Holograph inscription of

Silas Weir Mitchell

Strathedon House.

Mitchell Henry —
with the regards
of his cousin
S.W.M. —

60597

FIVE ESSAYS.

BY

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EDITED BY

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TO

THE GRADUATES

OF

JEFFERSON MEDICAL COLLEGE OF PHILADELPHIA,

These Essays,

BY ONE WHO FOR SEVENTEEN YEARS
ZEALOUSLY AND CONSCIENTIOUSLY FULFILLED THE DUTIES
OF THE CHAIR OF PRACTICE OF MEDICINE
IN THAT INSTITUTION,

ARE RESPECTFULLY DEDICATED,

BY THE EDITOR.

PREFACE.

With one exception, the essays which constitute the present volume, have already been given to the public. The treatise upon Animal Magnetism, written some years ago, and partially prepared for the press, was intended, in the first instance, to be read to the college of physicians of this city, and still bears the traces of its destination. The late Dr. Mitchell finally contented himself with announcing, to that learned body, the conclusions at which he had arrived. His reasons for so long delaying the publication of the essay itself, will readily suggest themselves to those who remember with what distrust the more conservative part of the community looked upon any one whose name had the misfortune to be linked with a subject which most prudent men, at that time, unjustly regarded as the province of the empiric and the traveling exhibitor, rather than the rightful domain of scientific inquiry. What qualifications the author brought to this ungrateful task, are best known to the physicians of this country, to so many of whom, as students, his voice was familiar in the clinic and the lecture-room. To them these essays will need no introduction. As regards others, the editor feels glad to be able in this connection to point to Dr. Mitchell's essays upon the Passage of Gases and Fluids through Membranes, as evidences of his skill in the exact investigations of the laboratory, as well as of the love of truth, and the cautious temper with which he approached every subject of research.

The essay upon the Fungous origin of Fevers, needs but little comment. It is well known as the leader of medical thought in a new direction, and has for some time been out of print; the first and only edition having been early disposed of.

From a number of papers by Dr. Mitchell, upon strictly practical medicine, the editor has selected that upon the treatment of Acute Articular Rheumatism, as the most important. The publication of this essay affected the treatment of rheumatism throughout the country, and is still a favorite mode of practice in this formidable malady.

The essays which are not comprised in this volume, are to be found scattered through various medical journals. They include papers upon the Treatment of Dysentery, Spinal Curvatures, the Tests for Arsenic, Smallpox, and Vaccinia, in conjunction with Dr. John Bell; on the Solidification of Carbonic Acid Gas, and others of minor note.

With this brief preface, the editor resigns to the medical public these essays by one, of whose scientific labors and personal example, in all the relations of life, his medical brethren have at least no reason to feel ashamed.

S. WEIR MITCHELL, M.D.,
1226 *Walnut Street, Philadelphia.*

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TO THE CANDIDATES FOR GRADUATION IN THE JEFFERSON MEDICAL COLLEGE, OF THE SESSION OF 1846–1847.

GENTLEMEN :—

To you I had the honor of delivering, nearly in their present shape, the lectures which I now send to the press. Previously, I had not put my ideas on the subject of which they treat into so formal a shape, although I had announced for years, to each successive class, my impression that possibly the protophytes might afford a good explanation of the causation of malarious and other diseases of a febrile nature. Of the production thus, at least of yellow fever and cholera, I entertained less doubt, and taught, therefore, the sentiment with less reserve. But, although urged by some of you, and more formally requested by the class by which you were immediately succeeded, to place my opinions on this subject before the public, I refrained from their publication through aversion to controversy, and the hope that time would bring more conclusive evidence of their truth or falsehood. Other friends, whose age, position, and learning entitled their opinions to the highest respect, did me the honor to listen to my elucidations, and to recommend their publication. Indeed, one of them, well known to you for his great learning and refined eloquence, wrote to Dr. Forbes, of London, offering these lectures to him for his reprint of American Medical Tracts. His plan, not embracing unpublished manuscripts, excluded them; but he kindly suggested the propriety of their immediate publication by myself, as he thought an essay on a subject of so much novelty ought not, through my aversion to publicity, to remain inedited.

Since that time, a work of some merit has been printed in England, and dedicated, by permission, to John Forbes, M.D., by its author, Charles Cowdell, M.B., M.R.C.S., London, 1848. It professes to be, "*A Disquisition on Pestilential Cholera*, being an attempt to explain its phenomena, nature, cause, prevention, and treatment, by reference to an extrinsic fungous origin." A review of works on cholera, inclusive of that of Dr. Cowdell, appeared in the July number of the *British and Foreign Medico-Chirurgical Review* for 1848, in which the reviewer recommends to Dr. Cowdell to extend his hypothesis, which he thinks ingenious and interesting, "to all epidemics. He would, perhaps, find yellow fever and plague still more to his purpose than cholera."

Dr. Cowdell's book, and the review of it, reached me nearly at the same time, and left me no further excuse for withholding these lectures from the public, unless I preferred to lose what little reputation might be obtained by sending them to the press.

It will be seen that I have not attempted to conceal the sentiments of former writers on this subject, although my ignorance of German prevents me from knowing exactly how far the authors of that country, Henle, Müller, and others, have carried their ideas. Nothing in Dr. Cowdell's book occurs to show that he was aware of any pre-existent fungous theory of fevers, nor of the wide dissemination of that hypothesis on this side of the Atlantic; so that he is apparently entitled to the credit of having made, if not a new, at least an original theory of the cause of cholera.

As you have heard these lectures, gentlemen, you may not have forgotten that, in making my selection of facts and observations, I have, with a single exception, studiously avoided an appeal to phenomena perceived only by myself. I have created no facts for this subject; because I have long learned, as you will learn, to trust reservedly to alleged truths observed by a theorist, who cannot avoid, however just he may be, the coloring which, through a blinding partiality for a new discovery or hypothesis, is too often given.

I have not, however, been idle. Experiments are in progress which seem to promise more direct and unquestionable proof of the validity of our hypothesis; but they are yet incomplete, and therefore should not now appear, lest they might load so young a conception with a too dubious weight.

As there may, in the future, arise some dispute respecting the paternity of the theory which is now proposed, I may be indulged with the liberty of quoting the following extract from a letter by Professor J. W. Bailey, in answer to one from me:—

"*West Point*, March 5, 1845.

"Doctor J. K. Mitchell:—

"My dear Sir:—Please accept my thanks for your favor of the 29th ultimo. I was interested in your letter on the fungous origin of fevers; and it appears to me that you make out a very strong case, and one which appears more satisfactory than Liebig's somewhat vague ideas of 'communication of motion,' being the cause of the propagation of contagious poisons, fermentation, etc. Your theory will, at least, lead to experiment, while his, *if I comprehend it*, leads to nothing, and is only a way of saying that we don't understand the subject."

It would scarcely be proper, Gentlemen, to overlook the kind note addressed to me by a committee from the class which immediately followed you, and which formed a principal inducement for correcting for the press the following lectures.

"*Jefferson Medical College*, December 8, 1847.

"PROFESSOR MITCHELL:—

"SIR:—At a meeting of the class held last evening, the following resolution was unanimously adopted:—

"'*Resolved*, That a committee be appointed to wait upon Professor Mitchell, and request him to furnish, for publication, his new and original views of the nature and cause of malarious diseases.'

"Allow us, in fulfilling the agreeable duty imposed by the class, to express the high gratification we have derived from listening to the lectures referred to, and to add our personal solicitations that you will grant the favor which it is the object of the resolution to ask.

"Yours, respectfully,

"W. P. THORNTON, of Mississippi.
"R. S. HAYNES, of Virginia.
"JNO. HORACE SELTZER, of Pennsylvania.

"CHAS. F. STANSBURY, of the District of Columbia, Chairman.
"JOHN O. MCREYNOLDS, of Kentucky, Secretary."

In reply to this kind request, I promised, when at leisure, to cause the lectures to be published; and now commence the work by offering to you, who heard them first in the form and substance in which they now appear, a dedication of them. With the most sincere desire for the promotion of your welfare, and with the greatest respect,

I have the honor to be, Gentlemen,

Your friend and preceptor,

J. K. MITCHELL.

ON THE

CRYPTOGAMOUS ORIGIN

OF

MALARIOUS AND EPIDEMIC FEVERS.

LECTURE I.

THEORIES OF MALARIA.

THE most ancient authors allude to the noxious influence of the air of marshes and stagnant pools. Some of them indulged in speculations respecting the immediate cause of its morbific power; and here and there, in their writings, may be detected, more or less vaguely expressed by them, the opinions, by the publication of which Lancisi, less than two centuries ago, acquired so much reputation. His treatise, *de noxiis paludum effluviis*, gave consistency and authority to the impression of the miasmatists, and the loose idea of a former age became the accepted sentiment of the eighteenth century. By degrees the medical profession, almost everywhere, adopted tho theory of the causation of periodical fevers by marsh air, and even ascribed the poison to a decomposition of the vegetable remains of low and wet places. After that time, (1695,) with occasional modification from the fancy of each author, the vegetable theory of miasm became almost an established dogma of the schools, not often questioned until the very time in which we live. Now, writers, dissatisfied with the inexact condition of the subject, demand proofs in favor of the marsh theory, which they cannot find; and I may, perhaps, feel safe in asserting that, at the present day, few well-informed physicians accept the theory of the miasmatists, as detailed by McCulloch.

Whatever view may be taken of the nature of the pestilential cause, it is usually most potent in places of a moist and marshy character, such as are the borders of lakes and rivers; and in such places it commonly most abounds, when accompanied by a luxuriant vegetation and a high temperature. As heat, moisture, and vegetation, so commonly attend the production of malarious influence, careless observers, naturally enough, believe the action of heat and moisture upon the vegetation to be the efficient cause of miasm; while they refer to contrasts of temperature and moisture, as exhibited by day and night, as the exciting causes of the periodical fevers of such places.

Such conditions, predisposing and exciting, no doubt cause such maladies; but inquirers take very different views of the mode of production, and of the immediate agents concerned. Some conceive, as already stated, that by decomposition a predisposing poison is produced, sufficient of itself often to excite disease, while dews and change of temperature may occasionally precipitate or determine an attack. Others think that the mephitic vapors of marshes only enfeeble health, and thus enable the obvious changes of heat and moisture to excite disease, which they often produce without any such preparation. A third party refers all cases of periodical disease exclusively to sensible changes, and thinks the proximity of a marsh only efficient as presenting an evaporating surface, by which the air is made colder and damper.

Dissatisfied for many reasons, to be hereafter offered, with the vegetable theory, and with the evaporating theory, and indeed with the hypothesis by which both are united, authors of our own time have suggested a variety of explanations, which it may not be inexpedient to pass in cursory review.

The commonly received marsh theory is well stated and supported by McCulloch, to whose work on malaria I refer you for a view of that side of the question. It is, in a much more masterly and precise manner, sustained by Dr. Craigie, of Edinburgh, to whose volumes on the Practice of Medicine, you may most profitably resort for a learned, lucid, and, I think, impartial array of the facts and opinions bearing on that side of the question. McCulloch involves himself in difficulties without seeming to see them, while Craigie, although inclined to the same conclusions, views with a master's eye, the whole of the impedients and objections. The objections presented by the latter are, the low temperature at which these disease-producing changes may take place; the unaccountable production of them in places where there is no apparent vegetation and often no marsh; the exemption of certain places where occur all the seeming elements of decomposition; the inexplicable effects of rural cultivation; and the unexplained vicissitudes of health in the same places in different though similar years.

Denying the vegetable theory, and indeed assuming the position that we are as yet totally ignorant of the nature and true source of the cause of malarious fevers, my eminent colleague, Professor Dunglison, in his work on Hygiene, ably exposes the fallacy of the received opinions on this subject. He is not favorably impressed, indeed, by *any* of the many hypotheses with which an obscure, but highly important subject like this, is sure to be loaded.

Not less antagonistic to the received theory is my friend, Dr. John Bell, who, however ingenious and learned in his opposition to it, does not also arrive at a negative conclusion, but refers the morbid phenomena to the modification of the sensible or appreciable conditions of the atmosphere. His paper, contained in the *Medical and Physical Journal* for 1825, 1826, pp. 274–316, is worthy of an attentive perusal, although written at a very early period of his medical life.

Notwithstanding, therefore, the seeming supererogation, my duty as a teacher compels me to offer to you at least a summary of the objection to current opinions on this subject.

The most forcible argument against the vegeto-aerial theory, consists in the extraordinary exemption from malarious diseases of places which, were it true, could not escape a severe infliction. It is the more forcible, because the theory is founded mainly upon the concurrence of such diseases with heat, moisture, and vegetation. If, then, it can be shown that the alleged conditions exist in the most perfect state, *in very many places*, without morbid results, the universality of the coincidence can no longer be brought to sustain the opinion.

Again, if *many places* can be cited, where these supposed elements are not at work, which are nevertheless noted for their insalubrity, the opinion becomes even less tenable. It is still further weakened by the fact, often observed, that under precisely the same apparent circumstances, healthy places become unhealthy, and sickly places salubrious. The marsh, the heat, the moisture and the vegetation, remaining apparently the same, the health of a region may vary from one extreme to the other.

I will now offer you some examples in illustration of these positions:—

McCulloch, the unqualified advocate of the marsh theory, seems to have been very much perplexed by an exception to his rule, which lay just under his own eye. The canal in St. James's Park, London, was, at the time he wrote, notorious for the abundance of its aquatic plants, causing, in autumn, an intolerable stench. Yet he congratulates the inhabitants on their miraculous exemption from malarious fevers, "it being, perhaps, *the only exception in the world*, at least wherever the climate equals (in temperature) that of England." (*Page* 50.)

Let us see how far his assertion is sustainable. The town of Kingston, in the Island of St. Vincent, is situated at the bottom of a semicircular bay, and at the foot of a mountain range, with high land on each side. The soil consists of a black alluvial mould, evidently arising from decaying vegetable matter. In one place, the bed of a dried-up water-course, branches of trees were found, and the neighboring ground was covered with leaves in different stages of decomposition, for upwards of eight inches in depth, into which the feet sank at every step. "There, then," says the Deputy Inspector of British Hospitals and fleets, Robert Armstrong, "we have all the elements necessary for the production of this vegeto-animal poison, heat, moisture, decayed and decaying vegetable matter, with as large a proportion of reptiles, insects, and other animal matters, as is found in other tropical countries; yet, strange to say, the town of Kingston is one of the most healthy spots in the West Indies. I was informed by the staff-surgeon to the forces, who had long resided there, that *it was as healthy as the most favored spots in England.*" As a very curious contrast to the statement of Armstrong, we learn from Bishop Heber that the wood tracts of Nepaul and Malwa, *having neither swamps nor perceptible moisture*, become, in summer and autumn, so pestiferous as to cause their abandonment even by the birds and beasts.

Fordyce, too, tells us that, in a part of Peru where there is a total absence of water, and, of course, of vegetation, fever and dysenteries render the country almost unhabitable; and, according to Pringle, the dry, unproductive, sandy plains of Brabant excite malarious fevers of great intensity.

New South Wales extends from 10° 5′ to 38° south latitude, embracing a region similarly situated to that of America from the West Indies to the Chesapeake Bay. It is subject to a rainy season, has streams, estuaries, and extensive swamps. Around some of its towns there lies a deep, black, highly productive vegetable mould. It is liable to extraordinary inundations, which lay the country, as far as the eye can reach, under a sheet of muddy water. The temperature is quite as high as that of any other like latitude. The coast is covered with *mangroves*, and skirted by rocks, reefs, and islets. Among its products are mahogany, oranges, lemons, guavas. The mosquito, with myriads of insects and reptiles, parrots, paroquets, and other tropical birds, announce a hot, productive climate, and lead us to look for a tainted air and a pestilential habitude. But, notwithstanding all these threatening conditions, the usual symbols of a sickly clime, New Holland is remarkable for its healthfulness. Pulmonary diseases, and, in the wet season, dysenteries, are observed, but the fevers incident to warm climates elsewhere, are here of rare occurrence. In speaking of this country, Malte Brun has this expression: "Hitherto we have heard of no such fatal epidemic fevers as are frequent in some other colonies situated in warm climates."

Mr. Titian Peale, the zealous and successful naturalist, who accompanied Captain Wilkes on the exploring expedition to the Southern Ocean, writes to Professor Dunglison, that he *never* saw a case of intermitting fever in either natives or strangers, in the Polynesian Islands, although the officers and men of the expedition lived and slept in the midst of marsh stenches and mosquitoes, when the days were hot, and the huts open and exposed.

Captain Wilkes himself describes these islands as fertile, moist, hot,—but yet as remarkably salubrious, as is evinced by the general good health of the men, who were often exposed at night, by the shore duties of the service, to fatigue, night air, and heavy dews.

The following examples of the truth of his general statement, are found in the same work.

TONGATABOO is an *organic island*, formed by coral, is *rich*, *flat*, and *luxuriant*, and oppressed by a temperature rising to 98° F., offering a mean, during the sojourn of the expedition, of 79° 25. There was much rain, and, when clear, heavy dews. The writer supposes that these phenomena must create sickness, but he sees *many old* people, and admits that, *although ashore at night*, the people of the expedition were not sufferers. Mr. Peale, also, testifies to the good health of the place.

OVOLAU (Fegee) is a *volcanic island*, the mean temperature of which, for six weeks, was 77° 81; maximum, 96°; minimum, 62°. Turnips, radishes, and mustard-seed appeared above ground in twenty-four hours, melons in three days; while marrowfat peas, fit for use, were produced in five weeks. On this island, volcanic as Sardinia, and hot as the Maremma, "fevers, whether remittent or intermittent, were unknown."

In the two instances cited above, the islands closely resembled each other in climate, temperature, and fertility, but were contrasted as to origination, geology, and surface, the one being organic, the other volcanic; the one being flat, the other mountainous; yet both enjoyed a degree of salubrity totally at variance with our preconceptions.

The Island of Soloo, in latitude 6° 01′ north, enjoys a temperature seldom below 70°, or above 90°; that is, about the mean of that of the pestilential western coast of Africa. It is, however, healthy.

Menouf, the capital of Menoufyez, in Lower Egypt, is situated on the banks of a canal formerly navigable, but so no longer. This canal bathes the walls of Menouf from south to west. Within a few yards of it lies another canal of stagnant water, the space between forming a road into the town. To the right of the south gate, lie basins of water *to rot flax in*, which gave out a disagreeable odor. Here and there is a cemetery, and between them are pools for the same use, some of them broken, neglected, and full of stagnant water. Menouf has no gardens, its streets are narrow and dirty, and its houses small and badly constructed. The people drink the Nile water. The yearly inundation floods the country around Menouf up to the walls, but it does not continue long under water, to which fact Surgeon Carriè ascribes its healthfulness: "C'est pour cela sans doute que cette ville est assez saine."

In addition to its other defects, the place is surrounded by a second wall, formed of dirt and rubbish transported from town, by which the view is obstructed, and the town sheltered from the wind. Not only is Menouf *assez saine* in other respects, but even the plague does less damage here than in other parts of Egypt. (*Degenettes.*)

If the exception presented by the canal in St. James's Park puzzled McCulloch, and was, at the time he wrote that page, apparently, to him the only one, he alighted, in his progress, upon another at Singapore, which seemed still harder to dispose of, without a severe shock to his system.

My esteemed friend and former pupil, Dr. M. B. Hope, Professor of Belles-lettres in the College at Princeton, resided for some time as a missionary at Singapore, in the East Indies, and adds to the details given by McCulloch, respecting Singapore, the following facts and opinons:—

"The Island of Singapore is, in the main, *low* and *level*. There is one hill in the interior about 500 feet high, in which granite rocks make their appearance. Scattered here and there are low, round sand-hills, the level ground between which is formed of a ferruginous clay upon a sandy substratum. The greater part of the island is covered with jungle. Lofty trees and a most luxuriant vegetation are found in many places. The island is pretty well watered by streams, which descend from the hills to the sea. The tides have produced and sustained a chain of marshes nearly all round the coast. In some places, fresh and stagnant water covers the low grounds extensively.

"The city, which lies in latitude 1° 17′, contains a highly mixed population of about 20,000 souls: Chinese, Malays, Indians, Europeans, etc. It is nearly surrounded by marshes, the jungles of which are almost impervious, and are infested by tigers and other ferocious or wild animals. Here and there the Chinese have cleared and cultivated the ground; and there are, near the city, some sugar and nutmeg plantations.

"The vegetation is incredibly rapid in its growth; and *its decay is not less wonderfully great*, as may be supposed, when the soil is rich, and the mean annual temperature is, in the morning and evening, 79° 45′, and at noon 84°.

"Astonishing as it may seem under such circumstances, fevers *of any kind* were *very rare*, particularly among the natives. Now and then remittent fevers might occur, and, yet more rarely, intermittents. Foreigners were, of course, more readily attacked, but not often, except through imprudent exposure to fatigue, or the sun.

"Singapore is considered as a kind of *sanatarium* for the Oriental invalids, who go thither from every quarter of the Eastern world, to escape from malaria, or to recover from chronic diseases."

The empire of Brazil extends from the equator to the southern tropic. It is watered by vast rivers and countless streams, abounds in lakes and marshes, and, under a burning sun, smokes from the vapor of impetuous rains, and boasts a vegetation unsurpassed for abundance, variety, and rapid transitions. Along an extended coast, the mountain ranges are nearly parallel to the sea; so that behind them the sea-breeze exerts no cooling power, and the air is stagnant and hot. Even at Rio Janeiro, the latitude of which is nearly 23°, the temperature is very high, and the atmosphere often excessively languid and oppressive. "In the city," says Dr. Horner, of the United States Naval Service (293,) "the thermometer had been 90° in the shade. Night and day the temperature in my state-room was 86°." The sluggishness of the air at Rio may be known by the name *Nitheroy*, or *Dead Sea*, given by the aborigines to its harbor. The climate is hot and moist; high and thickly wooded mountains, the narrow entrance to the bay, and the numerous islands, impede the free passage of the wind. The site of the town is low, the streets are indescribably filthy, and the waters from the hills accumulate in the marshes which nearly encompass the city. (*Hist. of Brazil.*) "The proximity of the ocean, the great size of the harbor, the great height of the land about it, the many hills, narrow streets, and high temperature, keep Rio, almost without cessation, immersed in a heavy, sultry atmosphere, rendered more disagreeable by want of cleanliness, and the exhalations from the ravines and marshy grounds in its rear." (*Horner.*)

Notwithstanding the presence of all the alleged *materiel* for fevers, the American squadron, with a mean force of two thousand two hundred and eighty men, had, in seventeen months, only one hundred and fifty-five cases of fever, of which the Concord alone, had seventy in a crew of two hundred, when on a visit to the African coast. *Not one died of fever on the Brazil station.* The British ship Warspite, with a complement of six hundred men, lay a whole year in the harbor of Rio, and did not lose a man. *In that time she had but seven cases of fever.*

Travelers who spent some time in Rio, and who penetrated to every part of the country, are equally warm in their praise of the salubrity of the climate. "It was," says Walsh, "the rainy season, *a mortal period in other tropical climates.* For eight or nine hours a day, during some weeks, I never had dry clothes on me, and the clothes of which I divested myself at night, I put on quite wet in the morning. When it did not rain, there shone out, in some places, a burning sun, and we went smoking along, the wet exhaling by heat, as if we were dissolving into vapor. Such weather, in Africa, no human being could bear, but not so in Brazil; no one is affected by those states of the atmosphere which are so fatal elsewhere. It has, with some reason, therefore, grown into a proverb, that it is a country where a physician cannot live, and yet where he never dies. There was no doctor at S. Jose; but I was told there had been two at S. Joao d'el Rey, and that one of them had left because he could get no patients, and that the other had for a long time no patient but himself." (*Page* 297, vol. ii.)

In Africa, under the same latitude, the rains scarcely commence before the constitution begins to sink, even without external exposure. According to Lind, the first rains which fall in Guinea are supposed to be the most unhealthy; *and they have been known in forty-eight hours, to render the leather of shoes quite mouldy and rotten.* Mungo Park observes, "that the rain had not commenced three minutes, before many of the soldiers were affected with vomiting, *others fell asleep, and seemed as if intoxicated.* I felt a strong inclination to sleep during the storm, and as soon as it was over I fell asleep on the wet ground, although I used every exertion to keep myself awake. *Twelve of the soldiers were ill next day.*"

"The thermometer," says Boyle, "is seldom above 81° or below 69° at this period, but the process of decomposition proceeds so rapidly, that cloth and animal substances, such as leather, become putrid in a period hardly credible."

On one of the Isles de Loss, at Sierra Leone, a small force was soon destroyed, yet it is in the sea, only about from half a mile to a mile in diameter, and formed *of granite*, which rises to three hundred feet at its centre. It is apparently free from supposed causes of fever. There is but one piece of arable ground, no sulphur, no calcareous rock, no marsh, and very little soil, not a swamp, and the temperature seldom rises above 80°. (*Boyle*, p. 16.)

Other examples almost without number, might be given of the salubrity of places full of decomposing matter, and of the insalubrity of others, where scarcely a vegetable is to be seen. So that many reflecting men are now disposed to abandon a theory which cannot be rationally sustained by a reference to facts, and which is shaken the more, the more closely its pretensions are examined.

"We must be contented to place the explanation of the cause of plague," says Fodere, "in the category of that of all endemic maladies: that it is unknown."

"Malaria is a specific poison producing specific effects on the human body, and is probably gaseous or aeriform. Of its physical or chemical qualities we really know nothing." (*Watson.*)

According to Robert Armstrong, "we are utterly ignorant of the nature of this poison, and no two authors agree respecting its constitution, the circumstances under which it is generated, or its effects on the human body." (*Page* 70.) Again, "of the existence of miasm we have no positive proof. It has never been obtained in an insulated state, and, consequently, we are totally ignorant of its physical properties."

"If asked what is malaria, I answer I do not know." (*Caldwell.*)

"Hence, physicians have been reduced to the necessity of inferring the existence of hidden atmospheric influences, as a cloak for ignorance." (*Tweedie.*)

"Epidemic fever may be attributed to a *mysterious something*, an occult quality in the atmosphere." (*Med. Gaz.*, xvi. p. 515.)

But thinking men can scarcely rest satisfied with negative conclusions, and, therefore, new explanations of the cause of fever succeed to the uprooted theories of the age gone by.

The opinion which, next to that of "the malarial," seems to be most successfully sustained, refers intermittents to the obvious conditions of the air, altered by heat and moisture; hot days followed by cool evenings, dry days by dewy nights. The strongest argument for this conclusion rests on the fact that such diseases prevail at the season of greatest contrast, both as to heat and humidity, and in places where extensive wet grounds aid in the production of the strong vicissitudes.

As these phenomena are subject to observation, a close examination may be made of the relative condition in such respects, of the most healthy and the most deadly localities. The result is not favorable to the theory based on them, for many very salubrious places are remarkable for the most striking manifestations of the supposed causes of intermittents, while very sickly situations are not unfrequently distinguished by the uniformity of the climate, and the steadiness of the temperature and dew-point; nay, two places, in all observable respects alike in elevation, local relations, atmospheric phenomena and geological structure, may differ totally in their degree of healthfulness. Even in the same place, the line of limitation of disease-producing power may be a common road, a narrow street, a stone wall, or a belt of woods; things which could scarcely be supposed to affect, sensibly, the heat and moisture, or their fluctuations.

