AN

EXPERIMENTAL ESSAY

ON CUTANEOUS ABSORPTION:

PUBLISHED AS AN

INAUGURAL DISSERTATION.

SUBMITTED TO THE EXAMINATION

OF THE

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(*PRO TEMPORE*),

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FOR THE

DEGREE OF DOCTOR OF MEDICINE.

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"We ought, in every instance, to submit our reasoning to the test of experiment, and never to search for truth but by the natural road of experiment and observation." LAVOISIER.

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TO

DOCTOR W. A. DAINGERFIELD,

OF ALEXANDRIA,

AS A TESTIMONY OF RESPECT

FOR HIS

INFORMATION AND TALENTS AS A PHYSICIAN,

OF ESTEEM FOR HIS

CHARACTER AS A MAN,

AND OF WARM AFFECTION TOWARDS HIM

AS A BROTHER,

THIS ESSAY IS INSCRIBED,

BY HIS PUPIL AND FRIEND,

HENRY P. DAINGERFIELD.

INTRODUCTION

TO

AN EXPERIMENTAL ESSAY

ON ABSORPTION.

IN selecting the subject of cuticular absorption for an inaugural dissertation, I have not been misled, either by the hope of offering any thing new on so interesting a question, or of rendering that which we already know more valuable by the beauties of my style, or the force and perspicuity of my reasoning.

Medical science is accused of having hitherto advanced with a slow and halting pace ; and this too, perhaps, principally because it is an experimental science. Nature, although simple and determinate in her laws, nevertheless requires that he who consults her oracles, if he wishes to obtain correct responses, should undergo the fatigue of minute and laborious enquiry. It is this labour and fatigue that frequently determine physicians to remain in ignorance, or to be content with imperfect information. Instead of going to the fountain head, to ascertain whether the data from which they reason be founded in truth or in error, it is this pain and difficulty of making and of repeating experiments that determine them to receive theories as established, that are founded only on a single series of observations. It is in this way that one author has been induced to rely solely on another, and in this way that we have volumes on volumes that contain little else than a repetition of what had been previously known. In short, until within the present enlightened era of philosophy, those that have experimented, have done it with so little attention to the minutiæ by which the results of experiments are varied, have been so imperfect in the detail of their modes of proceeding, that the mind is seldom able to rest satisfied with their conclusions; and doubt and uncertainty have thereby been entailed upon us from century to century.

On the subject of cutaneous absorption, the medical world has not long been divided. From the times of Hippocrates and Galen down to those of Sabatier*, it has been received as an unquestionable fact, that substances are taken, by absorption, from the surface of the body into the general circulation. The contradiction of this doctrine, at once so novel, and apparently so absurd, could not fail to excite much attention. Much, therefore, has been said on both sides of the question, and physicians, once so unanimous in favour of such a power, are now, I believe, very much divided in relation to its existence.

Doctor Rousseau, a graduate in this university, was the first among us to institute a systematic examination of the question; and has, I think, by a set of ingenious and well-devised experiments, succeeded in proving, that there is no such function as cuticular absorption.

But for the establishment of so interesting a question, is it enough that we have only a single series of experiments? and those too supported only by the testimony of a single individual? May not the tests which he employed have been fallacious? May not the want of accuracy on the part of the experimenter have led to deductions widely different from truth? To decide, therefore, this point with accuracy, the only alternative left us is to repeat and extend his experiments. Should their results, when thus re-

* A late French author, who, in conjunction with Lavoisier, wrote a paper on this subject for the Academy of Arts and Sciences at Paris.

peated, be strikingly coincident, surely we shall then have greater reason to rest satisfied with the truth of his opinions than we now have. Mine, then, be the humble lot of such repetition; mine the satisfaction of contributing to render truth more certain, by multiplying the evidences on which it rests.

Upon superficial examination it might be imagined, that enquiries of this sort were better calculated to amuse than to furnish valuable practical information. But when we advert to the fact, that the assumption of the doctrine of cutaneous absorption has been employed to explain the origin of contagious and epidemical diseases; the similarity of effect produced by the external application and the internal use of the same medicine; and that on it has been predicated the use of numberless ointments and lotions; we at once perceive the futility of such an objection: since a negative conclusion relative thereto would, on the one hand, free us from absurd theories; and, on the other, save that time which would otherwise be wasted in an inert practice.

