cold all the while.*

It has already been said, that no anatomist has been able to show the inosculation of the extreme arteries with veins, while the structure of the body is not affected by disease ;

* *Extract of a Letter from Dr. Barclay.*

“ EDINBURGH, Jan. 26, 1815.

“ The case of aneurism, to which you refer, and of which I have still the preparation, was in the left subclavian ; the patient had complained of it twelve months before I saw it, and, about three weeks after I saw it, it had disappeared. The arm was cold from the commencement, (*period of obliteration,*) and till death, but an additional covering of flannel prevented any uneasiness from that cause. He survived the obliteration of the subclavian eighteen months, if not two years, and, for a great portion of that time, enjoyed tolerable health. With regard to your question concerning the veins of the arm, I was not prepared to give any answer till last evening ; I had only thought of the arteries, and ascertained that there was no pulsation at the wrist. I thank you, however, for the question ; it is highly proper and interesting, and will be of use to me in my future observations on similar cases. The

nor can a single instance be adduced, of any direct communication formed between newly produced arteries, and the veins that are supposed to convey off the blood from the obliterated artery in its yet healthy state. It will not be said, that when the iliac is tied, any

answer to it was sent me yesterday, in writing, by Mr. Allan Walker, an intelligent and observing Surgeon, who regularly attended, and was the first who pointed out to me, and procured me the preparation."

"I shall copy his Note verbatim :—*Dear Sir—*
 'With regard to the state of the vessels in that case of
 'aneurism, to which you were alluding this morning,
 'the veins on the back of the hand were very turgid.

Yours truly,

South Bridge Street.

A. B. WALKER."

"I was very anxious to have had the whole arm, to have injected it, but to obtain it was impracticable.—My own idea is, from a beautiful preparation in my own possession, from those which I have seen in the possession of others, and from what I have read in Scarpa, and others, that no limb is preserved, unless the circulation of even red blood be continued in the lateral vessels, which are always enlarged, and often rendered tortuous, from encreased impetus."

arterial blood can reach the toes, and yet the veins of the foot and leg contain blood, as if no obstacle had intervened; the limb only turns cold, as we may infer from the case given by Dr. Barclay.

In certain cases published by Mr. Astley Cooper, in the Transactions of the Medical and Chirurgical Society of London, accompanied with engravings, he particularly describes the enlarged and new branches formed after the obliteration of the main artery. In some of the patients, who lived for years after the operation for aneurism had been performed high up in the thigh, the engravings represent the arteries as terminating near the middle of the leg; but none appear to have reached the foot. In these cases Mr. Cooper appears to have paid no attention to the state of the veins, nor to have suspected that the appearances were not to be reconciled with the hypothesis of the circulation of the blood. But when Mr. Cooper tied the descending aorta of a dog, a little below the heart, where not an

arterial twig had been given off, that could possibly reach the lower extremities, the dog suffering apparently little uneasiness, and living for more than a year, when he was killed, in order that the state of the vessel might be observed, it is really wonderful that the insuperable difficulty of reconciling the phenomena to Harvey's hypothesis, did not occur to the experimentalist. According to that hypothesis, the venous blood must have been exhausted from the lower extremities, by the action of the right auricle and ventricle, and none could possibly be supplied by arteries. How happens it that the dog did not die of suffocation in a few minutes, the blood of the lower extremities being accumulated about the heart and vital organs? Dr. Parry, one of our first physiologists, who has particularly described and exhibited in engravings, the regeneration of arteries after some months, admitted, that he could not account for the difficulty I had stated, and why the dog appeared to suffer very little inconvenience immediately after the application of the ligature.

He was too ingenuous to talk of anastomosing branches which no eye hath seen, which, at once, may supply the place of large trunks, and carry on the *circulation* ; and had he lived, I have some reason to think, that he would have favored the world with his sentiments on the subject, and an account of the appearances upon dissection within a few days after such a ligature had been applied. Dr. Parry was the first to demonstrate newly formed arterial branches, passing over the part obliterated by ligature, and again inserting themselves into the main trunk. According to his experiments, this renewed communication took place in the carotid of a ram, within thirteen months after the ligature was applied ; but were red blood naturally conveyed by the arteries, how could the circulation be carried on immediately after the application of the ligature ? If the answer be, by *anastomosing branches*, where is the need for new arterial branches, which are certainly produced ?

Were we to admit the doctrine of anastomo-

ses to the extent the supporters of the Harveian hypothesis insist upon, we must concede that the contents of large vessels may be conveyed by channels not evident to our senses, and that small arteries given off from the main trunk of the descending aorta nearly at right angles, and apparently of very short course, may transmit the whole blood, contained in this great vessel, to the lower extremities, as soon as the trunk is tied, from which it is returned as if nothing had happened. Of this supposition I can only say, that it appears to me utterly unworthy of serious consideration; and if, in the dead subject, when fresh and warmed to the natural temperament of life, the finest injection pushed against a ligature of the descending aorta, will exhibit no traces of vessels that by any possibility could supply blood to the lower extremities, the presumption evidently is, that no such vessels exist.

But modern physiologists appear to think themselves bound to reconcile all phænomena to the Harveian hypothesis, and to *suppose* all

kinds of adminicles to support its probability. Haller, a most honorable man, and pains-taking experimentalist, tells us, that upon tying an artery, the part next to the heart swells, and that beyond the ligature *discharges itself of its contents*, although he professes to believe, and certainly did believe, the doctrine of Harvey and Walæus, who referred the pulse entirely to the action of the heart, the propelling power altogether depending upon the *vis a tergo*.

When the artery is compressed by a ligature firmly drawn, its communication is, for the time, cut off from the source of life and motion; but, because in the dead body, and between ligatures and the extremities, arteries are found empty, a wonderful contractile power is imagined, which even in *articulo mortis*, and after the application of ligatures, propels their contents, and leaves not a drop of arterial blood to be seen. It is, however, of consequence, to examine more particularly the opinions of Haller on this subject, be-

cause, after many experiments, amounting, as he tells us, to more than two hundred, performed upon both living and dead animals, although he qualified some of the opinions stated in his work upon physiology, he yet continued to believe in the reality of the circulation.

“D’abord je me suis assuré,” says he,* “que le sang, poussé par le cœur, dilate les artères, et forme ce battement, qu’on appelle le pouls.” Believing then that the blood propelled by the heart forms that beat which we call the pulse, he appears to overlook the fact that, after the action of the heart has ceased, blood propelled by it can no longer produce any effect in the arteries; nor could these vessels ever be found empty, were the blood propelled by the heart, the cause of the pulse, because the flow of blood from the heart being supposed constant, while life endures

* Deux Mémoires sur le Mouvement du Sang.

Lausanne, 1756, pp. 33, et seq.

“*undā superveniente undam.*” The last pulsation of the heart must of necessity leave the arteries full of blood. He also passes unnoticed the fact, that the pulse of the heart and extreme vessels is exactly synchronous—a fact irreconcilable with the progressive motion of blood, by whatever force propelled in elastic tubes; and particularly, as it is admitted that the sum of the diameters of arterial branches, exceeds considerably that of the aorta. He, however, differs from most physiologists of his time, and contends, that the velocity of the blood is not diminished in the smaller arteries—a position not easily to be maintained: but to this conclusion he says he was led, by observing that, after passing through an aneurismal sac, the blood recovers its original velocity. Why the observation should have led to the conclusion just mentioned, is not very apparent, although he speaks of it as of great importance, and as destroying almost entirely the doctrine maintained by Keill and others, of the gradual retardation of blood in the arteries.

But as Haller admits that the sum of the diameters of the arterial branches is greater than that of the aorta, he cannot possibly suppose or maintain that fluids move with equal velocity through wide and contracted channels, the quantity being always supposed equal; because the supposition is in direct opposition to fixed principles. His experiments were principally performed upon fishes and frogs, and comparatively few on warm-blooded animals. He however admits, that there is sometimes an *oscillation* of the blood in the arteries, sometimes a *retrocession*; and sometimes, he says, the arteries are found entirely empty. "This last phænomenon," he says, "may have occasioned the error into which the ancients fell, in believing the arteries to contain only air. It is very certain that they are sometimes entirely empty, although men of great eminence have maintained the contrary."

The contractile power of the arteries, which by many of our physiologists is made to

account for the empty state in which they are found after death, Haller denies : he says that arteries and veins, whether irritated by sharp instruments or caustic substances, undergo no change, nor manifest the least contractile power or irritability.

Farther he says, ossified arteries pulsate, when neither susceptible of dilatation or contraction : “ On vit très long-tems avec presque toutes les artères ossifiées, puisqu’on trouve souvent dans les cadavres une suite de lames osseuses depuis la tête jusqu’au pied, entre la tunique musculéuse et la tunique interne des artères des gens qui pendant leur vie avoient rempli toutes leurs fonctions, et ne s’étoient plaints d’aucune maladie dépendante du dérangement de la circulation.¹ Ces vieillards, dont les artères se sont ossifiées, se sont promenés, *ont eu du pouls*, ont conservé leur chaleur naturelle, et ont fait toutes

¹ Page 139. et seq.

leurs fonctions pendant un tems considérable."

This language is very explicit; and believing the facts stated on authority so highly respectable, we must be convinced that the supposed contractile power of arteries is a mere fiction, invented to help out the received doctrine of the circulation. It is not a little astonishing that, convinced of these facts, we yet find Haller throughout the work quoted, supposing the artery below a ligature to empty itself after the ligature is applied. "Quand on lie une artère, la partie au-dessous de la ligature chasse également dans les veines le sang qu'elle contient; si l'on fait deux ligatures à l'artère, le sang compris entre les deux passe également dans les rameaux voisins."¹

It is true that he mentions these points as

¹ Des Causes du Mouvement du Cœur, p. 137.

ascertained by others, and quotes Drelincourt and Schwenke ; but that he himself, believing in the doctrine of the circulation, must have believed in this extraordinary power, is evident, for there is no other means by which he could account for their being found empty after death. The experiment of applying two ligatures to an artery in the living subject, to ascertain whether blood be contained between, must be performed with great nicety before we can form any conclusion whether blood be naturally contained in the aortic system. If the ligature the most distant from the heart be first tightened, blood will be found ; but if the nearest to that viscus be first drawn close, not a drop. However, when an artery is to be tied in this experiment,—as it must be detached from the surrounding parts, no person of common sense would choose a part giving off branches, so that the blood might escape “ *dans les rameaux voisins.*”

