AN

INAUGURAL ESSAY,

CONTAINING

EXPERIMENTS AND OBSERVATIONS

IN DEFENCE OF THE

DOCTRINE

OF

CUTANEOUS ABSORPTION,

FOR THE DEGREE OF DOCTOR OF MEDICINE.

SUBMITTED TO THE EXAMINATION OF

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OF PENNSYLVANIA,

ON THE 18*th DAY OF APRIL*, 1810.

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INAUGURAL ESSAY, &c.

THE subject of *Cutaneous Absorption* having, within the last ten years, attracted the attention of some of the most eminent physicians of this country; and the doctrine of *Non-cutaneous Absorption* having been very ably supported, many have been induced to adopt it; and some have even supposed it established beyond the reach of controversy. But after having heard it ably advocated, and after having attentively perused the different papers published relative to it, by graduates in this University, though I was not persuaded by the eloquence of the former, nor convinced by the experiments and arguments of the latter, yet I was unable at that time to refute them. Having been early taught that "one truth in medicine is worth a thousand unconnected lifeless facts," I could not conscientiously rest my opinion on other men's bare assertions, however great their eminence, either as physiologists or physicians. I resolved, therefore, to avail myself of my first leisure moments, to repeat Dr. Mussey's experiments, with Madder, (the correctness of which had been by some doubted) and at the same time try such other articles as I might deem most proper, and to rest my opinion on the result of those experiments. Accordingly, having engaged my ingenious friend, Mr. Thos. P. Jones, to assist me, on the 17th of March, 1810, I instituted a course of experiments with the Rubia Tinctorum, Rad. Rhei, Rad. Curcuma, and Garlic.

Experiment I.—At 15 minutes past 4 P. M. having evacuated my urine, I immersed myself, my head and neck excepted, in a strong watery infusion of *Rubia Tinctorum*, and remained in it two hours and a half. The temperature of the atmosphere was 34°, that of the bath fluctuating from 82° to 90°. Urine was drawn at the expiration of 1, 3, 8, 13, 15, 18, 26 and 37 hours after leaving the bath. The first portion was very pale and unusual in quantity; all the other portions, except the last, (which was of its natural paleness) were much above the natural color, particularly the 2, 3, 4, and 5 portions, which were of a higher color than common Madeira wine. On adding a solution of the Carbonat of Pot-Ash to the urine drawn immediately before entering the bath, and to that drawn one hour after leaving it, no perceptible change whatever took place in the color

of either of them. Added to the other portions, it instantly changed the color of all of them except the last, to a bright cramberry red; but the portion drawn at the expiration of 8 hours, gave the highest color. The last portion, or that drawn at the expiration of 37 hours, was not in the least changed by the addition of Pot-Ash, any further than water, or any similar fluid, would weaken the color by diluting it. The different portions of urine which were sensibly changed by the addition of the Pot-Ash, on standing 8 hours, deposited a copious white sediment, which was not the case with the other portions. My pulse, while in the bath, became slower and fuller, and I felt considerable langour and slight head-ache, for two or three hours after leaving it.

Experiment II.—With a view to ascertain whether the change of color, produced by the addition of Pot-Ash to several of the portions of urine, as above mentioned, depended on the presence of the coloring matter of Madder—March 18th, I added to a portion of urine drawn before entering the bath in the preceding experiment, a watery infusion of Madder, until it became of the same color as that drawn three hours after leaving it. On adding the Pot-Ash to this, it immediately assumed the bright cramberry red color. The Pot-Ash produced the same change on a weak infusion of Madder in common pump water.

