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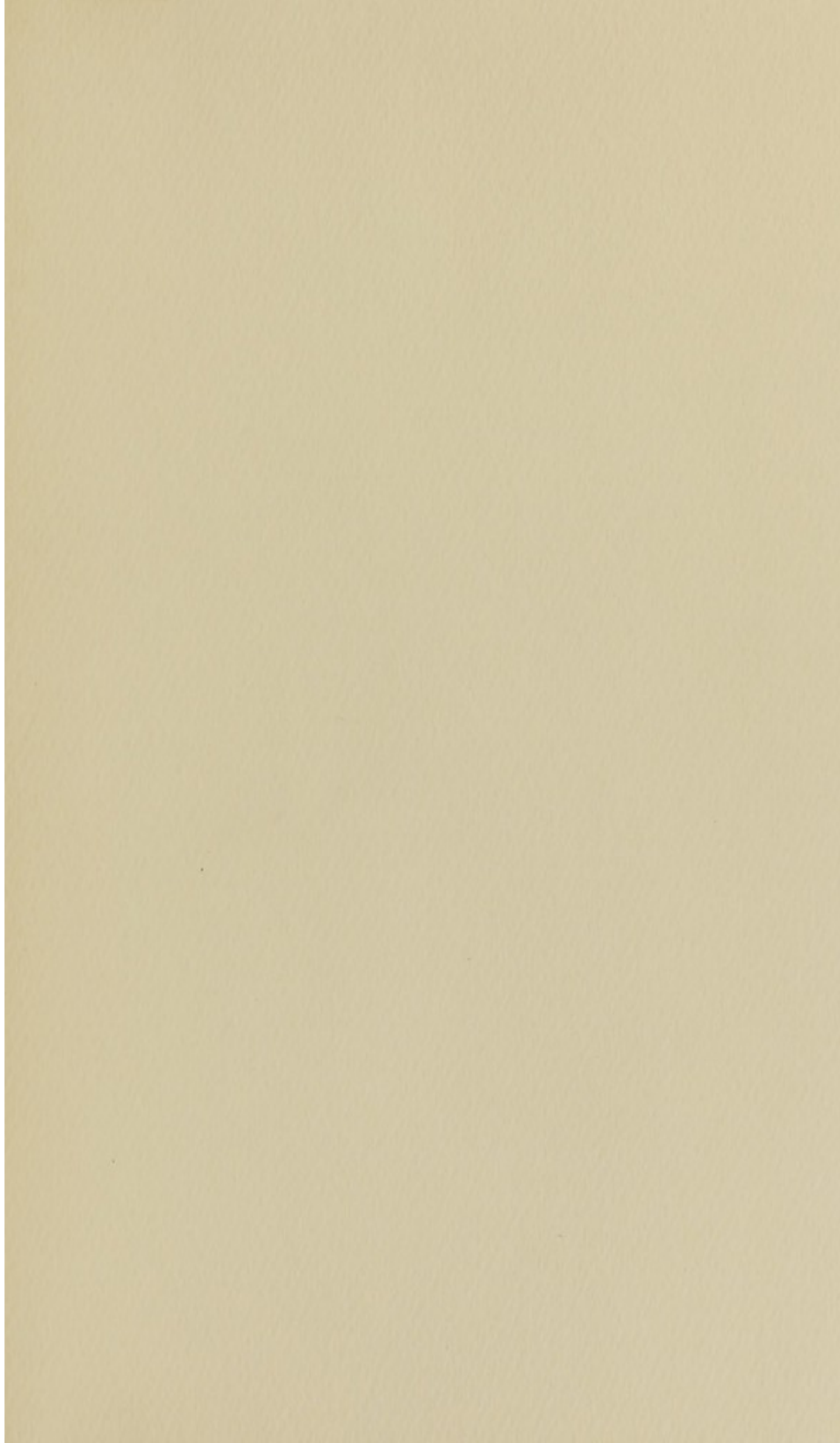
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