But let us consider for a moment what that contraction must be in degree, and how it

must act in order to expel the whole blood from the arteries. This contraction, in order to expel the blood from the arteries, must be sufficiently strong to bring the parietes of the vessels in contact, and completely extinguish the *lumen arteriosum* ; and it must proceed, whether instantaneously or progressively, from the heart towards the extreme branches. Near the heart, such is the strength of the vessel, and so great its diameter, that a ligature must be drawn with some considerable force before the cavity is obliterated : but if the arteries are to expel their contents when the animal is moribund by their own proper effort, that effort must be sufficient to produce the extinction of the *lumen arteriosum* mentioned, from the heart throughout their whole length, in order to force the blood into the veins. Is it possible to conceive arteries in old age turning rigid, and sometimes, as Haller observes, nearly all ossified, exerting such a force ? No attempts have proved successful to show this supposed contraction of the arteries in *articulo mortis*. The con-

traction of umbilical arteries after the child is separated from the placenta, is the effect of cold, the parietes being weak, and the contents condensed ; and the presence of blood in the arteries of the cord, affords no satisfactory proof of the natural presence of blood in the arteries, when the animal œconomy is altogether different. The experiments upon which J. Hunter founded his doctrine of the irritability of arteries, appear to have been not well contrived, and altogether inconclusive. He does not say that he repeated the experiments of Haller, and with different results, in the living body ; and the contraction or enlargement of parts after death, by different degrees of temperature, by drying, or the application of moisture, prove nothing with regard to the living functions of the parts. Elasticity is no proof of irritability or vitality ; for many animal productions retain their elasticity for years after death, after being pickled, and even boiled. Magendie, a very accurate experimentalist, has pointed out, in several instances, why the experiments of Hunter could not be held conclusive ; and

admitting the contraction and dilatation of arteries, he comes to the same conclusion as Haller. His words are :

“ Mais tout en considérant comme certaines la contraction et la dilatation des artères, je suis loin de penser avec quelques auteurs du siècle dernier, qu’elles dilatent d’elles-mêmes, et qu’elles se contractent à la manière des fibres musculaires : je crois, au contraire, qu’elles sont passives dans les deux cas, c’est-à-dire que leur dilatation et leur reserrement ne sont qu’un simple effet de l’élasticité de leurs parois mise en jeu par le sang que le cœur pousse continuellement dans leur cavité. Il n’y a sous ce rapport aucune différence entre les grosses et les petites artères. *J’ai constaté par des expériences directes, que dans aucun point les artères ne présentent d’indices d’irritabilité ; c’est-à-dire qu’elles restent immobiles sous l’action des instrumens piquans, de caustiques, et du courant galvanique.*”¹

¹ Précis de Physiologie, Tom. II. p. 320.

It has been observed, that Haller made the greatest part of his experiments on fishes and frogs, whose hearts, when compared with the human, are essentially different ; but some he made upon warm-blooded animals, which deserve attention.

He observed, that upon opening a vein, the blood flowed to the aperture both from above and from below ; and as we know that in amputation it is sometimes necessary to secure veins by ligature, it is certain that, in the human body, the course of the blood in a wounded vein is in such cases from the heart. The sudden flow of blood upon slackening the ligature in venesection, he says, is not to be accounted for by any supply immediately derived from the capillary arteries, but from the removal of pressure, which allows the venous column of blood above the ligature to descend.

Modern physiologists enumerate four different terminations of arteries—in veins, exhalants, in cells, and in the excretory ducts of

glands. The ancients believed that both arteries and veins terminate on the surface or in cavities. They believed that perspirable matter is thrown out by the arteries, and that moisture is derived from the surrounding atmosphere by the veins on the surface, and nourishment by those of the stomach and intestines: hence the dictum of Hippocrates, εἰσπνοον καὶ ἔκπνοον ὅλον τὸ σῶμα. Chyle they believed to be absorbed by the veins of the mesentery, whether conveying red blood, or the lacteals, called by them φλέβες λευκαί. Sanguification they believed to take place in the liver; and the secretion of urine, directly from the blood poured from the liver into the cava, by means of the emulgent veins and kidneys.

The recent experiments of Magendie establish in the clearest manner the fact, that urine is secreted, and that abundantly, while the thoracic duct is completely obliterated by ligature; and as it is agreed that the kidneys secrete urine from *blood*, it is a necessary con-

sequence, that blood must be formed otherwise than from chyle supplied by the thoracic duct. This conclusion, which contradicts modern opinions, renders it necessary to enquire after some more rational account of these functions of the animal œconomy ; and it will naturally occur to those interested in the investigation, that the doctrines which prevailed for so many centuries, and until the time of Harvey, deserve to be attentively reconsidered.

Magendie thus describes his experiments :—

1^e *Expérience sur la lymphe.* Un chien a avalé quatre onces d'une décoction de rhubarbe ; une demi-heure ensuite on a extrait la lymphe du canal thoracique. Ce fluide n'a présenté aucune trace de rhubarbe ; et cependant à-peu-près la moitié du liquide avait disparu du canal intestinal, et l'urine contenait sensiblement la rhubarbe.

2^e *Expérience.* On a fait boire à un chien

six onces d'une dissolution de prussiate de potasse dans l'eau ; un quart d'heure après l'urine contenait d'une manière très-apparente le prussiate : la lymphe extraite du canal thoracique n'en présentait point.

3^e *Expérience.* Trois onces d'alcool entendu d'eau furent donnés à un chien ; au bout d'un quart d'heure, le sang de l'animal avait une odeur d'alcool prononcé : la lymphe n'offrait rien de semblable.

4^e & 5^e *Expériences.* Le canal thoracique ayant été lié, et quelques onces de la décoction de noix vomique mises dans l'estomac et le rectum, firent mourir le chien, comme si le canal thoracique avait été ouvert.

Précis de la Physiologie, Tom. II. p. 182.

It being rendered evident by these experiments, that the generally received opinions concerning the secretion of urine are altogether erroneous, Magendie instituted several experiments in order to discover the passages

by which liquids find their way from the stomach into the urinary bladder ; and he ascertained, first, that always when prussiate of potass was injected into the veins, or was made to be absorbed from the alimentary canal, or a serous membrane, it passed immediately into the bladder, where it was easy to discover it mixed with urine. If the quantity injected has been considerable, it may be discovered by re-agents in the blood : but salt is discoverable in whatever proportion in the urine ; so that it is not extraordinary that Darwin and Brande did not discover in the blood substances that were distinctly perceptible in the urine. From these circumstances he concludes :—

“ Quant aux organes qui transportent les liquides de l'estomac et des intestines dans le *système circulatoire*, d'après ce que nous avons dit en parlant des vaisseaux chylifères, et de l'absorption des veines, il est évident que ce sont *les veines* qui absorbent directement les liquides, et qui les transportent aussitôt

au foie et au cœur ; en sorte que la route que suivent ces liquides pour arriver aux reins, est beaucoup plus courte que celle qui est admise généralement, c'est-à-dire, les vaisseaux lymphatiques, les glandes mésentériques, et le canal thoracique."—Tom. II. p. 380.

The French physiologists are admitted to be very accurate in performing experiments, and take all possible pains to render them conclusive. They assure us that many of J. Hunter's experiments were not correctly *designed*, while his reasoning upon the phænomena is exceedingly faulty. It was said to be established by his experiments and those of Cruikshank, that red veins do not absorb, and that arteries are endowed with a contractile power ; and both these facts are expressly denied by Magendie, who describes his experiments, performed before some of the first physiologists of the present age, entirely agreeing with him that they are conclusive. In both cases these physiologists have recurred to the ancient doctrines, and, in my opinion,

have established facts which at once subvert the hypothesis of Harvey, although they appear as yet unconscious of the inevitable consequences of their own investigations.

If we admit the fact that urine is secreted while the thoracic duct is tied, we must also admit that *blood is formed*; and this leads us back immediately to the theory of sanguification.

Cruikshank, who zealously contended that red veins do not absorb, candidly admits that he had often seen appearances in the veins of the intestines for which he could not account. "I mentioned," says he, "in the last chapter,* Swammerdam's having seen the blood in the mesenteric veins streaked white. I also mentioned that professor Mekel informed us that he had seen white lymph in the same veins. I have frequently seen this appearance in the

* Anatomy of the Absorbent Vessels, ch. v.

veins of the intestines. What this is owing to, I do not know; it cannot be absorption of chyle from the cavity of the intestines, for then the lacteals would also be found to contain the same colored fluid: but on every occasion where I have seen this appearance in the veins, the lacteals were constantly empty." But it does not follow, that chyle received into red veins from the cavity of the intestines, should be of the same color as in the pellucid lacteals in an unmixed state; neither does it follow, that because the lacteals were empty, the white chyle in the intestines could not have been absorbed from the intestines; and his observation immediately following, "that some hours after a full meal, blood taken from the veins of the arm has appeared streaked with white lines," does not prove that the white fluid he saw in the veins of the intestines was not really chyle. The Harveian hypothesis assumes that the red veins are continually occupied in returning to the heart the blood poured into them from the

arteries, and consequently their power of absorption has been denied by the greater number of physiologists for the last two centuries ; although some, and those of great name, have always supported the doctrine now established by the French experimentalists. Cruikshank gives at length the account of an experiment made by Kaaw Boerhaave, who immediately after the death of a dog injected warm water into the stomach and intestines, and saw it return by the veins of those parts, in such quantity as to wash out all the blood they contained, and leave them perfectly white. “ *Canis post mortem statim incidi thoracem et abdomen ; mox per œsophagum premendo leniter ventriculum, evomere contenta omnia feci. Dein immissa pura aqua tepida, movendo lenissime ventriculum, vidi a venis bibulis illam resorberi, ingredi venas gastricas majores, tandem portarum venæ tradi, et ex hac per hepar venæ cavæ reddi eandem. Tædioso labore, per horas lenissime immittere aquam, et premere ventriculum conti*

nuavi, donec pallerent omnia vasa sanguine orbata per resorptam aquam. Aqua vel cera per hæmorrhoidales venas injecta, in intestinorum cava exit.”—To this Cruikshank replies very shortly, “*The appearances which Kaaw Boerhaave saw, were entirely owing to transudation.*” Had they been entirely owing to *transudation*, how happened it that the fluid injected kept its regular course from the intestines into the smaller veins, then into the vena portarum, and through the liver into the cava?

Any injection propelled with great force, will rupture the vessels of the dead body; but in this case the greatest pains were taken, by the gentlest pressure and very gradual supply of fluid, to preserve the parts entire; and the completely regular course of the fluid gives reason to believe they were so preserved. The intention of Boerhaave was to imitate nature; and the warm water exactly observed that route, which the ancient phy-

sicians inform us the chyle takes in passing from the intestines into the cava. "Whatever," says Galen, "has been received into the stomach, and proves indigestible, is expelled downwards; and whatever is useful for the purposes of nutrition, is rendered still more useful by the veins distributed to the stomach and intestines; for they, like the *porters* employed in our cities, carry the food that has been prepared in the common store, to the common laboratory, that it may be concocted, and rendered fit for nutrition. Thus these veins carry off the food contained in the stomach, to the common laboratory of concoction for the whole body; and this we call the LIVER. The entrance to this receptacle is *one* composed of many minute passages; and to this a certain man of antiquity, of great power of mind as I think, gave the name of *portæ*; a name which, from him, remains to this day, adopted by Hippocrates and all physicians admiring the wisdom of this man, who compared this part of the animal

œconomy to an apposite department of our political institutions.”¹

The blood of the vena portarum, already of a red color, is, according to the ancient doctrine, received by the ramifications of the cava, on the convex part of the liver.

