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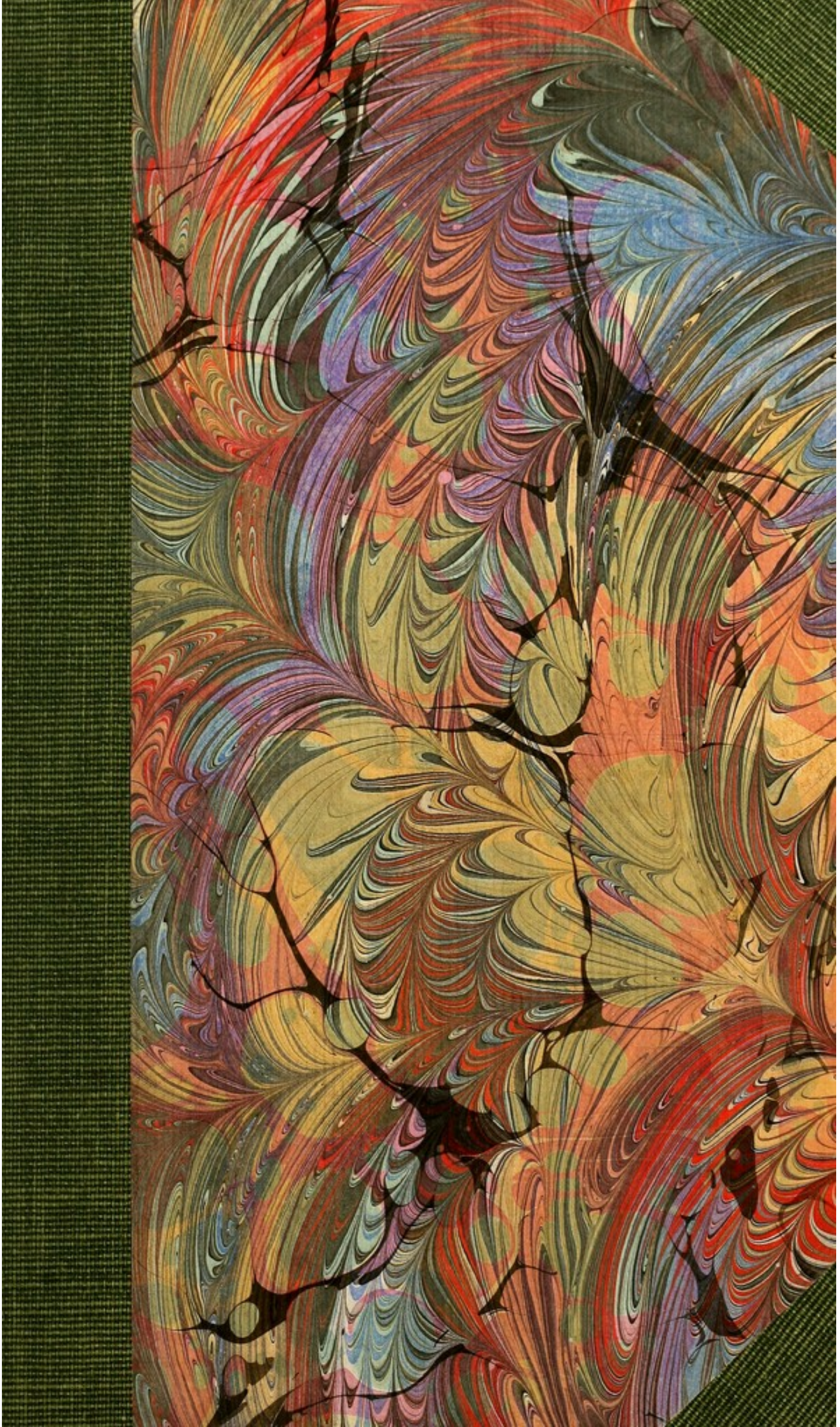

RESEARCHES,

CRITICAL AND EXPERIMENTAL,

ON THE CAPILLARY CIRCULATION.

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OF NEW-ORLEANS,

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There is neither rest nor pause, but ever movement and evolution,—a curse still clinging to standing still.—GŒTHE.

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THE CAPILLARY CIRCULATION.

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The dynamics of the capillary system has advanced but little since that memorable era, now two hundred and thirty years since, when Harvey discovered the circulation of the blood—an event that shed upon his native land an imperishable lustre, before which swords and helmets, coronets and crowns must "pale their ineffectual fires." This discovery, in a great degree created physiology or at least raised it from chaos to symmetry. It diffused a steadily increasing brightness, over a vast field where the darkness of countless ages had been broken, only by occasional glimmers of light, which served, to bewilder, rather than to direct the inquirer.

Although this discovery was very complete as to the mechanism of the heart and its action upon the arterial system, it did not, as Harvey supposed, account in a satisfactory manner, for the forces which circulate the blood in the capillaries, nor in the portal system, nor in the general venous system. He attributed all these motions to the heart alone,—a theory not at all tenable as will be seen hereafter.

The capillary force is a subject which has been long and ardently investigated in its two-fold character as a physical and a vital problem. The physical history, alone, which deserves a distinct treatise, would serve as the prelude, or introduction to the vital desiderata; hence a few remarks on the physical principles by which many attempt to explain the circulation, will be necessary. At the present time, one section of the physiological world maintains that the circulation is explicable upon mechanical principles, solely—a second, which is, perhaps, identical with the first, solves the problem by chemical affinity, nutritive action, the endosmtic forces of septa, suction from a vacuum in inspiration, sap-circulation, oxydation, and so on; another appeals to a nervous fluid, and ganglionic action, while, a fourth, relies upon a vital force. The separate, combined, and modifying results of these forces—their points of identity, analogy, contact, divergency, antagonism, and absolute difference, present a very complex question of the utmost importance, and of an almost limitless expansion, justifying a slight historical retrospection, prelusory to the hurried examination proposed on the present occasion.

The Italian philosophers, nearly two centuries ago, took the lead in research and discovery in the phenomenology of motion, comprehending the great principles of equilibrium, statics, dynamics, and hydrodynamics. The Italian, and, subsequently the French governments, expended the public treasure liberally in attempts to ascertain and fix the laws of motion, particularly the movements of fluids, constituting the science of hydrodynamics. Curious readers, who will examine the two hundred large quartos containing the transactions of the Academy of sciences at Paris, for nearly two centuries, will find many, if not satisfactory papers on these topics.

The following remarks by the learned and candid Mr. Whewell, will doubtlessly prove as acceptable to the reader, as they are pertinent to the general physical aspect of this subject, and the more so, as physical, or at least chemical force, is, at the present time, trenching upon animal dynamics. "The mode of solving the problems relating to the motion of fluids, has been, to introduce certain other hypotheses, often acknowledged to be false, and almost always in some measure arbitrary, to assist in determining and obtaining the solution."—"In most cases, the solutions of problems of hydrodynamics are not satisfactorily confirmed by the results of observation."—"The assumptions of the mathematician here do not represent the conditions of nature; the rules of theory, therefore, are not a good standard to which we may refer the aberrations of particular cases; and the laws which we obtain from experiment are imperfectly illustrated by *a priori* calculation. The case of this department of knowledge, hydrodynamics, is very peculiar; we have reached the highest point of the science,—the laws of extreme simplicity and generality from which the phenomena flow; we cannot doubt that the ultimate principles which we have obtained are true ones, and those which really apply to the facts; and yet we are far from being able to apply the principles to explain or find out the facts. In order to do this, we want, in addition to what we have, true and useful principles, intermediate between the highest and the lowest;—between the extreme and almost barren generality of the laws of motion, and the endless varieties and inextricable complexity of fluid motions in special cases. The reason of this peculiarity in the science of hydrodynamics appears to be, that its general principles were not discovered with reference to the science itself, but by extension from the sister science of the mechanics of solids: If we had lived in a world in which there were no solid bodies, we should probably not yet have discovered the laws of motion; if we had lived in a world in which there were no fluids, we should have no idea how insufficient a complete possession of the laws of motion may be, to give us a true knowledge of particular results."

"The same formula expresses the general condition of statics and that of the dynamics. The tendency to generalisation which is thus introduced by analysis, makes mathematicians unwilling to acknowledge a plurality of mechanical principles; and in the most recent analytical treatises on the subject, all the doctrines are deduced from the single law of inertia." *

Stevinus, in 1586, developed the principles of equilibrium and thereby

* Hist. Ind. Sci. i. 114 to 121.

founded the whole science of statics. This seemed to have served as the point of departure for the science of motion, for about the same period Benedetti published his *Speculationum* Liber, maintaining that the "motion of a body, separately from the mover, arises by a certain natural impression from the impetuosity (ex impetuositate) received from the mover, and that in natural motions this impetuosity continually increases by the continued action of the cause,—namely, the propension of going to the place assigned to it by nature, and that thus the velocity increases as the body moves from the beginning of its path." †

This "natural impression", "this propension of going to the place, assigned to it by nature", is, it may be said, nothing but an indirect confession of ignorance. It is too true; yet, it is the best that can be given in explanation of gravity, chemical affinity, the magnetic force and its cycles of increment and decrement. The heart's force, that of the capillaries, that of the portal, that of the pulmonary, that of the lymphatic, and that of the lacteal circulations, all belong to the same category.

It is probable that all Forces, physical and vital, are imponderable substances, essences, things in themselves, and not simple attributies, mere conditions. Electricity has been regarded by the experimental school as a nonentity in itself, or as a mere quality of matter; not a substratum; not a ponderable. But, it will be seen, according to the experiments recently reported in the London Lancet, that electricity is matter—is ponderable—overflows, has gravity, and pours from a jar like water.

Trancendentally speaking, few things would seem more *real* than Force, as manifested and proved by motion, and as selfrevealed in its many laws. By trancendentalism, I mean, in the first place, a class of hypotheses and probabilities which might be true, but which have not yet been proved by the experimental method, some of which, perhaps, never will; many of which, nevertheless, will be, as they now are, leading principles of action with the learned and unlearned, much as they may deny the imputation: I mean, furthermore, propositions which are not only probable, but absolutely true, and, which connot be proved experimentally, in all probability, which lie above, or beyond the reach of experiment, and which, in some instances, experiments and the testimony of the senses, seem to contradict. For example, the infinite divisibility of matter is not an experimental, but a trancendental truth. The infinity of space is in the same category.

If every material and immaterial entity were annihilated, space itself would remain, the negation of realism, the antithesis, nay, the very antinomy of experimental phenomenology. Here, one of the most absolute and necessary of all truths, stands in contrast to experimentalism; her longest sounding line reaches not the bottom; the Rosse telescope reaches not the *ultima thule;* the telegraphic wires stretched from planet to planet would not reach the frontiers of infinite space. Eternal duration is a truth transcendental and self evident, but strictly construed, it appears to be, really, another antinomy or contradiction to the experimental, that is, to time. Time is not even a part of eternal duration. Infinity cannot be composed of parts. Again, it is not possible to estab-

† Mr. Whewell.

lish experimentally, the fundamental law of modern dynamics, namely, that motion is perpetual, uniform, and rectilinear, nor this truth—two straight lines which cut each other, never again can meet. Experiment may have suggested this truth which it cannot prove. Though a logician may, perhaps, show that something of this kind lies at the foundation of all generalizations which transcend experience. The transcendental doctrines in medical science, are too numerous for enumeration in this place.

So real and positive did forces appear to the minds of many great philosophers, as Descartes, Newton, Berkeley, and others, that they attributed all motion to the immediate power of the Deity, and not to mere secondary qualities of inert matter as color, hardness, &c.