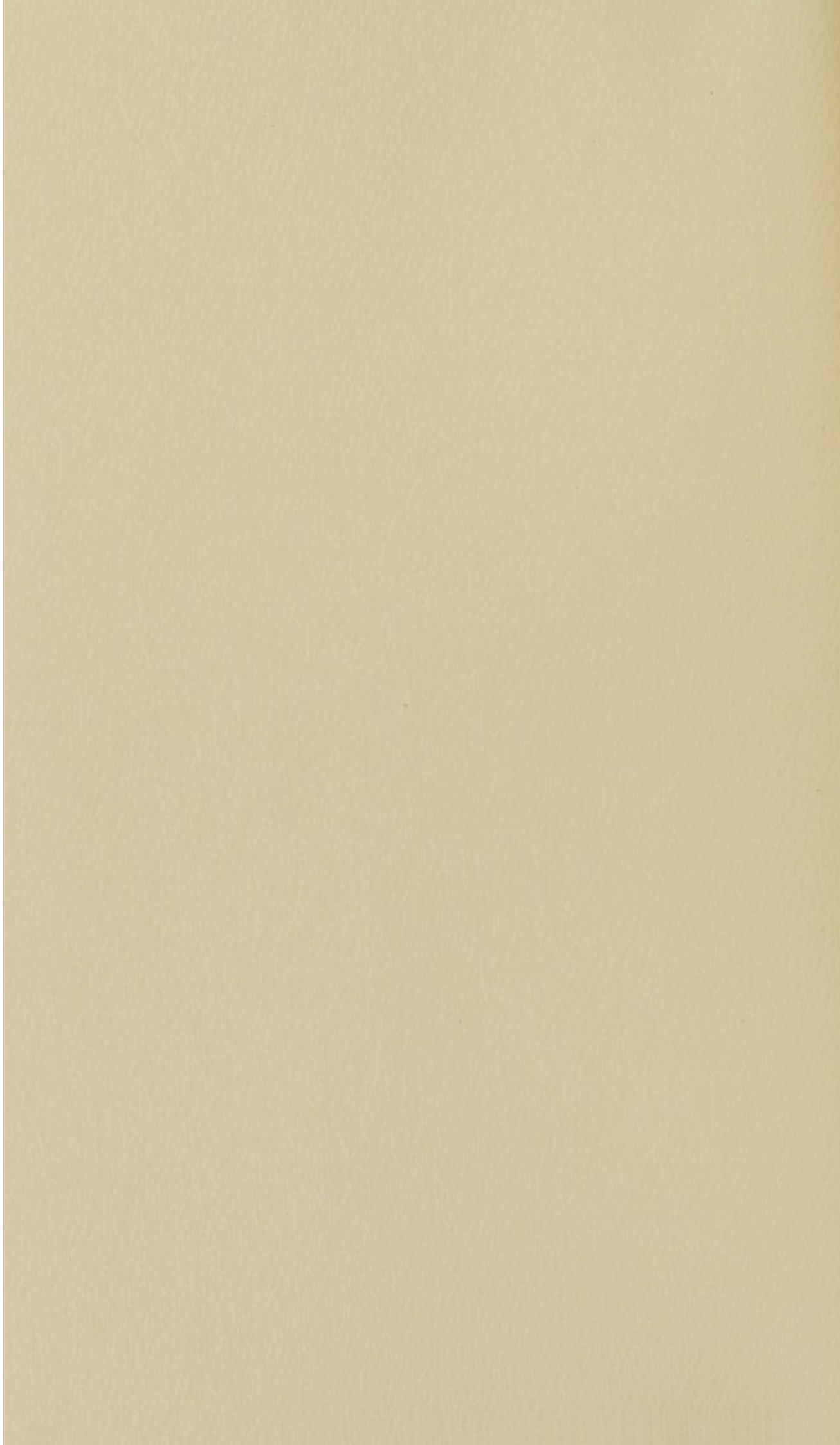
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EXPERIMENTS