But the most fatal argument against this theory is the fact that exposure for a single hour, at night, sometimes produces an almost immediate attack in some persons, while in others it creates a tendency to disease, not actively expressed until the lapse sometimes of months. It will be acknowledged, too, that in that hour there may be observed no unusual or contrasted conditions of the air, either as to temperature or moisture.

On the western coast of Africa, the sickness reaches its maximum in the height of the rainy season, when the diurnal temperature and moisture are almost invariable. While on the coast of Brazil the same meteoric phenomena are perfectly innocuous.

Like every other theory, therefore, this one owes its plausibility rather to the defects of a former hypothesis than to its own value.

Daniel in England, and Gardiner in this country, have adopted, with slight modification, an opinion of Rammanzani, that marsh exhalations owe their injurious activity to sulphureous emanations. It is not enough to demonstrate their destructiveness, that their presence in minute quantity may be detected in paludal air; for the same argument would equally favor the reference of marsh fevers to any one of the many other gases or vapors found in the same places. The innocuous qualities of these as manifested elsewhere, is not less than those of the compounds containing sulphur. Moreover, the sulphureous localities of the sickly Island of St. Lucia, are its only salubrious places. (*Evans.*) Cities, too, which abound in sulphur-products, should not, according to this theory, enjoy the immunity from agues for which they are everywhere noted.

Immediately around the sulphur-works and factories for making gunpowder and sulphuric acid, the vegetation and the ague disappear together. Facts of the same import might be almost indefinitely multiplied, but the task is unnecessary.

Hoffman attributed malarious fevers to a *lessened elasticity of the air.* His notion, obscurely conceived and inaccurately stated, is only excusable because of the loose philosophy of his day on every subject connected with atmospheric phenomena. Air is always equally elastic, and any modification of its density, except by adulteration, cannot be very partial for any length of time, so as to create a permanent insalubrity. When adulterated by excessive moisture or unusual gases, it is altered in composition, a cause of disease much more intelligible than that of modification merely of density.

Particular gases have also been supposed to exert malarious influence: carbonic acid, nitrogen, cyanogen, carburetted hydrogen, phosphuretted hydrogen, ammonia, and all the imaginable *effluvia* from decomposing organic compounds, have had each its advocate. As yet, however, no one has been able to show that marshy or insalubrious places abound most, or peculiarly, in such emanations; nor has it been made even probable that any one of these can, or ever did, produce an ague; while we know that in the busy haunts of non-malarial districts, the arts produce indefinitely diversified decompositions and emanations, both animal and vegetable, with wonderful impunity to artisans.

The great difficulties in the way of other theories have induced some authors to suppose, that the emanations productive of fevers result from the action on water of *living vegetables*, or of vegetables *not dead but dying*. Others have found more astringent vegetables in hot than in cold climates, and have conjectured that *some combination of animal matter with tannin constituted malaria*. Not yet satisfied with conjectures, a few presume that the decomposition merely of *certain* vegetables forms or diversifies miasmata. "Thus," says McCulloch, "might *the cruciform plants*, OR THE TRIBE OF FUNGI, produce a malaria differing from that poison as resulting from the gramineous ones, or the consequence of the putrefaction of seeds differ from that of leaves." Some French writers lay great stress on the influence of *narcotic vegetables* in the causation of malaria.

McCulloch, after a very elaborate citation of facts and opinions, arrives at the *indefinite* conclusion "that the presence of *vegetables* or vegetable *matter*, in *some mode* or *form*, is necessary to the extrication of malaria; while the conclusion has sometimes been, that it is a production formed between *the living vegetable and water*: more generally that it is generated between *that* and the *latter, in some stage intermediate between life and absolute decomposition*; or, *lastly, that it is the consequence of absolute putrefaction*."

I need scarcely say that the *ifs* and *ands*, and *buts* and *ors*, in McCulloch's book, show the utter inefficiency of his undefined cause to explain the difficulties of this vexed question. Nor is it necessary to offer objections to the other theories cited, since no one has sustained them by even plausible reasoning or pertinent facts. They are not received or respected by the "profession."

The last of the theories, to which I shall invite your attention, are those of Doctors Ferguson and Robert Jackson.

The latter gentleman, once a firm believer in malaria as usually understood, saw, during his West India service, so many antagonistic phenomena, as to incline him to the opinion that it is, sometimes at least, *an emanation from living vegetables*, through the exuberance of organic life, the excess of vital vegetable action. To use his own language, "It would appear that the materials of vegetation abounding in excess, acted upon by a powerful cause, give out a principle which, not being expended on the growth and nourishment of plants, is diffused to a certain extent in the atmosphere, causing a derangement of such bodies as come within the sphere of its action."

Mr. Doughty offers a modification of this sentiment, in the supposition that by the separation of their nutrition from the soil, especially when their growth is very rapid, plants cause in the earth new combinations of rejected elements, which thus become aerial, and poison the neighboring atmosphere. As many highly malarious places are barren, and naked of apparent vegetation, the theory of Jackson falls at once to the ground. But if not, then is there the additional difficulty of explaining by such a cause, the existence of malarious diseases when the season of active phenogamous vegetation has passed. It is also a pure hypothesis unsustained by facts or reasoning.

The theory of Ferguson is received now by the profession more favorably than perhaps any other. It narrows the malarial question down to this, that the only conditions essential to the production of miasmata are soil and water, especially a porous soil; and the only relation between these elements is that of successive moisture and dryness. Stating it in the words of Dr. Watson, of London: "There is reason to believe that the flooding of a porous earthy surface with water, and a subsequent drying of that surface, under a certain degree of heat, constitute the sole or main conditions of the generation of the poison."

If these are the sole conditions, only moisture can come from the soil, for if anything else does, *it* must be a miasm, and we revert to the old opinion of Lancisi. If only moisture is exhaled, why does it sometimes poison its subjects in a single hour, or make an impression actively expressed sometimes months afterwards? Ferguson has himself adduced a fact irreconcilable with his theory, where the army suffered in long droughts when at a distance from porous and wet soils. His hypothesis is also in opposition to the fact that *in Africa the greatest mortality is during the rains*, when the earth is always drenched with water. On the other hand, the shores of the Mediterranean are most pestilential when a long drought has parched up the earth.

It would be a waste of time to even enumerate the theories of malaria, founded on the supposition of an unusual disproportion of the ordinary atmospheric elements, such as an excess or deficiency of oxygen, or nitrogen, or carbonic acid, or water. Nor would it be of more use to cite the electrical and magnetic theories of disease, since no analysis of malarious atmosphere has revealed any defect of its elements or of its imponderable constituents. Not a fact sustains any of these opinions, and observations extensively made thoroughly falsify them.* Whatever of change from such causes is observable in malarious places, must be ascribed to their power to excite, not to predispose.

The only theoretic view of malaria to which I incline, is that which refers marsh fevers, and some of the epidemic diseases, to a living organic cause, capable of reproduction by germs, as is alleged of contagious diseases; but unlike the latter in this, that the germs are not reproduced by the organism of the sick, but exteriorly to, and independently of, the human body. In other words, that as the germs of contagious diseases are reproduced *in* the body, the germs productive of malarious and other non-contagious diseases are elaborated and re-elaborated *out of* the body, and independently of its agency. One is the product of *person*, the other of *place*. This notion is sustained by the fact that organic azotized substances are the only things detected in marsh air or dew which can possibly affect the health injuriously.

Although I approve of the reference of malarious diseases to the causation by organic germs, I am far from

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* M. Peltier, by constant observations, found the clouds, in 1835, almost always positive, in 1836 generally neutral or negative, yet no marked difference in health was observed in those years.

Since these lectures were written, Sir James Murray has defended, with much ability, the electrical theory of malarious and epidemic diseases. (*Lancet*, October, 1848.)

being satisfied with the animalcular direction taken by all who have elaborated a theory on this foundation. Hitherto it has been so feebly sustained by proofs, as to have at no time received general favor from the profession, although supported by some eminent men in almost every period of medical history. The chief objections to the animalcular theory are: 1st. That it has never been shown that animalcules are poisonous in any way. 2d. That none of the difficulties of this puzzling subject are thus removed. 3d. That the assumption is hypothetical at first, and does not, in the progress of an examination, become at any time more demonstratively probable, or logically acceptable. 4th. But the strongest objection is founded on the superior probability of *the vegetable branch of the organic theory*, by which I hope to show that very much of the obscurity of this subject may be dissipated. This last objection will, as we advance, rise into more remarkable prominency.

It is painful to be thus compelled to abandon the ingenious theories of our fathers, built up so elaborately and so industriously; to brush away the whole labor of the lives of many eminent men, and to reflect upon the time and talent lavished wastefully upon mere day-dreams. We cannot also fail to perceive the great fallibility of human opinion, as thus exemplified, nor can we avoid the dread that we ourselves may have to mourn hereafter over the unproductive labors of our own lives, or leave to our children the thankless office of removing our worthless mental rubbish to make way for, perhaps, not more substantial edifices. Be this as it may, we derive from the review the useful lesson of philosophic humility, which teaches us to state or receive new doctrines with becoming hesitation, and to bring them into *practical* application with prudent caution, and then only when sustained by the prolonged observation of many persons in many places, and at various times.

LECTURE II.

THE CHARACTER, GROWTH, MINUTENESS, DIFFUSION, ALTERATION BY CLIMATE, AND AUTUMNAL PROFUSION OF THE FUNGI.

In offering to your attention and consideration a theory of malaria, I profess to do no more than present a review of the phenomena which seem to render it probable, without supposing that, on so difficult and important a subject, I can produce in your minds the thorough conviction which nothing short of a positive demonstration could bring home to my own. Not thoroughly convinced myself, I can only be excused for occupying your time, by the belief that the theory I am about to offer is not only very plausible, but is associated with agreeable and useful collateral inquiries. If we should not discover, at the end of our journey, the truth, the search after which has lured us to the path of observation, we shall enjoy at least beautiful scenery by the way, and sometimes pluck a flower, and sometimes find a gem.

Standing at St. George's, in Delaware, more than twenty years ago, upon the bottom of what had been, a short time before, a mill-dam, I found around me the undecayed stumps of trees which had been for one hundred and seventeen years submerged in fresh water. Two or three years thereafter I again visited the spot, and saw that these stumps, no longer wet, but damp, had been entirely disintegrated by the dry rot, and that they crumbled in the handling. In the handful of dust which I picked up, I found innumerable spores of what I supposed to be Polyporus Destructor and Merulius Vastator, cryptogamous plants, whose active existence had been bought at the expense of the old stumps. In a moment I conceived that, perhaps, the miasm, so much dreaded in that place, might be, *directly* or *indirectly*, the product of these urgers-on of a more rapid decomposition. It was a loose thought at the time, but it gave me a disposition to collect the phenomena which might prove or disprove the agency, in the generation of malaria, of living, not of dead plants.

A part of my collection I now offer you. In doing so, I shall present only the affirmative side of the question, believing that no one else is likely immediately to sustain so revolutionary an opinion, while professional emulation, habitual prejudice, and even love of truth, will subject it to a sufficiently rigorous opposition. You have, therefore, due notice of the guarded manner in which you are to receive my *ex parte* observations, a notice which I cheerfully give, for I have much confidence in the force of my subject, and do not love my theory well enough to wish its establishment at the expense of truth or reason. Take it, then, for what I may show to be its worth.

Just on the line which faintly marks the division between the animal and vegetable kingdoms, lie the *lichens*, the *algæ* and the *fungi*. These cryptogamous plants are so closely allied to each other, as to be indistinctly separated by naturalists; some of whom include under one division, species which are found differently disposed of by other phytologists. Lindley, following the great continental cryptogamists, admits that the location, rather than the structure of these plants, affords a final distinction, and that, while the lichens live on dry and scanty soils, and algæ in water, salt or fresh, the fungi occupy the intermediate place, *loving a damp and unsound or loaded atmosphere*, and feeding on organized matter, the vitality of which is gone, or going.

In all of them, the element is a very minute cell, not often distinguishable when isolated, from the elementary cells of even animal organisms. Indeed, some of the confervæ, obviously vegetable in one state of existence, as the *arthrodieæ*, offer in another, the plainest character of animal life; supposing that animal life is to be inferred from motions indicating a well-marked power of volition. Some of the *oscillarias* have an oscillatory movement extremely active and perceptible, and the *ulva labyrinthiformis* and *anabaina*, with all the other conditions of a vegetable, have, according to Vauquelin and Chaptal, all the chemical characters of an animal. We have, therefore, chemically constituted plants with animal motions and volition; and those of animal composition, with the exclusive habitudes and structure of vegetables.

All plants are liable to curious and often great alterations by climate, soil, and season; but the dubious beings I am now describing undergo such astonishing modifications, even by the slightest causes, as to perplex, by their morphology, the sagacity of the best informed naturalists. They, at least the simplest of them, seem to have so little inherent tendency to the assumption of form and nutrition, as to take their shape and products almost exclusively from the hand of accident. "One might call it," says Lindley, "a provisional creation, waiting to be organized, and then assuming different forms, according to the nature of the corpuscles, which penetrate it, or are developed among it."

For these reasons, the lowest of the vegetable groups, the fungi, are, in the opinion of some naturalists, equally distinct from plants and animals, mere fortuitous developments of vegeto-animal matter, called into varied action by special conditions, or by combinations of heat, light, and moisture, and capable of existing and of being propagated, under circumstances apparently the most contrasted.

Of all vegetable substances the fungi are the most highly animalized. Like animals, they disengage carbonic acid and imbibe a quantity of oxygen; nay, some of them extricate hydrogen, and even nitrogen. Their chemical composition also allies them to animal structures. They yield the vegetable products, resin, sugar, gum, fungic acid, and a number of saline compounds; but they also afford the adipocire, albumen, and osmazome of the animal kingdom. The basis of these plants is fungin, a tasteless but highly nutritious substance, white, soft, and doughy. It yields, by nitric acid, nitrogen, hydrocyanic, oxalic, and some other acids, and fatty substances, like wax, tallow, and, in some instances, oil.

Of the cryptogamous plants, the *fungi* are distinguished for their *diffusion and number*, for their *poisonous properties, and their peculiar seasons of growth*, for *the minuteness of their spores*, and for *their love of darkness and tainted soils, and heavy atmospheres.* While, then, I shall present their claims to be considered as the *principal cause* of fevers, I do not mean to exclude the occasional agency of other cryptogamous vegetables; and beg, when using the convenient word *fungus*, to be understood as not entirely denying the agencies of cognate beings of kindred subdivisions, which are hardly distinguishable from it.

Here and there among writers, ancient and modern, a hint is thrown out, that, possibly, plants of the lowest orders may cause malarious fevers; and in some countries, as Spain, for example, even the populace believe that the fungi cause fevers. For, to the practice of eating mushrooms at a sickly season, the Estramadurans ascribed the febrile diseases by which the British army suffered so severely.

A treatise was published at Vienna, in 1775, by J. S. Michael Leger, "concerning the mildew, considered as the principal cause of epidemic disease among the cattle, etc." "The mildew producing the disease is that which dries and burns the grass and leaves. It falls usually in the morning, *particularly after a thunder-storm.* Its poisonous quality, which does not continue above twenty-four hours, never operates but when it is swallowed immediately after its falling."

"Should the too bold notion of Nees Von Eisenbeck, that fungi of the most minute forms have their origin in the higher regions of air, and, descending to the earth, produce spots and stains, be confirmed, these *signacula* would have a much more important connection with epidemics than can be otherwise conceded to them." (*Hecker*, p. 205.)

Müller thinks (*Archives*, 1841,) that if vegetable cells were to be *seminia morborum*, they could scarcely be microscopically distinguished from the primordial formative cells of our own tissues.

The theory, therefore, which I now offer, is not entirely new. Nay, the learned microscopists, who are making, on the nature and action of elementary cells, such immense contributions to our STORE OF FACTS, not only prepare us for such a theoretic step, but actually, by pregnant allusions, lead the way. It is, then, not so much a rash generalization, in advance of the opinion and knowledge of the age, as a very natural result of that knowledge collected and classified. It is an expression, if not of the *sentiment*, at least of the *science* of the present era.

Under this impression, I undertake the adventurous duty of developing a theoretic result, not expecting to do more than obtain for it, at present, a hearing and an examination, since its demonstration, if ever completed, must exact, for years, the enlightened and patient toils of many philosophers. "There never is," says Bischoff, "an important and comprehensive discovery made at once; the elements of it are generally obtained from different quarters, and from all these truth at last results."

Imitating the natural philosophers, I have constructed a theory, not to be esteemed devoutly true, but as, in the present state of knowledge, the most perfect explanation of the known phenomena of the case; and as the least exposed to the many objections easily brought against any other hypothesis.

It may be thought that the cause assigned is not adequate to the rapid production of the effect. Can a minute vegetable, however distributed, contaminate the air of a large marsh or field, in the course of a few minutes or hours? When we remember how minute a quantity of a reproductive organic virus is, in other cases, necessary to the infection of a proper subject, we might leave the argument to that defence alone; but I think there is a better one, in the wonderful growth and ready diffusion of the plants to whose nocturnal potency I am inclined to ascribe malarious fevers.

A mushroom growth is proverbial in every language. In a single night, under favorable circumstances, leather, or moist vegetable matter, may be completely covered with mould. Of the more minute fungi, some species pass through their whole existence in a few minutes, from the invisible spore to the perfect plant. Lind says, that the first rains in Guinea have been known to make the leather of shoes quite *mouldy* and rotten in forty-eight hours; showing that the plants which disorganize the leather must have drawn their nutrition, even from its heart, in that time, and, by many successive generations, extended themselves over its total surface. Mr. Berkeley describes a *Polyporus squamosus* which, in three weeks, acquired a circumference of seven feet five inches, and a weight of thirty-four pounds. The *Polyporus frondosus* described by John Bapt. Porta, sometimes transcends a weight of twelve pounds in a few days.

The *Bovista giganteum*, on the authority of Carpenter, the eminent physiologist, has been known to increase in a *single night*, from a mere point to the size of a large gourd, estimated to contain four thousand seven hundred millions of cells; a number which, when counted at the rapid rate of three hundred per minute, or five per second, would take the whole time of one person, *night and day, for three hundred years*. A square mile contains upwards of 3,000,000 square yards, or 27,000,000 square feet, so that a single *Bovista giganteum* may present, at evening, an almost invisible single cell, and yet place before morning, nearly one thousand eight hundred such cells in every square foot of a square mile.

Notwithstanding the wondrous productions of a *single* individual of one *species*, Fries, the Swedish naturalist, observed not less than *two thousand species*, within the compass of a square *furlong*. The same author tells us, that he has counted above 10,000,000 of sporules in a single individual of the *Reticularia maxima*, so minute as to look like smoke as they rose in the air.

Webster, when writing of the malignant fever of 1795, informs us that *sound potatoes from market* perished in his cellar, in thirty-six hours; and we know now how they perished. It was a parasitic death.

In the *Philosophical Transactions*, Lond. (vol. iv. p. 308, Abridg.,) it is stated, that a green *mould* attacked a split melon, and took three hours to sprout, and six to ripen and produce, and let fall new seeds.

At New York, the pestilential season of 1798, Webster says, that he saw a cotton garment covered with dark gray-colored spots of *mildew in a single night*, and that such events were then and there common.

I might multiply examples of the rapid growth and extensive diffusion of fungi, which, like the lowest classes of animals, seem to have a power of development and propagation inversely as their magnitude. The more minute the plants, the more rapid their multiplication; until, as they descend to those of the smallest scale, a microscope shows them in even visible growth. Nothing astonishes one more than to see in the bottom of a watch-

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glass a drop of yeast swelling up, as the *torula cercvisiæ* unfolds itself, and exhibts a forest of fungi, where but a few minutes before, only a spore or two were visible.

"The family of the funguses," says Badham, "is immense. Merely catalogued and described, there are sufficient to fill an octavo volume of four hundred pages of close print, of British species alone. Altogether there cannot be less than five thousand recognized species at present known, and each year adds new ones to the list. For the single mushroom that we eat, how many hundreds there be that prey upon us in return. To enumerate but a few and those of the microscopic kinds (there are some which the arms could scarcely embrace;) the mucor mucedo that spawns upon our dried preserves; the *ascophora mucedo* that makes our bread mouldy; the *uredo segetum*, that burns Ceres out of her corn-fields; the *uredo rubigo*, whose rust is till more destructive; and the *puccinia graminis*, whose voracity sets corn-laws and farmers at defiance, are all funguses. So is the gray *monilia* that rots, and then fattens upon our fruits; and the *mucor herbariorum*, that destroys the careful gleanings of the pains-taking botanist. When our beer becomes mothery, the mother of that mischief is a fungus. If pickles acquire a bad taste, if ketchup turns ropy and putrefies, funguses have a finger in it all! Their reign stops not here; they prey upon each other; they even select their victims! There is the *myrothecium viride*, which will only grow upon dry agarics. The *mucor chrysospermus* attacks the flesh of a particular *Boletus;* the *sclerotium cornutum* which visits some other moist mushrooms in decay. There are some *xylomas* that will spot the leaves of the maple, and some, those of the willow, exclusively. The naked seeds of some are found burrowing between the opposite surfaces of leaves; some love the neighborhood of burned stubble and charred wood; some visit the sculptor in his studio. The *racodium* of the low cellar festoons its ceilings, shags its walls, and keeps our wines in bonds; while the *geastrum* has been found suspended on the very highest pinnacle of St. Paul's. The close cavities of nuts afford concealment to some species; others, like leeches, stick to the bulbs of plants and suck them dry; these pick timber to pieces, as men pick oakum; nor do they confine their selective ravages to plants alone; they attach themselves to animal structures and destroy animal life; the *oxygena equina* has a particular fancy for the hoofs of horses, and for the horns of cattle, sticking to these alone; the belly of a tropical fly is liable, *in autumn*, to break out into vegetable tufts of fungous growth; and the caterpillar to carry about a *clavaria* larger than himself. The fungous disease called *muscardine* destroys many silkworms, and the vegetating wasp, of which everybody has heard, is only another mysterious blending of vegetable with insect life. Funguses visit the wards of our hospitals and grow out of the products of surgical diseases. Where then are they not to be found? Do they not abound, like Pharaoh's plagues, everywhere? Is not their name legion, and their province ubiquity?"

An ingenious friend proposes as an objection to my theory, that as malarious fevers are specifically the same everywhere, and as the plants of temperate differ totally from those of tropical regions, how are we to account for their identity? The intermittent is a native of Russia and Sweden, while it is also an endemic of the coast of Guinea, and of the banks of the *Orinoco*.

The answer given is, that of all plants of the same species, only the fungi are known to be natural inhabitants of the various climates of the earth; for, to use the words of Mr. Roques, "*we find mushrooms in every climate.*" We saw on a piece of damp leather, at the Cape of Good Hope, the same *mucor mucedo* that penetrates its tissue at Sierra Leone, or St. Petersburg. Like man, the fungi generally live in any climate, though there are among them some that infest only the steppes of Tartary, and others that revel solely on the sands of *Sahara. This ubiquity is one of their most peculiar qualities.*

But why is it, then, if the same fungi create diseases in Lapland and Senegal, that there is so fatal a difference in the intensity of them at these two places? As the fungi of a poisonous character possess acrid and narcotic properties, it is scarcely necessary to consistency to presume that the same species are everywhere the cause of malarious fevers. Yet, if that were an imperative supposition, it would not embarrass the question materially, because naturalists affirm, that *the poisonous cryptogami are rendered yet more poisonous by increased temperature and moisture.* The amanita muscaria, only narcotic or intoxicating in Siberia, and used there for the purpose of agreeable exhilaration, is mainly irritating in France and Italy, and therefore, there, a very deadly poison to the mucous surface and nervous system.

We have an analogous example of the poison-enforcing power of climate in the fact, that the common hemp evolves a strong narcotic, in the tropics, while no such excretion is thrown out from it in temperate regions. In the Crimea, the *conium maculatum* is used as an esculent vegetable. The tendency to cause moulds so intensely expressed in hot climates is seconded by the aggravation of their activity when produced. It is curious, too, that tropical regions excite only the more minute forms in a greater degree, which according to many writers are poisonous. "Those that are most injurious are generally of the microscopic kinds." (*Badham.*) If, too, the excess of rain may make poisonous, in our climate, even the esculent mushrooms, what may we not expect from the influence exerted upon the noxious fungi by the prolonged and heavy rains of the tropics!

May we not find a difficulty in believing that the sporules of the fungi are absorbable into the circulation? Their volume, or the selective appetency of the mouths of the absorbents and the lacteals, or of the pores of the venous radicles, may offer insuperable impediments to their entrance. The chyle globules are about two-thirds of the size of blood globules in man, and they are supposed to be readily absorbed by the lacteals. Fries states that he has seen cryptogamous sporules of the size of one ten-thousandth of an inch, which would give them a volume one-third of that of blood globules, and two-thirds only of that of chyle globules. In examining, when mixed together, blood globules and the spori of various minute fungi, I have often seen the latter, in line along the disk of the former, when it required fourteen of them to subtend its long diameter. They were, therefore, at least ten times as small as the chyle globules. So much for *size*. As to the selective power of the lacteals, we know that they suffer very many and various poisons to pass into the circulation, and that in this respect, they are much less particular than our fathers imagined. Besides this, we know that fungous growths, both in man and the lower animals, have been found in places, to which their germs could have gained access only by the circulation, or by imbibition. There is, therefore, no good reason for doubting that the spores of fungi find their way to the channels of the circulation, as do the cells of exanthematous diseases, and the virus of syphilis.

The cause of the uniform excess of malarious diseases at the end of summer and in autumn has been an interesting subject of discussion and wonder. Boot, in his life of Armstrong, observes that, "*the most remarkable circumstance* connected with the diseases supposed to arise from malaria, is their general prevalence *in autumn in every country where they occur.*" Even the yellow fever of places in which it is an ordinary endemic, is not an exception to this law, for Baron Humboldt says that, at Vera Cruz, where "May and June are hotter than September and October, the latter months greatly exceed the former in the number and vigor of the fevers."*

If mere vegetable decomposition were the cause of such fevers, we should find them most active in May and June, when, after the previous autumnal death, and the distintegrating effects of winter frosts, or soaking rains, the warmth and moisture of spring and early summer rapidly decompose the softened textures, to feed the tender spongioles of the swelling vegetation. The great

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* The regular return and continuance of this fever in the months of July, August, and September, every year, more or less since its first appearance in these islands. (*Jas. Clark, Dominica.*)

Yellow fever is most active in September, when the temperature has fallen *much below* that of July and August. (*William Currie.*)

chemists, heat, light, and moisture, are then most active; and the dead relics of the former year, prepared by *time, frost*, and *rain*, are ready for the process of decomposition, as the electrical and vital agencies of the countless and threadlike *radices* open up their intendered store-houses of nutrition. Although, therefore, almost every one has supposed that the autumn is the season of the greatest decomposition, that process is really conducted in the spring and early summer *with a tenfold energy*, as may be easily recognized by the extraordinary smell of the earth after a shower at this season.

Malarious diseases, therefore, are not probably the effect of ordinary vegetable decomposition; for they occur most when that is not at or near to its *maximum*. Everywhere they abound, when the general vegetation has just passed through its great orgasm. But there is another and special vegetation, *which*, *whatever may be the climate, has its spring-time and summer in the autumnal season* of the year. On the exhausted *debris*, and the varied *exuviæ* of plants, weeds, and grasses; from root to leaf; under ground and above ground; feeds a race of vegetables which wait for *their* food to the latest period of the season of heat, and then flourish most when the more perfect forms have completed their annual task, and submit to the inroads of these Goths of phytology.