Experiment III.—March 19th, at half past 9 A. M. Mr. Jones, having evacuated his urine, immersed himself, his head and neck excepted, in a strong watery infusion of the *Rad. Rhei*, and remained in it for two hours and a half. The temperature of the atmosphere was 40°, that of the bath fluctuating from 84° to 95°. Urine was drawn on leaving the bath, and at the expiration of 2, 4, 6, 9, 11, 21, 26 and 34 hours afterwards. All the portions, except the first and last, were very high colored. On adding a solution of the Carbonat of Pot-Ash to the urine drawn before entering the bath, and to that drawn on leaving it, no perceptible change took place in the color of either of them. Added to the other portions, it instantly changed all of them except the last, to a deep red color; but that portion drawn four hours after leaving the bath, gave the highest color. On the last portion it produced no visible change. All those portions that were redened by the addition of Pot-Ash, on

standing 20 hours, deposited a copious sediment; in those drawn at the expiration of 9 and 11 hours, it was most copious, and of a pale red color. While in the bath, his pulse was increased in force, but not much, if any, in frequency. No langour or head-ache succeeded.

Experiment IV.—In order to ascertain whether the color of the urine, as last mentioned, depended on the presence of the coloring matter of the *Rhei*, I made a watery infusion of *Rhei*, of a similar color to that of the urine drawn four hours after leaving the bath. On adding the Pot-Ash, it instantly assumed the same deep red color of the urine above mentioned.

Experiment V.—March 20th, at 15 minutes past 3 P. M. I immersed myself, my head and neck excepted, in a strong watery infusion of the *Rad. Curcuma*, and remained in it for two hours and a half. The temperature of the atmosphere was 45°, that of the bath fluctuating from 86° to 95° degrees. Urine was drawn on leaving the bath, and at the expiration of 2, 5, 12, 16, 21, 28 and 34 hours afterwards. All these portions, except the first and last, were much above the natural color; and on adding a solution of *Caustic Pot-Ash*, they instantly assumed a reddish hue, though in a much less degree than in either of the preceding articles. Those drawn at the expiration of 2 and 5 hours gave the highest color. That drawn 12 hours after leaving the bath, on standing 15 hours, deposited a copious sediment of a dusky brown color. On adding the *Caustic Pot-Ash* to the urine drawn on leaving the bath, and at the expiration of 34 hours, no perceptible change took place in the color of either of them, except their becoming paler by dilution.

Experiment VI.—In order to ascertain whether the color of the urine could be imitated, and also to determine whether the *Caustic Pot-Ash* was a proper *test* to detect the presence of *Curcuma*, I made a watery infusion of Curcuma, similar in appearance to the highest colored urine in Experiment V. On adding the Caustic Pot-Ash to this, and also to the urine above mentioned, they both assumed precisely the same red color. Wishing to ascertain whether the odor of certain volatile substances could be taken into the system in a manner similar to the coloring matter of the preceding articles, I made the following experiment with *Garlic*, taking the state of the urine, and breath, as a proper criterion.

Experiment VII.—March 21st, at 4 P. M. I took one end of a tube into my mouth, the other end of which was passed out of a window: pieces of adhesive plaster was then applied over my mouth and nose, so as to completely prevent the passage of air either to or from my lungs, except what passed through the tube. Mr. Jones then applied cataplasms of bruised garlic to my axillars, to the inside of my thighs and my ancles. At the expiration of one hour and a half, as they produced considerable pain, they were removed, and the parts (to which they had been applied) washed with warm soap-suds. I then left the room immediately, and a few minutes after, again washed the parts with soap and water, and changed my clothes; after which I took a walk of a mile. One hour and a quarter after the garlic was removed, my breath was sensibly tainted with the odor of garlic, so much so that two gentlemen who were in company with me at the time, mentioned it to me. Two hours after this, the smell of garlic was so strong in my breath, that it was not only very disagreeable to myself, but very perceptible to several persons with whom I was in company, and it continued so until late bed time. On rising from bed next morning, 14 hours after making the experiment, nothing of the odor of garlic could be perceived in my breath. The urine was frequently examined during the 30 hours succeeding the experiment. The portions drawn during the first two hours after removing the garlic, exhibited nothing peculiar either in color or odor. At the expiration of five hours, it had a disagreeable pungent smell, and at the expiration of fourteen hours, it was still more so; but the precise smell of garlic could not be perceived in it. This disagreeable pungent smell continued for 26 hours, after which it became imperceptible.