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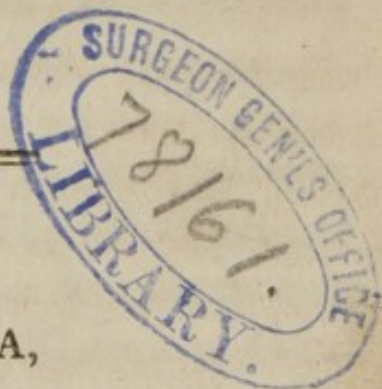
OBSERVATIONS

ON

CUTANEOUS ABSORPTION.

BY REUBEN D. MUSSEY, M. D.,
OF MASSACHUSETTS.

*[From the Philadelphia Medical and Physical Journal,
conducted by Professor Barton.]*



PHILADELPHIA,

FROM THE PRESS OF T. & G. PALMER.

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1809.

EXPERIMENTS

EXPERIMENTS AND OBSERVATIONS

OBSERVATIONS

ON THE ABSORPTION

The following experiments were performed to determine the rate of absorption of a gas in water. The apparatus used was a gasometer of known capacity, connected to a trough of water. The gas was introduced into the gasometer, and the volume of gas absorbed was measured by the displacement of water. The temperature of the water was kept constant throughout the experiment.

The results of the experiments are as follows: In the first experiment, the gas was introduced at a pressure of 1 atmosphere, and the volume of gas absorbed in 10 minutes was found to be 1.5 cubic centimeters. In the second experiment, the gas was introduced at a pressure of 2 atmospheres, and the volume of gas absorbed in 10 minutes was found to be 3.0 cubic centimeters. In the third experiment, the gas was introduced at a pressure of 3 atmospheres, and the volume of gas absorbed in 10 minutes was found to be 4.5 cubic centimeters.

From these results it is seen that the volume of gas absorbed is directly proportional to the pressure of the gas. This is in accordance with the law of Boyle, which states that the volume of a gas is inversely proportional to its pressure, provided the temperature remains constant.

The following table gives the results of the experiments:

Pressure (atmospheres)	Volume of gas absorbed (cubic centimeters)
1	1.5
2	3.0
3	4.5

It is also seen that the rate of absorption of the gas increases with the pressure of the gas. This is because the higher the pressure, the greater the number of gas molecules per unit volume, and therefore the greater the number of molecules that can be absorbed in a given time.

EXPERIMENTS AND OBSERVATIONS

ON

Cutaneous Absorption.

THE analogy of structure between man and other animals, and the great number of absorbent vessels found running along the cellular substance under the skin, seem strongly to favour the doctrine of Cutaneous Absorption. For what purpose, it is natural to ask, were absorbents thrown in such profusion under the cutis vera, if some of their extremities do not open externally, and occasionally take in substances applied to them? But to produce conviction on a mind cautiously inquiring after truth, stronger evidence than is furnished by analogy is often necessary; and, on a subject so important as the present, our opinions should be fixed by experiment alone.

Doubts respecting the existence of this long-acknowledged function of the skin were raised by Mr. Seguin, of Paris, and acquired great strength from the experiments made in this city by Drs. Rousseau, Klapp, and Daingerfield. These experiments, in which mercury and stimulating odorous substances were the principal articles

employed, had labour, ingenuity, and candour to commend them to the public, and induced many physiologists to renounce their former opinions on this point.

It long since occurred to me, that coloured substances which are but moderately stimulating, and the colouring part of which is known to be absorbed from the intestinal canal, and to enter the circulation unchanged, ought not to be neglected in the prosecution of an experimental inquiry into this subject; and I resolved, should no one anticipate me, to make some experiments myself, when a convenient opportunity should occur. Accordingly, March 22, 1809, I instituted a course of experiments, with the *Rubia Tinctorum*, or Madder.

EXPERIMENT I.

I immersed myself, my head and anterior part of the thorax excepted, in a pretty strong watery infusion of the *rubia tinctorum*, and remained in it two hours and forty-five minutes. The urine discharged immediately after leaving the bath, was pale, and considerable in quantity. Three hours afterwards, the urine was again discharged; it was slightly tinged with red. The bladder was not again emptied till the following morning; and, though the time elapsed was ten hours, the urine had no perceptible redness, and in appearance was nearly or quite natural. Treated with a solution of the common sulphat of iron, the highest-coloured portion of urine gave a tinge of a purplish-brown. The

other portions, treated in the same way, were not sensibly changed in their colour.

EXPERIMENT II.