Although the errors into which physicians have fallen on this subject have been numerous, much may be said in extenuation of them. That phenomena should have been ascribed to the lymphatic system, in early times, which the better judgment of the present day has referred to a different origin, ceases to be a matter of astonishment, when we recollect, that this system of vessels is so minute, as to have eluded the observation of the best anatomists prior to the last fifty or sixty years. How then could it be expected that, within so short a lapse of time, the true character and functions of so large a portion of the animal economy, so minute in its parts and infinite in its ramifications, could be marked and defined without falling into error! Men, too, who make discoveries in philosophy or the arts, are ever anxious to enhance their value, by ascribing to them every possible importance. Accordingly, no sooner was the existence of this system announced to the world, than it became necessary to assign to it uses commensurate with the ideas entertained of its value by its discoverers. Hence we find it made, not only the vehicle by which every digestible substance taken into the stomach is distributed throughout our bodies, but the medium by which every medicine, whether of internal or external application, produces its appropriate effects on the animal system.

The doctrine of absorption, as now taught, may perhaps be properly divided into lymphatic and lacteal. Of the truth of our opinions relative to the latter, as thus delineated by the elegant and learned author of the Botanic Garden, there can be no question:

> "Thus where the veins their confluent branches bend,
> And milky eddies with the purple blend;
> The chyle's white trunk, diverging from its source,
> Seeks through the vital mass its shining course;
> O'er each red cell and tissued membrane spreads,
> In living net-work, all its branching threads;
> Maze within maze its tortuous path pursues,
> Winds into glands inextricable clues;
> Steals through the stomach's velvet sides, and sips
> The silver surges with a thousand lips."

But when authors, proceeding still further, would explain the *modus operandi* of medicines, by conveying them unaltered into the general circulation, by means of absorption, we are compelled from facts to deny their assertions, and to adduce the experiments of Doctors Hodges and Walmsley* to prove that such is not the case in relation to medicines taken into the stomach; and that it is not in relation to those applied to the skin, will, I hope, be no less satisfactorily demonstrated, in the sequel of this essay.

Before we proceed further, it may not be improper to pass in review some of the principal arguments in favour of absorption from the external surface. An attention to the perspirable mat-

* See their Inaugural Dissertations.

ter, with which our bodies are incessantly bedewed, appears first to have led modern physiologists to suspect the existence of a set of vessels appropriated to this effect; and the subsequent discovery of the lymphatics, opening to their view the strong analogy between it and the lacteals, where, perhaps, they had already contemplated the function of absorption, determined them to convert this similitude into a further proof of the existence of cuticular absorption. Those vessels then, no sooner discovered than charged with the important office of absorption, have given birth to the theories already noticed; and the skin, which would appear to have been formed as a covering to the more delicate parts of our system, has been made the ready inlet of a thousand predisposing and exciting causes of disease. The rapid diminution that succeeds the application of carbonic acid to the skin has been supposed to depend on its transmission through those vessels into the general circulation. The narrative of the unfortunate sailors, who resisted the dreadful effects of thirst, by wearing wet jackets; the presence of spirits of turpentine and garlic in our breath and urine, when we have been exposed to their emanations; and, above all, the fact that mercurial frictions salivate, have severally been adduced in proof of such transmission.

But let us turn to the results of a more rigorous examination of facts. In reply to the first proposition, it may be affirmed, that positive experiment has proved the diminution of the carbonic acid to proceed from its absorption by the moisture of the hand, for which it has a very great affinity, and, consequently, that this phenomenon has no connection whatever with organic life. The second position is no less objectionable. Thirst doubtlessly depends upon a waste of fluids; a cold application, therefore, constringing the pores, checks perspiration, and thus mitigates its pains: not to add the insurmountable objection, that the absorption of a single drachm of water never could be detected by the best ballance that ever was invented, although the body was subjected to long and repeated immersions in baths of every variety of temperature*. To the third it may be replied, that whenever spirits of turpentine, or garlic, have passed into the general circulation, they may be decidedly proved to have entered it through the medium of the lungs; organs whose powers of absorption have been sufficiently demonstrated. And to the fourth and final proposition, it is also objected, that although it is difficult to assign a satisfactory theory of the mode in which mercurial frictions operate, yet it may be affirmed, from incontestable evidence, that the one now under consideration is perhaps as wide of the mark as any that could have been suggested.