“The vena portarum, and the cava,” says

¹ — Οὕτω καὶ ἡ τῆς γαστρὸς δύναμις, εἰ μὲν τι τοιοῦτον (matter indigestible), ὠθεῖ κάτω· τὸ λοιπὸν δὲ πᾶν, ὅσον ἂν ἡ φύσις χρηστὸν, ἔτι χρηστότερον ἐργασαμένη ταῖς εἰς αὐτὴν τε καὶ τὰ ἔντερα καθηκούσαις διανέμει φλεψίν. αἱ δὲ, ὥσπερ οἱ ἐν ταῖς πόλεσιν ἀχθόφοροι, τὸν κεκαθαρμένον ἐν τῷ ταμείῳ σῖτον εἰς τι κοινὸν τῆς πόλεως φέρουσιν ἐργαστήριον, ἵνα πεφθαρμένον τε καὶ χρήσιμον εἰς τὸ τρέφειν ἢ δὲ γενησόμενον. οὕτω καὶ αὐταὶ τὴν ἐν τῇ γαστρὶ κατειργασμένην τροφὴν ἀναφέρουσιν εἰς τι κοινὸν ὅλου τοῦ ζώου πεψέως χωρίον, ὃ καλοῦμεν ἥπαρ.—Εἰσοδος δ' εἰς τὸ χωρίον τοῦτο πολλοῖς στενωποῖς κατατετμημένη ὑπάρχει μία, καὶ τις αὐτὴν ἀνὴρ παλαιὸς, δεινὸς οἶμαι περὶ φύσιν, ὠνόμασε ΠΥΛΑΣ, ἀπ' ἐκείνου τε μένει τοῦτομα διῦρο αἰεὶ· καὶ Ἰπποκράτης τε οὕτω καὶ πᾶς ὁ σὺν αὐτῷ χορὸς Ἀσκληπιάδων ὀνομάζουσι—ἐπαινοῦντες τὴν σοφίαν τοῦ πρώτου πολιτικῇ διοικήσει τὴν κατὰ τὸ ζῶον εἰκάσαντος.

Galenus de Usu Partium, Lib. iv.

Theophilus, "communicate by exceedingly small anastomoses; and by this means the cava receives the blood from the liver, purified and concocted, but still containing a watery excrement,"—to be afterwards drained off.¹

The secretion of urine, modern physiologists inform us, takes place from the blood which has already been distributed over the whole body. Liquids are taken up by the lacteals from the intestines, by them conveyed into the thoracic duct, and into the left subclavian vein, to be assimilated with the blood, from which the urine is separated by means of the emulgent arteries and kidneys. The Greek physicians, on the contrary, say, that the blood is not fit for the purposes of nutrition

¹ — καὶ συμβάλλουσιν ἀλλήλαις ἢ τε στελεχιαῖα φλέψ
καὶ ἡ κοίλη, κατὰ τὰς περατώσεις αὐτῶν στενοτάτας οὔσας.
Διὰ οὖν τῶν περατώσεων τούτων μεταλαμβάνει ἡ κοίλη φλέψ
τὸ αἷμα ἀπὸ τῆς στελεχιαίας, καθαρὸν, καὶ πεπεμμένον σὺν
τῇ ὑδατῶδαι περιττώματι.

until it has been deprived of the excrementitious fluid we call urine; and this fluid, they inform us, is immediately drained off from the blood returned from the liver to the cava.

How this doctrine is confirmed by the experiments of the French physiologists, we have seen.

Several experiments have been made by Magendie, which appear to establish the fact that red veins actually do absorb. Assisted by M. De Lille, he performed an experiment, cruel indeed, but apparently decisive, upon a living dog.

“ M. De Lille et moi nous séparâmes du corps la cuisse d'un chien, assoupi précédemment par l'opium (afin de lui éviter les douleurs inséparables d'une expérience laborieuse) : nous laissâmes seulement intactes l'artère et la veine crurale, qui conservaient la communication entre la cuisse et le tronc. Ces deux vaisseaux furent disséqués avec le

plus grand soin, c'est-à-dire, qu'ils furent isolés dans l'entendue de quatre centimètres : leur tunique cellulaire fut enlevée, dans la crainte qu'elle ne recélât quelques vaisseaux lymphatiques.

“ Deux grains d'un poison très-subtil (l'upastiente) furent alors enfoncés dans la patte. Les effets de ce poison furent tout aussi prompts et aussi intenses que si la cuisse n'eût point été séparée du corps ; en sorte qu'ils se manifestèrent avant la quatrième minute, et que l'animal était mort avant la dixième. On pouvait objecter, que malgré toutes les précautions prises, les parois de l'artère et de la veine crurale contenaient encore des lymphatiques, et que ces vaisseaux suffisaient pour donner passage au poison. Pour lever cette difficulté, je répétai sur un autre chien l'expérience précédente, avec cette modification, que j'introduisis dans l'artère crurale un petit tuyau de plume, sur lequel je fixai ce vaisseau par deux ligatures ; l'artère fut ensuite coupée circulairement entre les deux

ligatures ; j'en fis autant pour la veine crurale : par-là, il n'y eut plus de communication entre la cuisse et le reste du corps, si ce n'est pas le sang artériel qui arrivait à la cuisse, et le veineux qui retournait au tronc. — Cette expérience ne laisse point douter que le poison n'ait passé de la patte au tronc, à travers la veine crurale. Pour rendre le phénomène encore plus évident, il faut presser cette veine entre les doigts au moment où les effets du poison commencent à se développer : ces effets cessent bientôt ; ils reparaissent dès qu'on laisse la veine libre, et cessent encore si on la comprime de nouveau. On peut ainsi graduer à volonté."

That this experiment was performed with much care, is evident from the description ; but to me it does not appear more conclusive than that of professor Boerhaave. The Harveian hypothesis assumes that the current of blood in the veins is towards the heart ; and, as in this case the artery was preserved, it might be said that the contents of the vein were forced

forward by blood received from the arteries. Would not the experiment have been more satisfactory, had the artery been tied—the heat of the limb being kept up to the natural standard by artificial means? A few minutes were sufficient to ascertain the fact; and no good reason appears why the vein was not left the only remaining communication between the limb and trunk.

The French physiologists however, although they have given us the results of many valuable experiments, continue to believe in the *système circulatoire*; and another experiment made by Magendie, which he thinks a direct proof of the circulation, deserves particular consideration, as it really appears more conclusive than any one of Harvey's.

“Après avoir passé une ligature autour de la cuisse d'un chien sans comprendre ni l'artère ni la veine crurale, appliquez une ligature séparément sur la veine près de l'aîne,

et faites une légère ouverture à ce vaisseau ; aussitôt le sang s'échappera en formant un jet assez élevé. Pressez ensuite l'artère entre les doigts, pour empêcher le sang artériel d'arriver au membre : le jet de sang veineux ne s'arrêtera pas pour cela, il continuera quelques instans ; mais il ira en diminuant, et l'écoulement finira par s'arrêter, quoique la veine soit pleine dans toute sa longueur. Si pendant la production de ces phénomènes on examine l'artère, on verra qu'elle se resserre peu à peu, et qu'elle finit par se vider complètement ; c'est alors que le sang de la veine s'arrête : à cette époque de l'expérience cessez de comprimer l'artère : le sang poussé par le cœur s'y précipitera, et aussitôt qu'il sera arrivé dans les dernières divisions, le sang recommencera à couler par l'ouverture de la veine, et petit à petit le jet se rétablira comme auparavant. Maintenant comprimez de nouveau l'artère jusqu'à ce qu'elle se soit vidée, ensuite n'y laissez pénétrer que lentement le sang artériel : dans ce cas l'écoule-

ment du sang par la veine se fera, mais il n'y aura pas de jet, tandis qu'il se développera, dès que l'artère sera entièrement libre."

The whole phænomena here described, admit of very easy explanation, upon the simple principle that all the motions of the living body depend upon *animal heat*. Every surgeon knows that blood is with difficulty obtained from the veins of one under the influence of cold ; and that when, from whatever cause, the heat of the body is considerably encreased, hæmorrhage from even a small opening in a vein is with difficulty suppressed. If the principal artery leading to a limb be *altogether* obstructed, the limb immediately becomes cold, and no blood will be obtained from the corresponding vein, unless by pressure, as in the dead body ; and if the temperature of the limb be considerably reduced from partial obstruction of the artery, corresponding effects will be produced, the blood from the vein issuing sluggishly, and without any jet. That the artery no longer distended

by the elastic vapor which, according to ancient doctrine, it conveys, and, losing heat, should gradually contract, as all soft parts of the body do from the action of cold, is to be expected.

If those who support the Harveian hypothesis insist that the phænomena described by Magendie depend, as he says, upon the blood propelled from the heart, will they undertake to show blood, or even warm water, propelled into the artery of a dead body issuing in a jet from the corresponding vein? But a phænomenon which frequently occurs in practice, and I have often observed, makes directly against the supposition that blood is supplied to the veins by the extreme arteries, viz.: that after opening a vein in the arm, suppose the median, and towards the close of the operation applying frequent pressure with the finger, and exhausting the vessel below the ligature of its contents, if after twelve or fourteen hours it is necessary to take away more blood, we still find the vessel

empty as we left it, nor does it rise upon removing the dressings, and applying the ligature anew, and we have to open another vein. That this is often found to be the case in practice is beyond dispute, although for the most part we find the vein again full, and ready to pour out blood upon touching the orifice recently made : but were it true that the veins naturally receive blood from the extreme arteries, the vein never could remain empty in the manner described in any one case, the corresponding artery continuing its action.

The experiments of the French physiologists are liable to the objection that they were made upon animals under the influence of great pain, when the whole animal œconomy suffers from violence ; but some facts they do unquestionably ascertain, and these very important in the present question. Cruel as such experiments are, they are better calculated to show the natural functions of parts, than anatomical investigations after death;

because these functions depend upon an agent, whose mode of action is no longer to be perceived.

Continuity of vessels, and the nature of their contents, we may ascertain after death; but the moving power is gone, and the blood, to which some have attributed vitality, and called it the cause of motion in the microcosm, tends immediately to putrefaction.

According to the ancient doctrine, the atmospheric air is collected by the lungs, and, mixed with a very small quantity of blood, passes into the arteries, and exhales in cavities, or upon the surface of the body; and the phænomena upon the division of any considerable artery, exactly agree with what the ancients taught. When a considerable pulsating artery is divided, a particular whizzing noise is heard, the blood appears of a very light color, as if mixed with vapor, and falls in small drops exactly in the manner of the electrical shower. The first jets are very

strong, throwing the blood to a very considerable distance ; but upon every succeeding pulsation, the blood appears of a darker color, it issues with less violence, and, as soon as the vessel is full of blood, pulsation is lost, and the blood does not flow *per saltum*, but as if from a vein somewhat affected by the pulsation of a neighbouring artery.