March 23, I continued three hours in the madder-bath. On leaving it, the urine was, as before, pale, and considerable in quantity. Five hours after, the urine discharged was strongly tinged with a reddish colour. Three hours from this, it was much paler; and the next morning, or eighteen hours after leaving the bath, the urine discharged was of its natural paleness. The portion discharged five hours after leaving the bath, was a little deeper coloured than common Sherry or Sicily wine. Treated with the sulphat of iron, a strong purplish-brown precipitate was produced. With the same test, the urine drawn seven hours after coming out of the bath, gave much less of the colour; the succeeding portions gave none.

EXPERIMENT III.

Recollecting an observation of Haller, that 'as heat increases perspiration, so cold increases absorption,' I determined to enter the bath at as low a temperature as I could bear. In an hour and five minutes, a strong chill and some spasms compelled me to retire. The urine immediately discharged, was, as in the two first experiments, pale. At the end of three, and of five hours afterwards, the urine had a little more colour, but the difference was small; and I am not certain that the sulphat of iron produced any of the brown colour.

The urine, at succeeding intervals, had nothing peculiar in its appearance.

EXPERIMENT IV.

March 25, I continued three hours in the madder-bath. Urine was drawn at the time I left the bath, and at the expiration of five, and of seven, and of fifteen hours afterwards. The first portion was pale, and unusual in quantity. The second portion exhibited a deeper colour than had appeared in either of the preceding experiments, and, with the sulphat, a very strong purple-brown was produced. The third portion, or that taken seven hours after I left the bath, had a feeble tinge of red, and was acted upon in a small degree by the sulphat. The fourth portion was natural.

Blood was drawn from my arm at the end of three, and again at the end of five hours after I left the bath. The different portions were suffered to stand unagitated until a separation had taken place. The serum was then carefully decanted off. No difference appeared in the two portions; each was opaque, and of a pale-red colour, slightly tinged with yellow. The sulphat added to them produced a very slight change, but I am not positive that the purplish-brown appeared at all.

In all the above experiments, the urinary bladder was emptied immediately before I entered the bath. The urine was uniformly pale, and, tested with the sulphat, exhibited none of the purplish tinge. In the first, second, and fourth experiments, the bath, by the occa-

sional addition of hot water, was kept at a comfortable temperature. I had no thermometer (it is said to be unphilosophical to experiment without a thermometer); but, fortunately, the results of these experiments have shown that very nice attention to temperature was not necessary.

The *pulse* was a little accelerated soon after I entered the bath, and retarded before I left it. In the first, second, and fourth experiments, I felt a small degree of languor and slight head-ache for two or three hours after bathing. During this time the pulse was increased in frequency, not falling below seventy, and in one instance going as high as eighty-four in a minute.

I invariably took a full meal after each experiment; but, during the four days in which I was engaged, I avoided eating or drinking coloured substances. In the three first experiments I avoided friction; in the fourth, I employed considerable friction upon the lower extremities.

EXPERIMENT V.

In order to ascertain whether the colour of the urine could be imitated, I made a saturated infusion of madder in water. This was introduced *guttatim* into a portion of clear water, until it acquired nearly the same tinge with the urine. I then threw in a few drops of the solution of the sulphat of iron; the purplish-brown was instantly visible.

EXPERIMENT VI.

Into a quantity of pale, recently-drawn urine, I dropt the saturated infusion of madder, until the urine acquired precisely the same hue as the red urine of the fourth experiment. The sulphat of iron, added to the urine thus tinged, and to the urine of the fourth experiment, produced the same purple-brown colour in each. This experiment was repeated in the presence of my friend Mr. French, and he declared that he could discover no difference of colour in the two portions.

EXPERIMENT VII.

Doubtful whether the colour of the serum, in the fourth experiment, could, with propriety, be referrible to the presence of madder, I took blood from my arm, March 27. I found the serum of this blood considerably redder than the former, but it had nothing of the yellow tinge, and was transparent. The sulphat of iron added to it produced no change in the colour.

EXPERIMENT VIII.