Experiment VIII.—With a view to ascertain whether garlic, when taken into the stomach, communicates any odor to the urine, March 23d, Mr. Jones eat several cloves of garlic. His urine was frequently examined during the succeeding 36 hours. At the expiration of two hours after eating the garlic, nothing peculiar could be perceived in the ordor of his urine; but at the expiration of 4, 6, 8, 12 and 24 hours, it had precisely the same disagreeable pungent odor of the urine mentioned in Experiment VII. At the expiration of 36 hours, it had entirely disappeared,

The result of this experiment, I think, fully establishes two points in question—1st. That garlic, when taken into the system, does communicate a peculiar odor to the urine; but it is one essentially different from that of the garlic.—2d. That the disagreeable pungent odor of certain portions of the urine mentioned in Experiment VII, was produced by some portion of the garlic used in the experiment.

Having concluded the preceding experiments, I could not for a moment hesitate which side of the question to advocate; for the articles used, or at least some part of them, appear to have actually entered the system; and can any one for a moment suppose that it is possible for the three first articles (which it is known are not volatile) to have been taken in by the lungs. Through what other medium then could they pass, if not by that of the cuticle? As it respects the experiment with the garlic, considering the compleat manner in which the lungs were excluded from any agency in the business, I think the result fully as conclusive as either of the preceding. The very result itself proves clearly, to my mind, that the odor was not taken into the system by inspiration; for if it had been, would not my breath have been more strongly impregnated with it at the conclusion of the experiment than at any subsequent period? And instead of increasing for some hours, would it not have gradually decreased?

If, then, not only the coloring matter (and probably something more) of certain articles, but the volatile odor of others, when applied to the surface of the human body, is conveyed into the system by means of *cutaneous absorption*, may we not reasonably suppose that mercury is conveyed into the system in a similar manner, when applied to the surface of the body in the form of *unguentum hydragyri?* If this is not the case, how, I would ask, is a salivation produced by its use in this way? I have heard some of the advocates for non-cutaneous absorption say, in answer to this, "That the *mercury was volatilized*, and afterwards inhaled into the lungs; or, that it was induced by a sympathy existing between the glands of the mouth and those to which the mercury had been applied; or, probably in both ways." To the first I would beg leave to observe, that it requires a degree of heat far above that of the human body, to volatalize mercury, and therefore, until they bring some proof

of its being the case, I have good reason for doubting it. But admitting it to be true, I would ask, how it happens that the attendants in the venerael wards of hospitals (who would, in such a case, be constantly in a mercurial atmosphere) are not frequently salivated? Such instances have, perhaps, seldom, if ever, occurred.—To the 2d I would only observe, that of late it has become so fashionable to refer every thing to sympathy which cannot be readily accounted for some other way; that it would have been *heresy* in any one to doubt it, or to have overlooked it on the present occasion.

By some it has been said—"Whereas the matter which is the cause of most of the diseases (particularly those which are strictly febrile) which affect mankind, floats in the atmosphere, it is not *reasonable* to suppose that the surface of the *human body* is endowed with the power of absorbing; for if this be the case, it would be hardly possible for persons who expose themselves to the open air (particularly in sickly seasons) to escape disease." To me this appears a very futile objection; for if we were to determine the question by reasoning in this way, it would be much more plausible to deprive the lungs of the power of absorbing, (a power that no one at this day denies) and confine it exclusively to the skin; in which case we could in a great measure guard the system by means of proper cloathing; whereas we are always under the necessity of breathing the circumambient air.

In making the preceding experiments, every attention was paid to have them done accurately; for having espoused neither side of the question, I felt no further interested, as to the result, than truth might be concerned. If the experiments have been correctly and properly made, I think the *doctrine* of *cutaneous absorption* must (at least to a certain degree) be admitted. And I think I am warranted in concluding, that certain substances (probably all those which are either nutricient or medicinal) do, when applied to the surface of the human body, pass into the system by means of *cutanepus absorption*. But should it hereafter appear that there has been a fallacy (which if there has I protest I am ignorant of) in the foregoing experiments; and that the substances used passed into the system through some other medium than that of the *cuticle*, I pledge myself to be one of the first to renounce the doctrine which I have here advocated. The End.