All this is admitted by Harvey in express terms; and he thinks it sufficient to say, in accounting for the equable flow of blood from a wounded or divided artery: “Sanguis præterit, non distendit.” But why does the divided vessel pulsate at first, and gradually lose that pulsation, the other arteries continuing their action, and the patient conversing and in good spirits? The blood is *præteriens* while it is impetuously thrown out *per saltum*; it is *præteriens*, when immediately after it flows quietly as from a vein, and is of the same dark color as venous blood. The fact certainly is, that an artery full of blood pulsates no longer; and it appears to me ab-

solutely certain, that were the left ventricle of the heart and aortic system filled of blood, neither pulse nor life could remain.

In cases of children, where from some mal-conformation the blood passes directly from the right side of the heart to the left, there is a certain quantity of blood in the aortic system, their pulse is feeble and irregular, their temperature does not come up to that of the healthy human body, the complexion is livid, and they die at an early age.

The pulsating vessels originating in the left ventricle of the heart, the ancient physicians called *arteries*, because they believed them to convey the air received by the lungs blended with a very small quantity of blood throughout the animal system; not all the quantity inhaled, but a part separated by the action of the lungs, and transmitted to the left ventricle. According to received modern opinions, the arteries convey blood, and that only ; of a lighter color indeed than that con-

tained in the veins, in consequence of some chemical changes it is supposed to undergo in passing through the lungs, and by two degrees warmer than venous blood, but essentially the same. Of this blood the arteries are said to be always full; and the air inhaled by the lungs, is supposed to be again thrown out in very nearly the same quantity in expiration.

We are indebted to Bichât for the result of some interesting experiments, which prove that the air inhaled passes immediately into the arteries without any rupture of vessels, and that when it passes from the lungs into the veins, a læsion of parts is always perceptible. "Je me suis assuré," says this ingenious physiologist, "par un grand nombre d'expériences, qu'on peut sur un animal vivant faire passer dans le sang, par la voie du poumon, l'air atmosphérique en nature, ou tout autre fluide aériforme. Coupez la trachée artère d'un chien, pour y adapter un robinet; poussez, par ce moyen, et avec une seringue, une quantité de gaz plus considérable que celle

que le poumon contient dans une inspiration ordinaire ; retenez le gaz dans les branches, en fermant le robinet : aussitôt l'animal s'agite, se débat, fait de grands efforts avec les muscles pectoraux. Ouvrez alors un des artères, même parmi celles qui sont le plus éloignées du cœur, comme à la jambe, au pied : le sang jaillit aussitôt écumeux, et présente une grande quantité de bulles d'air.

“ Si c'est du gaz hydrogène que vous avez employé, vous vous assurerez qu'il a passé en nature dans le sang, en approchant de ces bulles une bougie allumée, qui les enflammera. Je fais ordinairement l'expérience de cette manière-là.

“ Dans l'ouverture cadavérique des animaux morts à la suite de ces expériences, on trouve tout l'appareil vasculaire à *sang rouge*, en commençant par l'oreillette et le ventricule aortique, plein de bulles d'air plus ou moins importantes. Dans quelques circonstances le sang passe aussi en cet état par le système

capillaire général, et tout l'appareil vasculaire à *sang noir* est également rempli d'un fluide écumeux. *D'autres fois, les capillaires de tout le corps sont le terme où s'arrête l'air mêlé au sang ; et alors, quoique la CIRCULATION ait encore continué quelque temps après l'interruption de la vie animale, cependant le sang NOIR ne présente pas la moindre bulle aérienne, tandis que le ROUGE en est surchargé."*

He proceeds to mention that he had never observed in these experiments any rupture of the bronchia, and says, that if the air be propelled with a moderate degree of force, and to the quantity that may be supposed to be inhaled in a full inspiration, the air will, by the natural passages, enter the blood without any infiltration into the cellular membrane.

These experiments prove that aeriform fluids from the lungs pass into the left side of the heart and aortic system by natural passages. What the circumstances are under which they reach the venous system, we are not informed. There appears reason to

believe, that it must be by the rupture of vessels ; but his last observation, “ that air thrown into the lungs has filled the whole arteries with bubbles, mixed with blood, even to the capillaries, while not a single air-bubble, even the most minute, could be found in the venous system, although the circulation had gone on for some time after the air had been thrown in, deserves particular attention. According to the Harveian hypothesis, the blood is delivered by the capillary arteries into anastomosing veins, adapted to carry back to the heart what the arteries bring towards surfaces. The air-bubbles described pass as we see into the capillary arteries, where blood is also present, the respiration having been violently disordered, and an artery wounded ; but upon the supposition that continual circulation goes on, the air-bubbles ought to be discernible in the veins the very instant after it has reached the extreme arteries. The action of the heart is still going on, and, according to Harvey’s hypothesis, while that continues, the contents of arteries and veins are

rapidly performing their usual circuit. To me it seems not a little surprising that the French physiologists have not been led by the results of their own experiments to question the Harveian doctrines ; but both Bichât and Magendie speak of the *circulation*, and the *sang rouge* and *sang noir*, as if the truth of the hypothesis were incontrovertible, and all phænomena to be reconciled to it, by whatever means.

It would be desirable that some experiments could be agreed upon as decisive of the point in dispute, whether blood be really conveyed by the arteries in the natural state or not ; for, although the experiments performed by men of unquestionable respectability, as well as accuracy of observation, are, in my opinion, completely conclusive, I know from experience how difficult it is to drive a staunch Harveian from all his retreats, and compel him to admit that his ground is untenable

When it is proposed to extinguish life in-

stantaneously, or rather to put a stop to respiration and the action of the heart at once, and then to show the arterial system empty, we have seen that a wonderful contractile power is ascribed to the arteries, of evacuating themselves at the very instant that life becomes extinct. When we say that the quantity of blood contained in the human body is not sufficient to fill both arteries and veins, it is replied, that at death the blood is accumulated in the venous system. Wounds of the left ventricle of the heart occasion instant death, while the patient survives for hours, and sometimes for days, after the right has been wounded, and pounds of blood effused; while after death occasioned by a penetrating wound of the left cavity, very little blood is found in the ventricle. In such a case the sudden death very evidently does not proceed from loss of blood; but the words of the poet are literally true—

Omnis et una

Dilapsus calor, atque in ventos vita recedit.

Diemerbrock relates a case of this kind very distinctly: "Vidi quondam Novio Magi virum cum alio pugnante, cui, me adstante, medius thorax ab adversario suo perfossus fuit gladio, ictu sinistrum cordis ventriculum (quod postea in aperto cadavere vidimus) penetrante. Ille, eo ipso momento quo vulnus accepit, quasi fulmine ictus concidit, moxque extinctus est. Cum autem concideret, statim accessi, et ejus pulsum in carpo ac temporibus exploravi, sed nusquam arteriarum ullam pulsationem percipere potui, propterea scilicet quod sanguis per vulnus cordis in thoracis cavitatem effluens non impellebatur in arteriam magnam, sicque sanguis arteriarum immobilis substitit, nec ulla arteria pulsavit. Similem casum antea etiam Leydæ videram."¹

¹ Since the first edition of these Observations was published, Dr. Barclay of Edinburgh has very obligingly favored me with a case drawn up by Dr. Fogo, which I shall give in his words:—

This clear description of the immediate extinction of life appears to be very accurate, and exactly agrees with common experience ; but the *rationale* of the sudden death is not satisfactory, for wounds of the right ventricle, from the effusion of blood, would prevent its passage into the aorta ; and we have many

“ A German, Mullins, of the Royal Artillery, belonging to one of the companies stationed in the island of Leon, near Cadiz, in the early part of 1811, having quarrelled with his comrade, was accidentally wounded in the scuffle, by a knife which the other happened to have in his hand at the time. I was called to see him soon after the accident, and probed the wound, but could not introduce it into the cavity of the thorax ; and as the wound was not half an inch long, I rather thought the instrument had hit upon the rib. The man did not complain of any particular pain, but felt somewhat faint, which I imputed more to fear than to the effect of the injury. The fray took place in the evening, between eight and nine o'clock, and when I got him to the regimental hospital, I ordered him some wine, and to be put to bed.

“ To the astonishment of those in the ward, he died suddenly about four o'clock in the morning, almost without a struggle, seven hours after the accident.

cases, well authenticated, in which great effusion of blood had taken place, and yet the

“The singularity of the case made me open him the following day, and, aided by assistants, we examined the thorax.

“We found the knife had penetrated under the seventh rib of the left side, without injuring the lung (as far as we could see), and made a wound in the pericardium of nearly the same size as the external one, hardly so long. When we opened this cavity, we found blood (I think perhaps an ounce, or thereabouts,) mixed with the fluid ; and on examining farther, to our surprise we found an incision into the left ventricle of the heart, itself about an inch and a half in length. The disparity of this wound, with the external one, surprised us somewhat, but we concluded that it must have been caused by the muscular contraction of the heart ; and that the lung was collapsed in the act of respiration at the time the knife penetrated the cavity of the thorax, otherwise it must have been wounded.

“I have stated merely the leading points, which I could not well forget, nor do I wish to be more particular, in case my memory might mislead me.

patients had lived for a very considerable time—some three days.

“I am sorry I did not keep a detailed statement of every circumstance, as it would have been more satisfactory; but it proves sufficiently that though wounds of the heart eventually may prove fatal, yet they do not always cause instantaneous death.

“THOMAS M. FOGO,

“Surgeon of his Majesty’s Royal Artillery.”

Dr. Barclay adds:—“Dr. Fogo, who writes the above, is a very intelligent young man, and I have the utmost confidence in his accuracy. You may make what use of it you please.” That the case is narrated with a strict regard to veracity is evident; but I am inclined to doubt whether the ventricle was penetrated at the time of the infliction of the wound; for supposing the heart wounded during the *systole*, it appears difficult to conceive how the wound of the viscus should so far exceed in dimension that of the external parts and pericardium. Is it not more probable that the heart was partially wounded at first, and afterwards ruptured—an accident which we know sometimes takes place without the infliction of any wound? Had the heart been penetrated at first, must there not have been a much greater effusion of blood?