Extending the principle of absorption from the lungs, as contended for by Saguin, Dr. Rousseau has attempted to explain their operation, by supposing the mercury to be volatilized during its application, and to be inhaled by the lungs. With this view, he has adduced numerous instances of persons who have been salivated, during their attendance on venereal wards, or after having been exposed to the influence of the more volatile saline preparations of this metal.

That the atmosphere of a room, in which a number of persons have been daily in the habit of using large quantities of different mercurial preparations, should become sufficiently impregnated with this fluid to excite salivation in their attendants, I can readily believe: nor do I find more difficulty in admitting a fact which is thus stated by Chaptal†: "Oxygenous gas, obtained from the mercurial oxides, almost always holds a small quantity of mercury in solution. I have been a witness to its having produced a speedy salivation, in two persons, who used it for disorders of the lungs. In consequence of these observations, I filled bottles with this gas, exposed them to an intense cold, and the sides became obscured with mercurial oxide, in a state of extreme division."

* See Dr. Currie's elegant experiments on this subject, in his work on the hot and cold bath.

† Vide his Elements of Chemistry.

Here, as in the first instance, we perceive the means by which mercury may be volatilized, and taken into the lungs. But when we consider the nature of mercurial frictions, this explanation certainly fails us ; since it is neither essential to their operation that the patient should inhale oxygen gas, obtained as above described, nor that he should frequent a venereal ward. " May it not be supposed," it is contended, " that, during the frictions, some of the mercury is, by the action of the air, assisted by the heat of the body, oxyded, and that it afterwards parts by degrees with its oxygen, which carries along with it to the lungs some parts of the mercury, which is with difficulty separated from it," and thereby produces its specific effects?

In reply to this overstained construction of possibilities, it may be affirmed, that positive experiment has proved, that neither the blue pill nor mercurial ointment are oxyded by friction*. Hence the doctor is deprived at once of the vehicle by which he conveys this fluid to its place of destination, and of course of his theory. But even though we grant, for argument's sake, that mercury is oxyded in the case of which we speak: how does it happen that a few drachms of ointment, and the friction of a few days, will excite ptyalism, whilst those who are engaged for years, and during every variety of temperature, in preparing this article for sale, are never so affected? And yet in the history of one or two men, whose employment this was, and of whom I made it my business to enquire particularly, I could never find that they had been affected by mercury in the slightest degree.

Again, if mercury is not absorbed from the skin (as I hope to prove hereafter), and if the absorbents which lie on the surface of the body are governed by the same laws with those that are seated in the lungs ; what reason have we to believe that they will take up mercury, when those on the cuticle have refused to do so? Should it, though, be contended, that those two sets of lympha-

* See Mr. J. Douglass's Inaugural Dissertation.

tics are governed by different laws; that those seated in the lungs are even as active as the lacteals; may it not be inferred, that if the last named set of vessels are so fastidious as to refuse to take up foreign matter, that the absorbents of the respiratory organs would also do the same? At least I can perceive no reason why we should deny the one set of vessels the power of absorbing extraneous matter, while we so liberally grant it to another.

If it be impossible then to explain the modus operandi of mercurial frictions, by calling to our aid the doctrine of cuticular absorption, the great question naturally occurs, how do they produce their effects?

On this subject, mysterious as it may appear, we are not, I conceive, more at a loss than we are to explain the mode of operation of any article of the materia medica, whether it be taken into the stomach, or applied to the skin. At one time, the absorption of medicine from either of those surfaces into the general circulation afforded an apt and satisfactory explanation of every phenomenon. Driven, though, by stubborn fact from this strong hold, we were compelled to resort to the doctrine of sympathy to explain, by a natural or acquired consent of parts, whatever that of absorption is inadequate to. Thus, for example, by long habit, the actions of the stomach and salivary glands are so connected together by the principle of association, that when a substance is applied to the stomach, calculated to produce a specific effect thereon, we invariably observe a correspondent effect to take place in the salivary glands. Mercury is a substance so calculated, and thus are the phenomena of salivation at once explained. Why not, then, extend this explanation to medicines applied to the skin? Why not suppose that habit or nature having established a certain consent between it and the salivary glands, mercurial frictions act in the same way?