To satisfy myself what allowances should be made for the increased action of the blood-vessels induced by the bath, in accounting for the deep colour of the urine, I placed myself, March 30th, in a bath of pure water, at a comfortable temperature. I remained in it three hours. My pulse, on my entering the bath, was about 72 in a minute. At the end of the first hour it

beat 67, at the end of the second hour, 62, and at the end of the third hour, 62. Four hours after leaving the bath, my pulse was 80 in a minute; but I had very slight languor and no head-ache. In this, as in former experiments, I ate a full meal, and took a walk soon after coming out of the bath.

Urine was discharged at the time of my going into the bath, at the time of my leaving it, and at the different intervals of 3, 5, 8, and 17 hours afterwards. The urine drawn at the time of leaving the bath was a little paler than either of the other portions, and was much more abundant. The other portions were scarcely distinguishable, by their colour, and in neither of them could the red tinge be at all perceived. Tested with the sulphat, these different portions exhibited no change of colour, or rather they exhibited nothing of the purple, or the brown. A large proportion of the sulphat produces a slight change in the appearance of common urine. I have occasionally seen a semi-opaque, whitish appearance, after mixing the sulphat with pale urine.

EXPERIMENT IX.

I accidentally discovered (which, however, I ought before to have learned from books) that potash is an excellent test for the presence of madder. A saturated solution, in water, of caustic, or moderately carbonated potash, dropped into a weak infusion of madder, turns

it to a bright cranberry-red, without destroying the transparency of the infusion*.

This test was applied to the red urine of the second and fourth experiments, portions of which I had fortunately preserved; the same cranberry-hue was instantly visible.

I made a saturated infusion of madder in recently-drawn pale urine. I dropped this saturated infusion into a quantity of pale urine, until it acquired precisely the colour of the red urine of the fourth experiment. The potash added to each, produced the same degree of cranberry-redness. I should have applied this test to the serum of the fourth experiment, but it had commenced a spontaneous decomposition. The serum of the seventh experiment was not changed by the addition of the potash, in any other way than in becoming paler in colour, in proportion to the degree it was diluted.

EXPERIMENT X.

To throw additional light on the query, did the colouring matter of the blood enter the urine, and produce the red appearance exhibited in the 1st, 2nd, and 4th experiments?, I agitated a small bit of crassamentum with a quantity of pale recent urine, until it acquired nearly the colour of the urine of the fourth experiment. The urine thus coloured was tested with the solution of

* All the alkalies are good tests for the presence of madder.

potash, but no change followed, except a diminution of colour, in proportion to the quantity of the solution added. The sulphat of iron did not change its appearance.

EXPERIMENT XI.

With a view to compare the appearances of the urine after taking madder internally, with those which followed its external application, I took, March 31, two ounces of a strong infusion of madder. At this time the urine was discharged: it was pale, and was not altered by the addition of the potash. At the end of two hours, the urine was again discharged: it was pale, as before, and was not affected by the potash. At the end of five hours, the urine drawn was faintly tinged with a reddish hue. The solution of the potash sensibly reddened it. Succeeding portions were pale.

EXPERIMENT XII.

April 1, half past 3, P. M., I took six ounces of a strong infusion of madder. The urine discharged at this time was pale, nor was it in the least altered by the addition of the potash. Two hours and a half after I took the madder, the urine was considerably tinged, and, on the addition of the potash, it assumed the bright cranberry hue. Five hours from the time the madder was taken, the urine had a deeper colour than the last mentioned portion, and, with the potash, it gave a proportionably deeper-coloured result. Treated with the sulphat of iron, it assumed the purplish-brown tinge. The urine discharged the following morning,

was of a faint red, and was perceptibly changed by the potash. The pulse in the two last experiments was somewhat accelerated, owing either to the madder or to exercise—more probably to exercise. I had no head-ache.

EXPERIMENT XIII.

I made an infusion of two pounds and a half of the best madder in about thirty gallons of water. April 4, at half past 2 o'clock, P. M., I entered this infusion, and remained in it until half past 6 o'clock, P. M. The temperature of the atmosphere was 68° , that of the bath fluctuating from 85° to 87° . In consequence of considerable previous exercise, my pulse stood at 100 beats in a minute, at the time of entering the bath. In half an hour it was 87 in a minute; in an hour and a half it was 79; in two and a half it was at 76; in three and a half it was at 72; and in four hours it was at 68 in a minute.