Governed in a great measure by the phenomena immediately around him, an observer, seeing the period of sickness succeeding to the active vegetative season, assigns the cause to the climatic events which then ordinarily arise. Thus African writers believe that the rains are the immediate producers of malaria, for they descend in torrents in July, when the vegetation of that torrid clime is on the decline. On the other hand, the Sardinian supposes that the sickness of his *hot* and *dry autumn*, is the result of the heat and aridity, and that droughts after rains, and not rains after droughts, cause his *miasmata*.

In the *insular* West Indies, there are heavy rains in August and September, which are sickly months; whereas, the pestilential season of Demarara is also in August and September, although they are there the dry months.

Egypt, although placed in the northern hemisphere, enjoys a climate almost the reverse of that of other countries similarly situated. During the summer scarcely any rain descends. At Cairo there are but four or five showers in a year, and in Upper Egypt only one or two. Near the sea, showers are not quite so rare. Everything, therefore, is, in the hot months, brown and dry and hard—dews rarely descend, and the parched land lies locked up in a barren drought. About the first of June the Nile begins to rise rapidly. Its channel becomes full early in the month, and, at the summer solstice, it pours its waters over its usual barriers. The country is covered with water during the hot summer and autumn, and there is no vegetation, and no disease.

At the winter solstice, the spring-time of Egypt begins, and while nature leaps into amazing activity, the husbandman enters on his annual labor of sowing and planting. Toward the end of January, oranges and citrons blossom, and the sugar-cane is cut down. In February all the fields are verdant: the sowing of rice begins, the first barley crop is harvested, and cabbages, cucumbers, and melons ripen. The sickly season of

Egypt should, therefore, on my view, commence in the winter or spring, and accordingly, here as elsewhere, the ravages of disease follow the decline of active vegetation, and *the plague begins.* In 1834, the deaths by plague in December were 109; January, 1835, 151; February, 821; *March*, 4329; April, 1897; May, 321; June, 41 cases. About St. John's-day, the country being covered with water, the plague ceases.

We see, then, that *the insalubrity of a place has the most constant relation to the habits of the living vegetation.* Whatever may be the temperature or humidity, the most unhealthy period of the year is, in any given locality, that when the phanerogamous vegetation has completed its annual task of growth, and flowering, and fruitage, and feels the weakness of an exhausting effort, and when to triumph as it were over a worn-out foe, the cryptogamous plants plunder and destroy it.

A reference to books, whose authors did not perhaps even dream of this theory of fever, shows, that *the fungi are active chiefly in the end of summer and in autumn.* Dr. Badham observes, that "a wet *autumn* is generally found to be exceedingly prolific in these plants." This in England, but in Italy the very scenery is beautified by the number, variety, and coloring of these vegetables. "Well may their *sudden apparition* surprise us, for not ten days since, the waters were all out, and only three or four nights back peals of thunder rattled against the casements; and now, behold the meadows, by natural magic, studded with countless fairy rings of every diameter, formed of such species as grow upon the ground, while the chestnut and the oak are teeming with a new class of fruits, that had no previous blossoming, many of which had *already attained their full* *growth.*" "These are the fungus tribe, a new class of objects which have sprung up suddenly and now beset our path on every side, beautiful as the fairest flowers, and more useful than most of the fruits." "The extremely limited time during which funguses are to be found, their fragility, their *infinite diversity*, their ephemeral existence, these too add to the interest of an *autumnal* walk." "In such rambles he will see what I have *this autumn* myself witnessed, whole hundred weights of rich wholesome diet rotting under trees; woods teeming with food, and not one hand to gather it."

Merat and Lens say: "It is usually in *autumn* that they (fungi) are developed in humid places, where the air is thick and unwholesome, with a rapidity that has passed into a proverb."

In the *Cyclop. Amer.* we learn that the best time for gathering mushrooms is *August and September*, and Miller's *Horticultural Dictionary* remarks that *September* is the chief season of their growth.

In the beautiful work of Mons. J. Roques, on the poisonous and edible fungi, is the following language: "*We find mushrooms in every climate. A very large proportion* of them is met with *only toward the end of summer and in autumn.* Heavy rains, and unseasonably early heat, may force them in May or June; but they are then never so perfect. Thunder-storms with rain, develop them prodigiously."

Of one hundred and five species of fungi treated of by M. Roques, only one grows at all seasons; four in spring; one in spring and autumn; five at the very end of summer; eight exclusively in summer; twenty-eight in summer and autumn; and sixty-two exclusively in autumn. Of the one hundred and five, therefore, ninety-two are active in autumn, and thirty-six in summer.

Were this essay not necessarily very long, it would not be uninteresting to inquire, in how marked a degree the proportion of diseases to seasons, corresponds with that of the above table: but most of you know already that the relation is remarkable enough, since the growth of the fungi and of malarious fevers is generally in the order directly of autumn—summer—spring—winter.

LECTURE III.

THE FUNGI ARE ACTIVE ALMOST EXCLUSIVELY AT NIGHT, AND ABOUND DURING THE PREVALENCE OF EPIDEMICS AND EPIZOOTICS.

THE most common malarious diseases are not producible by exposure in sickly places *during the daytime.* Whatever may be their cause, it seems to have activity almost solely at night. *Darkness* appears to be essential to either its existence or its power. As this position is not generally acknowledged, I may be pardoned for going into some detail on it.

Dr. James Lind cites the following case: The Phœnix sloop-of-war, of forty guns, was employed, in 1766, on the coast of Africa, where also was the Hound on the same duty. Both vessels, after a healthful cruise, put into the African Island of St. Thomas, notorious for its pestilential character. Here, of the crew of the Phœnix, slept on shore, seven officers and servants, while three midshipmen, five seamen, and one boy, were also employed on a watering party, which detained them on land at night. Of these sixteen persons, only two survived the malignant fever which followed. The remainder of the crew of two hundred and eighty men were permitted to go ashore in the *daytime*, where the men rambled about at pleasure, followed field-sports, and washed their soiled clothing. *Not one of these was attacked with any kind of fever;* and before her return home the ship lost only one man, and he died of the effects of a blow on the head. The crew of the other vessel, the Hound, were permitted to visit the shore only in the daytime, although no other restriction seems to have been laid on them. *Of these not one died of fever.*

Another equally remarkable case is given by Lind. In 1766, some French Protestants settled in a paludal part of Florida, where finally most of them perished. On some business, they were visited by eight gentlemen, more healthfully seated at a considerable distance, who spent one night there. On the following day seven other persons, from the same place, paid them a visit, but left their district before nightfall. Of the first party, every one was attacked with intermittent fever, and two died; while of the other party, not one individual suffered in the slightest degree.

The judicious Dr. Hunter, of Jamaica, relates cases of nocturnal damage of the same character. In one instance, out of sixty or seventy men sent ashore to water, not an individual escaped fever, while the rest of the crew enjoyed good health.

Dr. James Johnson, in treating of this subject, remarks that, while cruising or at anchor between Batavia and Malacca, his crew lost but one man, by fever, who had not spent the night ashore; whereas, almost every one who slept even a single night at Edam died. No ill effects were experienced from going on shore in the day-time. Even *being awake during the night, when on land, did not protect the seamen from danger.**

Tratter (*Med. Naut.* i. 456) says, when speaking of the danger of exposure to the land-air at night, "every man who slept ashore died, and the rest of the ship's company remained remarkably healthy."

On the authority of Surgeon Allen, we learn that, at Zanzibar, *all* who slept on board ship escaped; *every victim* seen or heard of had passed at least *one night on shore.* The captain and forty men, from a French corvette, who passed a night on land, were attacked by the coast-fever, and not one survived.

Dr. Evans, writing from the unhealthy Island of St. Lucia, observes, that during the day, "the sportsman wades through the stagnant waters and mangrove-bushes, which cover the surface of West India fens, with comparative impunity; but long before the sun has disappeared, he places himself beyond the reach of their poisonous effluvia."

Mr. Webb, Inspector of Hospitals, stated, before a committee of the British House of Commons, that the men who remained on board the ships in the noxious climate of Walcheren, were *extremely healthy, although they went ashore to bathe and exercise daily, but never remained on land at night.* Yet it was in that very

* I do not find this remark indorsed by any other writer, but my eloquent friend, Professor Dickson, confirmed it to me in a recent conversation on the subject.

place that the English army, encamped or lodged on shore, was almost annihilated by malignant intermittents.

Robert Armstrong says, that of the crew of the ship-of-war Monarch, employed to collect, at Xanthus, specimens of ancient art, the large body of men employed on shore were, without exception, attacked with remittent fever, and twenty-four of them died; while those who remained on board the ship were, to a man, exempted from fever.

The inhabitants of our southern country are fully aware of the important truth I have just illustrated, for they avoid, as a deadly poison, the night air of malarious regions, but visit them and travel through them fearlessly during the daytime. The precincts of the City of Charleston are especially pestilential, and the country fever, as it is called, is remarkably fatal to the residents of towns and of the upper country. To sleep one night in this district, puts in peril the life of the unacclimated; but no one believes that the most prolonged visit by day is attended with any danger.

In Major Tulloch's masterly report on the health of the military and naval service, he observes that "the sickness of the shore very rarely extends to the shipping, *though only a few hundred yards from the land.* The visits of sailors to the shore *by day* did not produce disease. In the Ceylon service, the mortality of the marine force, by fever, was three in one thousand, of the military, 24·6, or more than eight times as great."

The frigate Potomac, on a three years' cruise, visited some of the most insalubrious stations of tropical regions, and yet lost only twenty-six men, of whom not one died of fever. Dr. Foltz accounts for this happy exemption from malarious diseases, by stating that his men were never permitted to remain ashore during the night.

During *three voyages into tropical regions*, I always advised the adherence to the safe rule of compelling the seamen to return to the ship at night; and although we watered in places notorious for their insalubrity, and eminently destructive to parties which ventured to remain ashore at night, we did not lose one man by fevers of any kind. In some of these places, the water was stagnant and irritating at first, and caused inflammation of the skin of the legs of the waterers. The heavy odor of the rank vegetation, and the damp feel of the air among shallow pools, where myriads of insects sported, gave lively evidence of a pestilential locality. Besides that, the sickly and bloated looks of the white inhabitants of some of these places evinced the presence of active malaria.

In vain do we search in the works on received theories for the cause of this curious influence of night. It is in the daytime that evaporation goes on most rapidly, and that chemical changes, produced both by heat and light, are in most active operation. The water is warmer, the common vegetation more vivid, and the great chemist, the sun, is urging on the processes of the laboratory of nature. This is of course admitted by many writers, some of whom confess manfully the difficulty of this part of their subject, while others suppose that the miasm, evolved during the day, descends at night. Were this really so, it would scarcely account for the extraordinary difference of disease-producing power between the night and day; but when we consider how currents of air must sweep away the diurnal emanations, and how late in the night it is before the earth becomes cool enough to detain its proximate atmosphere, we can with difficulty admit this mode of explanation.

It has been also said that the baneful effect is due to the great change of temperature which follows the advent of night, by which moisture is precipitated by the air, and the human frame is chilled and sickened. As there is in the most unhealthy regions (coast of Africa) the slightest diurnal change; as rocky islets (De Loss in Africa) are sometimes most pestilential; as a wall, a road, or a screen of trees, sometimes separates a bad from a good locality; and as no such meteorological differences appear to explain the vicissitudes of the health of different years, we must reject such causes, except as excitants of the power of some poison yet to be discovered.

But when we observe the extraordinary tendency of fungous vegetables to develop their power only at night, we detect *another analogy between malaria and the fungi.* In vain do we search in the latter part of a day for young mushrooms. The early riser finds them in their prime and abundance. A field which at evening exhibited not a single plant, is often whitened by their little umbrellas in the morning. "It is well known," writes Comstock, "that this tribe of plants springs up almost everywhere, especially among decaying substances, and that *thousands* may be seen in the *morning*, where none existed the evening before."

Even the more durable kinds of fungi appear to add during the day little to their bulk, preferring to grow almost solely under the eye of night; so that these anomalous vegetables not only choose for their growth *the season of vegetable repose*, but *the hours of vegetable* SLEEP. In another respect they are beings of contrast, for, while other vegetables are adding oxygen to the air, from which they have extracted its carbon, these, as if they were averse to agreeing with phenogamous plants in any respect, are eliminating carbonic acid, having extracted from the undecomposed organic matter, on which they live, its more peculiar animal elements, the hydrogen and nitrogen.

Mr. Sowerby, the best of authorities on this subject, took the minute unopened *volva* of a *Phallus Inodorus* into the house in the evening, and found it in the morning a full-grown plant. In his experiments nothing occurred to show that this fungus grows in the daytime.

Supposing that the minutest fungi possess the general properties of the class to which they belong, we may readily perceive what prodigious influence must be exerted on them by the damp, rich air of a swamp—and if they have, as Heusinger alleges, a polarizing membrane, and, consequently, electrical relations to the polarized vesicles of a marsh mist, that mist, imbued with moisture, enriched by the terrestrial exhalations, and screened by the shadows of night, may form the most fruitful floating soil for the invisible cells of microscopic *cryptogami:* so that from the damp earth, or the nebulous air, or both, may come out to propagate disease, the cells of an anomalous vegetation.

But it may be reasonably objected, that if these things do grow at night, they should, sometimes at least, taint the day air of their vicinity, from which they can scarcely be entirely eliminated by an absorbing earth or a dissipating mist. It might be enough to say that, if they have electrical relations to the mist, or ascend only during the night, the quantity necessary to produce morbid results may not remain during the day, but the study of the habitudes of the fungi has revealed *other* reasons for the diurnal changes of salubrity in malarious regions.

In the first plaec, let me say, that no other vegetables are so strietly limited as these, as to existenee or properties, by apparently slight changes in their relations to extrinsies, and yet their germs resist eauses of destruction of the most active nature. Boiling water, many of the aeids, and eaustie ammonia, fail to destroy them, and they sustain the eold of solid earbonie aeid* (Cagniard de la Tour) without the abatement of their produetive power.

Dutroehet found that by slightly aeidulating a weak sulution of the albumen of an egg, various *monilia* were produced, but that when it was made alkaline the genus *Botrytis* appeared. On a neutral or simple solution no fungi showed themselves.

The *torula eerevesiæ*, or yeast-plant, grows in one form in a saeeharine solution eontaining yeast, and in another very dissimilar shape in stale beer. "There are some (fungi) which are seen only onee or twice in an age, and that in plaees where it is very diffieult to aceount for their formation." (Art. MUSHR., *Rees.*)

According to Pereira, "the fungi consist of eells and fibres, always sprouting from organized and generally deeayed or deeaying substances, *not perfeeted when entirely immersed in water.*"

Fungi (on the authority of Merat et Lens) appear susceptible of remarkable diversifieation, aeeording to climate, season, and soil, whieh *polymorphia* makes their study diffieult.

Almost every mineral, however poisonous, supports a

* Minus 118° F

peculiar eryptogamous vegetation. Thus we have *hydrocrocis arsenici* in solutions of arsenic, *hyd. barytica* in solutions of baryta. Fungi grow in ink, in wine, indeed, in everything; and naturalists are yet in doubt whether these seemingly diverse things owe their differences to soil, water, and temperature, or to different germs, each eapable of growing only in its restrieted field.

Some fungi are confined to particular plants, both above and beneath the surface of the ground, and some, as the entophytes, exist only in the *interior* of living vegetables. Even within hard, dried wood, a fungus creates a species of fermentation, by which moisture is evolved. The fungus appears finally at the surfaee, and the ligneous fibre erumbles to dust. It is dry rot; and the destroyers are the *Polyporus destructor* and the *Merulius lacrymans*, and *Vastator*.

Most writers on this subject, including Christison and Foderé, believe that the climate alone greatly alters the fungi; so that some, which are *generally* eaten with safety, become poisonous, and some of the poisonous kinds beeome esculent. The Ag. Piperatus, aecording to Haller, is poisonous in France, but esculent in Russia. The Amanita Muscaria, an intoxieating food in Siberia, becomes a deadly poison in the south of Europe. Foderé states that the most delightful of the eseulent mushrooms of France *become unsafe after prolonged rains.* The same thing oeeurs in South Carolina, where, in very wet weather, it becomes necessary to remove the mushrooms, or keep up the hogs, that they may not be poisoned by that which, in eommon weather, is eaten to advantage.

As the power of growth, and the quality of the fungi, are so dependent on slight causes, we can searcely wonder that a plant of this class may be noxious as produced at night, and hurtless as developed by day. Even if produced alike in both, the poison of the cryptogami is so subtle and fugacious, that a little daylight or sunshine may totally alter its properties. Foderé (*Méd. Legal*) tells us that most fungi become safe *when they have been dried*, which Christison thinks probable, as their poisonous properties appear to depend on a volatile principle. Finally, Letellier assures us, that the acrid principle of the agarics is so very fugacious, that it disappears on boiling, or drying, or by maceration in weak acids, alkalies, or alcohol. If, after all this, we find a malarious poison active at night, and not by day, it does not present an objection to the theory proposed, but affords some support to it, since we know of no *other* things which are so materially affected by light and heat.

I am now, gentlemen, about to show you a very curious part of this singular subject, the extraordinary association of fungous life with the existence and propagation of great epidemics and intense endemics. Not only are common moulds more common on such occasions, but there often appear new, or unusual productions of this kind. Hecker and Webster abound with examples of this truth. These moulds were chiefly red, but sometimes *white*, yellow, gray, or even black. They arose in an incredibly short time, on the roofs of houses, on the pavements of cities; on clothes, on the veils, and handkerchiefs of women; on various wooden domestic utensils, and on the meats in the larder. Even the depths of cellars, and the inmost recesses of cupboards and chests were invaded by a bloodlike mould, which filled the observers with disgust and horror.

Joseph Mather Smith remarks, "that pestilence; a strong tendency in dead animal and vegetable substances to rapid decomposition; morbid and immature fruits; and a vast amount of insect life, seem to have a common cause in epidemic meteoration." Admitting the extraordinary tendency to decomposition manifested in epidemic periods, Craigie observes, "that the rapid decomposition of vegetable and animal matters is to be regarded, not as a cause of fevers, but an effect of the febriferous state of the atmosphere, which thus displayed its insalubrious influence, not only on the human race, but on the vegetable world, and on *dead animal and vegetable matter.*"

Plutarch, in his life of Romulus, says, that in the first great plague at Rome, it seemed to "*rain blood.*"

On the 3d of July, 1529, when the continental sweating sickness prevailed, a *blood rain*, as it was termed, appeared at Cremona. In the sweating sickness of the English, there was remarked "an exuberance of the lowest cryptogamous vegetation."

In that calamitous period, which commenced An. Dom. 250, Decius being emperor, a pestilence began, which cut off, in the next fifteen years, half the human race. Eusebius relates that the air was so impure, as to "cast a *mould* like *turbid dew*, of a cadaverous hue, *on every object.*" Cadenus likened it to *the gore* or *blood* of the dead.

In 1813, at Malta, on the 14th of March, "the light showers that fell in some parts of the island, brought down a *reddish earth* with them. The same phenomena were observed at Palermo on the same day. In April, the plague commenced its ravages in Malta. (*Maclean.*)

Boyle makes the observation that, at Naples, in 1660, there was a pestilence, during which curious mould spots appeared on garments. So, in the plague of 746, spots in the form of crosses were observed on clothes.

At Brussels, in 1502, a pestilence drove the people from the city. On their return, they observed that, in that single season, a cryptogamous vegetation had covered the roofs of the houses. (*Webster.*)

At New York, in 1795, Bailey describes the destructive influence of that sickly season on cabbages, different kinds of fruit, etc. "It was remarkable that cherries did not come to perfection, and very soon showed *a disposition to decay.* The apples began to fall nearly a month before the usual time. *Those which came to maturity could not be kept so long as it is common for them to be preserved.*"

Webster also speaks of that year, (1795,) as peculiarly fungiferous. To use his own words, "the air of New York produced astonishing effects in the way of mould." Garments were spotted in a single night, the pavements became mouldy, and wooden furniture and utensils were spotted. Even desks, carefully closed, were invaded by mildew.

The year 1798 was remarkable for its prolonged heat and drought. Looking over the distinct and simple daily narrative of Dr. S. P. Griffitts, I find no mention of rain from the 20th of July to the 4th of September; and other writers describe this season as peculiarly hot and dry. Notwithstanding this, Condie and Folwell inform us, that "*the different kinds of mushrooms were found in great abundance* during their season." Webster also states, that in this year there were *fogs which had a singular odor*, and "even the pavements were covered with a mouldy dew." Through Dr. Rush, we also learn that the great heat of the season brought peaches to perfection nearly three weeks before the usual time; while apples, after being gathered, rotted much sooner than is commonly observed.

In 1799, at New York, similar phenomena were observed; and Webster noticed the extraordinary death of multitudes of flies, which became *white exteriorly*. This disease seems analogous to that of the muscardine of the silk-worm.

In pestilential Africa, when the rains and the sickness commence together, the fungiferous powers are fearfully developed. According to Park and Lind, the first rains *stain the clothes*, and make even woolens and leather *mouldy* and rotten in a day or two.

In St. Lucia, the most unhealthy station of the West Indies, "during the driest period of the year, a pair of boots are covered with vegetation, within twenty-four hours after being cleaned." (*Evans.*) In confined places, in unhealthy stations, the air is of a mouldy odor, "*earthy and mouldy.*" (*Robt. Armstrong.*)

During the epidemics of yellow fever at Natchez, in both 1823 and 1825, Cartwright noticed an extraordinary tendency to the production of mould, so that the shoemakers complained of the extreme difficulty of preserving even new articles in their line. Cartwright was surprised at this, because the meteorological state of the atmosphere would not account for it. *It was a fungiferous power irrespective of unusual dampness.*

During the prevalence of the cholera in Philadelphia, in 1832, I was shown in several different places a splendid vermilion-colored *mucor*, which attached itself to paste, starch, and other vegetable preparations. The housekeepers who noticed it then, had not observed it previously, nor have any of them seen it since. At that time, the flies died as in New York, in 1799, and were covered with a whitish dust. Confirmatory of these observations is the assertion of Copplez, Lamoth, and Coulin, that alimentary substances putrefied with unusual rapidity in the season of cholera.

In a letter addressed to me on the 3d of December, 1847, by Josiah G. Cable, M.D., of the United States service, I am informed, that, at Monterey, in a season always excessively dry, and then peculiarly so, under a burning sun, and on a lofty range of country, the men suffered greatly from miasmatic disorders. He also remarked the uncommon fungiferous tendencies of the place, as manifested by the mould on fruit, and the cacti, and aloe, and even "when a dead Mexican was turned over on the battle-field, his clothes were found to be covered *with a white fungus.*"

In fine, the history of epidemics abounds everywhere in examples of the cryptogamous luxuriancy of epidemic seasons. It is noticed by the careless observers of the middle ages, in more than half the recorded cases; and the ancients speak, not unfrequently, of offensive fogs and frightful mists and moulds. The spirit of the mist, according to Hecker, stinking and pestilential, moved over the face of devoted England, where, as it went, were scattered the seeds of the *sudor anglicanus*, by which that kingdom was almost depopulated, and sometimes the people of the villages were entirely exterminated. Many epidemics, as cholera and plague, select for peculiar residence and ravage, damp, dark, noisome places, where want of light and dryness and ventilation, must especially favor fungiferous processes. The instinctive aversion to mouldiness, as to serpents, seems to be, therefore, not without its utility, and in seeking the elevation in society which gives to man cleanly habits and an airy residence, individuals find a physical exemption from disease and pain even more valuable than the social enjoyments.

In the history of epizootics, are related a multitude of examples of the production of destructive diseases, apparently brought upon cattle and other animals by mould.

The fatal *angina maligna* of cattle, a gangrenous disease, which prevailed in 1682, was attended by a *blue mist* or dew on the herbage of pastures.

The milzbrand, *a gangrenous* disease of cattle, not unusual in France and Germany, is, according to Thomasin, very prevalent in Burgundy and Provence, where it affects the herds chiefly of low and humid districts in *summer* or *autumn* after inundations, by which the pasturage is deteriorated and the fodder *moulded* and *mildewed.* The disease thus acquired by cattle, may be conveyed to other animals, including man, by ingestion, (Chaussier, etc.) or even contact with the skin, (Morand, Duhamel, Thomasin,) producing in either way symptoms of fever in some persons, and malignant pustules in others. Sometimes a gangrenous fever is the consequence, and at other times only a local gangrene of a very intractable character ensues. That the poison upon which this very curious disease depends, is vegetable, may be strongly inferred from the fact that its *virus* is capable of resisting not only the heat of boiling water, but the action of caustic lime prolonged for at least two weeks. (*Gruby.*) No animal substance or even ovum, is known to have the power of resisting such agents, while, according to Cagniard de la Tour, the spori of the fungi can withstand means of destruction quite as potent. And, on the authority of many authors, we know that "unlike most seeds, they (the spori of the fungi) seem capable of resisting the prolonged heat of boiling water, infused in which, and poured upon the ground, they are still capable of producing each after its kind." (*Badham.*) So tenacious of its integrity and power is the *virus* of malignant pustule, that it can retain its destructive properties even when the wool or hair has been cleansed and woven into cloth, or the hide converted into leather. (*Bayer.*)

In this instance we have a disease originating in a grazing animal, probably from its food, when mildewed, which disease may be propagated by inoculation or ingestion, and of which the germs resist the heat of boiling water, the caustic action of lime, the detergents of the washer and weaver, and the prolonged tanning of the leather-dresser. Nothing known to us but the spores or nucleoli of the fungi are capable of accounting for these phenomena. Vimat, a commissioner of the Royal Academy of Medicine, made a report to his constituents, on an epidemic which occurred in the department of La Muerthe, near Marsal, which began in the cattle fed on recently inundated swamps. It was a *carbunculous* affection, which, *without material change of character*, affected subsequently the inhabitants of the same district. (*Fourcroy, Med. Eclairée.*)

J. S. Michael Leger, published at Vienna, in 1775, a treatise concerning the *mildew* as the principal cause of epidemic disease among cattle. The mildew is that which *burns and dries* the grass and leaves. It is observed early in the morning, *particularly after thunder-storms.* Its poisonous quality, which does not last above twenty-four hours, never operates but when it is swallowed immediately after its falling.

There is, in the wild regions of our own Western country, a disease called the *milk-sickness*, the *trembles*, the *tires*, the *slows*, the *stiff-joints*, the *puking fever*, *etc.* Of this curious malady, I have already, gentlemen, given you in its proper place, an elaborate history; but it may not be useless here to recapitulate the leading thoughts of that lecture.