In the absence of proof showing that the animal forces are strictly chemical, it is as convenient as it is necessary, to classify them separately, and to call them by another name, that is, vital forces. To suppose that there is a capillary force, the intrinsic nature of which tends to accomplish the circulations of the economy, as gravitation tends to the centre, and which is as necessary to the capillary system, as essentially inherent to it, as atoms, weight, and extension are to matter, may be called a hypothesis. So it is. The Experimental, be it remembered, is indebted to the hypothetical method for almost every advance in science. The former has, often nothing to do, but to test or verify the anticipations of the latter. A hypothesis, which, in the absence of all direct experimental proof, proceeds upon known analogies, so as to account for, or harmonize, a class of phenomena, with any degree of probability, whatsoever, is, so far, an absolute advantage. It is very different, however, when incoherent hypotheses, and scholastic abstractions, claim to be absolute truths and all-explaining theories, and, consequently, become barriers to experimentation, especially, when sanctioned by great names.

The phenomenal history of vital dynamism, presents a very strong contrast to the physico-chemical forces. What can be more unlike than a burning lamp, and the portal circulation, or than the muscular and gravitative forces? The one is intermittent, exhaustible, renewable, variable; the other constant, at the same distance, diminishing in one direction, increasing in another, and always rectilinear. Is the willow that waves in the wind a type of muscular motion? Does the sap-circulation of a tree resemble the portal circulation of man? Is it identical?

The position of the vitalist, in this relation, is by no means the worst, even if vitalism be construed in its worst sense, namely, as provisional admission of ignorance. For, the erroneous explanations, confidently advanced and received as physical axioms, are so much dead weight against the movements of true science.

The vitalist cannot display the muscular, or the capillary force in an insulated concrete form, any more than the physicist can gravitation, how real soever the former may be,—for, it is only known by its effects, is to be studied through its laws, and is to be traced in its essential conditions as conjoined with living masses.

The transcendental doctrine that the capillary force inheres in the blood, or in the capillaries themselves, as an independent one, or as a mere property, or condition of life, will be countenanced, strongly, it is believed, by the experimental part of this paper, while in itself, it is more or less plausible; but, however true, this hypothesis may be, it

cannot explain everything in medical science, as most theories pretend to do. Stahl, proclaimed the Anima, as the All of life, the body being inert matter, while the Anima is an all-controlling, but an immaterial principle acting intelligibly without intelligence, rationally without reason, contriving by instinct. This, instead of being received as a hypothesis, was bowed to as a true system.

Teleology, (a method which Cuvier followed with much success), though often very uncertain, is, nevertheless, a fruitful principle in physiological research. It is always a source of pleasure to be able to discern the final purposes or ends of nature displayed in the adaptations of organic structure. But this principle is not, always, very obvious in the capillary system, and if the doctrine that the heart's force be received as the motive power in the capillaries, teleology, will afford but little countenance, to this assumption. Hence, a slight examination of the structures concerned is as unavoidable, as I fear it will be unsatisfactory. Teleologically speaking, if the circulation be due to mechanical forces or physical principles, adaptations to such ends, might be expected in the structures concerned. A glance in this direction, is, therefore, indispensable.

As the capillary system belongs to Microscopic Anatomy, it will be necessary to dwell a few moments upon this aspect of the subject—a necessity, that I regret, not because my observations have been few, but for a different reason, namely, an extended monograph would be required to place my views on this subject in such a light, as would enable me to indulge a reasonable expectation of escaping the charge of presumption, in dissenting from the highest authorities, not, indeed, so much with respect to the exact structure of the capillary vessels, (which is little known) but, in respect to other elementary points still more uncertain. For example, the globules of the blood, so often minutely described, pictured, classified (as belonging to different animals) nay, absolutely anatomized, are probably, to a great extent illusory. Air, or rather carbonic acid gas, so abundant in the blood, with no other anatomical membrane than that which envelope a soap or champaigne bubble, would appear like globules,—or these supposed blood globules may be heterogenous liquids, having different densities, different affinities for each other, for the serum of the blood and for the surrounding tissues. Hence, upon the physical principle of homogenous attraction, the albumen, the fibrin, or the oily matters in the blood, would, like oil in water, assume the globular form. Besides, the microscopic analysis in the direct light of the sun, gives to blood and certain vascular tissues, a wholly new, different, and unvaried appearance. Of this method, I have much to say, but not on this occasion. I wish now, to be understood as referring to the usual degree of light, though unfortunately this important point remains without any standard of comparison, except the negative one, requiring the light of a lamp, or the indirect light of the sun. Passing by this source of illusion, the reader will be kind enough to read the following extracts, taken almost at random from a few books, lying on my table,—more would be tedious.

Prof. H. Milne Edwards, represents the globule opened, dissected, and the flaps turned back! (*Anat. and Phys.* 25.) A microscopic object, would, of course, require a microscopic dissector, and microscopic scalpels! Dr. Golding Bird, in his account of Prof. Mulder's chemical

physiology of the red coloring matter of the blood, writes with apparent good faith, many passages like the following:

"The coloring matter is normally enclosed in a sac or cell composed of a thin membrane, consisting chiefly of protein, and capable of admitting of percolation through its walls under certain circumstances, although it generally retains the colored fluid it encloses sufficiently firmly to prevent its admixture with the serum. Within the sac is a nucleus of similar chemical composition to the membrane, and the whole thus organized constitutes the *blood-disk*, *globule*, or *red particle:* The bright red corpuscules are always *biconcave:* in the dark-red blood, the corpuscules are *convex*, and the enveloping membrane is *much thinner;* they readily burst, produce exosmosis, emptying the sack, &c. &c.: On reaching the capillaries, the coating of oxyprotein is removed, the protein being employed for the repair of tissue, and the oxygen used for effecting the metamorphosis of effete and exhausted structures. The corpuscules losing their opaque covering, have their power of reflecting light diminished, their concave surfaces are lost, and the whole assumes a venous tint." (N. Y. Jour. Med, and Lond. Med. Gaz. 1845.)

Mr. G. Newport, in a paper read before the Royal Society, undertakes to define the corpuscules of insects, and other invertebrata, comparing them with those of man, and the vertebrata: He says "in the articulata the forms are first, the *molecules;* secondly, the *nucleated* or *oat-shaped corpuscle;* thirdly, the *spherules*, or minute rounded bodies developed from the oat-shaped corpuscle, analogous to the free nucleoli of Valentin; and lastly, the *discs*, which are further developements of the spherules, and analogous to the true red-blood-discs of the higher animals." (Bullet. Med. Sci. 1845.)

"No single description (of the red globules of the blood) has tallied with that which went before. Leeuwenhoeck believed that he saw them consisting each of *six well compacted smaller globules*. Hewson believed that they were *bladders* which had within them *some* central body, loose and moveable; that often the central part might be seen *rolling* in its bag, &c. The Abbé Torre, said that these were not globules, but *rings*." (Bell's Anat. and Phys.)

Dr. Carpenter calls the *globules*, "*flattened* discs having a circular form." (Phys. §570.)

Sir Charles Bell relies on the microscopic examination of the frog's foot, for the proof of the capillary vessels. He says, naively enough: "We do not discover the coats of the vessels, but conclude that they exist, from the confined and certain course of the particles (of blood) which are in motion." (Anat ii. 26).

Micography envisages much which seems

"For man's illusion given."

Thus a convex or a concave surface may under certain circumstances appear globular. The different elements of the blood, as air, water, fibrin, fatty matter, albumen, not to mention their probable form, possessing as they do, different degrees of transparency, different densities, different degrees of shading power, of irisation, and more than all, different degrees of refracting force, would all conspire to throw doubt upon the anatomical accuracy of these microscopic membranes, and central nuclei, and the configurations which are too confidently proclaimed as absolutely characterizing the globules. Still more. Physical capillarity

itself, would be illusory, that is to say, a portion of liquid, or several liquids in a capillary tube, would assume a concave or convex form, according to their wetting or non-wetting properties. These and some similar facts were forced on my attention, in making dried preparations between narrow strips or plates of glass; pressure and desiccation for a whole season, did not deprive many preparations of these globules of air, water, &c. Heat, instead of expelling them, often augmented their number, by expansion, at the same time giving fixity to their enveloping tissues.

I might adduce still further argument from dead or physical chemistry, favorable to the doctrine here advanced, as to the globules of the blood being in all probability nothing more than one or several of the constituents of the blood taking the spherical form, as may be observed, out of the living body, without an organization high and complicated. I have observed with the naked eye, in the recently dead human subject, a rapid circulation or flow of chyle in the blood; the former had not yet fully mixed or combined with the latter, as will be seen in the sequel. Hence, this element alone, might be expected to assume the form of globules, without admitting anatomical sacks, nuclei, corpuscules, &c. The very pictures of these condemn them, on the physical principles of light. The centre is said and is known to be colorless, nay, luminous, if the globule be organized with red envelopes, the rays would be absorbed to a great extent, while upon the supposition that it is simply, like a drop of water, or oil, or like a double convex lens, it would converge the light, so as to give a central luminosity corresponding with the pictorial representations. This piece of anatomy, this complete and sudden organization of the blood globule, considering the rapidity with which it is destined to pass through the minutest tubes, is, in a teleological point of view, very improbable, andamust be, upon the filtering, wetting, or endosmotic theory of the circultion, somewhat unfavorable to permeation.

I do not deny that there are globules in the blood, but I deny that they are peculiar to it, since they appear to exist equally in every animal and vegetable substances which I have examined. But of all objects, they are the least likely to appear truly represented in themi croscope either as to figure, color, or anatomical organization.

Color is an unimportant, and unexceedingly variable, not to say a deceptive element, in microscopic research. A very thin stratum of blood absorbs little, transmits much, and reflects almost no light in the direct solar ray. The so called red globules, may be absolutely colorless, though coloring matter near them, or in the axial line of vision, gives them a red hue, in the same way that a red curtain, in a room with a mild light, colors microscopic objects red? Are the peaks of an elevated mountain chain, really *blue?* the clouds that shimmer in an autumnal setting sun, really *red!* a full moon in Indian summer, *bloody?* Are solar and lunar coronæ and halos, and the rainbow reflected and refracted from falling globules of rain, *real colorations?* The tissues and their enclosed liquids, differing in density, in figure, and in diaphaneity, might be expected, themselves, in some instances, according to position, to magnify like the microscope, and to decompose light like prisms, or falling rain drops, while on the other hand, absolute transparency in the object, renders it as invisible, and as structureless, as the

air itself. Moreover, a dim light, at certain angles, throws enormously long, or very short shadows. One object rises above another, while the focal point or field of vision, is always, if accurate, extremely limited; and, hence, diagrams and descriptions, however correct, explain little, where the thing itself is involved in an almost inextricable confusion, whether the fluids, or their containing vessels be the object of research. For example, in the examination of fluids, I have observed a very striking, possible source of fallacy, namely, an apparently spontaneous formation of very regular capillaries, resulting from the desiccation of animal fluids. During this process, air, water, or some portion of the liquid, perhaps influenced by homogenous attraction, arranges itself at first, into river shaped channels, or tubes, with many branches, all of which contract more, until they become fixed, while innumerable anastomosing connections join with other similar trunks and twigs, forming a net-work very like capillaries, though more open, and straight. Other fluids, in desiccating, form innumerable fissures, different, it is true, from the above, and from capillaries, but the tracks of these fissures correspond very closely to some portions of the net-work of capillaries, and may be even mistaken for them. Before I ascertained the fact, first mentioned above, I had concluded, erroneously, as to the quality of mucus expectorated by a patient. I found, as I supposed, capillary vessels under the microscope. There is, as I accidentally noticed, another possible source of error, in making capillary preparations between plates of glass, which though closely bound together, and kept in box, permitted, nevertheless, the formation of cryptogamous plants, (chiefly *Fungi*) of several kinds, invisible to the naked eye, but exceedingly beautiful in the microscope, having delicate stems, which by crossing each other's path, seemed to form a capillary net-work, among the tissues, and in other places. Their radicles and trunks, though extremely minute, are much more linear and uniform in size than capillaries.