The opinion that blood naturally passes through the arteries in the healthy state, as we see it does upon the infliction of wounds, is evidently erroneous. A wound of the femoral artery will evacuate nearly all the blood of the body within two minutes; and it is clearly impossible that the same quantity could have passed through it in the healthy state; for the veins may be very completely compressed for more than half an hour, the artery continuing its action without any remarkable turgescence of the limb. When, therefore, we speak of measuring the velocity of arterial blood by the *impetus* with which it issues from a wounded artery, we use an unfounded assumption from which it is impossible to reason truly; and the phænomena that attend the wound of a considerable artery, ought, in my opinion, to convince us that the infliction of the wound occasions the flow of blood through the vessel. Upon the supposition that the blood is always passing rapidly and freely along the trunks of considerable arteries, how happens it that it rushes with violence through

the smallest wound in the parietes, while the natural course remains unobstructed? The current is supposed to have taken its impulse and direction from the heart, and yet we find the blood issues with impetuosity from an aperture perhaps not one-third of the diameter of the vessel, and nearly at right angles to its supposed natural course.¹ The ancient physiologists accounted for this in the following manner:—The pulse being occasioned by elemental fire, collected by the lungs from the surrounding atmosphere, and delivered to the heart, passes instantaneously through the whole system, to reunite itself again with the atmosphere, from whence it was derived by means of the exhalants: while the human body is in

¹ The jet of blood from a wounded artery is finely described by Ovid:—

Cruor emicat altè,
Non aliter quam cum vitiato fistula plumbo
Scinditur, et tenui stridente foramine longas
Ejaculatur aquas, atque ictibus aëra rumpit.

good health, it thus regularly proceeds through the arterial system, the membranous lining being a non-conductor ; but when a considerable artery is wounded, the vital spirits instantly escape, and are as instantly followed by blood from the venous system, which is, in fact, a wise provision of nature to prevent the immediate death of the animal ; for, as blood enters the wounded vessel, the loss of vital spirits is less, and the animal heat is better preserved. In large arteries indeed, wounds may occasion almost immediate death ; but in those of smaller size, the bleeding will frequently stop of itself as soon as the pulsation is stifled by the presence of blood, when the vessel contracts from the effects of cold.

It has been said that the ancients knew nothing of the phænomena of electricity or galvanism, and consequently could not reason justly concerning the operation of elemental fire ; but this assumption is not well founded, for we find that Numa Pompilius was acquainted with the means of bringing down

lightning from the clouds, and consecrated an altar to Jupiter Elicius; and Tullus Hostilius bringing down fire from heaven, was destroyed, and his house burnt.

Lucan, describing the dismal omens that preceded the civil war, says of Aruns the augur, that he was *edoctus fulminis motus*; and adds, that after he had sent the citizens to walk in sad procession round the city—

Dumque illi effusam longis anfractibus urbem,
Circumeunt, *Aruns dispersos fulminis ignes*
*Colligit, et terræ mæsto cum murmure condit.*¹

¹ In a preceding passage we find that Lucan knew that the effects of lightning depend upon an unequal distribution of elemental fire, and that the balance once restored, it again becomes quiescent.

Qualiter expressum ventis per nubila fulmen,
Ætheris impulsu sonitu, mundique fragore,
Emicuit rupitque diem, populosque paventes

A late author, without appearing to be acquainted with the ancient doctrine, has again introduced it, and suggests that the lungs collect elemental fire from the atmosphere—that the bronchiæ, resembling in structure the cells of the torpedo, possess an action somewhat similar—and that the pulse is really a shock sent through the arteries. This doctrine is supported by the facts that the pulse is transmitted instantaneously, and not according to the law of fluids propelled through elastic tubes : that pure air is known to contain elemental fire as we inhale it, and in expiration is found to have lost it : and in cases of increased heat of the body, attended with pain, the most immediate relief is obtained by the application of cold water, or the contact of cold metals, our most perfect conduc-

Terruit : obliquâ præstringens lumina flammâ,
 In sua templa furit ; nullâque exire vetante
 Materiâ, magnamque cadens, magnamque
 revertens
 Dat stragem latè, sparsosque recolligit ignes.

tors: our warm clothing is made of non-conducting productions of the epidermis of various animals—woollen and furs; and the same process which renders our cuticle a conductor of galvanism, diminishes the heat of the body.—On this subject I wished to have the opinion of a justly celebrated experimentalist, who very obligingly and readily informed me, that, “he had always looked upon the hypothesis as gratuitous, unsupported by any one fact, and contradicted by many simple experiments. Hydrogen gas may be breathed for nearly a minute, and yet the heart beats, and the functions of life go on; and the accounts on record of divers, and the history of the Blue Boy, prove that the air cannot perform any electrical function in the lungs, for the imagined electrical shock of course would immediately cease the moment the air was withdrawn.” He adds, that “he regards the muscular structure of the arteries, and the flow of blood, as sufficient proofs of the Harveian doctrine. An electrical shock could not create a wave, nor make the blood spout

several feet from a divided artery." It is no doubt inaccurate to term any impulse that may be derived from the lungs to the arteries, or the shock of the torpedo, *electrical*; as if it closely resembled the effect produced by the friction of dry and hard substances which we call Electricity. In both cases the animal function must be more closely allied to Galvanism; for a succession of shocks (if shocks they really be) are delivered by the lungs, when no external air is admitted, although each succeeding impulse is weaker than the former, and the action soon to cease entirely. But after the lungs have been once inflated, they retain a certain quantity of air even after expiration; and few noxious gases are so destructive of life as to destroy it instantaneously. By practice we may accustom ourselves to hold the breath, as divers do under water, for several minutes; but such suspension of respiration obviously does violence to the vital powers, and cannot be carried beyond a certain extent without producing the most violent symptoms. In the case of the *blue* boy, if the ancient

doctrine may be relied upon in opposition to Harvey's, he was blue, because blood was propelled to the extreme arteries.' For the

'In the first edition of these observations it was hypothetically stated, that *if*, by warming a recently dead body to about 98° of Fahrenheit, and injecting blood of the usual temperature into the carotids, we produce the exact complexion of the blue boy, the chemical solution of this uncommon phænomenon—(*the blue complexion*), by supposing a want of oxygenation, may be spared.

The experiment has since been tried, and upon injecting blood with even the slightest impulse into the carotids, the body being previously heated to 98°, the complexion instantly became livid. Warm blood diluted so as to have the brighter hue of what we call arterial blood, gives a tinge scarce perceptibly lighter;—and a solution of gamboge gives the countenance the appearance as if the patient had died of a deep jaundice.

The penis injected with blood in the same manner, assumes very nearly the hue of the purple grape.

The application of a ligature to a considerable

same reason his temperature was cold ; he had an obscure, feeble, and irregular pulse, and was unable long to support life. Many children have the bluish tinge at birth ; but if there be no mal-conformation of parts in the thorax, they acquire the natural complexion soon after respiration has commenced. It must be needless for me to repeat that the structure of parts, the muscular coat of arteries, and the flow of blood, afford, in my opinion, no proof whatever of the natural presence of blood in these vessels, or of the circulation. Upon the supposition that the arteries convey an elastic vapor, and are all of them exhalants, a muscular coat is necessary to

artery immediately brings blood into the vessel from the heart, while none is to be found beyond the ligature. Harvey, describing the consequences of tying an artery near the heart, says of that viscus, “ *purpureum contrahit colorem usque ad livorem ;*” a very just account of the appearance of any part in which blood is forced into the arteries.

enable them to accommodate themselves to the full hard pulse occasioned by fever or violent exercise, or a contrary state when the powers are debilitated. The steam pipes that warm our public buildings, are well enough calculated to convey water, but their structure affords no proof that such is their real use; and had the flow of blood from a wounded artery, together with the structure of the vessel, been really sufficient to establish the doctrine of a circulation of red blood, that doctrine must have been received from all antiquity; for arteriotomy was practised in the earlier ages as a remedy; and the effects of the wounds of arteries must have been observed from the time that battles were fought, and fatal accidents, destroying their continuity, occurred in civil life. I also conceive that the phænomena of the electrical water-works prove satisfactorily that a shock will create a jet in fluids passing through tubes, and, the excess of the heat of the human body above that of the atmosphere taken into conside-

ration,' the whole phænomena attending the wound of a considerable artery may be accounted for without the assistance of Harvey's hypothesis. Neither the structure of the parts, nor the flow of blood, had escaped the observation of the ancient physiologists. Whether they had particularly noticed the valves of the veins, I do not know ; but as they very particularly describe the coats of both arteries and veins, I think it as improbable that they had overlooked them, as that they had never seen the lobe of the liver, called by the name of Spigelius, and by him said to have been discovered.

¹ Every one must have observed, that a slight scratch of the skin bleeds profusely when the body is warm, whether from exercise or other causes ; and that when under the influence of severe cold, blood is with much difficulty procured even from a considerable vein. When the body comes to the temperature of the surrounding atmosphere, blood will no longer flow unless under circumstances that rarely occur.

It is well observed by Galen, that no medical doctrine ought to be received unless it possess three essential qualifications: 1st. It must be true: 2d. It must be useful: and, 3d. It must follow from established principles. *Πρῶτον μὲν ἀληθὲς εἶναι δεῖ—εἴτα χρήσιμον—εἴτα ἀκόλουθον ταῖς ὑποτιθείσαις ἀρχαῖς.* I have stated reasons why I do not think the Harveian doctrine *true*:—it is a matter of notoriety that it has not proved *useful*;—and I also regard it as *inconsistent with established principles* to suppose, that there is a continued circulation of red blood in the healthy state filling both arteries and veins, while the whole quantity in the human body is only sufficient to fill the venous system and right side of the heart.

If red blood naturally fills both arteries and veins in the healthy state, how happens it that upon injecting blood into the arteries, *e. g.* the carotids, the veins remaining full, the complexion becomes livid? In the natural temperature of the body, we should expect blood thrown into the arteries might remove

the paleness of death ; but it actually produces the livid appearance which arises from contusion, or in certain cases where death has been occasioned by suffocation. It is true that the coats of arteries are not diaphanous in the same degree as veins ; but the blood seen through the coats of arteries of considerable size, appears as if seen through arsenical glass ; and in some cases, the color of the temporal artery is seen gradually to change when it is divided, until at last in young subjects it puts on the appearance of a vein, when the stream of blood through the vessel is full. It has been already observed, that although blood issues immediately from a wounded artery, its first appearance is that of bloody vapor falling in small drops, which seem to repel each other exactly in the manner of the electrical shower ; and this the ancient physiologists said must be the case, because the vital spirits rushing with impetuosity to rejoin the atmosphere from which they are derived, throw out at first but a small quantity of blood ; but in order to prevent the immediate

extinction of life by their instant dissipation, the blood flows in greater quantity into the vessel divided, till it flows equably as from a vein, pulsation being lost. In drawing blood from an artery, it becomes *darker* in color till it assume the appearance of venous blood; and in drawing it from a vein, the last taken away is always the *lightest* in color; and when, as in some severe attacks of pneumonia, several pounds must be taken away, the blood at last assumes the appearance of the water in which raw flesh has been washed. The ancient doctrine was, that in arteriotomy, blood is derived into the arterial system—in phlebotomy, vital spirits into the veins. The ease with which Le Clerc (following the Harveian doctrine) presumes gross ignorance on the part of Erasistratus, who accounted for the phænomena in the manner just mentioned, is really amusing. After observing that it is surprising that this physician, who had so attentively considered the heart, and gave the names to the valves, which they retain to this day, should yet have embraced an absurd opinion con-

cerning the natural contents of arteries, and denied that they contain blood in the healthy state, he adds: “ Il étoit aisé de se convaincre par la vue ; mais il avoit recours à ce subterfuge. *D’abord, disoit-il, que l’on ouvre le ventricule gauche du cœur, l’esprit évapore, sans qu’on le voye, et ce ventricule se remplit à l’instant de sang. S’il avoit eu connoissance du MYSTERE de la circulation, il n’auroit pas été si embarrassé sur cet article.*”

It is to be suspected that the MYSTERY of the circulation (an excellent term supplied by Le Clerc), would not have cleared up many difficulties in physiology to this eminent physician, who had done more to enable himself to form a correct judgment concerning the phænomena of respiration, action of the heart, and other vital functions, *par la vue*, than any other physician of ancient or modern times, Herophilus alone excepted.