An animal affected by the cause alluded to, usually exhibits the symptoms of the disease upon being driven hard for a very short distance, perhaps only a hundred yards.* It then trembles, loses its regular power of locomotion, staggers, falls, makes ineffectual attempts to rise, becomes convulsed, and dies. When the affection arrives under quietude, the animal seems to lose its voluntary route, and strays irregularly and apparently without motive. Its power of attention is impaired, the eyes become red and turgid, and the color deepens from a bright to a dark red. Finally, it trembles, staggers, and dies. When other animals—men, dogs, cats, poultry, crows, buzzards, and hogs—drink the milk or eat the flesh of a diseased cow, they suffer in a somewhat different manner. The attack in men is usually ushered in by nausea, followed by vomiting, which at irregular intervals recurs, until the close of the case in death or convalescence, a period usually of from four or five to ten days. In the first stage of the attack, the sufferer com-

* This reminds us of the tetanode state of a frog, which being affected by a small dose of strychnia, falls into convulsions at the touch even of a feather. Marshall Hall recognizes the resemblance in this, to a diseased predisposition, waiting for an exciting cause.

plains of severe pains in the limbs, but chiefly in the calves of the legs, and sometimes at the nape of the neck. A headache is a common event. Even before the open attack, during the incubative period, constipation is observed, and a very obstinate torpidity of the bowels is a marked feature during the whole case. The abdomen is commonly enlarged, and doughy, and presents a very singular, diffused pulsation, most conspicuous to the right of the navel. In some cases there is gastric or abdominal pain and tenderness, in others even the prolonged vomiting does not cause pain; but usually there is perceived a curious and *intense sense of heat* at the *epigastrium*, which produces a desire for cool drinks, independently of a sense of thirst.

As in most intense fevers, the pulse is often in this one even natural, or, while the face is flushed, the extremities became frightfully cold, and the pulse falls to preternatural slowness or is accelerated to one hundred and ten or one hundred and twenty per minute. (*Buck.*) In some cases no sensorial disturbance is perceived, in others there is intense nervousness, extending sometimes to delirium, vigilance, coma. Such cases commonly prove fatal after the occurrence of *singultus*, *subsultus*, a hurried irregular pulse, cold extremities and a sunken countenance.* There is, according to every detailed account, a singular fetidity of the breath, not like any smell known to the describers; which with obstinate vomiting and costiveness, peculiar, soft enlargement of the tongue, and an abdominal pulsation, most distinctly

* Sometimes the *hair*, cuticle, and *nails* drop off. (*Lea.*)

M. Roulin tells us that in Colombia, the maize is liable to a kind of fungus or ergot, which occasions the loss of nails and hair.

felt *to the right of the umbilicus*, constitute the marked distinctions of this malady.

The animals made sick by the beef of the first one, have been in their turn the cause of a like affection in others; so that three or four have thus fallen victims successively.

Whatever the poison may be, it resisted the influence of the cook, in all the customary modes of preparation, also the action of diluted acids, and alkaline solutions, and chlorine, and some of the chlorides. *Infusion of galls* alone seemed to abate, but not to destroy its virulence. The water in which poisoned beef had been boiled acquired no poisonous properties; while the beef remained as noxious as ever. Butter from diseased cows, heated until it caught fire, did not lose its deleterious properties. (*Graaf.*) The urine of diseased animals, collected and reduced by evaporation, produced the characteristic symptoms. Milk of affected cows, or sluts, was very poisonous to their own young as well as to other animals, while the lactation preserved themselves from the malady, so long as they were milked regularly.

The animals originally affected, are only such as live upon herbage, such as cows, horses, goats, and sheep. The pastures in which the disease is found are *always* the unbroken soil of the new country. The action of the plough, even for a single season, is regarded by most authors as a *permanent* corrective.

Whatever may be the poison, its most potent activity exists in the end of summer and in autumn, chiefly in September and October. One writer denies the truth of this statement, but a large number assert it very positively.

It also acts only *at night*, or until the dew has been exhaled from the grass in the morning; for even the worst ranges are safe during the day, except where they lie in thickly wooded districts.

This disease has been found in rich alluvial places, on high barren ridges, on open plains, and in the deepest woods. Its place is sometimes confined to a small space inclosed as a "sugar-orchard," and entirely destitute of water; while it extends in other cases throughout a long narrow range of country, for as much as one hundred miles.

From the testimony of authors, each of whom has a peculiar opinion on the point, milk-sickness may prevail in wet or dry, hot or cool autumns, the character of the season seeming to have no especial relation to the severity of the epizootic.

The period of incubation varies in cattle, from two to ten days, when an attack is not sooner excited by violent exercise. When the disease is produced by the swallowing of poisonous beef or milk, butter or cheese, the nausea and vomiting may occur almost instantaneously, or may not appear until after the lapse of several hours or even days.

Whatever may be the poison, it seems, according to the experiments of Graaf, to be reproductive within the system of the poisoned animals; for the quantity of flesh necessary to produce the diseased effect was about the same, whether taken from an animal originally affected, or from others successively poisoned by its flesh or milk.

Most writers say, that attempts to inoculate with the blood, milk, etc., of affected animals have failed, but Drake asserts, on the authority of two credible witnesses, that the milk-sickness was produced in them by skinning diseased cows.

The autopsy showed gastro-intestinal inflammation, enlargement and softening of the liver and spleen. The meninges and brain exhibited congestion, inflammation, serum, lymph, pus. In all the fatal cases, *the blood failed to coagulate*, and there was uniformly a contraction of the stomach and intestines.

Authors generally admit, that only the grazing animals take the disease originally, and that other animals can only receive it through the medium of their flesh or milk, after they have been poisoned. As all animals seem impressible, there is a fair inference against the aerial character of the cause of milk-sickness, by which if it exist they should be equally and originally tainted. The facility of the correction by the plough, the insoluble and non-volatile nature of the poison, evinced by the effects of boiling or roasting the beef, and of the evaporation of urine even to dryness, all show clearly that the poison is not atmospheric, not aëriform or vaporous. It seems, therefore, plain enough that cattle receive it into the stomach as *food* or *beverage*. That the poison is not found in the water taken by the grazing animals, seems highly probable, because it has not been found subsequently to be soluble in that *menstruum*, or indeed in any other simple liquid, while the truth of this position has been almost demonstrated by confining them in limited inclosures, where, notwithstanding the total absence of water, many of them have in repeated instances exhibited veritable symptoms of the trembles. A critical examination of the waters of infected regions has failed to show peculiar or poisonous properties, and the plough corrects the evil, without being shown to be able to alter the waters materially.

It seems then very probable that the poison, whatever it may be, is swallowed with the food. Now the food is more or less soiled with earth. It is, also, in its most hazardous condition covered with dew, and is infected by insects, and the seeds of various plants and flowers.

Of these, the soil cannot give the venom, as it would not lose such a power by the action of the plough. A mineral poison would also be easily detected in it, and could not propagate itself through a succession of animals; nor has it a reproductive power.

We are reduced, therefore, to the only remaining hypothesis, the introduction of an *organic poison* of some kind, animal or vegetable, into the nostrils or stomachs, (probably the latter,) of the affected animals. The long latent continuance of the poison in the body, the apparently small quantity of it necessary to create disease, and the seeming reproduction after reception, all enforce the conviction that the virus is *organic.*

Having rendered probable the presence in these cases of an organic agent, the usual course of medical reasoning would lead us to assume its *animal* derivation, especially as it seems to have even in the system a reproductive power. But just at this point of time, the microscopic discovery of the frequent connection of vegetations with cutaneous and mucous diseases, and the probability that in other, and somewhat analogous cases, cryptogamous plants exercise a disease-creating power, embarrass us with a new element of difficulty.

Animal poisons are usually soluble, are commonly innocuous in the stomach, are not most potent at night, do not affect particularly the autumnal season; nor can we see how the plough could correct the evil, if of an animal character. The extraordinary fixity and indestructibility of the germs of this disease, point strongly to a vegetable source.

We are thrown back, therefore, by a kind of necessity on its vegetable origin, and among vegetables we find none whose habitudes and modes of action, so strongly as the fungi, entitle them to the sad distinction of creating this singular malady. They grow in autumn, they grow at night, they are disarmed by light and heat, they have extraordinary tenacity of life and texture, and yet are repressed by very slight alterations of soil and circumstance. They are usually poisonous, and produce curious and diversified maladies. Women are less affected than men by their poison, and children escape more readily than men and women. Some of them, after sending their poison through the system, escape unchanged by some one of the emunctories, as the *amanita muscaria*, by the kidneys. As we are reasoning upon probabilities here, let me ask what animal poison, what mineral poison, offers so many and so strong analogies, to entitle us to esteem it a cause of the milk-sickness?

LECTURE IV.

MOST OF THE FUNGI ARE POISONOUS, AND PRODUCE DISEASES RESEMBLING MARSH FEVERS.

Not only are the fungi generally poisonous to a singular degree, but the phenomena attendant upon their introduction into the system are so peculiar as to arrest the attention both of the toxicologist and pathologist. In most cases, the poison lies dormant for a time after its ingestion, then excites a morbid action of a febrile character, continued in some instances, remittent or intermittent in others, which is sometimes followed by abscesses or gangrene, as observed in typhoid fever and plague, occasionally by locked jaw and yellow skin, as in yellow fever. Even when using habitually, fungous food of à slowly poisonous quality, such as rye affected with *ergotætia abortifaciens*, *females of adult age*, and *the richer classes of society* are, to a remarkable degree, exempted from the disease-producing potency, which exerts itself so disastrously, in some parts of France and Switzerland, on the poorer and more exposed portion of society.

Of late years, too, it has been found that many cutaneous disorders, and at least one mucous disease, are, if not absolutely dependent on, at least closely associated with, and aggravated by, the growth of minute cryptogami. That these predatory fungi are really causers of the maladies with which they are uniformly connected, is made still more probable by the demonstration of the existence in insects, and even many larger animals, of *contagious cryptogamous diseases, which, transferred from animals to plants, and from plants to animals*, become very destructive, not only to their immediate victims, but to important commercial interests dependent on them.

It is scarcely necessary to prove to any intelligent reader, that the fungi are commonly poisonous. The caution with which mushrooms are bought, and examined, and cooked, evinces a sufficient knowledge everywhere, of the danger of eating the wild kinds. But as I am elaborating an argument upon a new and difficult subject, a few quotations, to show the sentiments of the best informed persons, may not be inexpedient. "By far the greater part of the tribe," says Comstock, "are poisonous. Some of them are so exceedingly virulent as to destroy life in a short time. Adepts, therefore, in botany dread the wild kinds." "So poisonous," says the author of the article mushroom, in *Rees's Cyclopedia*, "is one species of agaric, as to kill the very flies as they settle on it. The *Agaricus muscarius* is therefore used to poison flies and bed-bugs." Burnett quotes several curious cases where death has arisen in persons who have eaten mouldy (fungiferous) bread, mouldy pork, mouldy cheese, mouldy ham, pie, etc.

But it is rather to the *peculiarities* of these poisonings, than to the general fact that I would direct your attention. The first of these is the production of FEVER. Pereira tells us, "that the symptoms produced by poisonous fungi, are those of *gastro-intestinal irritation, and a disordered state of the nervous system*," a not inexact general definition of a malarious fever. "In the human system it (Agaricus muscarious) produces *shivering*, followed by that kind of *delirium which attends an ardent fever*." *Rees's Cycl.*, Art. MUSHR.)

A careful examination of the diseased potatoes of the British Isles, from which that kingdom has of late suffered so much, shows the uniform existence in them of "the fibres of a fungus called *botrytis*, from its grape-like form, or of one called *uredo tuberosum*, which may be observed ramifying round the cells which inclose the starchy corpuscles. Now these plants, however minute, are not self-generated, but must be produced by some seminal impregnation, transported by the atmosphere, and peculiarly adapted to fructify upon the *Sol. Tub.* This vegetable distemper, like *that of the cholera*, while general in its diffusion, is determined to particular localities and plants, by predisposing causes; yet it is not always dependent on these, having occurred in many regions where such causes did not materially operate." (*Ure.*) "The effects of using diseased potatoes were, in the first stage, rigors, heat of skin, quick pulse, and abdominal pain; in the second stage, rose-colored spots, migratory and evanescent, and diarrhœa; in the third *stadium*, a tumefaction of the muscles of the neck, shoulders, and arms, acute pain there, and, in the worst cases, erysipelas of the face and scalp, and œdema of the eyelids." (*O'Brien.*)

The effects of heavy single doses of ERGOT are, first, anorexia, nausea, vomiting, dryness of the throat, and thirst; secondly, abdominal pain and tumefaction, and diarrhœa; thirdly, weight and pain of head, giddiness, delirium, dilated pupil, somnolency, coma; fourthly, disturbed circulation by *increased fullness and frequency*, or *feebleness and slowness of the pulse. Formication* is a not infrequent consequence, while protracted use creates, not only febrile symptoms, but, as in malignant fevers, *a disposition to gangrene.* Christison describes the effects of its prolonged use, as *weariness and formication.* "IN A FEW DAYS *fever sets in*, with a *hemorrhagic tendency, rending pains of the limbs*, and at length *dry gangrene of the fingers, toes*, or even *legs*, which drop off by the joints." In some cases, the author just quoted reports *contraction of the spleen** and enlargement of the liver, as among the effects of ergot.

* The spleen is sometimes lessened. (Art. TYPHUS, *Dic. de Médecine.*)

Dodart, who acted under a commission of the French Academy of Medicine, reported to that body that ergot occasioned "*nervous phenomena and malignant fever, with stupor.*"

In 1826, Dr. Westerhoff saw two children who had been poisoned by mouldy bread; their faces were *red and swollen*, excited and haggard, tongue dry, inextinguishable thirst, feeble and frequent pulse, abdominal pain, vomiting and purging, vertigo, headache, great depression of mind and body, mental indifference, and somnolency.

On the 10th of June, 1839, at a musical festival at Aldenfingen, about six hundred people ate various kinds of meat, which, after being cooked, had been kept *in a badly ventilated cellar for nearly three days.* Upwards of four hundred of them were, *within ten days*, attacked by nausea, vomiting, some mental disturbance, colic pains, tenderness of the epigastrium, and diarrhœa. In the progress of the cases, disturbed circulation, constipation, fetid evacuations, and tympanitis, allied the cases to typhoid fever, *and nine died of this fever.* An autopsy revealed inflammation or ulceration in the lower part of the *ileum.* Those who did not go to the festival, but partook of these cold meats at home, suffered in a similar manner; while those at the festival who dined on bread and cheese, escaped all disorder.

Diseased wheat (*Phil. Trans.*, Lond. 1762,) produced, at Wattisham, a sickness with sphacelation. Seven persons of one family suffered the loss of one or more of their limbs, and one had a blackness of two fingers, but recovered.

The febrile disease from the use of rye is, according to Thompson, (*Lect. on Infl.*,) most prevalent in *wet* or *moist* seasons; and in thirty-three years, M. Noel met with this malady three or four times, and always in *rainy and moist seasons.* He also says, that among fifty patients, he *did not find one woman;* and he makes the very curious statement, that *only the poor and ill-fed were its victims.*

Pereira describes almost *choleric* effects of the poison of fungi, when he states that, in some cases, the powers of the vascular system were "*remarkably suppressed*, the pulse being *small and feeble*, the *extremities cold*, and the body covered with a *cold sweat.*"

It may not be disadvantageous to insert, in this place, the description of a yellow fever which became epidemic in the United States frigate Macedonian. It was given under oath to a court-martial, by Surgeon Chase:—"There were pains in the head, loins, and limbs; tenderness at the epigastrium, and sometimes in the fauces; nausea, vomiting, diarrhœa, or constipation; *the face was flushed, and sometimes swollen, the pulse was either frequent and full, or slow and small;* the eyes were red and watery; the mind was dejected; and there was, *ab initio*, low delirium or violent madness."

The famous *sweating sickness* usually commenced with a short shivering fit, which, in malignant cases, convulsed even the extremities. Many experienced, at the beginning, a disagreeable creeping sensation, or *formication*, on the hands and feet, which passed into pricking pains, and an exceedingly painful sensation *under the nails.* Some persons were afflicted with swollen hands and feet. In many the countenance was *bloated and livid*, the heart "*trembled and palpitated*," and lividness and rapid decomposition evinced the tendency to sphacelation. *The plague*, with its symptoms, its abscesses, and its mortification, might be taken for a case of fungous poisoning in its more intense forms.

You may now, gentlemen, turn to another curious effect of the poison by fungi: I mean, *periodicity.* Many authors mention, among the phenomena, *intermittency*, or *remittency.* The most singular of such cases is cited by Christison, who tells us that a whole family, consisting of a woman and her four children, were attacked by a *tertian fever*, by living exclusively for four months on *edible mushrooms.* The peculiar cause of the fever was made more manifest by the fact, that the husband of the woman, who lived on other fare, escaped all disease; while a *cutaneous eruption and subsequent gangrene of the extremities* attacked finally those who had the fever. Westerhoff observed in those who were poisoned by mouldy food, an *intermittent somnolency*, which he terms a remarkable feature of the case. M. Gassand saw cases of ergotism where the sensations either of heat or cold were *intermittent.*

Several other writers mention this feature. The mental disturbance intermitted in one case, inflamed eyes in another, and all the phenomena in a third. A young woman who ate a dish of *Agaricus clypeatus*, and was attacked with nausea, vomiting, bilious stools, and a frequent pulse, had *a marked remission* on the fourth day. The patient was at ease throughout the night, the skin was moist, and the pulse better. The other symptoms all abated, and the patient slept. On the fifth day *the symptoms returned*, with delirium, sighing, anxiety, failing pulse, great dyspnœa, *partial yellowness of the skin*, and even a *locked jaw*, as in some cases of yellow fever.

Another author cites a case of fungous toxication, in which "*the remission* was so well marked as to attract attention. The *Dic. des Sci. Méd.* reports cases of this kind, in which occurred *the most acute pains*, which *were intermittent;* and often there was a pause of two or three days, during which the sick could attend to their affairs." A recent epidemic fever in Scotland presented both the yellow skin, and the long and curious intermissions described in the above cases.

A reverend gentleman, of the Protestant Episcopal Church, in the City of New York, in the preceding year (1845) went with his family to a place near Sing Sing, and about three miles from the Hudson, which was selected because of its reputation for health, and its exemption from malarious diseases. In August and September, when mushrooms were very abundant, and when the country people abstained from their use, under the impression that they disposed them to fevers, the clergyman's lady, in her frequent drives, collected them daily, and for some time subsisted almost exclusively on them. The remainder of the family ate them more sparingly, and less frequently. About the end of September, the lady was attacked by an irregular fever, without periodical chills, but marked by an exacerbation on every second day. Thus the nature of the case was not suspected, until the return of an attack in the spring, which became regularly periodical in June, and assumed a distinct tertian form. It was then readily cured by the sulphate of quinia, and other means approved for intermittents.

In 1844, I busied myself with collecting and examining various species of fungi, most of them of a poisonous quality. For several hours a day, I hung over these specimens, watching the successive growths of fungus superimposed on fungus, and endeavoring, with a microscope, to measure the relative size of their spores and nucleoli. While thus engaged, I was, for the first and only time since my early childhood, attacked by a tertian, and was compelled to resort, after the third paroxysm, to the usual treatment for an intermittent. Whether this attack was the result of the slight vegetable decomposition, or an effect of the inhalation of spores of invisible fungi, I know not, but the coincidence was at least singular. That the latter supposition is the more probable one, is sustained by the well-known fact, that after an evacuation by an emetic or cathartic, of the poisonous fungi, no remedy is so valuable, as a corrective of the febrile and other consequences, as the preparations of cinchona. Merat and Lens, after describing cases of disease produced by fungi, remark, that *preparations of the bark are the best remedy.* Confirmatory of this opinion is the statement of Dr. King, of New York, (*New York Med. and Phys. Journ.*, 1825,) that, in a case of ergotism, wine and bark constituted the most effective remedial agents.

We thus see, gentlemen, that when patients are slightly affected by the fungi, symptoms arise which closely ally the case to those of common marsh fevers; and that the resemblance is still further improved by the discovery that both are to be most successfully treated by the anti-periodics.

More intense poisonings, by superadding buboes and *mortification* to other symptoms; bring fungiform diseases into close resemblance to the *plague.* Indeed, when we read first of the course and character of most epidemics, and then turn to the history of *cryptogamism*, in its diversified groupings, we cannot fail to be surprised at the many points of resemblance.

The plague is esteemed by many persons but an exaggeration of paludal fever. Mirolanoff, among others, inclines to this sentiment, and says that, at Archial, both officers and soldiers, who had intermittent fevers, were attacked with buboes and *carbuncles.* At Adrianople, Dr. Rinx observed that the slighter forms of plague were not distinguishable from intermittent fever, until the appearance of the buboes. Begin and Baudin also concur in the supposition, that plague is of the family of intermittents. John Hunter, M.D., of Jamaica, saw carbuncles in intermittent fever. After some continuance the part mortifies. "I have seen this in the *scrotum*, and also in the foot, and occasionally the loss of a toe." He also enumerates locked jaw as among the incidents of such cases. In 1798, Dr. S. P. Griffitts observed, in one day, two cases of mortification in yellow fever: one around the anus, and the other in a finger. Arujula met with carbunculous cases of yellow fever, and several gangrenous tumors.

The Hungarian fever of 1566 presented a kind of crisis by tubercles on the top of the foot, which, if neglected, ended in mortification, and many suffered amputation. (*Skenkius.*) In 1600, there raged throughout Europe a *mortal* colic, which usually destroyed life within four days. The patient became almost immediately senseless, *the hair fell from his head*, a livid pustule appeared upon the nose, which consumed it, and the extremities became cold and mortified. (*Webster.*) M. Roulin relates that, in Colombia the maize is liable to a kind of fungus or ergot, which occasions the *loss of nails and hair.* The poisonous property is lost by conveying it across the Cordilleras. (*Merat and Lens.*)

Marcellinus tells us that there "arose, in the reign of

Marcus Antonius, a fatal pestilence, which began at the sacking of Seleucia, and extended over the civilized world, from Caledonia to Persia. It was supposed to have arisen *from the foul air from a box*, opened by a soldier in search of plunder. The symptoms were, *light fever, and a gangrene on the ends of the feet.* In Rome alone, ten thousand died of it daily." The dark, damp old box, the evidence of a reproductive power, and the light fever and severe gangrene, speak strongly in favor of the fungous origin of this epidemic. Something very like this happened at Canton, where three persons were attacked with fever, and two with gangrene, in consequence of breaking unexpectedly into a coffin, long buried. Fortunately, no reproduction took place, and the terrible malady ceased with its first victims.

In another pestilence, A.D. 262, described by St. Cyprian, the patients suffered from despondency, debility, involuntary evacuations, *inflamed mouth, swollen stomach, and sparkling eyes. The disease destroyed the feet, hands, sight, hearing, and organs of generation.*

Chirac thus describes an epidemic at Rochfort, in 1741. Chilliness, great pain in the head, sense of *intoxication*, small pulse, syncope, *epistaxis*, inexpressible loss of strength, constant agitation of the limbs, leaden, cadaverous face, eyes dull or sparkling, continual nausea or vomiting, suppuration of the parotids, buboes, carbuncles, especially on the head and hands.

Gualtier de Claubry abounds in descriptions of gangrenous fevers of a low type. Thus in the typhus at Mayence, in 1813 and 1814, there was "*often gangrene of the extremities.*" At Forgou, in 1813, there was "*often gangrene of the extremities.*" In the hospital at Langres, in 1806, there was sometimes "dry gangrene of the feet." Fouquier, in describing a fever in the department of the Moxelle, in 1813, speaks of partial gangrenes on the surface of the body.

Thouvenel, a physician at Pont a Mousson, describes a febrile gangrene of projecting parts. Roux, Gilbert, Descastaing, Reveille, Parise, Frisal, Boin, Mauguis, Thouvenel, Fleury, Latourette, Robert, Fouquier, Gras, Castel, etc., mention, as events in fever, partial gangrene of the nose, ears, fingers, toes, and the loss even of a whole limb. So, also, John Hunter, McGregor, Pringle, Griffitts, Hillary, Deveze, Fellowes, Arejula, and others, describe as accidents of yellow and other fevers, mortification of the stomach, intestines, lungs, arms, legs, and scrotum.

One of the most striking examples of a gangrenous fever, presented itself in the village of Deerfield, in New England, of which the following account is extracted from the *Walpole Observatory* of the 9th of November, 1807. "On Tuesday, 2d of September, 1807, Joshua Fink, an unmarried man, of about twenty-five years of age, returned from Hartford, in Connecticut, to his father's house in Deerfield, where he became very ill, but finally recovered his health. On the 25th, twenty-three days after his return, his mother, Amy Fink, and his niece, who had nursed him in his illness, were attacked with *chilliness and vomiting*, followed by *excruciating pains and soreness* throughout their whole frames. They both died within twenty-four hours, *in a putrefactive state*. In that family circle, thirteen or fourteen persons were similarly affected, and only three or four recovered. Most of them died within twenty-four hours, in *a putrid state*. On the 7th of October, Sally Blacker was taken ill of the same disease and died on the fifth day." The narrative declares that she did not putrefy immediately, like the others, EXCEPT ONE OF HER FINGERS.

While poisonous fungi create the usual signs of fever, affecting the mucous tissue of the *primæ viæ* with inflammation, congesting the brain, disordering the liver and spleen, disturbing the circulation, and lessening or vitiating all the secretions they produce when used to excess, or for a prolonged period of time, a marked tendency to the ulceration and sloughing of compressed parts, as in typhoid fever, or to the mortification of the intestines or extremities, as in yellow fever, epidemic, camp, jail, or hospital fever, or to carbuncular destruction, as in plague. Every fungus of a poisonous nature does not produce all these morbid phenomena; but even the most nutritive of the mushrooms will, when long and almost exclusively eaten, manifest *the characteristic effects of the class.* In sudden poisonings, the peculiar tendency to sphacelation does not often occur, and when a disease is occasioned by only one or two doses, we seldom meet with gangrenous phenomena; but dreadful mortification often follows their slow and protracted application. As far as I can obtain information, it is made apparent that the more minute fungous forms have the most poisonous and gangrenous influence. Thus the long use of bread made of diseased rye (Ergotætia abortifaciens) causes not only a distinctly formed fever of a remittent character, but gengrenous sloughs in the intestines, and the dry rot of the extremities. We can scarcely resist the conlusion that this last effect is the consequence of the absorption and vital action of the fungous spores in the parts thus destroyed. Vegetables furnish us with many analogies. The diseases to which

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fruits and bulbous and tuberous roots are liable, are often the effect of absorbed fungi. Thus, in the *Microscopic Journal*, we learn that Arthur Hill Hassall caused decay at will, in sound fruit, by inoculating it with the spawn of fungi from rotten specimens. The mere bruising of fruit would not cause decay, unless fungi or their spores were present. So, the dry gangrene of the potato, so fatal of late to that esculent in Germany, and since in Great Britain and Ireland, is produced by *the absorption and destructive reproduction* of fungous germs in its very substance.* The analogy seems complete; for in both sets of cases fungi produce the disease, and in both a destruction of the life of remote parts is the consequence. In the potato and apple, the result is demonstratively caused by fungi. In the animal, may we not safely infer it, especially as several instances are recorded where the putrid matter, conveyed to puerperal women by the hand of the surgeon-accoucheur, has appeared to produce gangrenous *phlebitis;* just as was similarly excited, a gangrene of the fruit and the root.†

Even to my own mind, gentlemen, arises the objection, that most of my analogies result from cases in which the poisonous articles were taken into the stomach, and that, too, in large doses, such as could not be received into the system in any other mode. That objection seems more specious than sound, when we remember that very small doses of poisons are highly effective when inhaled by the organs of respiration. Thus a very

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* Ann. des Sci. Nat., September, 1842. M. D. Martius.