I have examined hundreds of capillary specimens, both human and comparative, both wet and dry, healthy and morbid;—in the capsule of the lens, the hyaloid membrane, the iris, the retina, the choroid, the pia mater, tunica vasculosum, the serous, mucous, and other tissues in the principal structures of the human fœtus as early as the second month; also in the beautiful, and highly transparent tissues of young alligators, as the nictating membrane, retina, hyaloid, iris, mesentery, peritoneum etc. But, *a priori* conclusions and conceptions were not realized; capillaries like regular rivers growing from thousands of tributaries—capillaries like arteries and veins, with thousands of ramifications, forming a regular and continous net-work, were not seen with that uniformity, as to enable me (whatever others may have seen), to draw a satisfactory conclusion, where a capillary vessel begins, where it ends, and what is it common direction. Isolated points, centres or reservoirs, are sometimes seen, from which capillaries radiate as if wholly independent, as if possessed of circulations within themselves. Again, a large, short canal, a spongy islet, or a stellated point, receives small vessels without any apparent outlet! The same may be affirmed of many regular arborizations, which seem as isolated as one spider's web is from onother's. The capillaries, sometimes, appear rough, porous, and spongy;—conditions unfavorable to the perpetuation of motion generated in the heart.

They enlarge and diminish irregularly—tap each other at every possible angle—form every possible curve, if not every possible figure.

"From the result of microscopic examinations, little doubt rests on my mind," says Prof. Jackson, of Philadelphia, "that a large proportion of what is regarded as capillary circulation, is not in fact performed by vessels. On the contrary the blood circulates out of vessels, but in currents. The currents of globules flow in every direction; I have seen currents of globules commence when none existed—pursuing every course with great diversity in their velocity etc.,—some in a retrograde course." (Princip. Med. 23.)

The importance of the microscope, connot easily be overrated, in the revelation of minute objects, showing their outlines, organs, general conformation, and the *tout ensemble*, all of which, are very striking in a moderate light; but, unfortunately, it fails to a great extent, exactly where, judging *a priori*, it ought to afford the most satisfactory results: for, in many instances, instead of drawing more clearly the lines of distinction—instead of showing, in a striking manner, the specific differences between tissue and tissue, between fluid and fluid, it reveals a closer similarity, a nearer analogy, if not identity. Thus when properly prepared, the fibres of muscle, fasciæ, tendon, pleura, peritoneum, arachnoid, sclerotica, etc., will appear so much alike, that the microscopist will often have to look at the labels to tell the difference; at least such is the case as to myself, and it is still more so, to some of my friends. He who has a good eye, will probably distinguish certain objects, with it, better than with the microscope, as the difference between a muscular and nervous fibre, a nervous and arterial twig etc.; whereas, the contrary rule, holds good as to infusorial organizations, and multitudinous objects of a minute size, and of sufficient diapheneity.

It appears to me, quite impossible, to see, and describe with anatomical accuracy the capillary vessels, the circulation of globules, and their structure, or even the groups of organs in many objects called microscopic, not to mention entire animals, as large insects, the limbs of frogs etc. Even the most delicate and transparent infusorium, is a great mass of tissues, piled one above another, presenting different results, at different perpendicular foci, and giving different appearances, with the increment, or decrement of light etc. Suppose that an entire frog, or man, in a transparent state, were placed in a microscope; millions of vessels will be found in the same field of vision, or in the same axial line, the one above the other, traversing each other, in every direction; every change of glasses will present a new focus and a new phase, *ad infinitum*. It is probable that a life-time spent in this confused method of examination, would produce no satisfactory anatomical result. Suppose a thin layer of tissue is to be examined. Here, the chances of error are diminished, but they still exist—an object is selected, as a capillary. Is it a tube, a muscular fibre or something else? For it is difficult to determine in the microscope a solid cylinder from a tube. But, suppose a tube is found isolated, not one among many in the axis of vision,—is it a true capillary, a vein, a lymphatic, or an artery? How can its structure or coats be described, as some pretend to describe them, without first laying open this tube, in order, to examine its different strata, upon an exact level? Who can do this, in an object invisible to the naked eye? It is not the experimenter, but the reader, with pictorial

illustrations before him, that has clear conceptions of these things. Indeed, Schiller contended that the Ideal was always better than the Real. Thus, of morality and liberty, he says—(he might have included Micography):

— — — — — If we test
By the *idea* of the good, the best,
How mean our efforts and our actions are!
* * * * * *
Freedom is *only* in the land of *dreams*
And only blooms the beautiful in song.

The hesitating manner and the contradictory accounts of different authors, show that the anatomy of the capillary system is but little known. The micographers of the seventeenth and eighteenth centuries, investigated this subject with great industry and ability, and it is fair to presume that this part of anatomy is attended with great difficulties, since so little has been clearly established. Never were results more sterile, nothwithstanding the grandiloquence of the day.

Beclard says, that "minute injections and microscopical observations soon led anatomists to admit that instead of the supposed parenchyma interposed between the terminations of the arteries and veins, according to the ancients, everything in the body is composed of vessels; an opinion which yet divides all the cultivators of the science."—"The capillaries are intermediate between the arteries and veins. It is in these vessels that, insensibly and without any fixed point the arteries are converted into veins. However, the capillary vessels have been generally described as the last division of the arteries, rather than the beginning of the veins. Whether this be well founded etc., the texture of the capillary vessels cannot be observed with the naked eye. These vessels have very thin and soft parietes, invisible to the naked eye, and slightly visible with the microscope, very little different from the substance of organs, and also from the humors they convey. They seem rather formed out of the substance of the organs than provided with its (their) own parietes." "It would be impossible to stick the point of a fine needle without opening several of these vessels.—Beclard thinks it quite impossible to decide upon the existence of the colorless or serous capillaries on which much has been written. He adds,—"Doellinger thinks that the arteries, at their extremities, cease to have any parietes, and that the blood flows unconfined in the solid substance of the body. Wilbrand goes still further: asserting that the whole of the blood is converted into organs etc."

Professor Carpenter's anatomical history of the capillaries does not seem embarrassed with any doubts whatever, though it bears on its face very little that can be called absolute certainty. He says that "the capillary circulation is carried on through tubes which have *distinct membraneous parietes* ;—originating in cells"!—in another place, he says that "the capillaries *arise* from a minutely anastomosing *network*, into which the blood is brought by the ramifications of the arteries on *one side*, and from which it is returned by the radicles of the veins on *the other*." (Physiol. 1848.) Cells! network! ramifications! radicles! one side! and the other side!

The Medico-Chirurgical Review, unwilling to allow the capillary system any share in vital phenomena, espouses "the Cell-Theory, as the greatest of all physiological discoveries! Every animal and plant is developed from

a *nucleated cell*, by which all the organic tissues are formed, and by which, nutrition, secretion, absorption, assimilation, growth, decay, etc., take place; all these processes are *independent of the blood-vessels*, being *extra-vascular*, as all accurate observers in every part of Europe testify." (See this Journal for 1844–5.)

The micographical physiologists are now making great efforts to plant the tree of life in a "*cell!*"

A better type of life can be found in an animal never before described *, which, I have called, *the solar infusorium*, (because discerniable only in the direct ray of the sun,) entering as it does, like bricks into the cell wall, and into every other tissue. A Nilotic crocodile, supposed to have been dead five thousand years, is, to a considerable extent, composed of this animal, which springs into life, as soon as the tissue is sufficiently dissolved. I am fully able to show that the motion of this animal is not owing to evaporation, which, as I suppose, renders this discovery complete. This animalcule is very distinctly seen with a moderate magnifying power—a high power is too inaccurate.—It is probable that it cannot be killed, certainly not by boiling, and the strongest heat short of combustion. (The latter, perhaps, only dissipates it.) These animalcules are all exactly alike. The *Vibriones* of Ehrenberg, Mandl, Mantell and others, approximate the *solar infusoria*, but are essentially different, taking the descriptions and figures of these authors for a guide. The solar infusoria enter into, and constitute the chief material of the different tissues of every *infusorium* that I have examined. In all hard bodies they are, of course, motionless, until solution be effected. Whether they are the *ultimata* of organic life, is uncertain:

> "Great fleas have lesser fleas, and these were made to bite them,
> And these fleas have lesser fleas, and so on *ad infinitum*."

A brief examination of the principal prevailing theories which are supposed fully to explain the capillary circulation, is necessary. It would be a species of physiological cowardice, to pass these by without an attempt to answer them. If they be true, they cannot be injured—if false, they ought to be overthrown. The candid reader will judge of the argument and facts only, forgetting the great disparity between the humble writer of these lines, and the illustrious names to which allusion may be made. He who has not a perfectly satisfactory theory of his own, cannot do a better thing for science (discovery alone excepted), than to overthrow the theories of other people. Every untenable theory which is overthrown, is often one obstacle removed out of the way. Suppose that one of three theories must be true; as for example, that the capillary circulation is due to the heart—or to the chemical attraction of septa—or to an inherent vital force in the system. Now if the two first can be overthrown, the latter must be true, at least, upon this assumption, but if the latter, like the former, can be overthrown, even though none be offered in its place, so much the better. The question is then an open one. The more causative antecedents shall be restricted and limited by experiment and sound argument, the greater will be the chances of finding the true one, at last.—The era, if not expediency, justifies a free examination into scientific, as well as into governmental questions, unterrified by great names.

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* See New Orleans Med. and Surg. Journ. for Nov. 1846.

Dr. Copeland (with many others) attributes the capillary circulation, "functional and organic", wholly "to the influence exerted by the ganglial nerves"; this logic like that which attributes muscular motion to nervous fluid, may be called reasoning *per saltum*, not by the *lex continui*, still less by the *lex parsimoniæ*, since it supposes that nature does not proceed continuously, but by jumps, one tissue, without adaptations, does the work of another, having adaptations, employing many, instead of the fewest possible number of laws. Teleology or the science of causes, ends and adaptations, should always be esteemed paramount, until the contrary is proved. Dr. Copeland, curiously enough, says, that besides this ganglionic force, there is another "which tends to a state of *repose*, and is exerted in the organic structures themselves, at the point of contact, of the solids and the globules,—compared to a *vortex*, whence globules constantly pass from the arterial or terminal capillaries, and are lost in the different tissues,—each one attracting from it those constituents of which itself is formed, and which are always present in healthy blood. This latter force, which was *first* very minutely examined by Professor Schultz, and briefly stated by M. Andral, in his Pathological Anatomy, *without acknowledgement*"* etc. It is difficult to see, how this *statical* force, this force "*of repose*", can be an element in vital dynamics, or vital motion, unless the physiologist like the physicist, take for his point of departure for all the forces, the law of inertia. There is no difficulty, however, in showing that Dr. Copeland, notwithstanding his vast erudition, has fallen into an error, in claiming this supposed discovery for Professor Schultz. The following extracts from Haller's First Lines of Physiology, will, it is supposed, not only prove this, but, also, prove otherwise very interesting: "As the velocity, so the slow motion of the blood in the ultimate vessels, has its peculiar effects. In the larger arteries, the most heterogenous particles are *whirled about* amongst each other; in the smaller branches the progressive motion of the blood being diminished, the lighter particles separate from the very ponderous and red globules, and are forced towards the circumference etc. [In the capillaries] the *attractive powers* of the blood are also increased—oil is separated from the blood in one part, water in an other, mucus in a third. The coagulable juices are separated almost everywhere, from the arteries themselves—each in its respective place, milk is never secreted in the kidnies etc. The blood itself from which any liquid is to be secreted, assumes in *various places*, that *peculiarity* of character, that it contains more particles, of a like nature with those which nature wishes to predominate in the fluid to be secreted. Slowness [in the capillaries] *facilitates attraction* and viscidity; similar particles, when brought together, can better *attract and join each other*, so as to retain the larger canal, while the thinner parts go off by the lesser lateral branches. In the arteries there is a very great degree of friction—of the blood globules against the arteries—of the arteries contracting round the blood like an obstacle,—and of the particles of blood amongst each other by the confused and *vorticle* [*vortical*] manner in which they are propelled." Other passages of similar import, might be cited from Haller, showing that the Cartesian

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* Dict. Med. Art. Arteries. Blood.

vortices, the impulsion of the heart, and the inherent force of the capillaries themselves, produce the circulation. It would be difficult to imagine a more unsatisfactory explanation than that offered by Dr. Copeland, whether it be considered as a trancendental or an experimental one. No trancendental explanation is good, which is not probable, much less impossible; no remote experimental explanation is good, wherein all analogies and adaptations are rejectet for a hypothesis. Why should the nervous ganglions, without any adaptations, circulate the blood in the minute terminal arteries, in preference to the heart and arteries, which, certainly have a much more favorable mechanism by which to accomplish the end? Besides, I can assert from experiment, that the ganglionic masses may be destroyed, without destroying the capillary force in the human subject.