Is it not in favour of this supposition, that the bark shirt cures intermittent fever? that tobacco nauseates, when externally applied? and that blisters excite strangury? When, though, in addition to this, we find that mercury applied to one side of the body has been known to affect that side only, this explanation will become still more plausible. Mr. Richerand, in his excellent Treatise on Physiology, states, that a "young man for whom" he had "ordered frictions on the inner surface of the left leg and thigh, to resolve a large bubo, was seized with a salivation the third day, although only half a drachm of ointment had been used each night; the salivary glands on the left side only were swelled, the left half of the tongue was covered with apthæ, the right side of the body remained perfectly free from mercurial influence:" an evident proof that ptyalism, in this instance, could not have proceeded from absorption, but must have been the result of some secret and unexplained sympathy. I am well aware, however, of the objection, that if the operation of frictions depended on sympathy, any other sialagogue ought to produce the same effects, when similarly applied. Thus strangury is often produced by the application of blisters, an effect hitherto ascribed to absorption; for it is contended, that if this effect resulted from sympathy, we ought to experience similar results from any of the siliquosæ, and yet no other visicating substance whatever excites this complaint.

In reply to this statement, it will be sufficient to observe, that the objector has done little more than beg the question. Pursue but for a moment this species of reasoning, and mark the consequences. If bark, for example, when applied to the surface of the body, cures intermittent fever by being absorbed, as it must necessarily do, according to this doctrine, are we not authorized to expect equally beneficial effects from any article of the class of tonics, that could be as readily taken into the circulation as bark? Hence, if this reasoning proves any thing, it is simply that cantharides ~~is~~ are specifically different from any of the siliquosæ; that blisters will excite strangury, whilst cataplasms of mustard will not; for the same reason that a purgative does not generally prove an emetic, nor an emetic a purgative.

That no absorption of cantharides takes place in the case of strangury, I think sufficiently evident, for the following reasons:

First, If we admit of absorption in the above instance, we must also admit of specific determination to the bladder: and yet specific determination is more difficult of explanation than absorption.

Secondly, This effect ought not only invariably to occur, but to take place in a short space of time, as the absorbents are supposed to be always in a state of activity.

Thirdly, If this symptom be referable to the principle of absorption, it ought to be proportioned, in its degrees of violence, to the size of the blister, and to its length of continuance, neither of which circumstances are noticed by authors on the subject.

Lastly, Because for strangury no remedy, according to the practice of some physicians, is better than the application of a second blister.

Here then is an effect produced from the skin on the bladder, in which the principle of absorption could have had no agency. How then can it be better explained, than by referring it to that great law of the animal economy called sympathy? It is vain to object that the doctrine of sympathy explains nothing; that it is vague and indeterminate in its signification: for when defined to mean no more than that consent of parts, which has invariably been remarked to take place under certain circumstances, it clearly furnishes as full and satisfactory an explanation of many of the phenomena of life, as our present imperfect knowledge of cause and effect will permit us to obtain, from any source whatever. Thus, for example, when I say that a dose of turpith mineral will speedily excite a flow of saliva, in consequence of a sympathy that subsists between the stomach and salivary glands, is not the explanation of antecedent and consequent as complete as when I say this effect depends on absorption?

It would be easy, in this way, to multiply arguments against the doctrine of absorption; but as my introduction is, perhaps, already swelled beyond its proper limits, and as it is improper, on a subject so purely experimental as the present, to trust to analogy what can be made the subject of direct experiment, I shall proceed to relate such as I have made, in their proper order, and with the candour and impartiality of one who has no favourite theory to support, but whose only object is truth.