† In Simon's Chemistry, published since the first delivery of these lectures, we are told that Scherer obtained in the abdominal cavity of one who died of metroperitonitis, organisms resembling minute algæ.

few drops of chloroform will, by inhalation, produce effects on the nervous and vascular systems, more potent than can be created by any dose, however great, thrown into the stomach. A drachm of ether inhaled from a bag will intoxicate, stupefy, and prodigiously excite one whom ten or even twenty times that quantity would not greatly move by the stomach. So, while it requires not less than thirty grains of arsenic (Christison) to kill an adult, I have known nearly fatal results from the inhalation of less than half of a grain of arseniuretted hydrogen. Now it is obvious that, of the small quantity of the respired articles mentioned, a much smaller quantity is absorbed by the pulmonary membrane, and passes into the circulation. Of the few drops of chloroform used, at least nine-tenths must be exhaled by the breath, and thrown away. But when organized substances find their way into the tide of blood, and that too with vital energies capable of reacting on the elements of the sanguine current, it requires but little acquaintance with physiological and pathological phenomena to induce us to dread the most fearful results. Even when their vital powers are destroyed by mechanical or chemical processes, vegetable poisons act in the smallest portions with great violence. How much strychnia, or digitalia, or aconita is requisite for the disturbance of functions, or the arrest of vital action? Certainly much less than we may readily suppose could be inhaled by a sleeper, if such things were suspended in his atmosphere, even with faint diffusion. But the experiment of Prout during the cholera in London, in 1832, if to be relied on, showed a gain in atmospheric specific weight of one sixty-second part; which would give scarcely less than a drachm by weight of some poison, suspended in each cubic foot of the atmosphere of London. That quantity of air may be inhaled during common respiration in fifty inspirations; and, as most persons respire not less than fifteen times a minute, a cubic foot of air may pass through the bronchial tubes in three minutes and a half. How much, then, of such a poison may be presented to the bronchial surface in the course of a single night! With how much more force too will it act, when it assails the system through that channel! Substances presented to the gasto-intestinal surfaces are mixed up with various secretions, mucus, saliva, gastric juice, bile, pancreatic liquor, and special exudations from the peculiar glands of each successive section, while aërial poisons, unmixed and unfettered, are applied at once to a surface on which, behind scarcely a shadow of a film, circulates the blood prepared, by the habitual action of the respiratory function, to absorb almost every vapor, and every odor which may not be too irritating to pass the gates of the *glottis*. It is, perhaps, for this reason that we have so instinctive a dislike of mouldy smells, and of humid musty places, and unhappily we discover that *in the abodes of filth and poverty, where misery dwells and moulds do most abound*, the great non-contagious epidemics find and destroy the greatest number of victims, because there is the especial domain of fungiferous potency.

I have hitherto spoken to you of the action of fungi, when swallowed or when inhaled by the respiratory organs. I am now about to direct your attention to a not less curious department of our subject. I mean the association of obvious fungous growths with the cutaneous and mucous diseases both of men and animals.

In the very time in which we live, there has arisen almost a new science, founded on the discovery that many cutaneous diseases, some maladies of the mucous system, and a number of the disorders of insects and reptiles, seem to be produced by vegetations in the living tissues, by which comfort is impaired and sometimes life sacrificed.

Caffort alleges, that the *agaricus fimetarius* is found in ill-conditioned wounds, (*Annal. de Montpelier*, 1808,) and Mery and Lemery cite cases where fungi grew on the skins of animals, even when not wounded or ulcerated. Schoenlein and Remak observed, and Fuchs and Langenback confirmed the observation, that forms apparently vegetable, and of a fungiform structure, rooted themselves in the skin of *porrigo favosa*. . Gruby subsequently investigated the subject more fully, and alleged that the *crusts of porrigo are almost entirely composed of the plants.* The vegetable nature of the disease seemed to be established by the transfer of it by inoculation *to a phanerogamic plant*, thus imparting to a vegetable a disease contagious in man.

Since these striking discoveries have been made, microscopists have detected vegetations in *porrigo lupinosa, impetigo scrofulosa, serpiginous ulcers, sycosis menti, and porrigo decalvans.* To the latter, Gruby has given the name of *microsporon andouini*, in honor of the able writer on the muscardine of the silk-worm. We have now to encounter, among the phenomena of disease, porrigophytes, mentagrophytes, etc. etc. Each disease has its fungus, perfectly characterized by form, habits, position, and propagation. For example, porrigophytes are seated in the cells of the epidermis, while mentagrophytes reside in follicles between the hair and the walls of the follicles. The former have a proper capsule, are very rarely granular in the stem, and their spores are large and oval, while the latter have no capsule, granules almost always appear in the stem, and the spores are small and round. The former descend into the hair-follicles, the latter ascend from the roots of the hair to the epidermis.

Not alone the skin, but the mucous membrane affords a field for the growth of cryptogamous plants, at the expense of the health of that membrane. In the *Comptes Rendus* for 1842, M. Gruby describes a fungous plant, which seemed to be the cause of the aphthæ which so often annoy sucking children, and are not unfrequently a torment to older persons. So minute is this plant, that each little conical elevation of the milk thrush is composed of a *multitude* of these vegetables, each having its leaflets, branches, and sporules. The roots are implanted in the cells of the epithelion, and the spores are not more than the one ten-thousandth of an inch in diameter, or about a third of the diameter, or a ninth of the volume of a blood globule.*

Vogel, in the same year, discovered vegetable *Paras* in the aphthæ, and found their organic covering capable of resisting the action of the water of ammonia and strong acetic acid.

Dr. Berg, a Swedish physician, has since treated this subject more at large, and shown that these aphthous protophytes are propagated not only from mouth to mouth, at the usual temperature of the body, but that

* The nucleolus in the cell germ frequently appears immeasurably small, or even entirely escapes the eye with the highest magnifying power, yet it probably serves as an introduction to the whole formative process. (*Schleiden.*)

they can live, and effect a reproduction out of the body, and at lower temperatures, when placed in contact with substances containing albumen or any nitrogenous compounds. These Paras are supposed by Dr. Berg to be active, even after being dried, and he suggests the idea of their transmissibility in this state through the atmosphere.

Dr. Arthur Farre, of London, read to the Microscopical Society a paper on the minute structure of some cryptogamous vegetable, which escaped in a kind of membranous mass from the bowels of a female who was slightly indisposed before, but who suffered severely for about twelve hours immediately previous to their expulsion. Dr. Farre was not able to refer them to known species, but supposes that the *reproductive spores* may have been swallowed in some beverage, *and become so altered, by receiving supplies from an organized surface*, as to present new and unknown appearances.*

Mr. Goodsir (*Ed. Med. and Surg. Journ.*, vii.) describes curious vegetable organisms developed in the stomach during indigestion.†

Mr. Gruby and Mr. Goodsir, without any concert, at different times and places, detected *transparent nucleated cells* in the glands of Peyer, in a diseased state, from *typhoid fever*. Whether these were animal or vegetable cells could not be determined, but that they were vegetable germs is made probable by the sub-

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* Confervæ, discharged in a case of dysentery, are described by Dr. Bennett.

† More recently, similar instances of this production, termed *sarcina* by Mr. Goodsir, have been noticed in pyrosis, by Mr. Benjamin Bell and Dr. Wilson.

sequent discovery by Schoenlein and Langenback, of *organized vegetable fungi in the body of a person who had died of typhoid fever.*

Hanover detected a species of leoptomitus agardh on the mucous membrane of the mouth and tongue of *two* typhoid patients, and also in the *bladder* of a young child.

Rayer found byssoid vegetations *on the pleura* of a tuberculous patient, and *in the intestinal canal* of a case of pneumothorax.*

In 1838, Boehm published the discovery of *vegetable filaments* on the mucous membrane of the intestines of those *who died of* CHOLERA.

Quevenne and Hanover found the yeast-plant (*torula cerevisiæ*) in diabetic urine.

The frequent action of the fungi in the production of disease is made analogically more probable by observing, also, how many diseases of the lower classes of animals are obviously dependent on the assaults of the cryptogami. Among the earliest observed and most thoroughly studied of these diseases is that of the *muscardine* of the silk-worm. This curious and costly malady was described for the first time in 1835, by Bassi, in Lodi, and M. Balsano, of Milan. Afterwards, in 1836, M. Andouin, who had devoted much time and attention to the subject, published a work on it, and in honor of the first describer gave to this deadly vegetable enemy of the silk-worm the name of *botrytis bassiano.* His statement is to the effect that there is found in *decaying* or *mouldy moss* a very minute fungus, which

* Scherer, cited by Simon, describes, as being found in the peritoneal cavity, after death by puerperal peritonitis, minute cells, *organisms resembling algæ*, granules, and nuclei.

bears very small whitish spores. These, placed near to the silk-worm, attach themselves to its surface, and by some unexplained means gain access to the pigment, under the cuticle, and to the subcutaneous adipose tissue. They are soon converted to the use of the vegetable; and indeed the acute observer of this subject could mark the transformation of the fatty tissue of the worm into radicles of the cryptogamic vegetation. By degrees the plants penetrate from within to the surface, where they have their fructification, and *whiten* it with sporules. Thus created, the germs attach themselves to other worms, and *a contagious disease, of vegetable origin*, devastates the cocoonery of the silk-producer.

The most curious part of this case is the capability of a plant to live at the expense of either another vegetable or of the silk-worm. A singular passage in the oldest book in the world carries this idea even beyond modern discovery, which, as often happens, seems to be rapidly approaching to the truth, as announced three thousand years ago. In the thirteenth and fourteenth chapters of Leviticus, where the subjects of scall and leprosy are discussed, we find the following singular language:—

Chapter xiii.—"The garment also that the plague of leprosy is in, whether it be in the warp or woof of *linen* or *woolen*, whether *in a skin* or anything made of skin; and if the plague be *greenish or reddish* in the garment, it is a plague of leprosy, and shall be showed unto the priest, and the priest shall shut up the plague seven days. If the plague be spread in the garment, the plague is a fretting leprosy. He therefore shall burn that garment.

"If the plague be not *spread* in the garment, then the priest shall command that they wash it, and shut it up seven days, and behold if the plague have not changed its color, it is unclean; and if the plague be somewhat dark after the washing, he shall rend it out of the garment; and if it still appear, it is a spreading plague, and then shall burn that wherein the plague is."

Chapter xiv.—"The priest shall command that they empty the *house*, and he shall look if the plague be *in the walls* of the house, with hollow strakes, *greenish* or *reddish*, which in sight are lower than the walls. Then the priest shall shut up the house seven days, and shall look, and behold if the plague be spread in the walls of the house, then the priest shall command to remove the stones, and he shall cause the house to be scraped within round about, and they shall replace them with new stones, and they shall take other mortar and plaster it. And if the plague come again, and break out in the house, then the priest shall come and look, and behold if the plague be spread in the house, it is a spreading leprosy, and he shall break down the house. * * * * *

"This is the law for all manner of plague of leprosy and scall, and for the *leprosy of a garment and of a house.*"

There is here described a disease whose cause must have been of organic growth, capable of living in the human being and of creating there a foul and painful disease of contagious character, while it could also live and reproduce itself in garments of wool, linen, or skin; nay more, it could attach itself to the walls of a house, and there also effect its own reproduction. Animalcules, always capable of choice, would scarcely be found so transferable; and we are therefore justified in supposing that *green or red fungi, so often seen in epidemic periods,* were the protean disease of man, and his garment, and his house.

Hecker also says, "These spots, (signacula,) and especially the blood spots, (red cryptogami,) were seen at a very early period, as, for instance, in the sixth century; and again during *the plagues* of 786 and 959, when it is said to have been remarked, that those on whose clothes they frequently appeared, and seemingly imparted to them a peculiar odor, were more liable than others to an attack of leprosy. Hence they were named clothes leprosy, (*lepra vestium.*")

Continuing my enumeration of the fungous diseases of animals, I cite Ehrenberg as having detected a vegetation, *chætophora meteorica*, growing on the scales of the *salmo eperlanus*, and creating disease. Henle has found vorticellæ on the toes of Tritons, producing *gangrene* and death. Hanover saw another kind of vegetable, which, accidentally attached to dead flies in damp places, could, by inoculation, be communicated even *to water salamanders.* Dr. Sterling, of Cassel, found similar products on frogs, weakened by other experiments; and Valentine tells us that *Achyla prolyfera*, a kind of mould, very often attacks animals, preventing the development of the ova of fishes, and rapidly extending from an individual to a group.

M. de Longchamps having occasion, in 1840, to dissect an eider duck (anus molissima) while yet warm, found a mould on the mucous surface of its air-tubes. The membrane beneath was diseased, and the spores of the plant were little more than half the size of blood globules. Rosseau and Serruier observed a different kind of mould in pigeons and fowls, as well as in the *cervus axis* and *testudo indica.* In a male parroquet, which died tuberculous, a greenish pulverulent mould was found on a false membrane *between the intestines* and *vertebral column.* Moulds in animals are also deseribed by Müller, Retzius, Mayer, Jæger, Heusinger, Thiele, etc.

A *stryx nictea*, (water fowl,) brought alive from Lapland to Stockholm, died dyspnœal. The lungs and *thoracic cavities* were found to be universally eovered with mushroomlike, flat, rounded bodies of a yellow-white eolor, separable from the mueous membrane without injury to its surfaee.

A *falco rufus*, in the zoologieal eolleetion at Berlin, was examined by Dubois, who found the same white umbilicoid bodies, quite fresh, in the air eavities, and also in *the abdominal cavity near the kidneys.* Müller, Link, Klotzseh, and others, deelared them to be vegetables.

I fear, gentlemen, that I have wearied you by the eitation of so many faets, which, all nearly alike, lose interest by repetition. But, on new ground like this, you must bear with me, if possible; as it is neeessary to show, by many witnesses, that fungi not only obviously produee diseases, but that they must be absorbed and carried into the eireulation, as they are frequently found by the best observers in the world, even in the shut saes of the body.

LECTURE V.

EXPLANATORY CHARACTER OF OUR THEORY—LATENCY—LIMITATION—DRYING—MOULDY SHEETS—YELLOW FEVER—CHOLERA—TROPICAL HEALTH—SUCCESSION OF EPIDEMICS.

A THEORY of malaria should not, in this enlightened age, be received, which does not, at least plausibly, account for the apparent irregularities, seeming contradictions, and anomalous inconsistencies of the subject, which now so greatly obscure all the usual modes of explanation. In this respect I hope to show the very great superiority of that which, I presume, is, by this time, not unfavorably viewed by my hearers. The diffusion of the fungi; their properties as a class; their aknowledged power of producing diseases of a febrile character, marked by periodicity; their nocturnal power and autumnal prevalence; their love of the damp, dark places in which febrile epidemics delight; their companionship with epidemics and epizootics; their obvious association with many cutaneous and some mucous diseases; their production of some contagious diseases of insects; and the progress of diseases from cattle, which are sickened by eating mildewed food, to human beings, sometimes by the use of the flesh, and sometimes, as in the cases reported by Vimat, by the simple exaltation of epidemic influence: all these details, numerous, diversified, and well sustained by authorities, should, I hope, induce my auditors to advance into the subject of the present lecture with at least some partiality for the new doctrine.

No one has yet attempted to explain satisfactorily the cause of the latency of the malarious poison. "The latent residence of narcotic marsh poison in the system," says Stevens, "is incredible." Lind says, that a man may be attacked by fever almost immediately after exposure to its causes, or after a day or two, or even after weeks. Usually the attack occurs within a few days of the time of exposure, and often on the following day. It is not easy to comprehend this, unless we suppose that the poison received into the system, is organic and vital, and that the phenomena of disease depend on its modification, and reactions in the body. In this way we can also understand how such a poison may remain dormant, like some of the animal poisons, and that its absorbed germs may be stimulated not only by time but season, following laws which we are just beginning to study.

This study is, necessarily, very limited as yet, for we are denied a direct examination, and trust often to analogies, feeble sometimes, and at others scarcely perceptible. On this part of the subject, as in one already discussed, we can only examine the effects of visible fungi, when swallowed, and trust to the light thus imperfectly obtained for a farther progress. It is, however, a very curious fact, that, of all the known poisons, that of the fungi lies dormant in the system for the longest time.

One of the greatest peculiarities (Christison) of fungous poison is, *the interval before attack*, and the *difference in that interval.* He endeavors to explain both these phenomena by ascribing them to the difficult solubility of the poisonous matter, surrounded as it is by vegetable pulp and fibre. But, in the splendid work on mushrooms, by M. Paulet, published in 1812, we are told that the extract and alcoholic tincture, and even the juice of the *agaricus bulbosus* and *vernus*, when given to dogs, did not make them sick in less than *ten hours* after their administration.

Christison mentions the poisoning of six persons by the *Hypophyllum sanguineum* or toad-stool (Puddock-stool,) in Scotland, most of whom were attacked, after the lapse of twelve hours, one after twenty hours, one after twenty-four hours, and the last in about *thirty hours.*

Gmelin quotes seventeen cases, which did not exhibit symptoms of toxication until the expiration of *a day and a half* after the meal at which the poison was swallowed.

Corvisart's Journal relates, that of some soldiers, who ate of the *agaricus muscaria*, a part were attacked with gastric symptoms almost immediately, but that others were indisposed only after the lapse of more than six hours, of whom four died.

In the *Histoire des Champignons* of J. Roques, we are told that a dog, fed on a *paté* made of the *agaricus venenatus*, exhibited symptoms of uneasiness only after an interval of *ten hours.* The same author relates cases where longer periods of time were necessary to develop the poisonous effects of the *amanita citrina* and the *agaricus maleficus.*

We see, then, that the poison of the fungi may remain apparently inactive for from an hour or two to even a day and a half, and that, too, when swallowed in large quantity. If we were now to look for any known poison as explanatory of the latency of malaria, should we not be inclined to say, that only that of the fungi exhibited, in this respeet, a strong analogy? We *know* of no other morbifie agent whose aetion is so uniformly and irregularly postponed.

Nothing more startles the student, who has been taught to believe in marsh or other exhalations as being the grand eause of autumnal diseases, than when told, that often a low wall, a eommon road, or a screen of trees, ean and does arrest the progress of marsh miasmata, though the wind from the marsh whistles freely past them, bringing with it even the paludal odor. He is also told by MeCulloeh, the great advoeate of the vegeto-aerial theory, that sometimes agues prevail exelusively on one side of a street, and that *inch by inch, and foot by foot*, the site of the Roman eapital is invaded by malarious diseases. The absurdity and inconsisteney of these various positions strike at the very root of all the old theories. On the other hand, when we suppose that the poison is a fungous one, *progressively marching over the soil*, sustained by the rieh air and pregnant moisture from the marsh, we ean readily suppose that the wall, or the road, or the wood, may limit its progress. Beside this, the spores of all fungi are more or less eleetrieal, and are, therefore, likely to be arrested by the trees of a wood.

Authors have admitted that malaria appears to aet in many instanees as if it eould exert no power, exeept when elose to the spot where it originated, while in othere eases, it seems to be wafted to a great distanee from its apparent souree. If we suppose the existenee of germs suseeptible of reproduetion, and progressive growth, these seeming eontradietions fall at onee. The interruption of progress by a road or wall justifies this view of the mode of eonveyanee, and the many facts which show the narrow limits of the poisonous activity, enforce it strongly. The place, the very spot, where disease is found, must reproduce the cause of it for itself, and if the conditions of growth are not present, then will the spot be exempt, even if very near to the most poisonous places. Thus may we, and only thus, explain the occurrence of agues, yellow fever, and cholera on only one side of a house, or one end of a room, or one side of a street, or wall, or road. A wind may indeed waft the spores in small quantity to a distance, but unless there are there the conditions essential to an adequate reproduction, the spores must lie dormant and harmless. For such reproduction, the marsh mist may be one of the most important elements, but that alone will not suffice, since we know that the disease is not proportional to its frequency or intensity. Other and very local conditions seem to exercise a peculiar power. Thus a new house is known to resist disease better than an old one, and a residence protected by an annual cultivation, immediately around it, is more safe than one which is encircled by lawns in grass. During some unusually sickly years, when scarcely an inhabitant of the skirts of the city escaped marsh fever, the wind set, often for a long period, directly from the infected regions into the heart of the city. In perhaps half a minute from the time when the southwestern air left the meadows and pestilential borders of the town, it had crept into every chamber of the place; yet physicians here, well know that no disease of a malarious character invaded these chambers, which were most of them left open during every night of the sultry autumn.

Writers entitled to credit and authority, by position and professional character, assert that a gauze veil, or a gauze screen in a window, adds much to the security of the wearer or the occupant of a chamber, in even the most unsound places. We can scarcely see how any gas or vapor, simple or compound, could be arrested by such a defence; but it is easy to suppose the detention of organized and comparatively bulky bodies electrical and glutinous, or moist.

However intense may be the virulence of a miasmatic atmosphere, its powers are greatly abated by artificially drying it. Hence, wood-cutters and waterers on the coast of Africa, find it advantageous to kindle a number of fires in the vicinity even of their sultry work. Lind attributed the greater health of the ship Edgar, compared with that of her consort, to the location of her cooking apparatus "between decks." Folchi, a Roman writer, says, "many persons are known to me who have, during many years, preserved themselves from fever, in the worst parts of the country around Rome, by adopting the most rigid caution in retiring within their houses before evening, closing the windows, *warming the rooms*, and taking care not to go out in the morning until the sun has been some time above the horizon." Old John Kaye speaks of the exemption of cooks and smiths from the sweating sickness. (*Sudor. Angl.*) There is no other poison, save that of the fungi, so far as we know, which is thus disarmed by dryness and heat. In any view of the case, the fact is inexplicable unless we suppose an organic cause, to which the absence of humidity is antagonistic.

Immemorially, the sleeping in damp sheets has been thought hazardous to health; but the keepers of hotels and boarding-houses know that the danger is very slight, unless the sheets have been put away in a damp state, and have acquired a *mouldy* smell. The constant practice of the hydropathists shows the little hazard of a wet sheet, while daily experience demonstrates the certainty of at least stiffened and painful muscles, and an arrest of the Schneiderian secretions, after spending an hour or two between damp and musty bedclothes. The Scottish Highlanders are said to dip themselves, dress and all, into the sea, when obliged to sleep out of doors, after being drenched by rain. As water is supposed to act unfavorably by means of its coldness, we cannot easily explain the known benefit of this substitution, except by a reference to the acknowledged power of salt to prevent the growth of fungi.

It may seem rather curiously nice to notice another point connected with this part of our subject; but as you are all students now, and will I hope become true scholars hereafter, I will observe, that every one who searches for knowledge among old books and manuscripts, has been occasionally attacked by sternutation, and at least a temporary coryza, when he has disturbed the dust which has long slumbered within their leaves. As the dust of a room swept daily, and the pulverulent clouds of a summer road do not so affect him, he seizes his microscope and detects the cause of his sufferings, in the numerous organic spores which have grown into power to torment, among the dampness and darkness of the leafy envelopes.*

We can scarcely doubt the events recorded by Lind, Rush, Webster, Hosack, and others, of the partial intro-

* My distinguished friend, Professor Hare, finds this experiment among his old papers, even a hazardous one, as it always seriously affects his health.

duction of yellow fever into places always otherwise exempted from it, by trunks of unwashed clothes, brought from infected regions. Boerhaave, Cullen, Lind and Russell think fomites, which are soiled and placed in a confined depository, are more to be dreaded than the excretions of the sick.

Hosack asserts, that the virus is, under such circumstances, *augmented in quantity*.

Hecker, to whose opinions I have already referred, holds that *fomites may even aggravate the infectious powers of a virus.*

Doctor Rush mentions one trunk case in detail, and says that he heard of two other instances, in all of which only those suffered who opened the packages. According to William Stevens, of Santa Cruz, "*The poison is made more intense by being confined in clothes and bedding.*"

In 1747, the trunk of a young supercargo who died at Barbadoes, was opened in Philadelphia in the presence of Mr. Powell, Mr. Hatton, three Welshmen, a cooper and a boy of Mr. Powell's; all sickened and died of yellow fever within a few days.

"I have seen the cases of some servants in Mr. O.'s family, attacked by yellow fever, upon receiving the clothing of a relative who had died of that disease in the West Indies, at a time, too, when no yellow fever prevailed in New York." (*Hosack.*)

On the same authority, we learn that, after the death by yellow fever of the late Gardiner Baker, while on a visit to Boston, where it prevailed epidemically, his clothes were sent home to his wife, then a resident of Long Island. The opening of the trunk was followed by yellow fever, of which Mrs. Baker died. No disease of the kind existed at that time in New York or its vicinity.

A recent report to the Legislature of New York on the subject of Quarantine, contains unanswerable facts of this kind, both numerous and well authenticated. Were yellow fever a contagious disease, these examples of propagation by fomites might be easily explained; but as its non-contagiousness is clearly shown, by even stronger testimony than that above cited in favor of introduction by fomites, we are left to explain the difficulty, as best we may, consistently with a belief in its importation by trunks and clothes, and a thorough conviction of its total want of contagious power. There is left but one escape, and that lies in the supposition that fungi, when lodged in the trunks among filth and animal matter, find in darkness and dampness the fittest imaginable growing place. That in scarcely any of these cases the disease advanced beyond those who inspected or handled the clothes, is only proof of the usual difficulty of sowing successfully tropical seeds in temperate climates, and of the inaptitude of fungi to grow under any but the nicely adjusted conditions upon which many of the tribe rely. Were I disposed to support further the opinion just defended, I might cite Dr. John Bard, of New York, Dr. Lining, of Charleston, the late Dr. John C. Otto, Drs. Bond, Cadwallader, and Graham, of the last century, Dr. Holt, of New Orleans, Dr. W. S. W. Ruschenberger, Dr. Joseph Bailey, Dr. Westerveldt, Dr. Vaché, and a host of others of the present day for examples of propagation by trunks and clothes.

Of a similar character is the question of the importation of yellow fever in ships. From the angrily mooted case of the Hanckey, in 1793, by which the yellow fever was brought from Africa to the Island of Granada, to that of the Éclair steamer, which, in 1845, carried it from the same coast to Buena Vista, and even to England, there has been a tempestuous dispute about importation and contagion. The contagionists point to the Bann, at Ascension, and even at Bahia, and to the Buck, at Bristol, a high and healthy village on the Delaware, and to the Vanda at the usually salubrious town of Roundout, one hundred miles up the North River, as evidence of importation, and, *of course*, of *contagion.* They can go even farther, and show that there are at least eighty recorded examples of the production of yellow fever in unusual places by vessels which came from its ordinary *habitat.*

On the other hand, physicians very generally reject the doctrine of its contagiousness, because it is not carried about by infected persons, because its victims, however much crowded together in a hospital which is removed to a short distance from the infected spot, do not produce it in those who visit or nurse them, or sleep with them at night. Persons thus habitually exposed, show their susceptibility, by suffering an attack by visiting, even for a few minutes, only the open *streets* of the morbific place. This objection is so strong as to throw the contagionists into all kinds of devices to defend their untenable position; such as conditional contagion, contingent contagion, concurrent local causes, *tertium quids,* between the imported and local agents, all of which, entirely hypothetical, depend for existence, even in the minds of their expounders, upon the first assumption, the contagion of yellow fever; an assumption which owes its acceptability solely to the fact of importation in ships, and propagation by fomites, together with the hitherto insuperable difficulty of giving to it a different explanation. "There is our position!" say they to their opponents; "destroy it if you can!" The opponents are reduced to the necessity of giving to numerous well attested phenomena a flat denial. The anti-contagionists, on the other hand, point to the dispersing invalids of a pestilential city, and ask, why they carry not disease to the country. They exult in the immunity of the hospitals, and, in their turn, inquire with confidence, "Where is your contagion?" They are answered by subtleties, and suppositions, and hypotheses. Is not all this very contrary to the true spirit of philosophy? Would it not be better to admit that yellow fever is often imported in ships, is now and then carried in trunks, and may possibly be sometimes an accident of the locality? Might it not be also said, that we know of no contagious disease which presents any analogy to the contingent contagion claimed for yellow fever, and that, therefore, we must for the present suppose that *it is portable* and yet *is not contagious?*

If I have made a good footing for the fungi, as producers of diseases very like to yellow fever, I may be indulged in *my* hypothesis, which alleges that a tropical fungus, carried off in dark, damp, animalized holds of ships, or in the offensive clothes of sick or dead seamen, may be introduced into the summer-clime of unaccustomed places, and there, as it came from, may go to the shore, and be sometimes reproductive. May I not suppose that the germs, when once ashore, may slowly migrate landward, and even by chance be carried or wafted to other neighboring spots, where they may grow, and create new *foci* of disease? that the requirements of an exotic may make such visitations rare, and such dispersions unusual? and that the equatorial plants may be nipped, and even totally destroyed by an unaccustomed frost?