It is observed by Boyle, that experiments, unless wisely contrived and carefully

executed, must ever mislead ;' and for upwards of two hundred years, we have had daily proofs of the accuracy of the observation. Rejecting the precepts of antiquity, every individual pursued his own path in philosophical research, made experiments and drew conclusions according to the talents he possessed, the extent of his information, and strength of his reasoning powers ; and the consequence is, that we have experiments upon record, to reconcile all possible contradictions. In physiology the discordant results of experiments and calculations are very striking. Borelli, an author much admired in his day, by calculation made the force of the heart to be equal to the pressure of *one hundred and eighty thousand pounds* ; Bernouilli corrected an error in his calculation, and reduced it to *five ounces*. Harvey supposed the whole blood of the body to circulate through the heart and lungs about twenty times in an hour, or four

* Vide Tentamina de infido Experimentorum successu.

hundred and eighty times in a day. Blumenbach says, the whole quantity circulates through the heart several times every minute ; and Richerand affirms that a thousand ounces of blood passes through the venal tissue in an hour.

The physiologists whose opinions I think correct in saying, that colored substances will hardly pass the lacteals, because nothing passing through the alimentary canal is found to alter the sensible qualities of the blood are opposed by others, who affirm that the bones may be colored by madder carried by the lacteals into the circulation.'

It is evident that some of these opinions must be wrong, because they are contradic-

' There can be no doubt that colored substances received into the stomach, alter the sensible qualities of some of the secretions. Rhubarb soon changes the color of the urine, and after a large dose of nitre,

tory ; yet they have been all maintained and publicly taught in our times, and apparently without producing conviction in the minds of medical men, that our physiological doctrines are essentially wrong in principle.—Recent experiments, however, appear to be leading us back to more consistent physiological rules ; and the phænomena of Electricity and Galvanism, with which we suppose the physicians of antiquity to have been altogether unacquainted, directly tend to restore the ancient doctrines. That there is one active principle pervading nature is now admitted ; and whether called *aërial acid*, *phlogiston*, *caloric*, *elemental fire*, or *æther*, it is allowed to produce every effect, *great, marvellous, or terrible*,

blood drawn from the arm exhibits not the faintest trace of its presence, while paper dipped in urine voided at the same time, and dried, in burning shows the deflagration of the nitre, and is, in fact, converted into what we call *match paper*. On this point the experiments of Mr. C. Darwin exactly agree with those of M. Magendie.

that occurs. It is the cause of *motion*, and without motion no change is effected in nature; so that the greatest philosopher the world has ever seen, called his books concerning natural phænomena, *Περὶ Φύσεως, ἡ Κινήσεως* —“Of Nature, or Motion.”

“With regard to the microcosm, (says a philosophic writer of the last century,) it is highly probable that it is phlogiston in its pure electric state, that actuates the vital frame, and is the immediate cause of motion, the source of sensation, of irritability and sympathy; in a word, that it is what has been generally denominated by physiologists, the animal spirit or nervous fluid.”¹

¹ Principio, cælum, ac terras, camposque liquentes,
 Lucentemque globum Lunæ, Titaniaque astra,
Spiritus intus alit; totamque infusa per artus
 Mens agitat molem, et magno se corpore miscet,
 Inde hominum pecudumque genus, vitæque volantum,
 Et quæ marmoreo fert monstra sub æquore pontus.
Igneus est ollis vigor, et cœlestis origo
 Seminibus.

The *Platonists* and *Pythagoreans* maintained, that next to the infinite mind, and great creative power, which presides in the mundane system, the immediate instrumental cause, that moves and actuates all its parts, is the pure element of fire. Such was the doctrine of these sages, nor did they widely err. From a too contracted view of the laws of nature, that diffused and active principle has been distinguished by almost as many names as energies; but we have shown that it is the same powerful agent which we at one time contemplate in the form of concentrated light, tearing asunder the densest adamant, and at another rending the clouds, and threatening with resistless destruction the lofty oak and the towering citadel.

Plato defined light to be a *rare* and *subtle* flame, φλὸξ μανὴ καὶ λεπτή: and he came nearer the truth than later philosophers in general imagined. Can we desire a more

¹ Light is related to fire, as vapor is to water.

convincing proof of the solid judgment and penetration of that ancient sage, than that after the lapse of so many centuries, and the vast progress made in the science of nature, we are under the necessity of rejecting the theories of modern times, in order to revive his long exploded doctrines as the most consonant with facts and experiments?¹

That animal heat is derived from the atmosphere, and that it is diffused over the body by means of the arteries, is the doctrine of the philosophers as well as the physicians of antiquity. Aristotle says, *Fire (elemental) warms the air, and air the body*; τὸν μὲν γὰρ αἶρα τὸ πῦρ, ὁ δ' αἶρ τὸ σῶμα θερμαίνει,—and in this he agrees with Erasistratus concerning the origin of the pulse.

De Generatione et Corruptione, c. ix.

¹ Leslie on Animal Heat.

“ The *calidum innatum*, (says Bishop Berkeley) the vital flame, or animal spirit of man, is supposed the cause of all motions in the several parts of the body, whether voluntary or natural. That is, it is the instrument by means whereof the mind exerts and manifests herself in the motions of the body.” No eye could ever hitherto discern, and no sense perceive, the animal spirit in a human body, otherwise than from its effects. The same may be said of pure fire, or the spirit of the universe, which is perceived only by means of some other bodies on which it operates, or with which it is joined. Thus Hippocrates, in his book de Diæta, speaks of a strong but invisible fire that rules all things without noise. Herein, saith he, resides soul, understanding, prudence, growth, motion, diminution, change, sleep, and waking. And what hath been delivered by Hippocrates agrees with the notions of other philosophers—Heraclitus, for instance, who held fire to be the principle and cause of the generation of all things, did not mean thereby an inanimate

element, but as he termed it, πῦρ αἰζῶν, an ever-living fire."

The ancient physicians called the excess of heat in the human body, when not arising from any perceptible external cause, πυρετός : and stating this excess of heat to be the characteristic of all fevers, they classed them according to concomitant symptoms. The low fever, in which the heat of the body was not very much increased, they called *Typhus* ; comparing it to a culinary fire *smoking* : and the fever in which the heat and consequent affection of the heart was excessive, they termed *Causus*, our Febris ardens, in allusion to the rapid combustion of fuel by *flame*. These modifications of *heat* formed the two great classes of fevers, and accidents of intermission, or the appearance of various other symptoms occasioned many subdivisions. Their practice in the cure of fever, had for its object to reduce the excessive heat, and bring it to the natural standard of health ; a practice in which we find nothing to alter,

even after the adoption of Harvey's hypothesis. Blood-letting, the admission of fresh air to the patient, and the judicious use of purgatives, were their principal remedies ; but sudorifics were also in use, as well as the application of hot and cold water to the skin, with the view of rendering it permeable, or, in our language, a *conductor*, that the heat in excess might escape into the surrounding atmosphere, of removing any constriction of the pores, and preparing the way for the expulsion of whatever vitiated matter hindered free exhalation. In eruptive diseases the lower classes believe that the eruption proceeds from the heart, and familiarly speak of the danger of *driving it in* ; and certain it is, that fever precedes all cutical eruptions, and is relinquished when the eruption has without interruption run its natural course. In cases of ischuria, when urine is no longer secreted by the kidneys, upon the approach of death a highly offensive urinous smell exhales from all the surface of the body, while blood drawn from the veins has its sensible qualities very little

changed. In jaundice, the skin becomes of a deep yellow color, while the serum of venous blood retains its usual appearance; and these phænomena agree with that described by BICHAT—where aeriform fluids pass readily through the arterial system, while there is not the least appearance of their presence in the veins. The serum of the blood sometimes appears variously colored, in a manner not easily to be accounted for; having at times, as Cruikshank has observed, a resemblance to milk, and that when the health was but little impaired; but in general it remains unaffected by substances taken into the stomach, which sensibly affect what is secreted by the kidneys, and exhaled from the arteries.

That the arteries contain an elastic vapor that during health distends them, and distributes heat to the whole body by regular pulsations; and that, on the contrary, the veins contain almost all the blood of the body appears to me evident from the facts

already stated, and arguments fairly deducible from these facts. Some of the phænomena attending the flow of blood from wounded vessels, have been supposed to favor the hypothesis of Harvey ; and at one time an attempt was made to transfuse the blood of animals, upon the supposition, that what was introduced into the sanguiferous system of a tabid animal from one in full health, would enter into the supposed circulation, and produce renovated vigor. That attempt, however, completely failed ; and there is reason to believe that very incorrect statements were given of some of the experiments tried while the project was still thought feasible. Strange anomalies in the flow of blood from the body we meet with occasionally, fatal hæmorrhage sometimes taking place from very slight wounds ; but these accidents are not to be accounted for from any propelling power in the heart or arteries forcing forward red blood. About three years ago I was called to see a middle aged man who was said to have fallen down suddenly in a fit. It appeared to me that

he was dead ; but in order to satisfy his friends I tried some means of resuscitation, and amongst others, opened a vein in the arm, from which but a few drops of blood could be obtained. Two hours after I had left him, I was called again, the blood issuing freely from the wound in the vein, but all motion of the heart and arteries had ceased from the first, and the body was quite cold. During more than twelve hours the blood continued issuing from this dead body, to the amount of not less, as I should suppose from the appearance, than five or six pounds ; and it was with some difficulty finally stopped. How such a quantity could issue from the vein opened under such circumstances I do not know ; but of the fact I can have no doubt, as I was repeatedly sent for, upon the supposition that I might be able to restore the man to life. Here then was a case in which blood continued to issue long and in large quantity, after the heart had ceased to act, and the body had become cold : a proof that we must cautiously examine all circumstances before we draw conclusions

from the phænomena attending the flow of blood from wounded vessels, whether during life, or soon after death. That the cause assigned by Harvey for the flow of blood from a wounded vein, did not in this case exist, and could not possibly act, is certain; although, as I have said, I am ignorant of the real cause of the flow, which I had never witnessed under similar circumstances. Generally speaking, it is true that all evacuations reduce the heat of the human body; but in the case of a patient reduced to the last extremity by an attack of pneumonia, the extremities cold, and the pulse no longer perceptible at the wrist, we frequently, by letting blood, freely bring back the heat of the body to its natural standard, by relieving the lungs and enabling them to perform their proper functions. Exceptions are held to be proofs of general rules; and although in physiology we meet with some facts which at first appear irreconcilable with what has for many ages been considered true theory, we shall find that, upon attentive consideration, the apparent

difficulty admits of solution from the *accidents* by which the phænomena have been attended.