Urine drawn immediately before entering the bath was pale, and yielded no colour when treated with the potash. Urine drawn three hours after entering the bath was pale. Urine discharged one hour after leaving the bath had considerable colour; and, with the potash, yielded the bright-red. In three hours from the time of leaving the bath, I again examined the urine: it had more colour than the last portion, and rendered a deeper red with the alkali. The urine discharged the following morning had a faint tinge of red, and was visibly affected by the alkali.

Blood was drawn from my arm two hours before I went into the bath, and again one hour and a half after I came out of it. The two portions of serum had considerable colour, though the last drawn portion was deeper than the first: neither of them were sensibly altered by the addition of the tests.

EXPERIMENT XIV.

My friend Mr. Clark politely offered to assist me in making an experiment. April 7, he entered the madder-bath, fifteen minutes before 1 o'clock, P. M., and left it fifteen minutes after 4 o'clock, P. M. The temperature of the atmosphere was 47° , that of the bath 65° , during the first 45 minutes; the remainder of the time it fluctuated from 85° to 95° . His pulse, on entering the bath, was 56 in a minute. An hour after, it was 83. At the end of two and a half hours, 72; and at the time of his leaving the bath, it was at 67.

The urine, on his entering the bath, was pale, and suffered no change by the addition of the alkali. On his leaving the bath, the urine was again discharged; and, though it was not perceptibly tinged, instantly turned of a bright-red when the potash was added. Five hours after the experiment, the urine gave a deep-red with the test; and the portion discharged the following morning, was sensibly reddened by the alkali.

Reflecting on the results of the foregoing experiments, I can account for them in no other way than on the supposition that the colouring matter of madder en-

tered the system in consequence of its external application; and, until a more satisfactory way of accounting for them be known, the doctrine of Cutaneous Absorption must be considered as placed beyond the reach of controversy.

In detailing my experiments, I have maintained as rigid an adherence to truth, as my knowledge of language would allow me. Every one knows how difficult it is, by means of words, to give precise ideas of colours. I have used the expressions *purplish-brown* and *cranberry-red*. Different language would probably have been adopted by a different experimenter; and it might, perhaps, more happily have expressed the ideas intended. But words cannot alter an impression made on the organ of vision.

In attending to the results of my experiments, I have not trusted merely to my own eyes. Many of my friends have examined the different portions of urine drawn in different experiments; they have witnessed the effects of the tests on these portions; they have compared these effects with those produced by the same tests on different fluids, as water, urine, and diluted serum *artificially* coloured with madder; and they have unanimously declared the results to be satisfactory and decisive. I think no person of common candour could have witnessed these results without acquiescing in their validity; indeed scepticism itself must have turned believer, in view of proof so plain. I speak with great confidence on this subject, since I consider

it impossible that so many of my scrutinizing friends, as well as myself, should have been deceived.

The red particles of the blood, inevitably retained by the serum, and the extremely diluted state of the madder it contained, and perhaps too the union of the colouring matter of madder with the carbonat of soda of the blood, will very well account for the want of effect produced on the different portions of serum to which the test was applied. This is certain, that I added several drops of a strong infusion of madder, to a very small quantity of serum, and could not detect it by my tests. One-sixth of the quantity of madder would have been detected by the tests, in the same quantity of water or urine.