It would be improper, to pass by the chemical theory of the capillary circulation, advocated by our distinguished countryman, Professor Draper, of New York—a theory, received in Insular Europe, with an alacrity, nay, with a rapture without parallel, judging from the encomia, by the late British Reviews, and systematic writers of established reputation—a theory which is distinctly claimed as original with Dr. Draper, and still more distinctly claimed as original with Professor Daniell, of England, who is said to have "preceded the former by several years," and "who took a more philosophical view of the subject!" Here a remark may be allowed, on the occasional mysteriousness of trans-atlantic logic! The above example literally reads thus: Dr. Draper made a discovery, but Mr. Daniell preceded him in it, and "took a more philosophical view of the same", but, then, Dr. Draper preceded Professor Liebig! Mr. Daniell is very quietly crowned with the originality, but then the English language is taxed to the uttermost to show the merits and claims of Dr. Draper in this particular. The British admiration for Dr. Draper is not misplaced, and being rare, is doubtlessly, very acceptable to his friends, but, in this instance, (the merits of which I do not pretend to know), if Mr. Daniell made a discovery, and "took a more philosophical view" of its import, then Dr. Draper is placed in the category, of which Malvolio discoursed,—"having greatness thrust upon him."—Dr. Copeland, in his valuable Dictionary of Medicine, denies that "Lænnec was the discoverer of the importance of auscultation in the investigation of disease." Dr. C. claims this discovery for his "countryman Hook". The latter must have preceded Lænnec nearly two centuries!

The British and Forign Medico-Chirurgical Review for July, 1848, asserts that the whole problem of the circulation is solved by this Draperian theory—"it is a satisfactory *vera causa* for the phenomenon in question—a beautiful application, fully and satisfactorily made by himself, in no hesitating or uncertain manner, with clearness and precision—a masterly demonstration of a *physical force in the capillaries*—adequate," etc.

Professor Draper's beautiful and learned folio, on the Forces which produce the organization of plants, published in 1845, utterly repudiates the doctrine which has prevailed ever since the days of Harvey, and which Professor Matteucci and others still adhere to, namely, that the heart carries on the entire circulation.

Prof. Carpenter, of England, seems to be perfectly satisfied with Prof. Draper's explanation of the capillary circulation, which he quotes and adopts. *

To say nothing of my own incompetency for the task, I do not possess the documents and dates necessary to a decision of the question of *originality* in this branch of Physico-Physiology. I believe, Mr. Daniell was preceded by Doctors Mitchell, Faust, Togno, Coates, Lawrence, Rogers, and last, though not least, by Dr. Draper. The publications of the gentleman last named, in the American Journal of Medical Sciences, anterior to that on Plants, afford, perhaps, the best proof, that could be desired, by a Physicist imbued with "mechanical principles", not to name those of teleology, showing the utter *impossibility* of this assumed physico-circulation. Hundreds of plates and figures, could be instanced, each being the *antinomy*, not the analogue, of the physical mechanism required in order to produce any motion identical with the circulatory systems of man. I have before me, at this moment, a paper, by Dr. Rogers, (who is by no means an *ultraist* in this "wetting" theory), in which there are 22 figured illustrations † and which with the remarks accompanying them, are very interesting in a physico-physiological point of view;—not, indeed, in the explanation of the circulation, —for endosmotic action is probably not even an auxiliary, but an antagonistic force, *to be overcome*, and necessarily so, whether the *vera causa* of the blood's motion be chemical, or vital.

Professor Harrison, of the University of Louisiana, in his work—"An Essay towards a correct Theory of the Nervous System"—published in 1844, has given a very able analysis of these experiments, with ingenious speculations on their physiological applications, under the head of ABSORPTION.

Professor Draper, says,--"Physiologists have long seen, in opposition to the popular opinion, that the heart can only exert a *very subsidiary* action." 37. "The flow of sap and the circulation of the blood are due to the *same* powers." 22. "The principles of the circulation of organized beings are *strictly physical*." 23. "All capillary phenomena are cases of *electrical attraction*"; 28; and, everywhere, he maintains, that this capillary, this electrical action, and, what is called *endosmosis*, consist in the *wetting* property of vessels, and that the circulation is strictly mechanical: "in man the circulation of the blood is caused by the oxydating action of that liquid on the solid structures with which it is brought into contact." 29. "The arterial blood has an *intense affinity for any of the tissues* with which it is brought into contact; the venous blood has *little affinity for any of those structures*, therefore, the arterial blood will drive it before it, with an *inexpressible force;* the pulmonary circulation is the converse of the former—drives the arterial blood with an inexpressible force" &c., 33–41.

To say that this is the most fanciful theory ever propounded by a great man, is but to express an opinion, which, in these days of experimental inquiry, cannot be regarded as a satisfactory test. But, when a theory appeals wholly to "*mechanical principles*," the mechanism being open

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* Physiology.

† Am. Jour. Med. Sci. Aug. 1836.

to all for examination, when a theory appeals to animal septa, the anatomy of which, the humblest dissector and micographer can see—when a theory appeals to chemical actions which every one may test in the laboratory, and when this same theory is propounded, as "a strictly physical" one, surely a little inquiry cannot shake, much less overthrow, "strictly physical principles," "animal septa," "the wetting properties of solid structures," "electrical actions," "intense chemical affinities," and "mechanical principles." The humblest student, and and the most authoritative professor, must, however, equally stand on the same Baconian Platform, as the servants and interpreters of nature: —"Homo, naturæ minister et interpres, tantum facit et intelligit quantum de naturæ ordine re vel mente observaverit; nec amplius scit, aut potest." "Here will I hold:" for this doctrine of the circulation cannot be received as a transcendental one; it expressly claims to rest on pure physics or mechanics, and by this it must be tested. In the early part of this paper, the physical principles of motion have been noticed. How do they apply here?

The fundamental principle in this theory is taken for granted, or rather kept totally out of view, namely, the mechanism, or the anatomical arrangement of septa or membranes, having on one side an "intense affinity for oxydated blood," and all so arranged, as to produce a flow in one direction, upon "strictly physical or mechanical principles."

The anatomical portion of the theory, is neither proved, nor probable. Where are these septa? Do not the great majority of the physiological anatomists deny that there are even parenchymatous divisions between the arteries and veins, seeing that injections pass, though rarely, from one class to the other? What is the anatomical character of these septa? which is the side that attracts arterial blood? Matteucci, not to mention our own countrymen, who preceded him shows, which side of a skin or membrane, as the inside or outside, has the strongest wetting property or endosmotic action. But admitting as an anatomical fact, which no one pretends to have seen, that there are septa;—that one side is venous, and the other arterial;—that the wetting property of the one side transcends that of the other;—that the oxydated blood is attracted by the one side, and not at all by the other; and that these septa are so accommodating as to interchange affinities—the arterial side attracting oxydated blood from the left side of the heart, while in the lungs the whole is reversed, the venous blood from the right side of the heart being attracted quite as strongly notwithstanding its deoxydation, admitting, I say, all this, still, the circulation is not accounted for, either transcendentally, or experimentally, nay, the induction from the whole shows, that the circulation is an impossibility upon "the mechanical principles," assumed,—for no one pretends to have seen any mechanism in the capillary system that could by any possibility accomplish the result. All the membraneous septa experimented with, have a definite arrangement, of which, diagrams and apparatusus are usually given, with descriptions, very mechanical, indeed.

Thus if a liquid or a gas permeate a membrane, in a certain direction, as from the interior to the exterior with a certain force, this direction, and this force, would be nullified by millions of septa, placed between heterogenous liquids, and gases, in every possible manner, so as to act,

not together, but in opposition : by no possibility could such a *mélange* of forces or septa, act upon purely mechanical principles, in a common direction, as in straight lines, regular curves, or circles ; by no dynamical principle, could the primary action of each septum, nor the resulting action of all the septa produce, especially among heterogenous fluids and opposing septa, circulations like those of the right and left sides of the heart, of the liver etc.; in fact, every experiment with gases, liquids, septa, tubes, jars, endosmometers, and the like, proves, not the human capillary circulation, but the existence of a force which often acts in opposition to it, like gravity, and, which, like it, is, and must be, constantly controlled by a *vital* or *contrary force.* The endosmotic action is not the type, but the *contrast* of the capillary system. And, more than all, the structural adaptations, in endosomose and vital capillarity, are altogether different ; at least, no proof has been given that they are alike.

According to Professor Draper, endosmosis is nothing but the *wetting* property, and, as the whole composition of forces in the circulations, systemic, pulmonary, portal, lymphatic, lacteal, are so completely chemical, that, (to use his own words), " had we known *nothing of the circulation* but been instructed in the *chemical* relations of the blood to the soft tissues and atmospheric air, we could upon *physical principles* have *predicted* the existence of that circulation, and *shown* what its direction in different organs *must be* "—36—Shade of Harvey! how easy it would be now, since all is known, to arrange a few hundred bladders in a trough, like a galvanic battery, with oxydated blood, showing disbelievers, how, by *wetting*, all the complex circulations can be carried on " upon strictly physical principles. " It is very meritorious in chemists to subdue the vital forces, and to bring them within chemical jnrisdiction ; but this cannot be done, by assuming conditions and forces, as similar, nay, identical, while they actually differ in every essential particular. Dead matter is not the type of living ; a *wet* bladder produces no motion analogous to that of the muscles, or that in the capillary system. It is highly probable " from physical principles, " as will be shown, that this assumed chemical action of septa, is really a cause of retardation, even more than gravity, nay, that it is an absolute contrast to vital capillarity, which latter, is only known inferentially by its multitudinous phenomena, and which, has not a single type in inorganic chemistry, nor in mere physics. Its name is nothing. " A rose would smell as sweet by any other name. "

Professor Draper's theory utterly fails to account for that portion of the triune-circulatory system, called the systemic ; but if it be admitted that this part of the circulation is fully proved, then it follows, that both the pulmonary and portal circulations are absolutely impossible, since, the two latter exhibit a complete negation, or opposition to the former. This can be shown as to the pulmonary circulation ; for if the arterial affinity for the supposed septa, or tissues, should drive the venous blood before it to the right side of the heart, there it ought to remain, seeing the heart's action " is only subsidiary, " for being venous, and the force mechanical that brought into the heart, and this force, being there neutralized, the blood ought to have no affinity ; it ought not to be drawn with an " inexpressible force, " into the tissues of the lungs, because the tissues ought to have no affinity for deoxydated blood, especially,

when that deoxydation has reached its maximum condition of deoxydation, that is, just before it reaches its distribution in the lungs, where it is to be oxydated. Here, again, another contradiction occurs, for the pulmonary veins take a sudden affinity (which they ought not to do), for this sort of blood. They chase it down into the left auricle, which, as a physical race, is quite impossible. It gets into the heart, which it ought not to do, without many septa; here, the affinity is metamorphosed into an arterial one! But, all these contradictions are surpassed by those of the portal or abdominal circulation. Imagine a live oak with two tops, (the roots representing one top); one of these tops, by innumerable branches, (capillaries), collects the blood of the abdominal viscera into the veins, and finally into a common trunk, which latter expands in like manner on entering the liver, and thereby, like an artery, distributes the same blood to the entire organ. Here the black, carbonized blood, which according to this theory, ought to have no affinity at all, is attracted by the supposed septa, as strongly as if it were fully oxydated! The deoxydated blood being thus distributed, new difficulties arise—a new set of septa is required to draw this same blood into the *vena cavæ hepaticæ*, and thence to the heart, a most unfortunate circumstance, seeing that this deoxydated blood has no affinity for these tissues! Nor is this all. There must be another set of septa to attract the arterialized blood of the hepatic artery. Still, more. The biliary circulation, will require septa. The end is not yet. The lympatic circulation must be provided with endosmometers. In fact these fancied septa do not appear, analogically and mechanically speaking, adapted for all these complex circulations. A town could be as well laid off into lots, streets and squares, by following a bushel of ants, poured out on the ground, each bent on a different route, as the circulation could be directed by such septa.