Arguments concerning physiological phænomena cannot be strictly demonstrative, but are of necessity dialectical, such as by fair and conclusive reasoning produce belief and conviction ; and I shall conclude the present imperfect sketch, by briefly enumerating some of the facts that induce me to believe the Harveian doctrine unfounded.

1. Harvey's first assumption, that the motion of the heart, and pulse of the arteries, is occasioned by the blood distending the left ventricle, and exciting it to contraction, is contradicted by the fact, that the heart separated from the body, and completely emptied of blood, continues its motion, at first vigorously, and thereafter in a more languid manner, till it come nearly to the temperature of

the surrounding atmosphere, when it finally ceases.

Lord Bacon mentions a case, in which the heart of a criminal was exposed round the scaffold palpitating, and when thrown into the fire, leaped up the height of several feet. In such cases the stimulus to motion cannot possibly be blood.

2. The pulse cannot be, as he assumes, the *impulsus sanguinis in arteriis*, because it is transmitted instantaneously; whereas a fluid propelled through dilatable tubes, must expend part of its force laterally, and its motion must therefore be progressive. The experiment of Harvey, in which water thrown into the intestine of an ox so as to distend the portion filled, transmits the impulse immediately of any body touching one extremity to the other, merely shows the incompressibility of the fluid, and proves nothing regarding the free passage of fluids through elastic tubes.

3. When a considerable artery is divided, and allowed to bleed freely, the color of the vessel, if exposed to view, will be found to change; the blood will soon have the appearance of venous blood, and pulsation is then lost: to me convincing proofs, that arteries, of a light color, and pulsating, are not full of blood.

4. The reasons assigned by Harvey for the empty state of the arteries after death, are altogether unsatisfactory; for while he supposes the distension of the left ventricle by blood, to be the cause of its contraction, and pulsation of the arteries; he yet assumes, that when blood is no longer furnished by the lungs to the left ventricle, the motion of the heart still continues, and that it *expels* the blood from the arteries.—In other words that an *effect* is for some considerable time produced without a *cause*.

5. In many cases of pulmonary consumption, before the patient dies, the lungs are al-

most entirely destroyed ; or, as Blumenbach expresses it, there is nearly a *total consumption*, when perhaps not one-fortieth part of the substance of the healthy and perfect lungs remains. In this state of the lungs, the Harveian doctrine assumes that the whole blood of the body passes through this remaining portion many hundreds of times within a few hours : yet no blood is found extravasated in the thorax on dissection, nor are the vessels of the remaining part of the lungs enlarged, the quantity of blood remaining nearly the same as in a state of health.¹

¹ Blumenbach says, “ The *corium* or cutis, and the internal surface of the alimentary canal, are so intimately connected with the lungs by sympathy, as in some degree to perform a part, and *occasionally the whole, of their function in their room*. This is exemplified in adults, laboring under nearly *total consumption*, or other violent affection of the lungs, and nevertheless existing for a length of time almost entirely without respiration.”

6. After death, when the veins of the extremities have collapsed, there consequently appears a congestion of blood about the heart ; a little is found in the left ventricle, and, unless in a few rare cases, none in the arteries ; nor is the quantity of blood in the body sufficient to fill both arteries and veins at the same time, according to the Harveian hypothesis.

7. The ligature of the saphena occasions no considerable swelling of the foot, from which it is said to return the greater part of the blood ; and the obliteration of the subclavian artery, is not followed by the exhaustion of blood from the veins of the hand and arm, which according to Harvey's doctrine

Here the circulation of the blood appears for the time forgotten ; although, according to Blumenbach, Richerand, and other modern physiologists, several thousand pounds of blood ought to pass, during each day, through the lungs.

ought to take place immediately after the obliteration of the artery is complete.

8. Heat, whether from violent exercise, or the application of warm air or water to the surface, causes a turgidity of the superficial vessels, which according to Harvey's doctrine arises from the increased action of the heart, driving the blood into the extreme vessels: but were this really true, the *vis a tergo*, the column of blood extending from the heart to these extreme branches, ought to be sometimes found, when death occurs suddenly under such circumstances; but the arteries as usual are found empty.

9. The existence of valves in the veins, demonstrated by Fabricius, first suggested to Harvey, as he informed the Hon. Robert Boyle, the idea of a circulation of red blood; but the lymphatics, in which no circulation can be supposed to exist, are supplied with the same apparatus of valves, and when a ligature is applied, swell as veins do on the side of

the ligature next the extremity; so that the presence of valves in the veins affords no proof of circulation, nor renders it in any degree probable.

10. Exhalants are admitted to be arteries opening upon the surface or in cavities, and by exercise we may when in health produce from them a profuse discharge, to the amount of many pounds in a few hours; when according to the Harveian hypothesis the blood is propelled with more than usual impulse towards the extreme vessels: the complexion however, although heightened, has the ruddy hue of health; whereas experience shows that blood injected into the carotids, the veins being full, immediately renders the countenance livid.¹

¹ It has already been observed, that the followers of *Harvey* have found themselves under the necessity of assuming as facts what cannot be proved, in order to support his hypothesis, and some of their assumptions are sufficiently extravagant. The wonderful contractile

11. It is unnecessary to give at length the antient physiology relative to the lungs, heart,

power of arteries, exerting itself with greater vigor in *articulo mortis*, than during life and health, was for a time believed; and the presence of *anastomosing branches* of arteries, always in readiness to perform the office of large trunks when obliterated, is still asserted by some. But if these anastomosing branches are adapted to supply a ready passage to the blood, what need is there for the production of new arteries, when a ligature obstructs a principal trunk?—These new vessels Dr. Parry has demonstrated; and how was the circulation carried on till they were formed? That eminent experimentalist candidly admitted that the difficulty in accounting for the life of the dog, whose descending aorta Mr. A. Cooper tied near the heart, appeared to him unsurmountable upon the Harveian hypothesis. Not an arterial twig, discernible by our senses, could possibly convey blood to the lower extremities in this case; and yet the Harveians insist that we shall believe in their existence although quite invisible, and that they are sufficient to supply the place of a vessel, which if divided freely would throw out several pounds of blood in less than one minute. No anatomist can show a twig of artery given off by

veins, and arteries. It is thus briefly stated by Theophilus : “ During the dilatation of the lungs, the external air is attracted, and this current of air created by the expansion of the lungs is called *inspiration*. In the lungs the air undergoes decomposition ; the most pure and rarified part is attracted by the heart, and arteries of the whole body, from the lungs, by the pulmonary vein. The fuliginous and excrementitious part is again returned to the external air by the channel of the trachea and nostrils ; and this is called *expiration* : these two operations being generally called *respiration*. *Perspiration*, is the inspiration and expiration from the surface of the whole body by invisible pores. There are two cavities of the heart, the right and left : the right containing blood, and a

the subclavian, and reaching half way down the humerus ; and yet we are required to believe, that upon the application of a ligature to the subclavian, anastomosing branches carry a full supply of blood even to the fingers.

very small proportion of aeriform spirit; the left, called *pneumatic*, containing such aeriform spirit, and very little blood. The arteries conveying the vital spirits contain a little blood for the purpose of nutrition, and the veins convey a small proportion of the vital spirits for the preservation of heat and life!"¹ The reader will judge how far this theory is confirmed by the above-mentioned facts; viz. the immediate passage of aeriform fluids from the

¹ "Ελκεται οὖν ὁ ἔξωθεν ἀήρ ὑπὸ τοῦ πνεύμονος διασταλέντος· αὐτὴ δὲ ἡ εἰσω φορὰ τοῦ πνεύμονος ὀνομάζεται εἰσπνοή· διακρίνεται δὲ ὁ ἐλχθεὶς ἀήρ ἐν τῷ πνεύμονι, καὶ ὁ μὲν καθαρώτατος καὶ χλιαρὸς ἐλκεται αὐθις ὑπὸ τῆς καρδίας, καὶ τῶν ἀρτηρίων ὅλου τοῦ σώματος ἀπὸ τοῦ πνεύμονος διὰ τῆς φλεβῶδους ἀρτηρίας· ὁ δὲ λιχνοῶδης καὶ περιττωματικὸς διὰ τῆς τραχείας ἀρτηρίας ἐκπέμπεται, καὶ τῶν ῥινῶν ἔξω, κ. τ. λ.

Εἰσὶν αἱ κοιλίαι τῆς καρδίας δύο· τούτων ἡ μὲν δεξιὰ, ἡ δὲ ἀριστερά· αἱματικὴ μὲν ἡ δεξιὰ περιέχουσα πνεῦμα ὀλιγοστόν· πνευματικὴ δὲ ἡ ἀριστερά περιέχουσα αἷμα ὀλίγον. Καὶ γὰρ αἱ ἀρτηρίαι πνεῦμα περιέχουσιν, καὶ αἵματος ὀλίγου μετέχουσι, τροφῆς χάριν, ὡς αὐτὴ καὶ αἱ φλέβες αἷμα περιέχουσιν καὶ πνεύματος μετέχουσι χάριν τοῦ ζῆν.

Theophilus, de Hominis Fabrica, Lib. III.

lungs into the arterial system, but not into the venous ; the secretion of urine while the thoracic duct is entirely obstructed ; the instant change of complexion to a livid hue, when but a small quantity of recent blood is injected into the carotids ; and the speedy change of the sensible qualities of the urine from substance taken into the stomach, while the blood is not in the least altered.

The experiment of Mr. Astley Cooper, in which he tied the descending aorta of a dog, a little below the heart, which dog lived for twelve months, and was then killed in order that the state of the vessel might be known, is in my mind an *experimentum crucis*, and proves incontestibly that the Harveian hypothesis cannot possibly be true. For were the whole blood of the body regularly passing through the heart and arterial system, as he supposes, every minute, the right ventricle supplying the lungs every instant with venous blood, the quantity contained in the descending cava, and whole lower extremi-

ties must very speedily be exhausted, and such an accumulation take place in the lungs, head, &c. as must speedily extinguish life. To pretend that anastomosing branches can in this case possibly supply the place of the descending aorta, is too much; for the vessel divided freely and left to bleed, would throw out very nearly the whole blood contained in the body in about a minute, and as small intercostal vessels leaving the trunk at nearly right angles are only given off in this case above the ligature, not a vestige of which can be traced below the pelvis, it is entirely out of the question to require us to believe in the existence of that which we cannot by the aid of any of our senses discover, or infer its existence from evident effects. In this very case new vessels had formed; and had the dog been permitted to live some months longer, would have formed a connexion between the upper and under part of the vessel, extending over that obliterated by the ligature, and for these new vessels there could have been no necessity had vessels already in existence

been ready to carry on the supposed circulation.