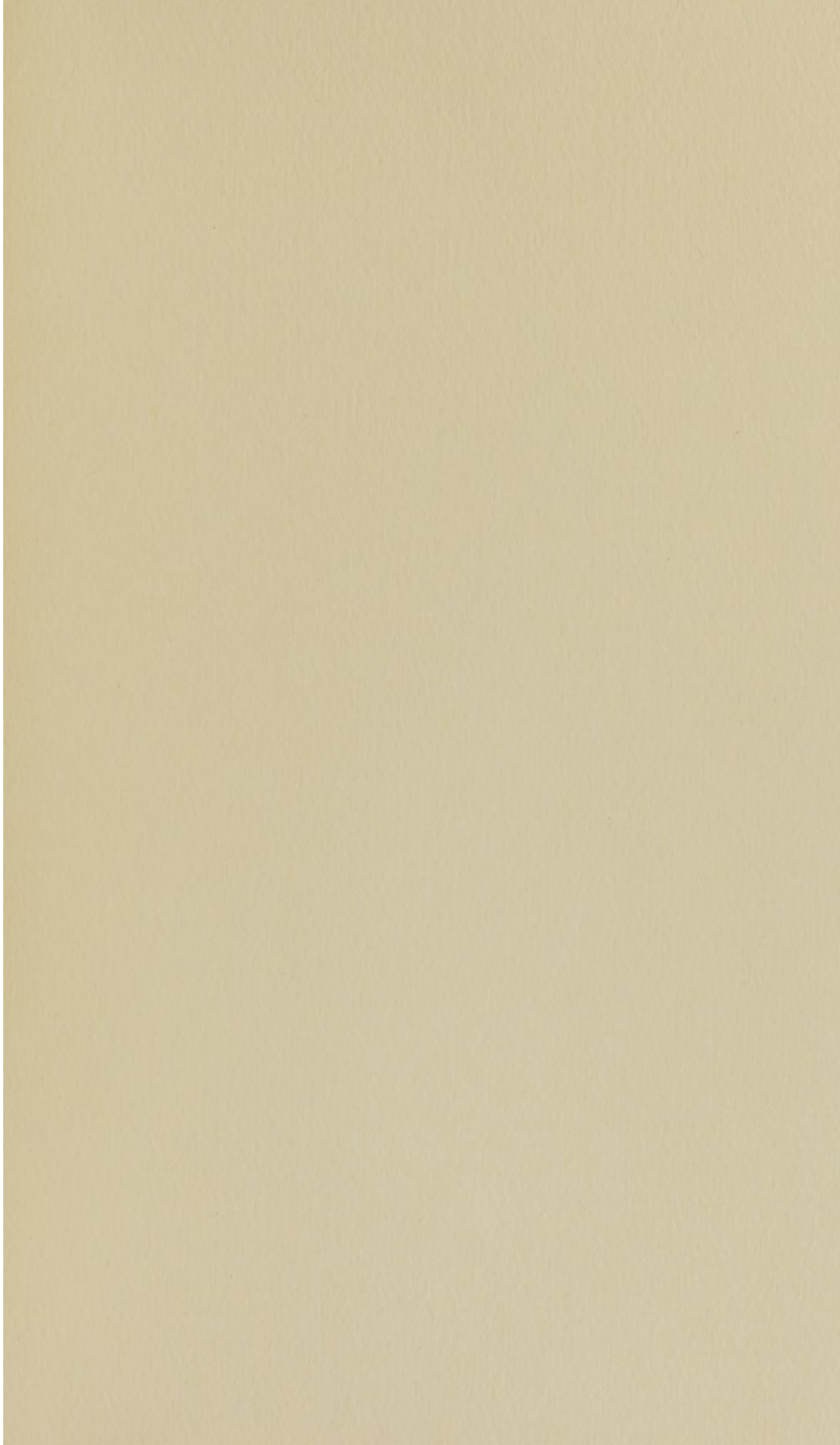
Comparing the quantity of coloured urine together with the degree of colour exhibited in the two last experiments, I should judge that at least twenty or thirty ounces of fluid were absorbed by the skin in the last experiment. But it would be very difficult to come at any thing like precision on this point.