Analysis gives nearly the same elements for both arterial and venous blood, water being about eight-tenths of the whole. Has any one offered a single experiment, showing that one kind wets an atery, a capillary, or a membrane, while the other does not?

I protest, therefore, altogether, against these exanimate liquids, bladders and glass tubes, and their "strictly mechanical actions," as true types of human structures, and of the living circulation; seeing that the former are not simply *contrasts*, but *antinomies* of the latter, in structure, modes of action, and general results,—seeing that the wetting and non-wetting septa are not proved,—seeing that the deoxydated blood actually performs more complex movements than the oxydated blood, I will dismiss this part of the subject, with two statements, one from M. Matteucci, relating to sap-circulation, and the other from Natural History.

Professor Matteucci, recently appointed by his government to deliver a course of lectures upon the Physical phenomena of living Beings, appears to have executed that task with much ability; certainly, he has not been backward in asserting the claims of that part of physiology which *is physical;* nor has he shown any unwillingness to include in that category, laws, which, many regard, as purely *vital;* yet, with all his predilections to appropriate the latter, in favor of the former, he admits, even with respect to the *vegetable* kingdom, that "the double movement of juices in the interior of vegetables, is a thing *inexplicable* by the mere

forces of *capillarity* and *imbibition*. This force of impulsion is *incompatible* with the effects of *capillarity* and *imbibition*—a force that has its *seat* in the *ultimate extremities of the roots*. A liquid rising in a capillary tube, cannot overflow the tube by the effect of the *same force* which raises it. He gives examples (a celebrated one from Hales), showing that the sap of a cut vine exerts an upward force, raising mercury equal to 43 feet 3½ inches of water. Hence, he concludes that, "the great height to which the liquid can ascend, appears to be *opposed* to the explanation of the phenomenon by considering it as an effect of *imbibition or of capillarity.*" 95-6.

Without delaying to explain the special anatomy of the crocodilian heart, (which has, I believe, been wholly misconceived by naturalists,) it is sufficient to say, that it has all the complexity of the human heart, with a most remarkable superaddition,—a wheel in a wheel—namely a large artery, which I will call the *Gastro-enteritic*, arising from the *right, or venous side* of the heart, which *distributes the deoxydated or venous blood to most of the abdominal viscera*. It is a matter of no importance to the present argument, to show that although the arterial or left side circulates only about half of the blood, there is a sort of provisional or supplemental structure—a short canal in the abdomen—by which the natural inequilibrium shall not be further augmented; it is sufficient to say, that deoxydated or venous blood is distributed—"attracted with an inexpressible force"—as Dr. Draper would say—*without* arterialization by "the soft tissues," just as forcibly, as if it were *oxydated* blood.*

When I come to the experimental portion of this paper, I will show that the capillary circulution in the human subject, cannot be dependent on the oxydating process.*

M. Matteucci being one of the latest, and most authoritative writers on the circulation, and withal, an advocate of the old doctrine which attributes this process to the exclusive motor power of the heart, a rapid summary of his views, will conclude the only remaining theory of importance, supposed to be explanatory of the capillary action.

M. Matteucci quotes and adopts M. Poiseulle's experiments and conclusions,—the induction from the whole, is thus expressed: "the movement of the blood in the capillaries ceases when the heart is raised or bound, and that its movement continued only for a few minutes, on account of the diminution of volume, and of the kind of contraction which the elastic coats of the vessels suffer when the blood ceases to be propelled by the heart.—The capillary circulation is uninfluenced either by a vacuum or by a pressure of eight or ten atmospheres."‡

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*The principles of Teleology—the *causæ finales*, are of the utmost value in experimentation. Had I not watched, for nearly one continuous year, the habits of the alligator, I could hardly have believed the anatomical fact here adverted to, seeing it is, apparently irreconcilable with physiology. An undisturbed alligator, be it observed, breathes but rarely, even in the hot, non-hibernating season! Hence, consuming but little oxygen by respiration, it is unnecessary that all the blood should be sent to the lungs. In the structure of the latter, there is but little parenchyma; they are chiefly sacculated, or vesicular.

‡ Living Beings. 327-8. These very inconclusive experiments, are summed up in the *Jour. des Prog. des Sciences.*-218. Paris, 1835.

After making due allowance for the illusions incidental to vivisections of the frog's heart, I must say, that both the theory and the facts—certainly the latter—appear to me to be fallacious, nay, self-contradictory. It is contradictory to say, that motion ceases, and yet continues a few minutes!

It is a mistake, to say, that the arteries contract, (in the human subject, at least,) after death, so as to empty themselves completely. This never happens "a few minutes after death,"—at which time there is occasionally blood in these vessels, but it soon disappears, leaving the arteries empty. It is even highly probable that the dead artery is larger than the living—the former by losing its natural or living tone, while the latter, as all know, is capable of contracting *præmortem*, upon its diminished contents, as in great losses of blood, and towards the close of life, so as to feel, under the finger, like a mere thread. This diminution of calibre, corresponding to the diminution or absence of the blood in the artery, is not seen, I repeat it, in the dead body. If "the motion of the blood ceases, "as says Prof. Matteucci, "when the heart is raised or bound"--(327)--how is it that the most complete act of the human circulation, far transcending the healthy, takes place soon after death, namely, the complete emptying of the whole arterial system, including the left side of the heart itself, where, by every principle of hydrodynamics, it ought to accumulate, leaving the right side, and the entire venous system empty, and not strongly distended, as they are found to be, almost invariably? Now "this diminished volume, and this kind of contraction," would make the great arteries, striking objecst, indeed, like solid rods, during contraction: for if the abdominal aorta should fail to contract completely into a solid cylinder,—if its cavity were open, and unobliterated, so as to admit even a hair, or a probe, that space would contain blood.

M. Matteucci, maintains, therefore, that the "binding or raising of the heart," completely arrests the capillary circulation, except a momentary impulse due to the mere elasticity and contraction of the arterial coats, "which these vessels suffer when the blood ceases to be propelled by the heart. By the aid of the microscope," he continues "there was seen an immoveable layer of serum, adherent to the coats, of the vessel; and the liquid thus moves in this tube, formed by its own substance. The same liquid in a glass capillary tube, and in a capillary blood vessel of a living or dead animal, follows the same laws in all cases. This fact assuredly proves, that in these various cases the liquid really circulates in a tube always formed of the same matter; that is to say, of an immoveable and adherent liquid layer, and which is the same as the liquid which flows through, whatever may be the materia of the tube." 328. Without stopping to point out the internal evidences of error, physical and microscopical, contained in this statement, it is sufficient to refer the reader to the experiments, in the sequel, showing that the fundamental doctrine of Matteucci cannot be true. The merest tyro can appreciate the physical impossibility implied by these illusory tubes—*tubes* formed of *serum or water*, *inside of the arterial tubes*—formed the moment the "heart is bound or raised," seeing, that of all the constituents of the blood, the serum is the least adapted for such tubes—tructures. The microscopic difficulties, here implied, may not be so self-

evident to the inexperienced, but they are not the less so, for that reason. Those who see the arterial tubes, and these firm tubes of water or, as M. Matteucci would say, "immoveable and adherent layers of serum," "must see as through a glass darkly."

The next piece of experimental logic offered is this,—"If we tie an artery in a living animal, the vessel almost entirely empties itself of blood, and the circulation continues in it only for a short space of time" (328)—a direct, though inadvertent admission that the circulatory force is more perfect without that from of the heart, than with it, for there is no proof that the forces of the latter can in any case, much less in this, completely expel the whole blood in the arteries, however vigorous may be its contraction. If the artery can thus suddenly empty itself completely by its own inherent powers, why, in the name of all that is teleological, does nature establish two dynamical organs to do a work which one can perform more perfectly? Verily, it is uncharitable to charge her with such platitudes, and absurdities, without better proof.

M. Matteucci having thus detailed what he calls "the most accurate and conclusive experiments upon the various questions relating to the sanguineous circulation,"—(328)—every important point of which I have noticed—triumphantly concludes, that the whole is due "to the simple alternating *impulsion* given to this fluid (the blood), by the sudden contraction of a species of *sack*, (that is, the heart) which makes a part of the tube itself."—331.

Dr. Marshall Hall, (like M. Matteucci,) says, "that the capillaries have no power to contribute to the motion of the blood, and that the capillary circulation depends altogether upon the action of the heart and arteries." (Lond. Lancet, 1830. ii. 540). Again, in his later Work on the Blood, he says, "the pulsatory power of the heart is capable of moving the blood through the arteries, the capillary vessels, and the veins, even in the extreme parts of the system." His observations were, as usual, made on frogs!

I have already shown that the capillary system itself, and all that is known of hydrodynamics, of retarding media, frictions, deflecting, and opposing currents, variable tubes, angles, and so on, render it improbable that this system should be exclusively under the control of the left ventricle. I could fortify this conclusion by many authorities. Let the following suffice: Mr. Samuel Cooper says, "the hypothesis of a *vis à tergo*, whether dependent on the heart alone, upon the arteries alone, or upon a combination of the two, has by no means proved sufficiently satisfactory, or been supported by evidence in respect to the entire circulation. Under no modification does it acconnt for the flow of the blood through the veins." (Good's Study, i. 314.)

Dr. Green, (New York Jour. Med. 1844) repeatedly observed with the microscope the capillary circulation in the foot of the frog, "in vessels so minute as to admit only a single file of globules. These may often be observed running *different directions* in parallel vessels—*in different directions in the same vessels*, so as to *pass* each other—independent of any *vis à tergo*;—other evidences of this inherent power have been observed" &c.

M. Magendie, in his work on the Blood, among a multitude of assumptions, (and most awful fulminations against all vitalists), going to show

that a great vivisector can chop logic,—asserts, "that the capillaries are *exclusively passive* in their circulation; in the movement of the globules, there is *nothing vital*." (158 to 169.) He maintains that the doctrine, that the capillary circulation is regulated by other laws than those of the great trunks, (purely physical) is one of the grand errors of the last and present century. (Ib. 169.)

Mr. Bryan, Surgeon, in the London Lancet, for April, 1845, maintains that the whole circulation begins in the left ventricle and proceeds *mechanically*, that is, the blood moves in the arteries, because the pressure is less than in the ventricle,—so of the capillaries, veins, on to the right auricle.

No one, so far as I know, has ever proved experimentally that the capillary, venous, and chyliferous circulations existed, or could exist after the termination of the oxydating process of respiration, after the cessation of the heart's action &c. Dr. Carpenter, the able physiologist, of England, inclines, it is true, to the opinion of the independent action of the capillaries. So do many others. But, he expressly declares that, although, "the movements of the blood in the capillaries of *cold-blooded animals*, after complete excission of the heart, has been repeatedly observed: In *warm-blooded animals this cannot be satisfactorily established by experiment*, since the shock oocasioned by so severe an operation," etc. (Phys. §. 506.) Now, according to the well established doctrine of discovery, it is not the man who *suggests* or avows an opinion, who is a discoverer, but the one who *establishes* it by indubitable evidence. Columbus was not the discoverer of a New World, until he saw the land itself. Almost every discovery has been claimed by many persons of different nations.

I could multiply by authorities similar to that of Dr. Carpenter. His own strong language is quite sufficient. I offer evidence. The decision is with the reader. If I shall prove, that in the human subject, the blood circulates or moves independently of the heart, long after death, I will have proved what he says "*cannot* be established by experiment." The *nature* of this force is not of any importance in this question.