Through this theory of ours, we can easily see why the disease may be imported, why it is imported rarely, and why it makes so slow a progress from the spot to which originally brought. It will, also, explain its non-contagious character, and even its occasional but rare visit to a village or hamlet. It may also account for its apparently spontaneous appearance in such places as Charleston, Savannah, and New Orleans, in which the winter may not be severe enough to kill the germs, but yet may so affect them as to make their reaction difficult or partial.

It is only thus that we can comprehend how a *perfectly healthy crew* may bring with them, in the closed hold of their ship, the germs of disease, which, after their dismissal, may pestilentially affect the "stevedores" who discharge her, or only the laborers *who disturb her ballast.*

We can thus, too, explain the *usual cause* between the first set of cases caught by visitors to, or laborers on board, the ship, and the attack upon the inhabitants of the vicinity. This curious interval, noticed by almost every writer, occupies about ten or fifteen days, while the period of incubation, after exposure to a known source of infection, is only about five days. (*Vaché.*)

This interval is only to be explained by the supposition that germs, of some kind, have gained a footing on shore, and have germinated and grown more numerous. *It is the crop in the hold which produces the first set of cases. It is the crop on the land which causes the second.*

It is only through the action of some organic cause that we can explain the tenacity of the attachment of yellow fever to certain ships, and these, too, among the cleanest and best-aired vessels in the British service. The Sybille had three several epidemic attacks between the 23d of June, 1829, and the middle of April, 1830. Two of these occurred while at sea. In the West India service, certain ships have usually an outbreak on going into even a healthy harbor.

Perhaps no disease has so much puzzled the etiologist as cholera. Its singular local origin, its yet more singular progress, its apparent inconsistencies, its diffusion from a tropical point over the habitable globe, and especially its invasion, in winter, of the frozen steppes of Tartary and Russia, all tend to confuse the observer of epidemics. At one time, slowly, against the monsoon, it advances on a long geographical line, at the rate of from one to two miles a day, while at another, it flies on the wings of commerce, almost as fast as there are means of conveyance for men and merchandise. At one time, it ascends or descends along the valley of an innavigable stream, slowly and regularly, as if progressive by its own locomotion; at another, it flies with the ship or the locomotive, across seas and continents. A stranded vessel throws it upon the shore of a lonely sea-island. (*Dickson.*) One ship conveys it from Dublin to the St. Lawrence,* another meets it *in the midst of the Atlantic*, and carries it to New York,† while a third, from the same source,‡ deposits it at New Orleans. Steamers

* The Carricks. † The packet-ship New York.

‡ The ship Swanton, Captain Duncan, from the healthy port of Havre, was assailed by cholera after being at sea for twenty-eight days, (lat. 25° N., long. 57° W.,) and after losing fifteen persons

scatter it far and wide as they ascend from New Orleans to the various branches of the river above. Contagion might explain the progress, where there are always materials to form a line of march, but contagion cannot account for its solitary advance over untraveled wastes or untenanted seas. Contagion cannot explain its presence in the atmosphere of the mid-ocean, nor its manner of assailing a city at once, at its most opposite points. Contagion is at fault as explanatory of the *exemption of classes*, the almost exclusive invasion of low, damp, dirty habitations, and the uniform appearance of a general premonitory state, before the irruption of the cholera itself.

The attacks of cholera within a few hours after exposure to infection; the introduction into hospitals of large numbers of cholera patients, while the old inmates enjoyed complete immunity, as at the Odinka, at St. Petersburg; the diseased condition of a single vessel, the Dreadnaught, in the Thames, in 1837; the great exemption of physicians and nurses; the attack of the old rather than of the young, or of those at puberty, all militate against the notion of a propagation by contagion.

On the other hand, many cases are cited where the cholera came with bodies of men, caravans, and ships, and seemed to be propagated by personal communication. At one time it confined itself to one wing of an

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in thirteen days, she arrived in the Mississippi, five days before the epidemic outbreak at New Orleans.

The ship New York, also from Havre, was attacked at sea, sixteen days out, and arrived at Staten Island two days before the cholera appeared at the New York Quarantine Station. (*Whiting.*)

army; at another, it spread progressively from left to right, along the line of encampment. Sometimes it affected but one out of thirty men in each of a great number of large tents, and sometimes it restricted itself to one or two such tents, which it completely desolated. No wonder that men were puzzled and perplexed, being contagionists at one time and place, and anti-contagionists at another. No wonder that Mojon and Holland should have endeavored to avoid the difficulty by reverting to the exploded doctrine of Kircher and Linnæus, the animalcular theory of disease.

The animalcular, being an organic theory, would explain well enough the phenomena of progress, were it not for the apparent absurdity of supposing that animalculæ of tropical origin could exist and procreate in a Russian winter. The want of proof that animalculæ are poisonous, or that they fulfill the conditions for such a theory, has been already stated.

But if we assume for cholera a fungous origin, all difficulties vanish; and, as in the case of yellow fever, an easy explanation may be given of every apparent incongruity. We have only to suppose, what is known to happen in other cases, that the fungi, on which the cholera is assumed to depend, acquire at times, as do the germs of some contagious diseases, an unusual power of reproduction and diffusion, a greater potency of expansion. Such germs may be carried by men, and goods, and ships, or may make a slower progress by their own unaided activity, or be scattered by the winds, to regerminate wherever special conditions are found. Thus can we see why the poison prefers the route of streams or infests the damp parts of cities; and why classes living in clean apartments in dry districts suffer so little.

We can see why women escape better than men; why both cholera and yellow fever, by the natural tendency of the vegetable cause to the organs of generation, almost always cause miscarriage of pregnant women; and why, when a city or country is unhealthy, the fungiferous causes of death, by over-stimulating the organs of reproduction, usually make a compensation by the births, for the unusual mortality.

Can we not thus explain the appearance of contagion where there is no contagion, and the absence of contagion while there is an obvious conveyance of the epidemic poison from place to place?

We are no longer surprised to learn that cholera advanced regularly from the tent nearest to the water, to the others successively, until it reached the end of the lines; nor do we feel astonished that it was, in another case, confined to the tent nearest the tank, or to the flank company, or the brigade on the left or right of the army. We now see why ninety men detached from a large corps, and attacked on the first night of absence, on the borders of a lake, were, without damage to the corps, promiscuously mingled again with it, after being brought back, totally disabled, to the original encampment. We can understand now, how, in the Odinka Hospital, whose salubrity was previously proved by the absence of cholera during an epidemic at St. Petersburg, its eight hundred inmates continued in their usual health, despite the introduction from without of five hundred cases of cholera. We can see how a corps, in its march through an irregularly infected country, may acquire and lose the cholera several times; how a healthy corps may enter a sickly army, *en route*, and not suffer from the prevailing malady. The diffusion, the limitation, the leaving the infection behind, or the carrying it forward, all admit of an easy explanation, if we assume the hypothesis that germs or spores, created exteriorly to the body, are the *seminia morbi*, and that they are liable to the usual accidents by which seeds are conveyed or lost, or favored or repressed.

It would now weary you, my young friends, were I to carry you over the same twice-trodden ground, in an endeavor to apply to the phenomena of the origination and propagation of THE PLAGUE the same explanatory theory. It fits it quite as well, nay, in some respects even more perfectly than it does the etiology of cholera and yellow fever, but, after what has been said, you can yourselves make the application.

In pursuit of our task of explanation, I am bound to give a reason for the extraordinary exemption of Brazil, New Holland, and the Polynesian Islands, from malarious diseases. They are volcanic, or organic, or alluvial. They have rank vegetation, and heat and moisture, as demanded by McCulloch, and sulphur-products as called for by Daniel and Gardiner, and a soil in process of drying after being wet, as suggested by Ferguson. They have the exuberance of vegeto-organic life of Armstrong and Doughty, and yet they are not infested by malarious diseases. Not a shadow of explanation do any of these hypotheses offer of this anomaly. But if we assume the fungous theory as a basis of explanation, we may readily believe, nay, *certainly might know*, that such exceptions are, on the doctrine of chances, to be expected. No plant is everywhere, and such plants as are here alluded to are especially capricious in habits and actions, according to causes which, though yet unstudied, obviously control them. On our theory, *the* *occasional exception should be looked for;* on any general chemical, or mechanical, or atmospheric theory, *it is inexplicable.* Under such a view, we are not astonished at finding Brazil healthy and Africa pestilential; for their obvious, much more their minute, vegetation is so dissimilar as to render a difference in their invisible phytology highly probable.

These considerations naturally lead us to inquire why the febrile diseases of various countries differ so much. Why have we no yellow fever in Brazil, or India, or Egypt, and why no plague in Florida or Calcutta? It is for the reason that, though of the same great general class, the fungi differ greatly from each other in special properties, and that the protophytes of each country, although many of them are nearly alike, present, some of them, almost contrasted properties. The *agaricus clypeatus* of the west of Europe poisons in one way, the *amanita muscaria* of Siberia in another. One irritates, the other intoxicates. So, a certain kind of *mucor* produces dysentery, another typhoid symptoms, and a third excessive vomiting. The ergot of rye excites formication, fever, and sphacelation; the ergot of maize, fever, loss of hair and nails. Is it then a matter of special wonder, if a fungus with one set of properties should germinate in India, another in Egypt, and a third in Cuba?

Nor should we be astonished at finding a surprising fecundity at certain times in certain classes of plants, by means of which they not only multiply prodigiously on their customary soil, but readily advance beyond their wonted boundaries. In this way I may explain the ravages of the plague in Europe, and of the yellow fever in North America and Spain; and account for the intrusion of cholera upon European ground, and its failure to maintain its conquests for any prolonged period of time. The plague retreats back to the Nile, Euphrates,* and Danube, its native home; the cholera withdraws to Hindostan, and the yellow fever to the southern coast of America and to the West Indies. It is twenty-six years since yellow fever visited Philadelphia. During that time, there have been many seasons of as great heat and drought as in 1793 and 1798, and, every year, arrive at our wharves vessels from infected ports; but the germs of disease do not bear transportation always, and our fungiferous tendencies at home have not invited a visit. Long may it so be in both respects!

To speak of quarantine regulations does not come properly within the scope of my subject, but the importance of the question may perhaps excuse me for the suggestion that, on the principles here laid down, the detention at quarantine even of the sick, is, for yellow fever and cholera, unnecessary; while the importance of detaining and purifying cargoes and soiled baggage becomes apparently more imperative.

I alluded, in the last paragraph, to the fungiferous tendencies at home, by which may be invited from abroad an exotic *fungus*. This idea affords an explanation of a fact universally noticed, but not easily other-

* At Erzeroum, the capital of Armenia, the winters are cold, the thermometer rarely rising above 32° F., and descending often as low as 25° or even 20°. In summer the heat has a range of from 66° to 81·5°; yet this place and its adjacent villages seem to generate the plague. It appeared there in 1840, about the middle of August, and in 1841, in the beginning of July.

wise explained,—I mean the growth of various diseases of a common character, before the irruption of a great pestilence. If these depend upon a fungous origin, their growth will be augmented by the augmentation of their cause, until the foreign intruder, urged by a new and inherent impulse, and welcomed by a domestic facilitation, enters upon a career of desolation. The fungiferous exaltation is shown by the early ripening and imperfect maturation of fruits and even roots, whose organs of reproduction are, by invisible ergots, over-stimulated. The decay of roots and fruits, the tainting of meats, and the moulding of other things, are but parts of the unwholesome "cryptogamism" which, at length, intrudes upon living things; when murrain among cattle, and pestilence among men, complete the history of a calamitous period.

Similar principles seem to govern the movements of diseases now generally acknowledged to proceed from germs. The contagious maladies—smallpox, measles, scarlatina, and hooping-cough—are almost always present in some part of a great metropolis, or at least in some part of a great country; yet their tendency to propagation is often, for years, so slight as to confine their ravages to a small number of victims. But at times, and sometimes after long intervals of comparative inactivity, these affections suddenly acquire a wondrous expansibility. Their germs are scattered far and wide. The slightest exposure brings on disease, and where but a few individuals suffered, thousands are attacked. A careful examination of the meteorological conditions affords no shadow of explanation. At all temperatures, in every variety of humidity, beneath every kind of skyey influence, these diseases become epidemic. Time seems to have for them some kind of bonds, for they seldom continue epidemic long, and do not usually return as such for a lapse of years. According to Humboldt, small-pox becomes epidemic in South America about once in from fifteen to twenty years, and that sometimes without a known re-introduction. These outbreaks seem to depend rather on germinal power than extrinsic enforcement, and remind one of the locusts, which, though every year present in small numbers, appear by myriads at periods of from seven to seventeen years. As the larvæ of these insects lie deeply buried in the earth, beyond the reach of anything but the mean annual moisture and temperature, which are but slightly varied, we have yet to learn what spell it is which calls them, in countless throngs, into active existence.

The plague-spell has not darkened the portals of Christian Europe for more than one hundred years, and the *sudor anglicanus* has not floated on its fetid mists, since the House of Tudor resigned to the Stuarts the throne of England. But these genii of a former age are but asleep. Their time is not yet. When they shall again recover their germinal vigor, and pass beyond their wonted limits, or awake from their long repose, they will retain probably, as before, their new activity or more extended dominion, for a series of years. It is true that a happier age, in comfort and cleanliness, and medical knowledge, has arrived to check their progress, and to limit their deadliness; but it is vain to hope that any disease has been entirely eradicated, or any germ totally lost. In a few years the cholera will, according to pestilential usage, retire to its old limits, and there perhaps seem to expire, until forgotten and contemned, it will, after a long repose, burst again over the fields of India, and the realms of Europe and America.*

LECTURE VI.

EXPLANATORY CHARACTER OF OUR THEORY: CONTRAST OF THE HEALTH OF SEASONS AND PLACES—SUDDEN ONSETS IN AFRICA—THE MAREMMA: VOLCANIC ERUPTIONS—SPUR TO VEGETATION—REVOLUTIONS IN LOCAL HEALTH—FAIRY RINGS—NON-RECURRENCE OF SOME DISEASES—LIEBIG'S THEORY—EPIDEMIC MOST FATAL AT ITS ONSET—DRY SANDY PLAINS SICKLY—RECAPITULATION.

Independently of any observable cause, the crops of various kinds differ in a remarkable manner in different seasons. Most of you must have seen the wonderful production of the fruits of all kinds in certain autumns. A year or two since, the trees actually bent down and broke under the immense load of apples, which were left to rot in the fields in many places, for want of the means of securing them. No cause for this

* Since this paragraph was written the cholera *has* returned to Europe and America, unchanged in character, and unmodified in severity. Again it haunts damp rural places and offensive urban localities. Again it selects its victims from among the poor, who are destitute of the opportunity of defending themselves from the circumambiency of the provocatives of infection. Filth, dampness, and innutrition; fatigue, bad habits, and neglect of premonitions, doom these unfortunates to the superadded evils of pestilence, torture, and death.

exuberance was observable. Farmers sometimes have good crops even in opposition to the inclemency of the season, and as often, under the most auspicious meteorology, are chagrined at the unaccountable shriveling, or paucity of their grain. So is it with the fungi, which, in opposition to hostile meteoration, spring up in unusual places, or abound prodigiously in customary positions. Thus in 1798, a year of protracted heat and drought, Condie and Folwell reported, as remarkable, the abundant production of various classes of mushrooms. So were there unaccountable moulds and mildews, in the driest periods of the pestilential years, in New York, Philadelphia, and Natchez. Sometimes but one kind of germ is stimulated, as in the case of the apples already cited; sometimes many are excited, as in some years of great and general "pomonal" luxuriancy. So is it with the fungi, as manifested by the extension of only one disease, or the coexistence of many. Of all plants, the cryptogami are the most capricious, or most susceptible of modification by unseen causes. Hence the quality of the season is scarcely ever an index to the morbid condition of any particular year, although heat, moisture, and a redundant vegetation are general precursors of malarious action.

We can, on our hypothesis, easily explain the arrival of the annual morbid orgasm, *after* the rains of one country, and *in* the rains of another. Whether hot or cool, wet or dry, the sickly season is the harvest time of the fungi, which lie tied by time and not by circumstance, until their customary period of activity has arrived, when, more or less stimulated by moisture, and food, and electricity, they show a feeble or a strong fecundity.

On our supposition alone, can we account for the sudden effect in Africa of the first rains. The dry season bakes the earth to a crust. The lesser vegetation is dried up under the scorching glare of a tropical sun, and nature seems almost at a stand. That is there the season of health. But the rains commence, and almost in a moment arises a morbid influence inexplicable by reference either to heat or moisture, or any ordinary decomposition. "The rain had scarcely commenced," says Mungo Park, "before many of the soldiers were affected with vomiting. Others fell asleep, and seemed *as if intoxicated.* I felt a strong inclination to sleep during the storm, and as soon as it was over, I fell asleep on the wet ground, although I used every exertion to keep myself awake. Twelve of the soldiers were ill *next day.*" Only some of the fungi, whose rapidity of growth is wonderful, and whose power of causing vomiting, drowsiness, and intoxication is acknowledged, can be plausibly brought to explain the phenomena described by Park. The very sudden production of excessive mould on everything, so as to rot to its centre, in forty-eight hours, a piece of cloth or leather, evinced the fungiferous force of the African rainy season. Moisture and heat alone could not produce such effects, for in Brazil no such phenomena are observed or recorded, although the rains are as heavy and the temperature even a little higher.

Contrasted with Africa, is a spot almost as unhealthy as "The Coast." While the latter is low, wet, marshy, and filled with the rankest vegetation, the Maremma of Tuscany and the Roman States is high, dry, free from perceptible moisture, and used chiefly as pasture-grounds, which are in no respect unusually fertile or productive.

Yet the Maremma, throughout its extended domain of nearly one hundred miles in length, is scourged by the most intense forms of malarious fevers. The Campagna di Roma, so celebrated for its pernicious fevers, is included in the Maremma.

This apparent deviation from the healthfulness which should pertain to a country so dry and so free from marshes and streams, has always presented to the miasmatists an especial stumbling-block; and a clever writer seems to think that a general malarious theory cannot be accredited by the profession, which will not explain satisfactorily the cause of the unexpected insalubrity of the Maremma.

The surface of the Maremma is formed throughout of volcanic tufa, which, when sufficiently softened, forms a pasturage on which feed large herds of cattle. It contains the finest pastures of Italy, on the soil of which are commingled the ordure of cattle and the disintegrated tufa. The former is known to be a favorite growing-ground of the fungi, and the latter, I shall now proceed to show, is even better calculated for the same offices.

According to M. Roques, the fine mushroom, *polyporus tuberaster*, of the Italians, grows in the environs of Naples, upon a species of volcanic tufa, very porous and of an argillo-calcareous nature. In the pores of this stone is deposited the *matrix* of the plant, from which, when moistened and shaded, grow up vast mushrooms, four or five inches high, and eight or ten inches broad. These stones are sent to France and England, where they are used as in Italy, for the production of mushrooms. The English philosopher, Boyle, first described this stone, under the title of *Lapis Lyncurias;* "which," to use his own language, "rubbed, moistened, and warmed, will in a very short time produce mushrooms fit to be eaten." Old John Hill, who wrote, a century ago, a volume on Materia Medica, published a book entitled *Lapis Fungifer*, in which he describes a stone of this kind in the possession of Lady Stafford. It was a hard, heavy mass, of an irregular shape, and granulated texture, like shagreen leather. This formed the *nidus* for the perennial root of a fungus superior to common mushrooms. One of these fungi weighed, according to Hill, two pounds two ounces, and measured six and a half inches on the head. The doctor presumes that the *Lapis Violaceus* of the Germans is of a similar nature.

The Neapolitans bring the tufa used in their horticultural processes from Calabria, where are found the samples of that volcanic earth of the finest quality. It is placed for cryptogamous purposes in shaded excavations, or in natural caves, or in cellars, where, by its means, are produced vast quantities of the best mushrooms.

In the Maremma, where the volcanic tufa is the basis of the soil, the surface is intermixed with the animal remains of departed empires and the ordure of cattle, is covered with grasses of old pasturages, and is wet with heavy dews. Everything, therefore, conspires there to a fungiferous end. The tufa is fungiferous, the manure is fungiferous, old pastures are always fungiferous, and the dews of the Maremma not only make night fungiferously hideous, but, by their chilly humectation, act as excitants of the train of nervous symptoms, and, as does driving the cattle in the milk-sickness, they bring on an attack, which, but for this element of the *suite*, might have been escaped. Instead, therefore, of being surprised at the ascendency of malarious diseases in the

Maremma, we should feel at a loss for a mode of explaining any want there of a miasmatic predominancy.

The fungiferous productiveness of the volcanic soil of Italy, is shown by reference to the report of Professor Sanguinetti, Official Inspector of the Fungi at Rome. Not having access to the original, I quote from Dr. Badham's beautiful work on "The Esculent Funguses of England." "For forty days *in autumn*, and for about half that period *every spring*, large quantities of funguses, picked in the immediate vicinity of Rome, from Frascati, Rocca di Papa, and Albano, are brought in at the different gates.

"The return of taxed mushrooms in the City of Rome gives a yearly average of between 60,000 and 80,000 pounds weight, and if we double this amount, as we may safely do, in order to include the smaller untaxed parcels, the commercial value is upwards of £2000 sterling, ($10,000.) But the fresh funguses form only a small part of the whole consumption, to which must be added the dried, the pickled, and the preserved."

Thus about 140,000 pounds of mushrooms are *sold* in Rome, a weight equal to that of 175 oxen.

A reference to the fungiferous power of the tufas enables us to explain a hitherto most puzzling fact, as recorded by many authors, and as specifically treated by writers on epidemics. It is remarked by Webster and Hecker, as well as by other writers, that volcanic eruptions and earthquakes, when productive of disease, do not cause it immediately, nor even in the current year, but usually in that which follows it. If mephitic vapors or gases were the cause of the epidemics in such cases, immediate consequences should ensue; but if the volcanic ashes, or the sulphur and calcareous products, excite the disease by evoking excessively the common cryptogamic growths, or exciting into action, the long slumbering spores of new or unusual protophytes, we ought to find their record in the morbid history of the succeeding year or years. So we learn that the year 79 of our era, was marked *by no unusual mortality*, although Vesuvius darkned, by its ejected ashes, the sun itself, and scattered its products through the atmosphere even to Syria and Africa. Herculaneum and Pompeii were so deeply buried as to be lost for nearly 1700 years, and the soil of Italy, from the Alps to Sicily, was dusted with the furnace-formed products of the volcano. But in the following year, when the now acknowledged fungiferous properties of the tufous ashes could exert on the soil their stimulating influence, disease desolated Italy, and a plague raged with resistless power. That fatal epidemic destroyed daily, for a prolonged season, 10,000 inhabitants of Rome. (*Webster.*)

One other difficulty remains to be removed, and I shall then, gentlemen, leave this subject for your future consideration, and, if worthy of it, your future investigation. Writers on malaria not unfrequently complain of the unaccountable irregularity of miasmatic action. Attributing, as they usually do, the diseases of the autumn to vegetable or other decomposition, they are disturbed by finding not the slightest relation between the supposed cause and the alleged effect. Heat, moisture, and vegetation, being the concurrent elements of their theory, some proportion should be observed between the amount of these, and the intensity or diffusiveness of malaria. But, alas for the speculation, disease sometimes most abounds in seasons remarkable for the negation of the alleged causes. Cool years are healthy, cool years are sickly. Dry years are salubrious, dry years are lethal.* Wet years present the extremes of health and sickness, and years of a mixed character have been in the *plus* and *minus* of the scale of salubrity. Only one element seems to make any approach to a constant relation to the state of health, and that is, *a tendency to excess of vegetable life.* In the autumn of fertile years, there is often the greatest mortality.

Can this arise from the decomposition of the vegetation of that year, which has just been completed? Does the vegetation submit, in the open air, to so rapid a change as that which is to be admitted, to rationally entertain the malarious theory, as usually received? I think not, and further, the occurrence of severe malarious diseases in barren places, on rocky heights, and sandy plains, shows that we may more rationally attribute the diseases of fertile seasons, rather to the spur given to the general vegetation, which is also communicated to the *cryptogamia*, than to a decomposition, which remains without proof, and which, when obviously most active, fails to excite disease.

As the fungi grow at the end of the phenogamous season, their production depends on causes which may or may not have been felt by the common vegetation. Hence, disease seems in this aspect of inscrutable origination, unless we look exclusively to the causes which may excite vegetation throughout a season, or only in the spring and summer, or only in the autumn.

There is a kind of corollary to the last proposition.

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* "For at Newtown, Long Island, and in most parts of this island, these diseases have existed in seasons of the greatest drought." (*Hosack.*)

Places of malarious character often become, at least for a time, quite salubrious, and places which have for a long course of time been healthy, unexpectedly and without apparent alterations, acquire morbid conditions. The streams run at their mean height, the pools are filled to their common capacity, the vegetation seems to follow its wonted course; but the health varies according to unseen influences, for all visible and measurable events move in a customary round. These diversities of salubrity are unexplained by the geology, the agriculture, the climate and the meteorology, which, remaining the same, or moving in defined and customary cycles of obvious similitude, leave no evidence of having any effect on the morbid irregularities. It would seem as if the unknown cause were migratory, or had long fits of irregular repose. Now, we *know* of nothing which possesses an acknowledged power of creating febrile diseases, by which such irregularity can be explained, save by reference to the habits of the fungi.

The cryptogami have, in a high degree, the curious property of destroying their own reproductive powers, or of poisoning against themselves the soil in which they grow. The *lapis fungifer* or volcanic tufa, if actively employed, loses, in about three or four years, its power of production, which is only required by a repose of several seasons. To this peculiarity is now ascribed the production of what are in Europe called *Fairy Rings*. These curious denuded circles, amid the vivid green of an English common, were once attributed to the tiny feet of fairies, who were supposed to make the spots, so marked, their place of revelry. Subsequently they were thought to be the effect of electrical action. Now they are known to be produced by the eccentric growth of various kinds of fungi, and might, therefore, be properly termed the vegetable ring-worms of the fields, or rather the ring-plants of the commons. Commencing, as do the ring-worms, at a spot, these fungi move progressively outward, leaving a bare unvegetating space behind them, upon which neither fungi nor grass will grow for a time. Finally, the grass returns, and filling up the centre, follows the protophytes, so as to produce a broad circular belt of scorched earth, which grows more and more in diameter. The fungi, evolved only on the outer edge of the belt, do not again attack the centre, in which the soil appears to have lost its power of sustaining them.