As the experiments of Doctor Rousseau, as far as I know to the contrary, are those that have excited most attention, or, at least, as they furnish almost all the direct evidence that we have against the existence of cutaneous absorption, I thought it my duty, for reasons previously assigned, to repeat some of the most important of them, and to communicate their results, before I proceeded to relate those which have been devised and executed by myself.

EXPERIMENT I.

With a view to determine whether spirits of turpentine would discover itself in my urine, by its characteristic property of imparting to it a violet smell, I took a few drops of it on a lump of sugar, and, in the course of three hours, discovered a strong smell of violets in my urine.

EXPERIMENT II.

Several days after the above experiment, when I could not expect that a particle of turpentine remained in my system, I proceeded to the following: At ten o'clock of a fine morning in April, the temperature of my room being 65 degrees of Fahrenheit, and my pulse 60, its natural standard, after a light breakfast, I exposed myself to the emanations of spirits of turpentine in a closed room, by placing some of this fluid in a saucer, on a table, at which I was busily engaged in writing. In about an hour, I referred to my urine, and found the smell of violets as strong as in the foregoing experiment, where the turpentine had been taken in substance. There then remained no doubt but that the emanations from this fluid, entering the body, had been conveyed into the general circulation. The difficulty, though, still remained: how had it entered the system? To ascertain this point, therefore, the following experiment was repeated.

EXPERIMENT III.

I provided myself with a long tin tube, by means of which I could, while sitting in my room, draw the air from a distant place without it, where none of the emanations, to which my body was exposed, could have previously existed, or have been conveyed during the experiment.

Thus provided, and in a room whose temperature was 75 degrees of Fahrenheit, I took the tube in my mouth, closed my nostrils, and immersed my hand and arm nearly up to the elbow in spirits of turpentine, when I directed the jar to be luted round my arm, in a manner that rendered it impossible that I should afterwards inhale any of the effluvia of this volatile substance.

Having previously taken a diuretic draught, my assistant was enabled, at the end of an hour, to examine my urine, but could discover no smell of violets. The perspirable matter of my body, and my breath, were also examined, and were found to have undergone no change. To render this experiment still more conclusive, another diuretic draught was taken, and it was determined to continue the immersion for an hour longer; but when half that period had elapsed, the pain became so intensely severe, that it could no longer be supported. The urine, perspiration, and breath were therefore again examined, but with the same results;

after which the hand and arm were withdrawn, swelled to nearly double their natural size, and so excessively painful, that I was unable to use my fingers or wrist for several hours. The pain and burning occasioned by this severe application gradually subsided, leaving behind them a scarlet redness, and great sensibility of the parts, which continuing for four or five days, terminated in a total destruction of the cuticle. From time to time, throughout the day, I repeated the examination of my urine, breath, and perspirable matter, but never could detect in them any thing that authorised the opinion that this substance was or could be absorbed from the external surface. Conclusive, therefore, as this experiment would appear, it has nevertheless been objected to it, that the surface exposed to the influence of the spirits of turpentine was too small, and that the activity of its absorbents might have been less than that of those seated on other parts of the body. To obviate this objection, I proceeded to

EXPERIMENT IV.

On a fine morning in April, the temperature of the room in which I proposed to make the experiment being between 70 and 80 of Fahrenheit, I adjusted my tube as formerly described, took off my clothes, placed the end of the tube in my mouth, stopped my nose, and directed my assistant to besmear my body and superior extremities by means of a spunge with spirits of turpentine. The pleasant sensations produced by the friction of the spunge counterbalancing the painful ones created by the turpentine, induced me to direct the attendant to continue it without intermission. The application of this fluid, therefore, so far from having been intermitted, was unceasingly continued throughout the experiment; so that the absorbents had the additional advantage of friction, by which to drink up the substance with which their mouths were besmeared. Three quarters of an hour elapsed, when my assistant received my urine in a vessel as I sat, but could not discover the least smell of violets. The hour being completed, my body was carefully washed. I then closed my mouth and nose, and walking precipitately into the next room, examined my urine, without being able to detect in it the odour by which the absorption of turpentine was to have been proved. My friends also examined my breath, and were unanimous in saying that it had undergone no change. The common tests were frequently resorted to during the succeeding twenty-four hours, but with the same results.