In pathology, vivisection, the method usually pursued, is an equivocal guide, a limited portion of pathological surgery excepted. A vivisector, in physiology, is apt to be from the very nature of his vocation, a *physicist*, seeing *vital* actions only through *physical* experimentations. Armed with mechanical instruments, he stuns, poisons, injects, ties, dissects, galvanizes, decomposes, waging a cruel war against poor, frail vitality, cutting the latter off, if too long, stretching her, if too short to suit his *physical standards*. The animals he selects for physical manipulation, are unusually of the lowest order, being quite unlike man. The *artificial* conditions he produces, though possibly illustrative of some *natural* conditions in the *same* kind of animal, do not often come up to the high behests of human pathology.

Sir Charles Bell, one of the most talented and laborious of experimental vivisectors, in referring to the mutilations and agonizing conditions artificially produced in animals, and fallaciously regarded as *types* of *natural conditions in man*, declares that "these experiments have never been the means of discovery; and that a survey of what bas been attempted in late years in physiology, will prove that the

opening of living animals has done more to perpetuate error, than to confirm the just views from the study of anatomy and *natural motions.*"

Prof. Caldwell, of Louisville, says, "Experiments that do no violence to nature, are useful; those which are the reverse of this, never fail to prove injurious."

The experiments I offer, are all prepared by the hand of nature. They were observed, not, *created*, by *artificial* methods. They are not taken from the *inferior* animals, *but* from *man*. This simplicity and naturalness, (so unlike the barren tortures of vivisection—the most unnatural thing in the world, except to show the natural history of agony), will no doubt be objects of derision, to those, who prefer whatsoever is complex and artificial, and, who, because the vital often *coincides* or *agrees* with the *physical* law, claim every thing for the latter! The facts that follow, are not referable in their main features to any merely physical, or non-vital law. They are the last acts of vitality, before yielding complete and endless submission to the laws which govern the inorganic world—the preludes to the entire extinction of life.

If these experiments shall prove nothing more than the doctrine of the independent circulatory force of the capillary system of man, they will have accomplished more in physiology, and in pathological anatomy, than most of the experiments made on frogs, with galvanism,—dignified with the name of *Electro-Physiology*, *Excito-motory-System*, &c. It is true that these vivisections (often made at the expense of the State, and by salaried professors) have, in the estimation of certain persons, so great a dignity, from the State ceremonial, as to compensate for their otherwise worthless character—experiments, as remote as *possible* from man and his healthy, and morbid conditions. It is difficult to see how yellow fever subjects can be slighted, since, no disease presents an equal proportion of young, vigorous, and muscular persons. A hundred cadavera from this disease, will, probably, present to the sculptor a greater per cent of model-forms, than a similar number of the army itself. Plants and frogs, endosmose and galvanism, (good in their way, but withal absolutely sterile), ought not altogether to supplant *man*, and his *natural conditions*, as subjects of medical science, human anatomy and pathology.

In the experiments on post-mortem venesection, elevated points and positions were chosen, when practicable, in order to cut off all possibility that the result might be influenced by gravity; at the same time, care was generally taken that the orifices in the arm, forehead, external jugular, &c., should be elevated but *little* above the level of the highest part of the body, for fear that gravity might favor the result—which it always does to a very limited extent at the first moment, though, it makes the subsequent part of the experiment far more striking and brilliant. Sometimes, the elevation was taken, by pouring water on a plank, the latter resting on the body. All the blood, at the moment, in the *distal* end of the vein, that is, beyond the orifice, would, if raised above the level, be discharged from gravity; perhaps an ounce or more would, in some cases, thus be lost in emptying the vein, but then, *no more could get there*, to replace the *first*, unless, by a power altogether *different from gravity* Hence, it appears, that all the blood, with this

inconsiderable exception, after the *first moment*, flowed by capillary or ces—without the aid of, or rather *against*, all that is called mechanifcal principles—against all the chemical forces incidental to respiration—against ganglionic forces, for the ganglions were often cut away, —and, always, without any aid from the heart. See, and apply for yourself.

1841. Yellow Fever. A man aged 34, died—was brought immediately &c.; a vein, in the left arm was opened—the blood flowed freely—on moving the muscles it jetted, and upon ligation, formed an arch of about eighteen inches in diameter, as in ordinary venesection. The left jugular being opened in the usual manner, bled copiously without jetting. The abdomen, and chest, were opened without delay; one of eight or ten little twigs of nearly equal size, belonging to the coronary vein of the heart, was opened upon its highest point; the blood shot out in a small, strong stream, a pint being discharged in a few minutes. The omenta and mesentery, were beautifully and forcibly distended, especially, in the venous vessels. The cavas discharged from three to four pounds of blood.

1841. Yellow Fever. A man, aged twenty-five, died, lying on his right side—soon after, he was placed on his back—in which position, in fifteen minutes after death, he was opened. The liver, which was very brittle, was penetrated upon its highest, convex surface; in a few minutes, three and a half pounds of blood were discharged. The blood was taken up from the abdomen, in a sponge, and squeezed out into a vessel and measured. A puncture with the *finger*, would not be likely to divide any large vein, nor even in the living state, produce much hæmorrhage. The capillaries of the portal system, supplied nearly all this blood, so rapidly delivered from the abdominal viscera. (At the close of life, this man had nasal hæmorrhage.) Those who practice anatomical injections, know that only a few ounces of wax can be forced into the vessels of the liver.

1841. A boatman, aged 32, of extraordinary size, muscularity, and weight—dead, from Yellow Fever, one hour: the jugulars being opened, the blood shot upward—in a short time, a gallon, by estimation, was discharged. [The time is not precisely noted]. At two and a half hours after death, when the dissection began, about one and a half pounds of blood, (not included in the above), had flowed from the orifices.

1841. An Englishman, aged 37, died while I was present, of congestive, erroneously diagnosed as yellow fever: in a few minutes, the body became extremely hot; the external veins, especially in the arms, distended, as in health, after exercise—two were opened—the blood flowed rapidly, projecting about one inch; by compressing the veins, and agitating the muscles, it shot out a foot, or more, clotting firmly, as usual. The arteries were empty. The cavas discharged about four pounds of blood. An hour after death, a fine, warm, natural sweat broke out on the face, neck, and chest; a strong febrile smell emanated from the body. (After death from *solar* asphyxia, [sunstroke], I have seen the veins of the forehead, suddenly become prominent; the face, cyanosed.)

1843. Yellow Fever. A New Yorker, aged 26:—Experiments,

from the third to the fifth hour, after death. Room 80°; body in the axilla, and stomach 101°;* liver 102½°; thigh 100°; brain 95°; moderate rigidity. The veins of the forehead extremely prominent, to a degree rarely seen, when a man in health stoops to raise a great weight. A block placed under the back-head, raising the forehead higher than any other part of the body, caused, for a second or more, a recession of the blood, but it quickly distended the frontal vein as before. This vein was opened at its highest point on the forehead—a strong, but not a jetting stream, ran down the temple for about a quarter of an hour, discharging about twelve ounces of blood—after which, a rapid dropping, for a similar period, produced about four ounces more. The skin of the head, face, and neck, which hitherto had been greatly congested, marbled, almost black, from blood in the capillaries, now became much more natural. During this venesection, an eye had been removed, and a thermometer thrust into the brain, and, also, the chest and abdomen had been opened, without any retardation of the current. It is supposed, that all the blood above the orifice in the frontal vein, subject to the flowing by gravity, did not exceed twenty drops. The orifice discharged nearly an ounce per minute—the original blood was replaced 24 times per minute—288 times in 12 minutes; or 1,440 times per hour—34,560 per day.

Here, it will be observed, is a large blood letting, without a bandage or pressure. Can any bleeder, by this method, get half as much blood from this vein, in a living man?

It is remarkable, that all the external veins, those of the face excepted, were, in this subject, collapsed! Capillarity, in this region, alone, persisted for five hours. This man before death had copious nasal hæmorrhages.

1843. Yellow Fever. A Baltimorean, dead half an hour, was bled from the arm in the usual manner, the orifice being on a level with the highest part of the body—the blood jetted about two inches, but soon subsided to a rapid dropping. In thirty minutes, about six ounces were discharged. Contractility, powerful—Caloricity, persisting: Axilla at successive periods of 3 to 5 minutes, thus: 105°–106°–107°–107½°–etc.

1843. Yellow Fever. An Irishman, aged 33–dead two hours: The subclavian veins discharged several pounds of blood. Three hours after death, the left lobe of the liver bled profusely from an incision on its convex surface, made in taking the temperature;—an incision in the right auricle of the heart, discharged much blood. At four hours after death, the eyes still continued minutely injected. Air 61°—Brain 80°; epigastrium 84°—left chest 80°—centre of the thigh 84°. Contractility, moderate.

1843. Yellow Fever. D., before, and for an hour after death, had minute and vivid injections of the eyes.

1842. Yellow Fever. An Englishman, dead ten minutes, was, after a few experiments on the muscles, opened: The peritoneum, omenta, mesentery, etc., had their minutest vessels, arterial and venous, dist-

* For the sake of brevity, I will generally omit the experimental history of post mortem caloricity and muscular contractitily,

ended, and bled freely, when punctured. The cavas etc., discharged, in less than an hour, from eight to ten pounds of blood, by estimation.

1842. Yellow Fever. Miss * * aged 23 : Dissection, without delay. The omentum, mesentery etc., distended with both venous and arterial blood, in a manner difficult to describe, flowing freely from the smallest cut, in the higher, as well as in the lower organs. The subclavians, in a few minutes, discharged about three pounds.

1842. A German, aged 23—Yellow Fever—Dead thirty minutes : Twenty ounces of blood flowed from the external jugular, in half an hour.

1843. Yellow Fever. M. dead, three hours. Hot. Blood oozing freely from a slight puncture, at an elevated point of the shoulder or deltoidal region.

1841. Yellow Fever. An Irishman aged 35, dead 12 hours : a circular incision of the scalp, discharged in a few minutes, about six ounces of blood ; incisions in the arms and legs bled freely ; blood issuing from the ears. Head elevated.

1841. Yellow Fever. A German aged 19. Dead 30 minutes : A circular incision of the scalp, discharged forty ounces of blood in as many minutes ; the head was elevated on a block.

1843. A supposed apoplectic aged, about 30 ; stout ; unknown—died with apoplectic symptoms.—Dead one and a half hours* : heat at every two to ten minutes, in the order following : axilla 91°, 89°; rectum 95°; thigh 92°; epigastrium and heart 94° ; brain 88°; slight rigidity. Face swollen, dark red flush, as in erysipilas ; pressure, as in this disease, caused the blood to recede, temporarily, but it returned quickly, even in the highest parts of the head, face etc. Incisions of the integuments of the highest part of the body, bled profusely ; eyes injected, for some time, say, about two and a half hours after death.

I will here simply remark, without copying cases illustrative of the vascularity of the subconjunctival tissue, that real, active post mortem injection, very different from the ecchymosed or infiltrated eye, takes place, or continues as it was before death, in considerable number of cases. As the body always rests opon the back, it disappears, of course, from gravity, as soon as the circulatory force is extinguished.

I forbear to detail cases, wherein the cadavera were not bled, though, the tension of the veins, and congestion of elevated parts, often showed an existing impulsive force,—an example of which I observed in the person of S., two and a half hours after death from Yellow fever :—1848. A sligt incision on the tip of the nose, in this subject, discharged in a few minutes four or five drachms of blood.—Two days afterward, I met with another subject which, half an hour after death, bled copiously, (as is observed, sometimes in the living body), from the scarifications in cupping. The heart of this subject, removed from the body, gave unequivocal evidence of a circulatory capillary force, upon its exterior, as branches of the coronary vein, which were emptied by pressure, soon after, become turgid again, in a manner inconsistent

* He was probably a laborer—was sent to the *Charity* Hospital, though, among his rags, 45 dollars in gold and some other money were found at the dissection.

with gravity. The heart, of course, exercises no mechanical influence over the post mortem capillary circulation. Neverthless, I have experimented, in every conceivable way, upon this organ, with a view of ascertaining whether pressure might not accelerate the flow during post mortem blood-letting. Nothing of that kind occurs. If, however, there be blood in the arteries, it can be readily expelled, by previously opening an artery, as one of the mesenterics ; I have, by a few artificial contractions, or rather compressions, spurted out to a distance, nearly half a pound of arterial blood. The removal of all the cranial, thoracic and abdominal viscera, does not arrest the capillary circulation in many regions.