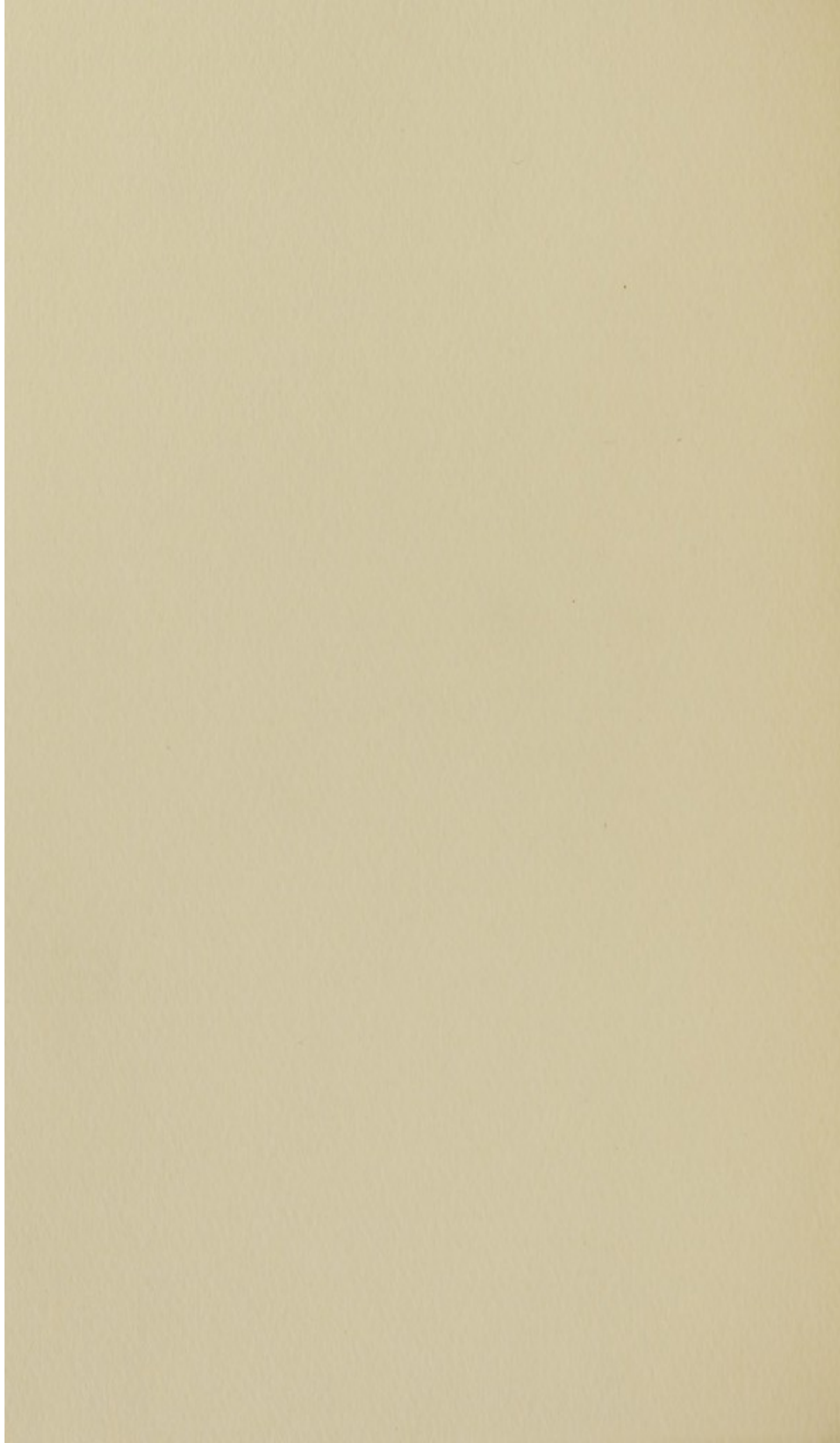
Will it, after all, be said, that the lungs might absorb the colouring part of the madder? I think not. Every *woollen-dyer* knows too much to raise an objection like this. The following experiment, however, is in point. I distilled a pretty strong infusion of madder at the boiling temperature. The fluid caught in the receiver was colourless as water, and did not receive any colour by the addition of the alkali. Nor did the boiling

heat in the least injure the colour of the infusion in the retort. A few drops of it in water or urine *were* detected by the potash in the same manner as before.

Although the doctrine of cutaneous absorption may now be considered as resting on an immoveable basis, yet it remains for future experiments to show what are the different substances which are absorbed, and with what facility they may be made to enter the skin. It would certainly be an extremely limited view of the subject, should we suppose that Nature prepared the cutaneous absorbents for the purpose merely of taking in an infusion of madder. The rational conclusion is, that they have a higher destination, and that they occasionally, if not constantly, take an active and important part among the animal functions.

Professors Barton, Wistar, and Woodhouse are requested to accept my acknowledgments for their generous attention to the results of the above experiments, for the interest they took in the inquiry, and for many suggestions relative to a further prosecution of the subject.





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