EXPERIMENT V.

If, then, it is satisfactorily demonstrated by the foregoing experiments, that whenever spirits of turpentine manifests itself in the urine, breath, or perspirable matter, it could not have been introduced by means of cuticular absorption, it remains to prove by experiment what has already been assumed, that it enters the general circulation through the medium of the lungs. With this view, having introduced my long tube into the mouth of a large bottle which contained a little spirits of turpentine, I inhaled several successive times the emanations of this fluid. At the end of an hour, referring to my urine as a test, I found it strongly tinctured with a violet smell. This experiment, therefore, while it proves the strong powers of absorption possessed by the lungs, amply accounts for the innumerable mistakes that have been made in ascribing the presence of certain substances in the general circulation to absorption from the skin.

Doctor Rousseau, after having related experiment No. 4, recommends it to those who may repeat it to be extremely cautious how they proceed, as the most trivial inattention might widely vary its results. To me, though, I hope this caution was unnecessary. I candidly confess, that the experiment was undertaken with the firm persuasion that conclusions would be drawn from it different from those that had been drawn; but having no favourite theory to support, I did not care to repeat it with the greatest circumspection. It was made in the presence of a worthy friend and ingenious fellow-graduate, who, believing that absorption from the external surface did actually take place, was so well convinced of its accuracy, that his opinions on this subject have thereby undergone considerable change.

Indeed, when I first heard the existence of this function questioned, I was thunderstruck with what I then believed to be the absurdity of the opposite opinion. I was in vain told, that a graduate in this university had experimented largely and with great acumen on the subject; that spirits of turpentine applied to the skin produced no sensible effect; that the poison of the viper, if brought into simple though close contact with the cuticle, was harmless; and that many of the phenomena hitherto ascribed to absorption had been distinctly traced to the inhalation by the lungs of the substances acknowledged to produce them. These were difficulties that my imagination readily surmounted. I admitted the facts, but gave very different explanations of them from those they had received at the hands of my opponents. The fundamental truth, that all medicines act specifically suggested itself as a weapon with which I could parry every objection, and confound my adversaries. Medicines, said I, when taken into the stomach, produce their several distinct and specific effects. The operations of opium are radically and invariably different from those of mercury, and the influence exerted on the animal economy by a purgative no less different than that exerted by an emetic. Let us then extend this general law of the system. The materia medica abounds with some substances that have, and some that have not, a specific operation on the lacteals and internal lymphatics. Why not then generalize the principle, and say, that it also abounds with some that have and some that have not a specific operation on the external lymphatics. Spirits of turpentine and the poison of the viper, for example, have no known tendency to promote absorption on the part of the lacteals and internal lymphatics. But the absorbents, seated both on the surface of the body and internally, it may be fairly presumed, are governed by the same laws; surely, therefore, we have no right to expect that the above substances will stimulate to increased external absorption, when they have been found to produce no such effect on the self-same congeries of vessels, as they exist in the different cavities of the body. But even though these two sets of vessels were admitted to be governed by different laws, still it would appear to follow, that the non-absorption of the spirits of turpentine and the poison of the viper proved no more than that those substances have not that specific operation calculated to awaken the cutaneous lymphatics into action. Again, too, it was further objected, that the highly acrid and stimulating substances, made use of in the experiments on this subject, were not such as were most likely to be taken into the general circulation, and of course that no reasoning from them could be received as conclusive.

To give, therefore, this question the certainty which it evidently deserved, it became necessary, not only to repeat the experiments previously related, but to extend them to a variety of articles, some of which at least should be milder and more bland than those heretofore employed. With this view the following series was instituted.

SECOND SERIES OF EXPERIMENTS.

EXPERIMENT VI.

To accomplish these important ends, I selected a substance which, from the uniformity with which it has been supposed to produce its effects through the medium of absorption, promised results highly satisfactory. The substance alluded to is mercurial ointment. To ascertain whether it did or did not produce salivation, together with its other effects, in consequence of being taken into the general circulation, I applied large plaisters of it to the calves of my legs. But as I was apprised of the objection, that the mercury might be volatilized and taken into the lungs, unless means were adopted to prevent it, and that if applied to an abraded cuticle, the experiment would be inconclusive, care was taken to avoid both the one and the other of these sources of error, by applying them to a perfectly sound cuticle, and by covering them with thick bladders, rollers, and a pair of stockings, which were not removed until the end of the experiment. Thus circumstanced I waited patiently the arrival of a speedy ptyalism, but at the end of eight days was completely disappointed, never having observed the slightest alteration in my general health, nor in my pulse, nor in the discharge of saliva.