The *quantity* of blood found in post mortem examinations of fever-victims, is an important, and often a highly characteristic element in pathological anatomy, though, it appears to have been almost wholly neglected ; while on the other hand, nearly everything that has been said, by foreign writers, of its *physical appearance* (so far as the fevers of New-Orleans are concerned) is remote from the truth. All that these writers say of the liquefaction, defibrination, putrefaction, etc., of the yellow-fever-blood, is unfounded ; though, at the late periods at which examinations are usually made, such appearances are quite common. In no fever, perhaps, is the blood so natural, physically speaking, as in yellow fever. In yellow fever subjects, the *quantity of blood discharged* from the divison of any great vein in the centre, as the subclavian, cava, etc., probably exceeds, in a given time, the quantity from any other class of fever-subjects, owing, no doubt, chiefly to a greater quantity of blood, and a greater activity of the capillary vessels after death. I examined a subject in St. Thomas street, two hours after death from a tedious remittent, without seeing more than six ounces of blood in the brain, chest, and abdomen ; the most thorough dissection of every organ was made.

I will add only one more case :

1848. Serous apoplexy : The history filling ten pages, will be slightly sketched. A stout man aged 35, ate his breakfast as usual, and labored in the hot sun, until 9½ A. M. ; the minimum of the day was 77°. He complained of indisposition, and on returning home, became insensible (as was supposed, from a stroke of the sun). This was the opinion of his comrades. At 11 A. M. he died. The experiments began twenty five minutes after death, and lasted one hour and thirty five minutes, before the general dissection. Contractility, too feeble to flex the fore-arm. Incipient rigidity. The thermometer remained 55 minutes in the axilla, without being removed ; the first 5 minutes, gave 105°;—5 m. 106½°, 5 m. 108°; 10 m. 108°;—10 m. 108°; 10 m. 108° ; 10 m. 108¼° ; stationary ; legs crossed—the knees 108°; perineum 108° ; rectum 7 m. 111° ;— 5 m. 111° ;—15 m. 110° ;—epigastrium 109¾° ;—middle of the thigh 108¾°; chest 107°.

Great distention of the veins of the head, face and neck, with red flushing of the skin, was observed. A ligature was placed on the right arm ; a vein was opened as usual ; about two ounces of blood jetted from the orifice, after which the blood trickled down the arm, and in the course of two hours discharged, in addition to the above, about twelve ounces.

The skin of the face and neck was injected, dark, livid, and somewhat mottled ; there was no cadaveric hyperæmia or injection of the dependent parts ; the external jugular veins were distended as if ready to burst. Greater tension, I had never witnessed, in glottidian œdema, nor in convulsions, nor in the last throes of parturition. The left jugular was opened, as for ordinary bloodletting, *but no bandage or pressure* was used, the head being raised, so that the orifice was nearly on a level with the breast-bone *. The blood jetted completely, without wetting the skin, forming an arch, the diameter of which, continued to extend for five minutes ; at the end of 8 minutes, the arch had contracted, owing, apparently to small clots on the margins of the orifice, and the skin having once become wet, the blood, without being materially diminished, ran down the neck, jetting occasionally on removing clots from the orifice. For about one hour the flow was copious, but, at the end of that time, was diminishing rapidly. I caught nearly three pounds at first, but as much of it did not jet out, but ran down the neck, I could only estimate the amount (which I did), at five pounds, or 80 ounces from the jugular alone. As the bloodletting progressed, the congestion and discoloration of the skin of the face, diminished.

Now it will be seen, that the orifice in the jugular did not discharge the blood as fast as the circulation replaced it—there was a surplus, because, the venous tension or jetting augmented for five minutes, and had not ceased during 8 minutes. There was, as already mentioned, no bandage, or pressure. It is fair to presume, that it would be quite impossible in this way, to bleed a living man, half as much, as collapse of the vein, clots, fainting, etc., would prevent it. Hence, the circulation in the veins was probably more active and persistent, than in health ! Let it be supposed, that the upper or *distal* end of the jugular, contained one ounce, when opened—this being discharged, no more could replace it, only by a circulatory force. But here, the tube is filled 80 times in a few minutes. This may seem incredible from but one among hundreds of veins. A portion of this blood, however, was not derived from the distal but from the proximal end of the vein, as will be shown, presently, by incontestable evidence. The explanation seems to be this : The capillaries filled the veins rapidly. The forces in the cavas, contended face to face. The right side of the heart had no outlet. The equilibrium between the ascending and descending cavas was broken. The latter was weakened by the force or pressure turned off by the orifice, in the jugular. The pressure from below caused a retrograde movement—a very short one—towards the point where there was the least pressure, that is, the orifice in the vein.

While these experiments were being made, the cavities were opened. The heart contained but little blood.

In the early part of the experiment, particularly after the jetting of blood was changed to a stream down the neck, I observed, for a long time, that is while the blood flowed, what then appeared to me, an inexplicable phenomenon, namely, white, milky streaks, or soft floc-

* This was ascertained by placing a straight or level plank on the highest points of the cadaver, and by pouring water on the plank. This body was also carefully measured, but no expansions, contractions, or elongations could be detected.

culent masses, passing out of the orifice in the blood, rather swimming on the top of the current—a diffused drop or white flocculent cloud of this kind, was carried out once or twice every second. The subsequent dissection, fully explained this. The stomach contained nothing but water, of a sourish scent, with a few, very slender, tendinous fibres, apparently of beef, perhaps not amounting to five grains; but the quantity of chyle, of a pale milky, white color, thinner than paste, in the small intestines, was very extraordinary. The lacteals were gorged with this liquid,—the very same kind that I had seen passing from the jugular. I had, fortunately, opened the left jugular, near the place where the chyle is poured into the subclavian, so that it found an easy exit, along with the refluent current of blood, which latter, as before suggested, passed, or a part of it, in a retrograde course, proving at once, the independent forces of the lacteal, and blood vessels,—modified in this instance, however, by an important law of hydrodynamics, which changed their physiological directions.

This last case, (not to name others), fruitful in physiological suggestions, shows that although life, in its popular or utilitarian sense, be completely extinct, several important sub-lives, in tissues, and several functions may survive for a considerable period, as manifested in the heat generating process, in muscular coutractility, in capillary action, and in the chylous circulation. As the chylous force, so clearly proved in this case, goes to confirm, collaterally, a capillary force after general death, it may be proper to allude to the subject briefly.

Haller, the greatest physiologist of his era, ascribes the chyliferous circulation,—"first to the attraction of capillary vessels—next to the peristaltic contraction of the intestines, and, lastly, to the alternate compressing force of the diaphragm. The attraction fills the villi, and peristaltic force empties them, and moves the chyle farther forwards. The rest of the motion seems to depend on the force of the membrane of the lacteal vessel *itself*." (Phys. DCCLXVIII). Here, several forces, altogether imaginary or inadequate, are invoked to do almost nothing, while the lacteals, *themselves*, do all the hard work by an *inherent force*. The latter hypothesis is the most rational, or, at least the most simple, and is therefore preferable. Certain writers of the present day, without any physical proof, or analogy, assume the existence of *molecules*, which, as they further assume, perform all the functions of nutrition and capillary circulation. This is, to say the least of it, a very obscure and unedifying explanation for professed *physicists* to make. The science of physics has developed no mechanical law or force analogous to the chylous.

Müller candidly acknowledges, that the powers by which the lymph and chyle are moved, are unknown. Sir Astley Cooper, in his work on the Thoracic Duct, details experiments showing that ligation of that duct in dogs, was followed by a rupture of the receptaculum chyli, by large extravasations of chyle, and by death, in a few days. This receptacle, though weaker than the duct, "is," as he affirms, "able to bear the pressure of a column of quicksilver more than two feet in height. The force, therefore, exerted by the absorbents must be greater than that of such a column; more especially when it is remembered that *living* parts will resist a force which would readily tear them

when *dead*. The contractile powers of the absorbents are proved to be very strong—sufficient to rupture their coats." (12.)

Another use of the above experiments, which I do not wish to dwell upon, is this : they completely overturn, as I conceive, the theory of Sir David Barry, namely, that the sole cause of the venous circulation, is a vacuum in the chest, caused by the act of inspiration, the atmospheric pressure causing the blood to rush into the lungs.

Endosmosis or physical capillarity, (the physiological imoprt of which has been already alluded to), like gravity, constantly acts upon the body and its organs ; but from its known laws, it appears not to be even an auxiliary to the chylous and capillary forces. In fact, it must from its *nature oppose* them in most instances. There is, however, a use of this doctrine which has been strangely neglected, and one which I do not now intend to point out at length, namely a use in pathological anatomy. As the vital force sinks, the endosmotic rises. Thus in a few minutes after death, *exosmose* is very apt to take place from the gall-bladder. As the bile has a peculiar color, it is easily detected in the surrounding tissues. In some dead bodies there is no exosmose from the gall-bladder, until the beginning of decomposition ; but in most bodies it begins soon after, nay, probably, *before* death—certain it is that in ten minutes after the extinction of life, large tracts of the peritoneal, or subperitoneal tissues, are often infiltrated and discolored with bile that has been attracted or transmitted through the gall-bladder.

Hence, endosmose and exosmose are fundamental considerations, if not in the healthy state, they are without doubt, during, and soon after death. An example that occurredt his day, (Nov. 27th), will illustrate the point. As others often refer to plants and frogs, I may be allowed to refer to crocodiles, a far more respectable animal. One of these having perished from cold, and I, not being able to complete the dissection on the first day, threw a wet cloth over the abdomen. The gall-bladder was much distended with bile, and was not wounded. But when I returned to the dissection, the cloth was green ; the whole of the bile had left the gall-bladder. The latter was shrunk almost to nothing.

Dr. Mitchell, of Virginia, from 1737 to 1741, dissected, in all, five yellow fever subjects. Some of these were kept only " two or three days" before examination ! (See Coxe's Med. Mus.) One of these, a negress, served the illustrious Rush, as a "clue in practice :"—"the liver," he says, "in the above mentioned slave, was turgid and plump on its outside, *but* on its *concave* surface, two thirds of it were of a deep *black color*, and round the gall-bladder *it seemed* to be mortified and corrupted." (Inq. iii. 90). Now the "*clue*" to this "*black* color" and this "*seeming* mortification", is found in the transudations of bile, and other changes consequent upon the delay in the autopsy ; at least, a different appearance would be extraordinary.

It would be difficult to show, in the whole circle of medical sciences, any branch more important than pathological anatomy, and, yet, is there a single work extant, which, to a greater or less extent is not based upon examinations vitiated by physical causes or changes, endosmosis being one ? Excepting a few diseases, as cancer, consumption, the lesions of which are as strong as those on the field of battle, what can

be learned of color, cohesion, congestion, vascularity, turgidity, effusions, infiltrations, &c., in acute diseases, as fevers, one, two, or three days after death? Magendie, the great vivisector, of Paris, thunders in the ears of his bookmaking colleagues, in this wise—"Are you not in truth convinced, that the lesions found at our autopsies are frequently produced *after* death, and that consequently the plan hitherto followed in such inquiries is *fallacious*, and can *only lead to vague information and error?*" [Lect. on the Blood].