When the body is heated after death to nearly the natural standard of health, say about 98° degrees of Fahrenheit, warm blood, or any colored fluid of like consistence, will pass readily into the extreme arteries so as to color the skin, and upon injecting in this manner an intercostal artery, its distribution on the surface will be distinctly seen, provided the body is fresh, and the injection has not been propelled with too much force, so as to rupture the vessels. It will then be evident whether the lower extremities can possibly be supplied with arterial blood by means of these vessels, when the large artery is tied. Vessels destined for a long course are given off at acute angles; the short, in the intercostals just mentioned, nearly at right angles, of which no trace can be discovered by injection, corroded preparation, or any other means, below the pelvis; and therefore the conclusion appears unquestionably just, that they cannot

possibly supply blood to the lower extremities.

For these reasons, among many others equally strong, I believe the Harveian doctrine of the circulation unfounded: for, according to the common laws of evidence, the facts are not to be reconciled to the hypothesis, nor can Harvey's inferences from his experiments, be born out by fair deduction or conclusive argument. From his whole train of argument in his tract "de motu Cordis," there is reason to believe that he was not well versed in the writings of the ancient Physicians; for if he had understood their physiology, he could hardly have formed the supposition that they had carelessly overlooked phænomena obvious to the meanest capacity, which according to him prove a circulation of red blood. With what reason could he take upon himself to say that the common appearance of the arteries being found empty after death, had probably *imposed* upon Erasistratus, a physician who, as well as Herophilus, had performed

hundreds of cruel experiments by the dissection of criminals *adhuc remanente spiritu*, that he might discover the real functions of the heart, vessels, and organs, whose action ceases immediately after death?¹ Could Harvey possibly believe that the cotemporary and friend of the greatest philosophers and mathematicians, at a time when the pursuit of truth was carried on with more zeal and success than in any succeeding age, had from want of genius or accurate observation omitted to perform those simple experiments upon which he founds his hypothesis?—that he did not know that a ligature upon a vein occasions the swelling of that part of the vessel next to the extremity; or that a wounded artery pours out blood that issues from the heart?—Could

¹ Herophilus ille Medicus aut Lanio qui *sexcentos* homines exsecuit, ut naturam scrutaretur; qui hominem odit ut nosset; nescio an omnia interna ejus liquido explorarit; ipsa morte mutante quæ vixerant, et morte non simplici, sed ipsa inter artificia exsectionis.

Tertullian.

Harvey believe that they who gave names to the valves of the heart which remain even to this day, left it for him to form the first rational opinion concerning their use; or that when philosophy and science flourished, physicians alone remained grossly ignorant, or believed in absurdities?' Whatever work of

' Harvey imagines that the ancient physicians had no clear ideas concerning the use of arteries and veins; and thinks that Erasistratus affirmed no portion of blood to be present in arteries; and that Galen, on the other hand, had maintained that they convey blood only.—Galen, however, takes great pains to describe particularly the difference between the contents of arteries and veins, and to explain why the coats of arteries should be stronger than those of veins; viz. because they are destined to convey a more subtle fluid, the motion of which is rapid; whereas the blood is thick, heavy, and with difficulty moved: Τὸ μὲν αἷμα παχὺ, καὶ βαρὺ, καὶ δυσκίνητον, τὸ δὲ πνεῦμα λεπτὸν, καὶ κοῦφον, καὶ ταχύ. De Usu Partium, Lib. vi.

Fons sanguinis est cor. Ex sanguine spiritus. Sed per cordis partem dextram manat sanguis; per

art of that age has come down to us, its excellence ascertains its æra; and although the writings of the philosophers and physicians of antiquity, have suffered by the ignorance or carelessness of transcribers, and the ravages of time, they are yet our most perfect models. Indeed, in these days the philosopher and physician were for the most part united in one person, and Galen entitles one of his books, Ὅτι ἄριστος ἰατρός, καὶ φιλόσοφος, *That the best Physician is also a Philosopher*: and he himself is introduced by Athenæus, as one of his Deipnosophists, a philosopher worthy of the first place. In his time, he admits that anatomy was not so accurate as in the time of the Asclepiades; but he himself is more minute in his descriptions than many demon-

sinistram *promicat* spiritus.—Est spiritus substantia subtilis, aëria, dilucida, ex tenuissima quaque sanguinis parte producta, ut deferatur virtus à præcipuis membris ad reliqua, quo proprios valeant actus exercere.

C. Rhodiginus.

strators of the present day, and in Nosology describes distinctions of disease altogether neglected and unknown of late years: so true is that which was said by Isaac Vossius, that even in his time, "*Vix aliquem hoc seculo invenias medicum, qui observata Galeni vel intelligat vel curet.*"

It was an observation of Galen, that although in his time it was fashionable to speak of Hippocrates as a very great physician, yet few were acquainted with his works; and those who took the necessary pains to understand them were sometimes ridiculed for their affectation of learning by the more superficial, whom he justly reprehends: "If," says he, "we really admire Hippocrates, we must first study his writings, and learn to philosophise; and thus we may not only approach his excellence, but even go beyond him, learning whatever has been so excellently taught by him, and what he has omitted, of ourselves finding out."

¹ ———Φιλοσοφητέον ἡμῖν ἐστὶ πρότερον εἴπερ Ἰπποκράτους

Cicero says, that to be ignorant of what has been done before our time, is ever to remain in a state of childhood, and his observation agrees with that just quoted of Galen; but in our times it has become a prevailing opinion that physicians had better know nothing of the precepts of antiquity, but trust solely to the doctrines of the day, which for two hundred years have continued changing, are still changing with amazing rapidity, and must continue to change if this hypothesis of Harvey be false. To that hypothesis we have attempted to reconcile all the phænomena of health and disease; and in many instances it has powerfully influenced medical practice, but certainly without improving it. The facts I have stated, the experiments I have detailed, made by eminent men deserving all credit, and arguments fairly deducible from

ἀληθῶς ἐσμὲν ζηλωταί· καὶ τοῦτο ποιῶμεν, οὐδὲν καλύει μὴ παραπλησίους, ἀλλὰ καὶ βελτίους αὐτοῦ γίνεσθαι, μαθάνοντας μὲν ὅσα καλῶς ἐκείνῳ γέγραπται, τὰ λείποντα δ' αὐτοὺς ἐξευρίσκοντας.

these, fully convince me that the doctrine is altogether unfounded and untenable; but we see daily proofs how differently men judge concerning the weight of evidence and deductions from facts, and I know that any change of opinions that have once been generally entertained must be gradual.

At one time it was my intention to have extended my observations upon this subject, and to have gone over particularly all Harvey's experiments, with the view of showing how they are to be accounted for without his assumed hypothesis of a circulation of red blood; but if my observations and objections be well founded, his reasoning from his experiments must be false; and although their inquiry is intimately connected with every branch of physiology, medicine, and chirurgery, upon second thoughts I think it more advisable to confine myself to these facts, which if established are decisive of the question.

P. S. In answer to the objection that has repeatedly been urged, that I have not adduced facts and experiments to prove the validity of my observations upon the hypothesis of Harvey, I think it quite enough to refer to the experiments made by men of eminence and acute observation, which appear to me to establish the point that Harvey's doctrine cannot possibly be true. When I find, according to the experiments of Parry, Cooper, and others our most eminent physiological experimentalists of the present day, that the largest arteries, and almost all the large arteries, may be tied without occasioning death or even much inconvenience, I conclude that this is a *fact*, the credit of the reporters being unquestionable; and taken in connexion with another established fact, viz. that after the application of a ligature, *e. g.* upon the descending aorta, when not an arterial twig can be demonstrated that might carry blood to the lower extremities, where the veins remain as usual full; I say that this *fact* is utterly inconsistent with the hypothe-

sis, that arteries and veins are naturally full of blood, and that blood in constant and rapid circulation.—An author for whom I entertain the highest respect has lately informed us, “that we can feel no surprise, that a large party of the medical profession should segregate themselves; and resolutely interdict the use of reasoning in medical practice, steadfastly resolving in their conduct to be guided solely by the dictates of experience.” I would inquire, how can any man profit by experience without the exercise of reason? I have seen a cylinder roll up an inclined plane, and, had the use of reason been interdicted, I might have formed the conclusion, that all cylinders placed upon inclined planes will of themselves roll upwards. To me it appears evident that false and inconclusive reasoning from experiments fairly enough conducted, has contributed more to the debasement of science in modern times than the most visionary speculations of former ages. Were reasoning really interdicted in medical science (the expression makes out a complete

bull, but that is not my fault) we have nothing for it but to suppose information supplied to the profession by supernatural means ; and a physician may be prized, as some of our statesmen were during the last century, when the Persian Ambassador expressed his conviction that Britain flourished, because its affairs were under the direction of an inspired ideot.

THE END.

The first of these is the fact that the
... in the ... of ...
... and ... may be found ...

ERRATA ET CORRIGENDA

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The ... of ...
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ERRATA ET CORRIGENDA.

This Note respecting the opinions of Servetus is to be inserted at page 48 :

“ In libro suo quem de sacra Trinitate scripsit, hæc habet Servetus a Physiologia deprompta.—Vitalis spiritus in sinistro cordis ventriculo, suam originem habet juvantibus maxime rationibus ad ipsius generationem. Est spiritus tenuis, aeris vi elaboratus, flavo colore, ignea potentia, ut sit quasi puriore sanguine *lucens vapor*.”

P. 109. l. 3 from bottom, for “ *compe* ” read “ *compel*.”

P. 138. l. 8 from bottom, for “ *cutical* ” read “ *critical*.”

—————, for “ *relinquished* ” read “ *extinguished*.”

ERRATA ET CORRIGENDA.

The Note respecting the opinions of Servetus is to be inserted at page 18 :

" In libro meo de rebus Tristitiae scriptis, nec habet
 tractum a Physiologia decepta. — Vitae spiritus in sinu
 matris ventriculo, cum originem habet juvenibus maxime
 alimenibus ad ipsos generationem. Isti spiritus tenuis
 et elapsus, hanc colorem, ignem potentia, ut illi quid
 a parente recipere possint magis."

- 2. pag. 1. 2. Item bottom, for "comp" read "compel."
- 3. pag. 1. 2. Item bottom, for "collat" read "collat."
- 4. pag. 1. 2. Item bottom, for "collat" read "collat."