Not content though with this, and willing to believe that the result of my experiment had failed to correspond with my expectations only in consequence of want of attention to a restricted diet, I accordingly resolved to live exclusively on vegetables, and to eat even of these with moderation. This determination was immediately carried into execution, but with no better success; for, at the end of another week, the plaisters had certainly produced no sensible effect. Unwilling, though, to abandon a doctrine so generally acquiesced in, and ardently desiring to know something conclusive on this subject, I reduced my system still farther, by the loss of fifteen or twenty ounces of blood, continued my vegetable diet, and applied, with the former precautions, two large mercurial plaisters to my fore arms, where they were suffered to remain seven or eight days; at the end of which time, as they had produced no effect whatever, I put an end to the experiment, after its having lasted the greater part of three weeks.

EXPERIMENT VII.

From the foregoing facts it appeared, that I was fairly authorised to conclude that there was no active power of cutaneous absorption; since, if there was, I ought to have been salivated, as having lived under every circumstance essential to that event. Afraid, though, to trust this conclusion to conjecture, when it could be made the subject of direct experiment, and apprehending that my experiments might possibly have failed from want of activity on the part of the absorbents, to which the plaisters were applied, or from some idiosyncrasy of constitution, or from some imperfection in my mode of living, or in the quality of the ointment used, I resolved to try the effect of frictions. Accordingly, on the same parts, with much less of the same ointment than had ever been applied at any one time in the form of plaisters, I succeeded in three nights in gently affecting my mouth.

EXPERIMENT VIII.

From the foregoing experiment it would appear, that the absorbents, stimulated by the friction used in the application of the ointment, were compelled to take it into the circulation.

To determine, therefore, how far this was really the case, it was necessary that the ointment should be applied to parts which, at the same time that they possessed the advantages of friction, should leave nothing to apprehend from its being volatilized and taken into the lungs. Accordingly, I provided a strong pair of oil-cloth socks, applied an ounce of unguentum hydrargyri fortius to the upper surface of each of my feet, put on the socks, drew a pair of stockings over them, and regularly walked a mile or two every day, that the friction of my boots against my feet might cause the mercury to be absorbed. At the end of ten days, though, I was greatly astonished to find that they had produced no effect. The socks were therefore taken off, and the same quantity of ointment again applied, after which they were renewed, and permitted to remain sixteen or eighteen days longer; but as in this time no alteration had taken place in the state of my salivary glands, they were again removed, and the experiment considered as concluded. It may not be improper to add, that my diet on this occasion was low, and strictly vegetable.

EXPERIMENT IX.

Apprehensive that there might not have been friction enough in the above experiment, to answer the purposes intended to be answered by this indispensable agent in the production of salivation by means of mercurial ointment, I was anxious to devise some remedy for this probable deficiency. For this purpose, it was recommended to apply strongly stimulating substances to my feet, which, at the same time that they excited the absorbents, would not impair the cuticle. With this view, having poured some boiling water on a quantity of bruised mustard seed, I immersed my feet in the infusion, until the irritation became so great as to be almost unsupportable, when they were taken out, and the socks, with the same quantity of unguentum hydrargyri fortius used in the former experiment, were again applied. This done, I immediately set out on a long walk, that the mercury might be rubbed into my feet, and the stimulus of the bath be thus co-operated with by that of friction. The socks were worn a week; but, as at the end of that time no appearance of mercurial affection was to be discovered, they were taken off, and the experiment regarded as complete.