In this paper, I have no wish, even had I space, to make any extended applications of these researches, to physiological and morbid conditions, although, deductions, fundamentally important, are involved in the same. The scalpel is truly the greatest, and most generally successful instrument of pathological research, yet made known. But how little does it reveal in some cases of acute fever, particularly in the south, where lesions, merely secondary, or tertiary, are infrequent? for in protracted fevers, positional lesions, doubtlessly, may form *internally*, as they do *externally*. Congestions of the lungs, of the cardia and great curvature of the stomach, of the mesenteric glands, of the mucous membranes &c, are sometimes, the result of position as much as bed sores. I was called twenty five miles, to see a young lady, [in Virginia,] affected with typhus, she refused all medicine; fell into a delirium for three weeks; the fever then declined, leaving a mortification of the back, from which she died three weeks after. Another young lady whom I attended daily for typhoid, during *one hundred days*, had, only a few days before death, the eruption of rose colored, lenticular spots, described by M. Louis.

There is a constant bias, in pathological anatomy, leading to the centre, as the stomach, intestines, or some other central organs exclusively for the explanation of the cause of death, while, the circumference scarcely falls within the radius of pathological vision. Observe, in the early stage of yellow fever, what red flushings or capillary engorgement discolors the conjunctiva, the skin of the face, neck, and breast. As death approaches, this often is replaced by yellowness. After death, in those cases, where capillary action is most energetic, these same parts, occasionally, become much more engorged than ever,—flushed, livid, mottled, marbled, cyanosed, nay black, apparently tumid,—and this too upon parts the most elevated. Now this is as much a morbid appearance, as it would be in the cerebral or abdominal centres. This appearance is of easy explanation, upon the theory of post mortem capillary circulation. The heart discharges no blood. The capillaries deliver their blood into the veins,—these fill, as well as those large subcutaneous capillaries, that had suffered by dilatation during the disease. It will be found, that as soon as the capillaries cease to act, the blood begins to gravitate; sometimes, subsiding like a water-line, or level; the higher portions of the skin as on the cheek, fore-head, tip of the nose, &c., become white. In cases where this capillary action is inconsiderable, and, in cases where it has ceased, the blood can be made to flow, and reflow, in a few seconds or minutes, by position, that is from side to side, by turning the body over; and, exactly, in those tissues where the engorgement was greatest during life, will the greatest degree of positional or gravitative engorgement be found after death. There is,

however, a variety of *post mortem cyanosis*, due to capillarity, a blueness of the face, neck, &c.

Capillary hyperæmias, particularly of organs beyond sight, occur, short of softening, induration, and other phenomena characterizing inflammation* in its extreme and concentrated conditions as met with in maladies less general. Now if the capillary circulation survives the general death, as it probably does, in some degree, in every case of fever, it is easy to understand, in acute hyperæmia, short of disorganization, how the capillaries, by filling up the great veins, emptying the arteries, and by altering the præ-mortem condition of their own vessels, may obliterate, almost all traces of some of the most fatal morbid conditions—for inequilibrium in the circulation, is as fatal as cancer, gangrene of the lungs, or aneurisms.

For reasons not necessary to explain, I beg leave to quote from Folio X. M S., and from the Western Journal of Medicine, some dates and statements: (see my paper, Post-Mortem Researches, in the Journal; dated Jan. 1843. April No.).

November 13th, 1841;—at this date, I find the following statement: The capillary circulation in yellow fever, and other acute fevers, probably survives respiration, and the heart's action; when it ceases, cadaveric hyperæmia takes place.

In perhaps one fourth of these dissections, the bodies were carried to the dissecting room a few minutes after death. The external veins, chiefly of the arms, and neck, sometimes became distended—these being opened, the blood often flowed in a good stream—sometimes, shot out a foot or more; (that is, formed an arch having that diameter). In some cases, by putting a ligature upon the arm, or by grasping it above the elbow, the blood was made to flow more freely, and, by moving the muscles, as the patient is wont to do in ordinary blood letting, the blood shot out some distance. Punctures in the middle of the subclavian, discharged blood, which, arose in a full stream, against gravity, two or three inches forming, sometimes, an arch as it fell. The coronary veins of the heart, discharged blood rapidly, and with surprising force: I have observed within a few minutes after death, the flow of from twenty to thirty ounces of blood from the scalp, the head being raised:—Prominence of the veins, after death, is doubtlessly owing to the capillary circulation, which first fills the central, and then the external veins to excess: These dissections show conclusively the independent action of the capillaries. If capillary action exist independently of the heart, congestions &c, may disappear, and others form during or after death &c. Hæmorrhages after death &c. Post-mortem blood letting proves, that the heart's action is not necessary to the capillary circulation. The proofs are many, though they might have been greatly multiplied, had I taken a just view of this matter sooner—distention of the veins, copious hæmorrhages, crimsoned flushings of the face, passed for nothing—not supposing it possible that the veins could discharge more blood than what they contained at the moment of death, nor that capillary life survived the extinction of the general life, as manifested in sensation,

* Professor Gross, of Louisville, in his able work on Pathological Anatomy, thus speaks concerning the *seat* of inflammation: "that this is in the capillary vessels is a fact concerning which there exists no dispute."—31.

respiration, systemic circulation &c. But, as soon as I had discovered the curious and complex laws of post-mortem contractility, the possibility of the doctrine of a post mortem, vital capillarity, flashed across my mind. (Various co-ordinating phenomena were subsequently noticed, particularly, post-mortem caloricity, the laws of which are so peculiar as to constitute a distinct branch of thermotics.) Are not hæmorrhages in yellow fever due to capillary congestion? &c. (Many other passages of similar date and import might be copied from the originals, which are open to any one).

From the Western Journal, I take the following extract, omitting the ten decided cases, each of which, proves the existence of the post-mortem capillary circulation: "The doctrine of the capillary circulation, as surviving that of the heart, and large arteries, renders tardy dissection a most fallacious guide in judging of venous congestions, vascular turgescence, organic reddenings, blanchings, &c." "I may be permitted to say, that the following facts prove that the direct action of the heart, and its indirect or suction power joined to the suction produced by respiration, are not necessary to the rapid motion of the venous blood. To say nothing of cadaveric injection and exudations, the transporting power of the capillaries might have deposited the blood in new situations, sponging it out from structures that had suffered during life from acute hyperæmia, engorging others that had been healthy, blanching the former, reddening the latter. At the right side of the heart, venous stagnation probably begins, its vessels being wholly passive. The capillary power is soon neutralized by an opposing mechanical force, or ended by an entire cessation of vital action."

I make these extracts for the consideration of honest Æsculapians, (these are not few) who are willing to give every man his due. If any one has ever before established the independent action of the capillary circulation in the living or dead human subject, if any one has applied this discovery, a fruitful one, in explanation of pathology and morbid anatomy, I am ignorant of the fact.

The number of illustrative experiments accompanying this paper, might have been greatly increased. I fear that some of the most valuable have been omitted, owing to my want of leisure to search for them in ten Folios, (from M S. Vol. IX to XIX)—some of which contain nearly 1000 pages,—all of which are imperfectly indexed, in relation to the subject now submitted to the reader. Although, long ago, I promised—voluntarily, it must be confessed—to give this experimental sketch, yet, nothing in the way of composition and arrangement was done, until late in November; one portion having been written, while the other was at the printer's hands. Of arrangement there is little or none—the cases were copied, exactly as they were found, in turning over my M S S. I, therefore, have now, as upon former occasions, to ask indulgence for hurried papers incidental to more extended aims, and plans. which require much labor and time. But of this self imposed task, I have no right to complain. Happy, if I shall, in the smallest degree contribute to advance that noblest science, whose high behests require of its votaries to do all they can, to promote the well-being, the health, longevity, and euthanasia of mankind.

New Orleans, Decr. 1st, 1848.

ADDENDUM.

Since this paper was sent to the press, Cholera has appeared in New Orleans. Not wishing at present to enter into the pathology, nor morbid anatomy of this remarkable malady, I may, nevertheless, be allowed to remark, that the *rôle* of the capillary system is a fundamental consideration, a point of departure in the investigation of this disease. Intimately connected with capillary derangement, or collapsion, including physical alterations of the blood, is that most characteristic feature, in fully developed Cholera, namely, a loss of animal heat, at every accessible point of the body, as under the tongue, in the palm, bend of the arm, popliteal region, axilla, and so on. In the axilla, the heat is least depressed. The temperature, before, and after death, from Cholera, has been noted with exactitude, probably for the first time in the history of this plague. Whether algidity be regarded as the cause or the effect of capillary derangement, it is an element of the greatest value, characterizing the disease, and indicating its pathology. The heat, in the dead body, sometimes, mounts to 110½°, but, it reaches its culminating point slowly, often requiring from one to two hours or more. It often remains stationary, or very nearly so, for a considerable period, and, what is the most extraordinary of all, this heat fluctuates, rising—falling—rising again;—a fact which I have observed in Yellow Fever. A portion of a case noted this day, (Decr. 24th, 1848), will serve as an illustration; at about an hour and a half after death from Cholera, the body of H. P., reached its maximum; in the mean time, the thermometrical readings of the rectum, at intervals of five minutes, noted consecutively, were as follows; 108°, 106½°, 106⅓°, 108°, 108½°, 109°.—On ligating the arm, and opening a vein, in this subject, the stream arose at least six inches perpendicularly.

In cases wherein indications of post-mortem capillarity exist, these are tardy in developement. The veins of the arms are rarely, the jugulars frequently, filled. From the latter, I have taken in a few minutes, as in ordinary venesection, six or seven ounces of blood, having, as is usual in this disease, a black color, and a thick, glutinous consistence, forming a soft, sticky, adhesive clot, yielding in twenty four hours, only a slight quantity of serosity. The subclavians, and other large venous vessels of the centre, usually discharge, in a short period, sometimes, in half an hour, from four to five pounds of blood, similar to that already mentioned, flowing, not all at once, but in a gradual manner, indicative of capillary action. In confirmation of this, I could relate a number of experiments on Cholera subjects, made from one to six hours after death, upon the small veins, and arteries of the heart, the mesentery, omentum, and pia mater, raised, artificially, above the highest part of the body, showing, that after these vessels have been emptied, they refill, in opposition to gravity, the blood flowing in the direction peculiar to the living condition of that part of the vascular system in which it is found. These results are the more remarkable, when the quality, and the reduced quantity of the entire mass of the blood are considered.

Heat, contractility, and capillarity, do not always bear towards each other a definite ratio, nor even a coincidence. P. K., whose temperature before death was low, after death not very high, and who had no exter-

nal indicants of capillarity, at an hour and a half after death, spontaneously contracted his thumb and fore-finger, six or seven times, as if about to take a pinch of snuff; spasmodic twitchings took place, also, in the little finger. Do the arteries and the heart undergo spasmodic contractions and collapsions? Even before the general collapse, paroxysms of pulselessness occur, not only during the cramps of the muscles, but in the intervals. In grave cases, the pulse or radial artery, is inexpressibly small, a mere thread—probably not the one tenth of the size of that observed in Yellow Fever, or even in the dead body. It is probable that the left side of the heart, and the arteries, (the pulmonary arteries only excepted,) are nearly empty in the collapsed stage, owing to the general failure of the capillary circulation. Hence the shriveled or rather *withered* condition of the skin, and often of the lungs, while it may reasonably be supposed, that the capillaries of the soft, spongy, mucous membrane of the stomach and bowels, including the imperfectly valved lacteals, assume, either an inverted action, or yield to the pressure from the circumference, and, consequently, pour out the white blood, and sometimes, chyle, into the intestinal canal, producing what might be called an acute, serous hæmorrhage, or an apoplexy of that vastly extended cavity, and, thereby, in a great degree washing away, or preventing the absorption of, and, consequently, the efficacy belonging to, remedial agents. Without pretending to know anything of the *vera causa* of this epidemic, which is absolutely unknown, I would ask—Is not capillary collapsion of the surface, and capillary effusions of the centre, *Cholera itself*, the invariable antecedents of algidity, asphyxia, cramps, pulselessness, and death?