Most persons attribute this fact to the probable exhaustion from the soil of some special element necessary to the growth of these fungi. That this view is erroneous, may be inferred from the observed decay of the fungi on the spot where they grow, by which the elements of their composition are restored to the soil at once.* Besides, if such elements were removed and not thus restored, it is not easy to see, how that soil could ever regain them by repose. But if we suppose the deposit of poisonous *exuviæ* in the soil, by these plants, we can understand how time, reactions, and soaking rains, may remove them, and again permit a reproduction, where for a time it is prevented. A curious exemplification of the poisoning of the soil against their own growth is afforded by the fungi which have so lately preyed on the potato crop. In Ireland the potatoes grow much better in the subsequent year, when the dis-

* The ploughing in of crops of clover is one of the best expedients for the enrichment of the soil. Land is impoverished only by removing its products.

eased potatoes have been left to rot in the soil, than when they are carefully removed.

We have other analogies for this idea. Macaire, who has given much scientific attention to the effect of plants upon soils, observes, that certain vegetables enrich the earth by their *exuviæ*, as, for example, the leguminous vegetables excrete much mucilage, and thus fertilize it for the *gramineæ*, but that the *papaveraceæ* injure the soil by the deposit of opiatelike substances, and thus prevent or render growths imperfect. So is it with the peach and bitter-almond trees, which, as well as other plants that produce prussic acid and the poisonous hydrocyanates, render the soil in which they grow incapable of successive crops of the same kind of trees. A nursery in which young peach-trees have been planted, and from which they have been soon removed, will not sustain the same kind of stock for eight or ten years afterwards.* Nature thus secures a variety, by a succession of dissimilar vegetations. I might multiply examples; but these are enough for our present purpose. In this way are the fairy rings formed, and in this way are the grasses protected from the endless destructive ravages of their enemies, as is the human body from the recurrence of violent diseases.

This view may explain the gradual extinction, or unexpected reappearance of trees, shrubs, and flowers. The prim, a New England hedge-bush, began to fail, according to Webster, in 1775, and finally perished. In 1664 commenced the mildew in wheat, in New England, which

* Manuring the soil from which a peach-tree has been removed does not mend the matter; removal of the soil, or long repose, will alone suffice.

long rendered it impossible to cultivate that grain on the Atlantic coast of three Eastern States. So have the Morillo cherry-tree, the buttonwood-tree, the linden, and some others, begun to decay, some in one way, some in another. The peach-tree is unhappily dying off in New Jersey, so that, perhaps, in a few years, we may have to look exclusively to the South for that delicious fruit.

Of all the known vegetable productions, the fungi appear to have the greatest variety of abating and destroying conditions. They poison their own soil, they depend for luxuriancy on nice contingencies, they are the food of many insects, who eat them up spores and stems, while they prey voraciously on one another, fungus being superimposed on fungus, in an almost indefinite series.

Thus, then, may we not improbably account for the occasional disappearance and reproduction of malarious diseases in malarious situations.

The obstruction to their own reproduction on the part of fungous vegetables may be—I speak it with great hesitation—the cause of the non-recurrence of certain violent diseases, such as yellow fever; while it may analogically explain the non-recurrence of diseases produced by contagious germs, such as smallpox, measles, etc.

May I be pardoned here for a short digression? Liebig has attempted to elucidate this difficult point by a chemical explanation. He avers that each contagious disease is produced by the action of a species of ferment peculiar to it, upon as peculiar a matter contained in the solids or the fluids of the body; by which means said matter is consumed, and thus is a reproduction of the disease prevented by the want of the material upon which the morbid action may be founded. This famous theory of the cause of the production and non-recurrence of certain contagious diseases, has great plausibility and a charming simplicity. It is, also, supported by analogies deduced from the fermentation of gluten, in the production of bread, and that of saccharine matter in the generation of alcohol. The fermentable substances having been consumed, the process ceases, and without the superaddition of new materials cannot be renewed.

The objections to the theory of Liebig are both numerous and, I think, unanswerable. The existence of the fermentable matters, as well as of the ferment, is purely hypothetical, no proof being offered of the detection of either. It must also be observed that there is, on this hypothesis, a peculiar substance to be acted on, for each of many diseases. Thus there must be one for *variola*, one for *rubeola*, one for *varicella*, one for *scarlatina*, one for *pertussis*, one for yellow fever, and one for every other non-recurrent disorder. Each of these substances must reside in the system without necessity, and apparently without cause. No influence seems to be exerted by them on the health or structure before disease comes, and their elimination leaves the system unaffected subsequently. But there is presented to my mind a still more important objection, which may be thus stated. For example, every one knows that persons who take smallpox in the natural way, have usually severe attacks, a multitude of pustules, and, according to the theory, a very extensive fermentation, and a reproduction of a large quantity of the products of the fermentation. Of course, there has been consumed a great mass of the peculiar fermentable substance, on the pre-existence of which the susceptibility to smallpox was founded. In inoculated cases, as a general rule, the disease is milder, the pustules are much less numerous, and the peculiar matter is consumed in much smaller quantity, while the products are consequently less. A vaccination produces usually only one small vesicle. Its fermenting power consumes, therefore, but a minute amount of matter, and produces but little *virus*. Yet, commonly, by each of these processes, the peculiar fermentable material is *totally consumed*, and the person is generally protected from a subsequent attack of smallpox.

This objection to the theory of a ferment seems unanswerable. But it may be strengthened by adverting to the fact, that by making many insertions of vaccine virus in different parts of the body, we may act on a great deal of the fermentable matter, or by making but one or a few we may consume but little. Yet, in either case, no one pretends to say that the degree of protection, or the liability to a revaccination is altered. These objections, while they unsettle the hypothetical basis of Liebig's explanation, totally destroy its theoretical conclusions. A peculiar matter is assumed as existing, is supposed to be consumed, and not to be usually reproducible. This matter, however, may be equally well consumed by a small or great fermentation, its own quantity seeming to have no relation to the extent or activity of the process, which is governed solely by the mode of using the ferment. How will the analogy, upon which the whole theory rests, sustain the argument of the great chemist? There is a certain quantity of gluten to be consumed in pannification, the action upon it of a ferment, by which the whole is changed, must be ever the same in amount, although it may not require exactly the same time. If the process be less active, it must be proportionally prolonged; if it be more energetic, it will be completed in a shorter time. But the more violent action of a *variola* is not sooner at an end than the gentler process of a *vaccinia*, both requiring for their completeness about the same period of time.

Taking it now for granted, that the chemical theory will not satisfy the physiologist or pathologist, I will proceed to make an argument for the non-recurrence, as producible by the leaving in the system the *exuviæ* of germs. A reference to former parts of these lectures show that many plants, and especially protophytes, poison, against themselves, the soil in which they grow; and that thus we may, not unsatisfactorily, explain the apparent capriciousness as to health of both places and seasons. Supposing that the cell germs, animal as well as vegetable, possess a like power when they grow in the animal frame, we can plausibly account for several things not otherwise explicable at all. Thus we may presume that some of these *exuviæ* having no emunctory capable of their elimination, remain always where the diseased processes left them, and thus stand as an obstacle to the future action of similar germs.* We can thus, and thus only, say why certain contagious diseases cannot recur, and why certain diseases which are not contagious, as yellow fever, for example, possess a like disability. Their germs having once reacted in the body leave behind a poison, or at least an impediment, by which their future reaction is there prevented.

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* Syphilitic poison lurks unexpressed in the system for years, or through life, exemplifying itself only in the offspring. So gout leaps over a generation, in which, however, its cause must be ever present, though latent.

But certain contagious, and even non-contagious diseases, obviously dependent on germs, have the power of recurrence for many times. Yet even these are subject, at least temporarily, to the same law; for, otherwise, none of these diseases could have a termination. The impedimental matter being either emulged or decomposed, after a period, shorter or longer, according to each disease, leaves the system open to a reinfection; and thus *syphilis*, *favus*, and *apthæ* may, for many successive times, disturb the health of the same individual.

Returning to the immediate object of the present lecture, I proceed to explain why it is that the first cases of an epidemic are usually so much more fatal than those which follow them. This is especially true of the diseases of a miasmatic character and non-contagious maladies, such as yellow fever and cholera. Toxication, when of vegetable origin, is made less potent by habit. Thus, in process of time the habitual drinker or opium eater tolerates enormous doses of alcohol or opium. Nay, even when made as obviously drunk, even to insensibility, the old toper is in less immediate danger than the beginner. His organism recovers better; and while the novice dies poisoned or apoplectic, the snoring habitual drunkard recovers from his coma and cerebral congestion. Thus is it with those who are toxicated by an atmospherically conveyed fungous poison. At first it proves eminently fatal; subsequently, although its symptomatic expression may be as strong, its danger steadily decreases, until at length almost every case recovers.

For this reason, medical men, at the commencement of a violent epidemic, are driven too often from a treatment founded on proper principles, into a loose and dangerous empiricism. For the same reason are they disposed, at a later period of the attack, to rely upon means of cure obviously inert, or improper, because the lessened mortality smiles an approval. Let any one found his treatment from the first upon a proper knowledge of the pathology, and a decent regard to prominent symptoms, and he will succeed in the end, not only much better, but also much more satisfactorily to himself, than those who lower themselves to the level of mere empirics. The deaths are at first owing, not to the greater potency of the cause, but to the keener susceptibility of the recipient of disease. While it increases the severity of the cases, this susceptibility is not greater for our remedies, and therefore we must necessarily have, at the outset, less success.

The malarious diseases commonly found in the rich alluvial courses of rivers, or shores of lakes, sometimes abound on sandy plains. Several writers describe the sickly plains of Brabant as superficially dry and almost bare of vegetation; and Dr. Ferguson informs us of the desolate aspect of Rosenthal and Oosterhout, in South Holland, where a level sandy plain bore nothing save some stunted heath-plants. Beneath the surface was found, at no great depth, clear potable water. The plain on the side of the river opposite to Lisbon, dreaded for its pestilential character, is also dry and sandy. Here no ordinary vegetation, no decomposition, can explain to us the cause of its malign power. But there is a teeming vegetation beneath, and almost at, the surface of such places, to which alone can we attribute their diseases. Truffles, a species of mushrooms, grow prodigiously in such places. They delight in sandy plains, and their microscopic congeners are also there in abundance. Such plains, in our own southern country, emit a fungous or mouldy odor* soon after night; which fact has not a little puzzled curious observers.

May not the healthful power of the plough be mainly attributed to its destruction of fungous growths of this, and of other kinds? Almost every writer on malaria speaks of the beneficial influence upon health of a constant cultivation. Now, we know, that when a country is covered with woods, it is usually salubrious, and that when cleared and put under imperfect tillage, it becomes more sickly; but that a regular system of husbandry by the plough, restores to it all its former healthfulness; while the placing it for some time in pasturage, causes it to again retrograde to a certain degree. The plough is the especial enemy of the fungi, which, either beneath the surface, as truffles, or upon it, as mushrooms, are obviously lessened or extirpated by the constant disturbance of an active tillage. Nothing else known to be capable of affecting the health of the inhabitants, is materially altered by agricultural processes.

I have now, gentlemen, brought to a close the prolonged examination of the cause of miasmatic fevers and non-contagious epidemics. Let me recapitulate, in a very cursory way, the most important elements of our argument.

I began by showing that all the usually received opinions on this subject are liable to insuperable objections, except that which refers to the causation by organic life, and especially by animalcules, as held by Columella, Kircher, Linnæus, Mojon, and Henry Holland.

* This is, probably, the cause of the musky odor noticed by Humboldt, when the soil of some tropical regions is disturbed.

While I was impressed, for the reasons so ably stated by Holland, with the greater probability of the organic theory, I prefer, for reasons stated by myself, the fungous, to the animalcular hypothesis.

My preference is founded on the vast number, extraordinary variety, minuteness, diffusion, and climatic peculiarities of the fungi.

The spores of these plants are not only numerous, minute, and indefinitely diffused, but they are so like to animal cells as to have the power of penetrating into, and germinating upon, the most interior tissues of the human body.

Introduced into the body through the stomach, or by the skin or lungs, cryptogamous poisons were shown to produce diseases of a febrile character, intermittent, remittent, and continued; which were most successfully treated by wine and bark.

Many cutaneous diseases, such as *favus* and *mentagra*, are proved to be dependent upon cryptogamous vegetations; and even the disease of the mucous membrane, termed aphthæ, arises from the presence of minute fungi.

As microscopic investigations become more minute, we discover protophytes in diseases, where, until our own time, their existence was not even suspected, as in the discharges of some kinds of dysentery, and in the *sarcina* of pyrosis. We are therefore entitled to believe that discovery will be, on this subject, progressive.

The detection of the origin of the muscardine of the silk-worm, and a great many analogous diseases of insects, fishes, and reptiles, and the demonstration of the cryptogamism of these maladies, their contagious character in one species of animals, their transfer to many other species, nay, even to vegetables themselves, all concur to render less improbable the agency of fungi in the causation of diseases of a febrile character.

A curious citation was subsequently made, of the fungiferous condition during epidemics and epizooties. These moulds—red, white, yellow, gray, or even black—stained garments, utensils, and pavements, made the fogs fetid, and caused disagreeable odors and spots, even in the recesses of closets and the interior of trunks and desks.

These moulds existed even when the hygrometric state did not give to the air any unusual moisture for their sustentation and propagation. Their germs seemed to have, as have epidemics, an inherent power of extension.

The singular prevalence of malarious diseases in the autumn, is best explained by supposing them to be produced by the fungi, which grow most commonly at the same season. The season of greatest photophytic activity is, in every country, the period of the greatest malarious disturbance. The sickly season is in the rains in Africa, in the very dry season in Majorca and Sardinia, in the rainy season of the insular West Indies, and in the dry season of Demerara and Surinam. Even when the vegetation is peculiarly controlled, as in Egypt, by the Nile, and the cryptogami are thus thrown into the season of winter and spring, that season becomes, contrary to rule, the pestilential part of the year.

Marshes are a safe residence by day, while they are often highly dangerous by night. In the most deadly localities of our southern country, and of Africa, the sportsman may tread the mazes of a swamp safely by day, although at every step he extricates vast quantities of the gases which lie entangled in mud and vegetable mould. This point, so readily explained by reference to the acknowledged nocturnal growth and power of the fungi, is a complete stumbling-block to the miasmatists.

The cryptogamous theory well explains the obstruction to the progess of malaria offered by a road, a wall, a screen of trees, a veil, or a gauze curtain.

It also accounts for the nice localization of an ague, or yellow fever, or cholera, and the want of power in steady winds to convey malarious diseases into the heart of a city, from the adjacent country.

It explains also well the security afforded by artificially drying the air of malarious places, the exemption of cooks and smiths from the sweating sickness, the cause of the danger from mouldy sheets, and of the sternutation from old books and papers.

On no other theory can we so well account, if account at all, for the phenomena of milzbrand and milk-sickness, the introduction of yellow fever into northern ports, and the wonderful irregularities of the progress of cholera.

The cryptogamous theory will well explain the peculiar domestication of different diseases in different regions which have a similar climate; the plague of Egypt, the yellow fever of the Antilles, and the cholera of India. It accounts, too, for their occasional expansion into unaccustomed places, and their retreat back to their original haunts.

Our hypothesis will also enable us tell why malarious sickness is disproportionate to the character of the seasons; why it infests some tropical countries and spares others; why the dry Maremma abounds with fevers, while the wet shores of Brazil and Australia actually luxuriate in healthfulness. The prolonged incubative period, the frequent relapses of intermittents, and the latency of the malarious poisons for months, can only be well explained by adopting the theory of a fungous causation.

Finally, it explains the cause of the non-recurrence of very potent maladies, better than the chemical theory of Liebig; and shows why the earliest cases of an epidemic are commonly the most fatal.

When I entered upon the task of elucidating for you this very difficult subject, gentlemen, I did not dream of its extent and importance, nor did I suppose that it would have imposed upon me so much research, or inflicted upon you so many lectures.

I have, therefore, not attempted to account, by this theory, for the periodicity of malarious diseases, rather for want of time than want of power, and from a desire not to tax too severely your patience.

The task is now completed. Yet, after all my labor and your polite attention, the theory presented to you may not be finally demonstrated. But it is the most consistent with the phenomena known at present, and is much better sustained by established facts than any other hypothesis yet presented to the world. It has, therefore, the requisites of a philosophical theory, which, in other and more exact sciences, would be accepted, not to be held as absolutely true, but as, in the present state of our knowledge, the most plausible and convenient explanation of the phenomena.

It has another value. It will revive the inquiry into the causes of fever, by giving to it a new direction, by offering new points of view, new motives for study, and new lights from analogy. If, too, its confirmation or refutation should give to future inquirers after truth half the pleasure which I have derived from excursions into this new field of mingled reason and fancy, these Lectures will not have been vainly elaborated.

AN ESSAY

UPON

ANIMAL MAGNETISM,

OR

VITAL INDUCTION.

ON

ANIMAL MAGNETISM,

OR

VITAL INDUCTION.

AFTER an examination and exposition of mesmerism, by a learned committee, of which Dr. Franklin was a member, it might appear supererogatory, and perhaps presumptuous, in an humble *individual*, to make a new report on that subject. But when it is known that long subsequent to the time of Franklin two committees of the French Academy of Medicine, composed, both of them, of highly distinguished men, made reports of the most opposite characters, I may be excused for thinking that the subject is yet far from being properly understood. At present, the world, learned and unlearned, is divided on this topic into two great bodies—the one giving full credence to the whole claim of the magnetizers, the other rejecting the entire suite of the alleged phenomena, from the slightest mesmeric excitation to the transcendentalism of *clairvoyance* and *prevision*. A few individuals, intermediately placed, think both parties wrong, and hold a qualified belief, more or less extensive, but agreeing in the rejection of the phenomena supposed to prove the existence of supernatural powers in those who are placed in the mesmeric state.

Of late, this subject of many pretensions has excited extraordinary interest, through its supposed phrenological bearings, and has attracted thus to its assistance the learning and ingenuity of the able disciples of the school of Gall and Spurzeim.

The varied fate of mesmerism, its extensive dissemination, its startling pretensions, and its present interest, might excuse me for bringing it before the notice of the College of Physicians; but it has stronger claims on their attention, because of its relation to the history and functions of the nervous system, and its supposed power over the diseases of that system. Viewed as a truth or a falsehood, mesmerism has so much possession of public attention as to demand from medical men some consideration, so much at least, as to enable them to decide upon its general probability. Yet how few (are there any ?) physicians who can at present offer more than mere conjectural opinions, since neither they, nor those in whom they confide, have given to the subject an experimental consideration; while the learned bodies who have professed to examine the matter have committed themselves to the guidance of interested quacks, or have set up arrogant presumptions as marks to shoot at. To begin at the beginning, and to examine mesmerism, step by step, without prejudice or partiality, excluding even the *presence* of interested magnetizers on the one hand, and resolute opponents on the other, was yet to be done, when I undertook the task of mesmeric experimentation. I call it a task, for I foresaw great difficulties, and a prolonged period of time before me; I anticipated the opposition of the mesmerists to a rigid scrutiny; the prejudices of the world against a contraband subject; the risk of being deceived by tho interested and the too credulous; and the hazard of being driven to extremes of opinion by authority or contradiction. The fate of former inquirers justified these fears; for they were many of them apparently well qualified for the enterprise, and yet the opposite results at which they arrived, showed at once the extreme subtlety of the subject and the difficulty of the examination. A closer view of former inquiries led me, however, to the hope of happier results. That, in the age of Mesmer, was obviously imperfect as to time and manner. It occupied a period of only a few months, and was, with a very few exceptions, confined to trained subjects, presenting sources of deception both for and against mesmerism, as will be shown in the course of this essay. The advantages derivable to the investigation from the alleged mesmeric sleep were either unknown until the completion of the report of that committee, or were totally neglected by it; and the waking phenomena might be commonly and with truth referred to imagination and imitation, as reported at that time.

The committee in our own time, of which Husson was the reporter, made the grand mistake of placing itself in the hands of Foissac and Dupotet, noted magnetizers, committed to, and interested for, the fate of their pretended science; and who, whether dupes or knaves, were equally in danger of being deceivers, in the one case directly of themselves, in the other directly of the committee.

The memorable memoir thus produced is yet in the recollection of the College. Notwithstanding the ostentation of precaution to prevent deception, even a careless reader of the report may see that deception was practiced in all the important cases, especially those which went to prove the existence of *clairvoyance* and *prevision*. In none of these were the patients free from the presence, and even active agency, of professed magnetizers; and although the experiments frequently failed, the occasional success was set down as a fact, and not as a phenomenon. Thus, M. Petit, who could not see through a bandage or even a sheet of paper, was supposed to see through his eyelids, so as to play at cards, read, and distinguish the color and form of minute objects. A little practice will teach any one, when awake, to present to others the appearance of well-closed eyelids, and yet to see, through an inappreciable chink, letters, forms, and colors. Again, Pierre Cazot, by trade a hatter, who had been subject for ten years to attacks of epilepsy, was supposed to have the power of predicting the exact time of the access of his epileptic paroxysms. The committee were weak enough to believe that Foissac concealed the somnolent predictions from the patient, or took it for granted that his alleged obliviousness of sleeping events was certain. They do not appear, physicians as they were, to have supposed it possible for the patient to produce his fits at pleasure, or to simulate them. But when, after many successful predictions, Paul failed to keep a promised time for his convulsionary efforts, because he was *unexpectedly* killed by a horse, the committee reduced itself to the supposed necessity of presuming that *the foresight was confined to events of the organism, and did not extend to extrinsic phenomena.* Is it possible that medical men should be ignorant of the fact, that irregularly recurrent morbid phenomena depend, *as to time*, upon numerous extrinsic events, affecting both mind and body; and that, therefore, no patient who does not foresee the train of external agencies, could predict the exact time of a coming paroxysm? If Paul could foresee these, he surely could not have overlooked the important one of his own violent death.

The committee were not only careless but inaccurate. As evidence of this, read the seventeenth paragraph of their "conclusions," where they say, "magnetism is as intense, and as speedily felt, at a distance of six feet as of six inches," and compare it with the eighteenth, immediately following it, where we find that "the action at a distance does not appear capable of being exerted with success, excepting upon individuals who have been already magnetized." That is to say, magnetism is equally potent at all distances, but is operative solely by close approximation.

I have said enough to show that whatever of belief one may have in any part of this subject, cannot derive any rational support from the labors of the committee, of which Husson was the reporter. Indeed, Magendie and Double retired from the commission chiefly because the experiments were not made with rigor and exactness, and refused to sign a report in which the public could not rationally confide. They thus left in the commission only those who were already prejudiced favorably toward magnetism.

The report of M. Husson was evidently unsatisfactory to the Academy of Medicine, for it was treated unusually, by being quietly strangled without adoption or discussion. It was, as we say, laid on the table.

Long afterwards, in 1837, a discussion, nearly accidental, arose on this report, in which members of the commission itself threw doubts on parts of it, of the utmost value to the validity of its conclusions.

Then it was that, at the instance of a magnetizer, M. Berua, a new commission was raised, (February 21st, 1837,) consisting of men who either doubted or resolutely denied the truth of mesmerism. Roux was its president; Bouillaud, H. Cloquet, Emery, Pelletier, Caventou, Cornac, Audet, and Dubois D'Amiens, were its members.

The report made by it, (July 17th, 1837,) through Dubois, to the Academy, shows that this committee gave to M. Berua every fair opportunity of exhibiting the wonders he had promised, and exercised a commendable degree of politeness and patience toward a petulant and unreasonable enthusiast. They confined themselves, in their inquiry, almost entirely to the phenomena supposed to manifest—1st. The ability of the magnetizer to govern the motive power of his subject by an unexpressed volition, so as to confer and remove at pleasure the muscular activity of any part, or the whole of the system. 2d. The power to control, by a mental order, the conversation of the patient, and therefore the thoughts. 3d. They examined the alleged evidence of the transposition of the senses, and of lucidity or *clairvoyance.*

In other words, they brought to exact and rigorous proof the alleged mysterious mental communication, the transposition of the senses, and the power of seeing through opaque barriers.

These pretensions, so far as the patients of M. Berua were concerned, they triumphantly demolished; and, indeed, as they publicly called in vain for other evidence, from other persons interested, they ought to be considered as having fully and perfectly settled the vexed questions to which they turned their attention. Notwithstanding the very conclusive character of the report, there were found, even in the Academy itself, persons of note, who objected to the results as well as to the observations of the committee. Among these, Husson, the reporter of the former committee, stood conspicuous. Seeing that the subject yet lacked a *coup de grâce*, M. Boudin, in the midst of a fierce discussion, proposed to put to the test the pretensions of the magnetizers, in a conclusive way. He offered to place in the hands of a public notary the sum of 3000 francs, to be given to the somnambulist who should be able to satisfy, by ocular proof, a committee of the Academy, that he or she could see without the use of eyes. The committee appointed for this almost unnecessary task was fairly composed. In it we see the names of Husson and Dubois D'Amiens, antipodes in belief; and we have the guaranty of perfect honor and fairness in the other great men who complete the commission. They were Double, Chomel, Louis, Gerardin, and Moreau.

The prize, its objects, and the names of the committee, were made extensively public. They were appointed in vain. Many offers of proof were made to them, but only one magnetizer came before the commission, and he insisted on veiling his patient in such a way as did not satisfy them that the sight would be perfectly obstructed. Indeed, one of the commissioners assured himself by actual trial, that he could see with his eyes covered by the veil presented by M. Pigeaire. The committee, therefore, did nothing. Happily, M. Gerdy saw the feats of the daughter of M. Pigeaire at another time and place, and he expressed himself little disposed by the exhibition to believe in her clairvoyance; *nay, he was profoundly convinced of the contrary.*

M. Burdin, being appealed to by several magnetizers, reduced his demands on the clairvoyants to the simple task of reading, with or without eyes, in or out of the magnetic sleep, through a single sheet of paper, placed at six inches from the face of the patient. Following this modification, we have a curious succession of frauds and failures, made out to the conviction of every reasonable man. Among these may be signalized the imposture of Miss Emily, detected by Frappart, and the failure of the somnambulist of M. Teste. Thus, from the 5th of September, 1837, to the 1st of October, 1840, stood open to competitors, in vain, the prize of M. Boudin. In France, with its thousands of poor somnambulists, to whom the prize would have been a little fortune directly, and a great one indirectly, there were not found more than two or three to contend for it, and of these one was convicted of imposture, and the others of entire impotency. Under the sense of time wasted, and talent misapplied to an absurd subject, M. Double, president of the last commission, said that he believed it essential, at length, to the dignity of the Academy, to put a stop to the endless demands of the magnetizers for experiments, which always failed; to place *magnetism*, on *its part*, in the same proscribed list, as did the associations of *natural philosophers*, *perpetual motion* and the *quadrature of the circle*. The proposition was carried by acclamation, and the Academy of Medicine, which had detailed not less than five able commissions on the subject of mesmerism, seemed reasonable in its demand for repose.

But the eminent body seemed to forget, in its satiety and disappointment, the history of the past. Magnetism was once thought to be extinct, when a Franklin, a Lavoisier, and a Bailly hung it on the gibbet of public derision. *It was only asleep.* Silently, slowly, under new names, and by new devices, it won on public attention and credulity, until it had the power to awaken commission after commission, and to occupy almost exclusively the consideration of the learned and the illiterate. M. Double and the Academy again laid the restless sprite, and supposed that hereafter no one would dare to raise his hand in its defence. Vain hope! True to its destiny, mesmerism, almost before the deadly verdict of the first of academies has been pronounced, is again on its feet, contending under a new flag for its share of public interest. Mesmerism finds an ally in phrenology, and, thus aided, marches with a vigor and confidence unknown to it since it quailed under the lash of Franklin and Thouret.