I am well aware, that this experiment may be conceived by some to militate against my explanation of the modus operandi of mercurial frictions; since it may be asked, if they produce their effects in consequence of a sympathy between the skin and salivary glands, why this sympathy was not excited in my experiments with the mercurial socks? The answer to this question is, I think, extremely obvious: for the feet are so far removed from the centre of circulation, and so far from the salivary glands, that it could not be expected they would be readily brought into sympathy with each other. Hence ptyalism was not produced; and for the analogical reason that " children under a certain age cannot be salivated, because those two sets of vessels" (the stomach and salivary glands) " have not acted long enough together for their motions to become associated*."

* See Dr. Young's Inaugural Dissertation.

EXPERIMENT X.

As I had now made every experiment with mercurial ointment that promised any thing conclusive on this subject, I thought it not improper to direct my attention to such other substances as should, by certain characteristic properties, enable us to detect them in the general circulation, should they be absorbed. Being therefore provided with large cataplasms of bruised garlic, I disposed of my tube as formerly described, took the end of it in my mouth, closed my nose, and directed my assistant to apply the plaisters under the axilla of both arms.

In the selection of this place, as the most proper for the application of the garlic, I was influenced principally by two reasons: 1st, Because it appeared to me, that if garlic failed to manifest itself in the urine, when applied to the feet, it might have proceeded from the languor of circulation in those parts; and 2dly, Because having been informed that mercurial applications succeeded no where so well as under the arms, it appeared clearly the most eligible diposition of my cataplasms that could be made. Thus situated, I suffered them to remain an hour, at the expiration of which time they were removed, and the parts carefully washed, when I quitted the room, with every precaution to avoid inhaling their penetrating odour. My urine, breath, and perspirable matter were now examined, but did not, at this or any subsequent period, discover the least smell of garlic.

EXPERIMENT XI.

Having heard the following fact advanced in favour of the absorption of foreign matters into the general circulation, I determined to repeat the experiment.

It is said, if a strong ligature be made round the arm, so as to stop the circulation in the subcutaneous veins, and that if the corresponding hand be then immersed in a strong solution of nitre, while it is occasionally chafed by the other hand, that the nitre will be absorbed. To prove which it is affirmed, that if blood be drawn from a vein in which its circulation had been stopped, and then dried on a piece of paper, it will flash when burned, as if containing nitre. Suffice it to say, that I repeated this experiment, with the precaution of washing my hand before the blood was drawn, and that I found the assertion to be entirely unfounded.

Thus, from a careful investigation of facts, have I been obliged to relinquish my former opinions, and to acknowledge, that the function of cuticular absorption has no other claim to our belief than the sanction of hoary-headed authority, than mental apathy, or inattention to the evidences by which it has been supported.

In thus denying the existence of cutaneous absorption, far be it from my intention to fly so directly in the face of anatomical demonstration as to deny that there exists a set of absorbents beneath the skin. All I contend for is, that they have not the power of conveying foreign substances into the general circulation, whether these substances be applied to a sound or abraded cuticle. To prove that such is not the case, in the first instance, nothing more, I presume, need be said; and that it is not in the second, may, I think, be fairly inferred, from the following statements:

First, As the lacteals will not carry extraneous matter into the general circulation, so we have no reason to believe that the lymphatics will.

Secondly, Because the spirit of turpentine was not absorbed in two of my experiments in which the cuticle was destroyed.

Thirdly, From the analogical fact, that mercurial frictions act by sympathy, and not by absorption*.

* This argument is conclusive, for the cuticle is often destroyed, where the frictions have been continued for a long time, or have been very violent.

Fourthly, Because congestions and obstructions have never been detected in the course of the lymphatics.

And lastly, Because it is easy to find a much wiser and better employment for this system of vessels, than that of converting our blood into one indescribable mass of heterogeneous matter. The arteries carry to the various parts of our bodies the matter out of which they are formed; the lymphatics mould and give to it its proper shape. One part is redundant, they prune it to its proper standard; another deficient, the arteries furnish the proper materials for its enlargement, the lymphatics manufacture them.

It is now time that I should put a period to this essay. How far I have succeeded in establishing the points contended for, is not for me to determine. All that I can ask, or flatter myself with obtaining, is, that the good sense of every person who dispassionately considers the subject will prevent the attempt from being considered as chimerical.

ERRATUM.

Page 6, line 10, for *Sabatier* read *Saguin.*