There must be some peculiar reason for this extraordinary vitality in an apparently absurd subject. We no longer hear of witchcraft or astrology among the educated and the wise. Belief in ghosts ceases with ignorance. But mesmerism has never lost its hold upon a portion of the *élite* of our enlightened age; and at this moment, the almost entire population of educated New England disregard the monitions of Franklin and the ridicule of Dubois, and has its hundreds of magnetizers and its thousands of somnambulists.

In this essay, I hope to be able to show the true causes of this singular state of the subject. Indeed, I am not without some little expectation of being able to put an end to the disputes which have so long disgraced mesmerism. But of this I leave others to judge.

In reviewing the past history of mesmerism, one is forcibly struck with the universality of the mode of examining it, adopted by the men of learning and learned bodies. They appear to have looked almost always through the eyes of others, or have neglected to reach the subject by the route taken by the magnetizers. They have also confined themselves chiefly to the examination, not of the foundation, but of the superstructure; and having found much, if not all of the edifice, unsubstantial, they have (how justly we shall presently see) decided that the basis is itself unreal. Every committee has occupied itself chiefly with charlatans and their subjects, or with learned but credulous men and their deceivers. None have sought, by original investigation and rigorous research among the educated and the virtuous, for the verification or refutation of magnetism. Or when they have in rare cases done so, they have either abandoned the pursuit too early, or suffered themselves to be carried away by the subtlety of the inquiry. It may be presumption in me to say that I have avoided the extremes of opinion. But I may say that I have devoted much of the leisure of five years to this subject, and have cautiously abstained from identifying myself in feeling or reputation with it. Uncommitted up to this moment, I have been entirely indifferent to the result, except so far as to desire that whatever is really true may be clearly demonstrated.

The first question which must engage us is, whether the acknowledged phenomena of mesmerism are the effect *exclusively* of an impression made on the minds of those who submit to the processes, *or whether they are not, at least, in the first sittings, a consequence of physical influence.*

It appears to me that this question goes to the very basis of the subject, and has not been satisfactorily answered, or even carefully examined by any one of the many learned commissioners to whom, from time to time, the question has been referred.

That there does exist such a power, appears to me probable, if not certain, for the following reasons. The most important indirect argument is that derived from the susceptibility of certain persons to be more or less violently agitated by the finger of another, when applied to certain parts of the body, as the armpit, flank, and upper part of the knee. That the effect is more than a mere mechanical one, is made apparent by the well-known fact, that even the most sensitive persons are *usually* incapable of tickling themselves. The same physical gesture may be made by both, but it is the finger of another which alone produces the effect, at least to any marked degree. It is also well known, that after any one of keen susceptibility has experienced these effects frequently, he becomes excited in the same manner by even the gestures which threaten him, and feels all the disagreeables of the operation without the physical action itself being applied. The latter may be easily referred to the mental sympathy of the subject, and is probably so produced; but it is not so easy to attribute the first impressions to the same cause. If these were purely or chiefly mental, *any one part of the body would be as susceptible as another*, and very young children would have no susceptibility at all, as they could not divine the intentions of the operator. A close examination of the phenomena attendant on this curious subject, shows that many persons have no such susceptibility, that others have it in very different degrees, and that some can exercise over it a mental control, so as to suffer or not from it at pleasure. Most of the latter require a moment to assume the defensive state, alleging that if they once give way they cannot recover themselves. A very few have even the power to resist *in cursu.* There appears, therefore, to be, I speak cautiously, a susceptibility to a physical impression, which by use becomes mental, and which is more or less under the control of a vigorous will or strong resolve. There is an analogy in the case of the stomach, when disordered by a physical cause, such as bad food or an emetic. Frequent repetitions render the food or emetic so disagreeable, as to excite a similar state of nausea or vomiting, when it is named or even thought of. Here, the impressibility, originally physically excited, becomes mentally so, at last; and we know that a strong resolve will often enable a person to resist both.* There is not known to the writer any other example of personal sympathy, so closely allied to the phenomena of animal magnetism, as that of tickling. There are, however, others, such as yawning, which seem, though less demonstratively so, to be of analogous, if not identical origin.

Considerations such as these, showing, though faintly, the existence of relations between individuals, of an unexplained character, first led me to an examination of the phenomena of mesmerism. Otherwise I should have acquiesced in the decision of the committee on the Burdin prize.

To determine that an unnatural state of the nervous

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* Van Swieten was present when the carcass of a dead dog exploded, and produced such a stench as caused nausea and vomiting. *Some years* afterwards, as he accidentally visited the same spot, he was excited to the same unpleasant degree by the recollection, and its associations with the place.

system may be produced by mesmeric processes was unnecessary, as every one knows what took place in the hands of Mesmer and his disciples, and few have failed to witness in credible subjects the alleged somnambulism of Puysegur and Deleuze. The task consists in bringing to proof the possibility of producing such a state, independently of the *mental co-operation* of the subject. That the greater part, if not all of the phenomena, may be mentally produced, is made evident, both by the experiments and observations of the Franklin committee, and the scenes presented in crowds excited by excessive religious enthusiasm. Hysteria and catalepsy are not unfrequent results of private grief, or anger, or fear, as most physicians can testify. But is it certain that such somnambulism as mesmerized patients present, can be produced *solely* by moral means? Is there no other, yet unexplained, mode of reaching the same effects? This question has as yet never been satisfactorily answered. It was not enough to show that it could be effected by moral causes. Nausea, as I have said, may be so produced, but generally it begins in a cause solely physical. Its subsequent *reproduction* may be caused by either physical or moral means. Now, if a physician can nauseate his patient by speaking of some article of food or medicine, does it follow that only moral means can nauseate him. So, if the imagination, or imitation, or both, produces disturbance of the nervous system, does that prove the exclusiveness of such power? Unquestionably not! You see, therefore, that the subject lies open for the examination of that point. To get rid of the mental aid of the patient, was a task of no small difficulty. It could only be done by operating on very young subjects, or upon very ignorant ones, or by concealing the object of the operator. Very young subjects offer the difficulty of constant restlessness and impatience under a prolonged operation, whose aim or use they cannot understand. Very ignorant ones are hard to find in our country, and it demands some address to conceal the motives of the operator. Time and opportunity come always at last to those who wait patiently; and accident supplies the want, which ingenuity and industry fail to satisfy. I need not say that I found some patient children, and some very ignorant people, whom I was able to somnambulize, as the magnetizers term it, without any *apparent* suspicion that such was my design. Avoiding the usual passes, the contact of thumbs, and the manual exercise of the craft, and professing only to carefully examine the phrenology of my subjects, I placed in the mesmeric state at least six persons who were, to all appearance, entirely unaware of my intentions. This on their subsequent testimony, and the evident chagrin which some of them showed at being entrapped. In other cases, I succeeded while professing to relieve headache, by compressing the vessels, and deadening the nervous sensibility. A recital of one or two cases will suffice to show how. A little girl from the country, aged eleven years, stood at my side while I was in attendance on the sick-bed of her sister. Neither of them knew anything of mesmerism. I seated her to examine her head, as I thought it remarkable, and while so engaged, conceived the design of mesmerizing her, which I succeeded in doing in sixteen minutes. In this sleep, she presented its usual phenomena, and did not recollect, when awakened, that she had conversed, sung, been pinched, tickled, punctured, etc. The recital of these things, and the marks on her hands, affected her, after she awoke, to an unpleasant degree.

A young lady, of respectable character and connections, sent for me to prescribe for an obstinate cephalalgia. Conceiving that the case presented a favorable opportunity for the mesmeric trial, I took one wrist in my hand, finger on pulse, and with the other compressed the brow. In a very few minutes she closed her eyes, and replied to a question to that purport, that her head felt easier. Just at this moment, two companions were admitted, to whom she spoke, and whom she requested to be seated, opening her eyes as she did so. I beckoned to them to obey her, and said, "I am endeavoring to relieve her headache." In a few minutes, thirteen in all, she appeared to be asleep, as indicated by the usual tests. After conversing, singing, walking to another seat, and seeming insensible to pain from moderate efforts to hurt her, she was aroused by passes, and declared her entire want of knowledge of what occurred during her sleep. In this case I found it difficult to appease the irritation of my patient, my only acceptable excuse being the desire to learn the truth on this subject, and that being impossible if I had consulted her.

In this manner, with various fortune, I treated many cases. Some saw through the design; others complained of being made drowsy and begged me to desist; while a few fell into the mesmeric somnambulism.

Perhaps the most conclusive experiments on this point were made at one of our asylums for children, where I was an entire stranger, and to which I was taken by one of its medical attendants. They are members, I believe, of this body, and were at that time, all of them, incredulous on the whole subject of mesmerism. Here we agreed to allege the usual phrenological excuse, to take the children apart, one by one, and to confine the time exactly to ten minutes. Out of thirty-six, ten fell asleep, all but two of whom awoke on being shaken or spoken to. These two were profoundly mesmerized, resisting all *ordinary* efforts to awaken them. Of the remaining seven, three exhibited the flushed face, cold clammy hands, and accelerated pulse of somnambulism, and were not disturbed by slight efforts to tickle and pinch them, but awoke on being spoken to.

It is not to be supposed that the people on whom I operated could be familiar with the multiform shape of the sleeping phenomena, which are within certain limits very peculiar and varied. Thus they could not, for example, easily know the effect of the direction of the passes, on the different conditions of the body; for a knowledge of these was to myself, at first, in some respects, a discovery. For instance, passes in the peripheral direction, are alone productive of positive effects in any case, although they do not produce any effect in some cases. Those in the central direction are only restorative of the natural state. But peripheral passes produce sometimes rigidity of the limbs, sometimes total paralysis. This happens according to the peculiarity of the subject, and not at the pleasure of the operator; *for the same subject always presented the same result.*

The centripetal passes always relieved the rigidity, or paralysis, leaving to the patient, in either case, the natural power of motion. Most susceptible persons may be thus affected, and relieved, even without the contact of the hand by which it is done. Supposing it probable, or even possible, that the many persons on whom I acted could have known the effect of the direction of the passes, it was easy, by carefully blindfolding them, to conceal the direction. This I did in several cases. I was so rigid in this scrutiny as to exclude the motion of the air by inclosing the arm in a glass tube, extending from the tips of the fingers to the shoulder, which was wide enough to admit of visible motion within. With all possible care to conceal the motion and direction of my hand, abstaining from touching the tube, and often making the motions in one direction only for several successive times, I uniformly found that the passes from the centre to the periphery of the nervous system, if productive of any effect, stiffened or paralyzed, while those in an opposite direction always restored the power of motion. These effects then were, as far as I, and those who witnessed them, could judge, entirely independent of the knowledge or volition of the patient. This part of the subject I elaborated until entirely satisfied. But that nothing may be wanting to complete this part of the evidence, I shall relate the following analogical case:—

J. C. S., a gentleman of the first standing, president of a prominent institution, but who knew absolutely nothing of the subject on which I am now reading, was attacked with total paralysis of the right side of the body—a complete hemiplegia. I saw him at midnight, within half an hour of the onset. I found that he had taken a long and fatiguing walk, had eaten a heavy supper, and retired immediately to bed. The paralysis was the result. His pulse was full, strong, and frequent;

his skin hot and dry; and he had vomited copiously without any sensible relief. I of course resolved to bleed him, and asked for a basin and bandage; pending the production of which, I examined carefully the condition of his side. The paralysis was so complete as to disable him from moving even a finger. He could not even draw his arm over the surface of the bed, much less sustain it in the air. His arm, when lifted by me and let go, fell instantly by his side. The resemblance to a mesmeric paralysis struck me so forcibly as to induce me to make some centrifugal passes, not, however, with the hope of any positive result. The passes were made lightly, and did not exceed a dozen. To my utter surprise, his hand remained in the air when I let it go; and, when requested, he moved it about; placed it on his head; grasped me firmly by the hand; and seemed to lack no part of his ordinary power. The leg remained paralyzed, and the difficulty of expression was not lessened. The basin and bandage came, and I bled him copiously in the left arm, so as to abate the force of the pulse. Immediately afterwards I found his right arm as paralytic as at first. Supposing it possible that the restoration had been a mere coincident of my manipulation, or that the patient might have observed and been impressed morally by the passes, I took every precaution in the repetition. Lifting his hand, I made a number of passes from the centre (a paralyzing gesture) and said, "You can NOW support and move your hand." It fell helpless on the bed! I lifted it, and repeated the same kind of passes. No result! I urged him to try "*after what I had done.*" "It is gone," was his answer. I then carelessly and lightly made passes toward the centre. Letting go the hand, it stood still in the air again, and was moved about as before, at his pleasure. I then, for the first time, inquired if he knew any difference in the friction used. I found that he had not observed me, and that he knew nothing on the subject, for he had never seen a case nor read a line about mesmerism. Not a word was said to intimate the use of magnetism, and I encouraged him with a confident tone only when I used the wrong passes. That was not necessary when the passes were made in the central direction. In about half an hour afterwards, his arm had again lost its locomotion, which I again restored, but much less perfectly than in the first two efforts. He could move his arm but could not grasp as strongly. In the morning his paralysis was found entire, and, with a very slight amendment, has affected him to this time.

The mode of awakening mesmerized patients affords another proof of the physical influence of which I now state the evidence. This may be done in so many different modes as to enable us to deceive the patients as to our intentions. It may be done by transverse or upward passes, or by rubbing the eyelids, or by shaking the hands rapidly, as if throwing water from the tips of the fingers. Each patient takes a peculiar time to awake, which is in most of them very uniform, seldom varying more than a minute or two. This enables us to mislead them as to our design, at least for several sittings, and we can thus prove that the patient may be and is awakened without his knowledge or consent. It would be presuming too much to suppose a degree of knowledge on the part of chance patients which few practiced mesmerizers have.

But this question can be settled in another way, as the following cases will show:—

C. E., subject to epilepsy and prolonged stupor, was attacked with convulsions soon after his marriage. In the alarm at the (to them) novelty of the stupor in which the family found him, I was sent for, and soon discovered from the stertor and wounded tongue what had happened. He had been found in bed, but was seated, at my arrival, in a large easy chair, with a view to arouse him, all the efforts for which had proved abortive. I took my turn at this duty, but to questions, tickling, water, and rough shaking, I received no other answer than the low grunt usual in such cases. I found his state so equivocal as to cause me to hesitate whether or not to take blood, as I knew that some epileptics were greatly injured by depletion. Desirous of asking the patient how it affected *him*, I renewed my efforts to get his attention, but in vain. At length I thought of the mesmeric passes, and using them for a few moments, saw the patient open his eyes, and heard him address me by name. I seized the opportunity to put my question. He replied, "Cups suit me," and pointed to the temples and nape of the neck. I sat down, wrote a few lines to a cupper, and found my patient again in his stupor. The effect was transient, so I forbore to repeat the passes, and left him to the more permanent effects of depletion. That immediately restored him to his wonted condition.

Master C., from Virginia, here for some time under my care, was about to depart with his mother for home. On the morning of his expected journey he had a fit, which was followed, as usual with him, by a long and profound coma. He slept usually from twelve to fourteen hours without being roused by any known means. On this occasion there was cause for new efforts, and after many fruitless trials, they dressed him and sent him into the street with his colored attendant. On his return I found him still asleep, and sitting, held up, on a sofa. I then took my turn at the task, but so long as I used common means, I was foiled. Then I bethought me of the mesmeric mode, and had no difficulty in awaking him by very gentle means. He opened his eyes, rose to his feet, asked for breakfast, and began to eat some bread and butter. In the midst of his meal he again fell asleep, was roused again, by passes, so as to finish it, but did not continue long awake. He was taken to Wilmington asleep.

A little girl, thirteen years of age, in feeble health, became naturally a somnambulist. She had never been magnetized, and had not seen, as far as I can learn, a mesmeric experiment. The somnambulism generally began soon after she fell asleep at bedtime, and kept the family in discomfort for two or three hours. After making many efforts to arouse her, which always failed, they sent for me. I contented myself at first with an examination of the case, and found that in all respects it tallied with the mesmeric somnambulism. She had closed eyes, free verbal expression, locomotive power, great sensibility to the touch, and but little to causes of pain; her face was flushed, her hands cold, and her pulse very frequent. She mistook the individuals around her; but although she gave wrong names, she always reapplied them to the same persons. She could be persuaded that she was in any given place; that she could fly through the air, etc. etc. The likeness was complete, even to the fruitlessness of all the usual efforts to awaken her. She insisted on being considered as awake, but gave evidence of being in a state of ignorance of everything around her. In this case *half a minute's* mesmeric action brought her to her senses, and she expressed great surprise at finding me in her chamber, and had no recollection of what had passed in her sleep. The effect was soon gone; her somnambulism almost immediately returned; she was again awakened mesmerically, and again relapsed. The attempt to keep her awake was finally abandoned, and she recovered her health and natural repose together, by the use of exercise, chalybeates, and the cold bath.*

Another, and perhaps the best mode of testing the theory of "imagination," is that of the awaking under a thick cover or shawl, and by peculiar manipulation, those who are not only ignorant of magnetic processes, but being so covered, cannot know what is done at the time, especially if the passes be made slowly and cautiously. H. S., a girl of eleven years of age, recently from the country, and unable to tell what was understood by the term animal magnetism, was placed in a mesmeric state in fifteen minutes. Her head was then

* It was curious to mark the sudden transitions from one state of consciousness to another; and the unvaried character of each state; while there existed as little resemblance between the two, as is found in the character of the most opposite persons. The ideas, sentiments, passions, forms of expression and gesticulation—even the temperaments, were those of two contrasted individuals. She was slow, indolent, and querulous, when awake; quick, energetic, and vivaciously witty, when asleep.

covered with a doubled shawl, through which I could not see the slightest ray of light. Being so covered, she was subjected to most of the usual experiments, and finally to one form of the awakening process, by upward passes with one hand, the fingers being kept apart to prevent any fanning. She awoke in less than two minutes, without a word being spoken until she opened her eyes, removed the shawl, and gave the usual look of surprise at the by-standers. In a second experiment, the hands were merely shaken, as if casting off water from them, and in a third, noiseless, moderate transverse passes were made. Experience had shown that such actions usually arouse the somnambulist. And they all restored her to her natural state.

Experiments of a similar character, indefinitely diversified as to concealment, all ended in establishing the certainty, that a mesmerized patient may be restored to the ordinary waking condition without his knowledge or consent. It is, however, unquestionable, that many subjects acquire, if they do not originally possess, the power of controlling the mesmeric state, so as to awaken themselves by their own volition. Such patients as these afford fertile sources of deception to the itinerant magnetizers and the private dupes to their own art. Seeing this, we can infer little positively from such experiments, unless the greatest care is taken to conceal our object and process. But, as I said before, by using very illiterate subjects, and concealing the design, by screens and varied processes, we are able to determine, beyond reasonable doubt, that persons may be brought out of the magnetic sleep, without any aid derived, directly or voluntarily, from either their minds or bodies. It follows, of course, if physical means may undo the knot, that it is not more difficult to believe that means of the same kind may tie it. This consideration, added to the direct proofs already brought to your notice, seems sufficient to prove, I think conclusively, that *there is a mesmeric state, and that it may be artificially produced by means which are independent of the volition or imagination, or any other known moral power of the subjects of it.**

In the course of this inquiry, other and numerous proofs of the truth of my first position will be hereafter adduced. They would be less advantageously cited in this place. In the mean time I shall proceed to consider—First, the real phenomena of the mesmeric state; secondly, the alleged phenomena which are not real; and thirdly, its medical relations. In other words, I shall bring to your view the extent, limits, and practical applicability of the mesmeric state.

The mesmeric sleep is so exactly like that of natural somnambulism, that, excepting as to cause, the two states might be esteemed identical. Favored by cir-

* The doctrine of the mesmerism of the imagination will not, I think, explain the extreme difficulty of somnambulizing a patient for the first time without contact. I see no good reason for supposing that so peculiar a state as mesmerism, if producible at first by imagination, should not be so done as readily without as with contact. What is there in the contact to aid in the process by imagination? When, after several operations, the patient becomes familiar with the state of mesmeric sleep, association and imagination may and will produce it as readily without as with contact; but no mesmerizer, as far as I know, has been able to dispense with contact in his experiment on any patient for the first time.

cumstances, I have had an opportunity of seeing both, often enough to learn, that even in their varieties they agree in phenomena. Thus, in both, the eyes are sometimes closed, sometimes open, without natural perception by sight, or disposition to wink. In both, there is the power of speech and locomotion, diminished sensibility to pain, but perfect retention of touch. In both, the subjects may usually be led to dream at pleasure of anything addressed to them verbally, etc. For these reasons the mesmeric sleep may be properly termed an ARTIFICIAL SOMNAMBULISM. The subjects in both are sometimes easily awakened, sometimes with great difficulty. In both, cases are sometimes found incapable of motion or of speech. Some subjects are conscious of the physical, but not the moral relations; while others are ideally in other places and times, or are cognizant of the persons and things around them. In all these curious diversities, the natural and artificial somnambulism agree; so that it seems no undue inference to suppose them the same state produced by different causes. As no definition of a state of multitudinous phenomena can embrace them all, that which I prefer to any other is the one I have selected above, because it is the most common state, and, from the general use of the term somnambulism, is the most intelligible. The term artificial is prefixed to distinguish it, as to cause, from accidental somnambulism.

Of the phenomena by which artificial somnambulism is distinguished, those presented by the circulation and respiration are not the least curious. The following table carries with it its own explanation :—

Table Showing State of Pulse and Respiration in Artificial Somnambulism.

Case.	Sex.	Pulse before sleep.	Pulse in sleep.	Respiration before sleep.	Respiration in sleep.
No.		1st. 2d. 3d.	1st. 2d. 3d.	1st. 2d. 3d.	1st. 2d. 3d.
1	m.	72... 72... 65	112...112...111	20	20
2	f.	80... 64... 78	98... 98...102	16	18
3	f.	90	120		
4	m.	64	84		
5	f.	92... 91... 88	116...116...132	unchanged.	unchanged.
6	f.	86	108		
7	f.	76	96	16	14
8	m.	80	96	22	24
9	f.	100... 82	120...108	28...32	36...32
10	f.	76... 84	100...104	12...16...24	12...16...18
11	m.	84... 84... 84	120...104...108		
12	m.	80	96		
13	f.	88	100		
14	f.	80... 70... 92	104... 92...108	15...15...24	16...16...20
15	m.	68	104	18	16
16	f.	81... 86... 81	96...104...104	20	20
17	f.	68	106	28	16
18	f.	78	116		
19	f.	80	108		
20	m.	80	96		
		1 2 3	1 2 3	1 2 3	1 2 3
21	f.	72... 72... 76	86... 84... 88	16...16...16	16...20...16
		4 5 6	4 5 6	4 5 6	4 5 6
		74... 92... 88	91...106...104	16...20...16	14...16...20
		7 8 9	7 8 9	7 8 9	7 8 9
		120...108... 96	128...124...112	16...16...16	20...16...16
22	m.	70	102		
23	m.	64	112		
24	f.	84	96		
		1 2	1 2		
25	f.	92... 92	116...116	22	44
26	f.	72	102		
		1 2 3	1 2 3		
27	f.	80... 88... 84	90...100...102		
Average...		81·7	105	19·04	19·68

Difference in pulse 23·3.

Difference in respiration 0·64, which, if proportional to pulse, should have been 4·75; or 74·2 times as great as it is.

The pulse of the sleep was *in every case* greater than that of the waking state; the least excess being eight pulsations, the greatest forty-eight, the average difference 23·3.

The respiration often remained *unchanged*. Of twenty-five instances in which it was examined, nine were without any alteration, nine were increased, and seven diminished in frequency by the mesmeric action.

Of the cases of excess, 2 were of one respiration; 2, of two; 3, of four; and 1, of twenty-two. In the last case (twenty-two) the somnambulist was in a constant state of moral excitement, talking incessantly, and seldom remaining even a moment quiescent.

Of the cases of diminution, 3 were of two respirations; 2, of four; 1, of six; and 1, of ten.

If the extraordinary case (twenty-two) be deducted, the average proportion of the number of respirations would be as 18·9 to 18; showing that the respiration is lessened by the mesmeric state. If that case be counted, the proportion is 19·04 to 19·68, showing the slight increase of $\frac{64}{100}$, a difference which might be accidental. The difference necessary to make the change proportional to that of the pulse, should be 4·75; that is, 7·42 times as great as it actually is, under the most favorable view of the case. On the whole, the conclusion seems fair, that the mesmeric state does not alter, in any marked degree, the frequency of the respirations. (See preceding table, and that on page 170.)

To make the bearing of the table more intelligible, it was necessary to institute some inquiry into the effect of natural sleep on the pulse and respiration, which resulted in this, that the pulse was sometimes unchanged, usually less frequent, *but in no case* more frequent. The respiration shared the same fate, being sometimes unchanged, but usually a little lower in sleep. These observations accord with those of the most distinguished physiologists, from Haller to Müller. Hamberger says that a child of 8 years of age has, when awake, 100 pulsations per minute; when asleep, 89. A child of 11 years, 90 when awake, and 80 when asleep; and one of 14 years shows a similar difference.

The following additional table has some value as showing the effect upon the pulse and respiration produced by abortive efforts to procure the mesmeric state.

In the greater number of these cases I find the words, "No change of the physical condition." In a few cases the change is more exactly stated thus:—

Table Showing State of Pulse and Respiration in Abortive Efforts to Mesmerize. No sleep.

No.	Pulsation.	Time.	Respiration.
1	76 to 109	30 minutes.	Not noted.
2	75 to 70	30 minutes.	Not noted.
3	130 to 90	30 minutes.	30 to 24
4	100 to 84	30 minutes.	20 to 14
5	100 to 86	30 minutes.	18 to 18
6	80 to 80	25 minutes.	24 to 24
7	72 to 72	30 minutes.	16 to 16
8	72 to 84	25 minutes.	16 to 16
9	68 to 60	30 minutes.	20 to 14
10	60 to 58	30 minutes.	14 to 16

Of ten cases where the numbers have been set down, only two showed a rise of the pulse, and six its depression; the other two showed, in this respect, no change.

Of the eight where the respiration is exactly noted, three fell, two rose, and five remained unchanged; all the rest were marked "unchanged."

The next phenomenon to which I call your attention is the disturbance of the calorification.

In a natural sleep, the extremities are warm, and the head cool. Even when the feet and hands of a person of good health are cold when awake, they become warm when asleep; and unless the bedclothes are too heavy, the face, during sleep, is rather paler than usual. These phenomena have been verified by several observations made not only by myself, but by others. In addition, I may state that, in almost all the abortive attempts to mesmerize, there has been either no change of temperature or an increased coolness of the head and greater warmth of the hands.

Thus of thirty-seven cases, twenty-five showed no appreciable change of the temperature of face or hands; five showed increased warmth of hands and coolness of face and head; while seven evinced clearly the change of temperature usually perceived in the mesmerized cases.

On the other hand, the annexed table shows that in the sixteen cases of mesmerized patients there noticed, only one retained warm, dry hands, and that but at one sitting. All the rest had cold hands and flushed faces; and almost all had wet or clammy hands.

Table of the Effects of Mesmeric Sleep on the Temperature of the Body, etc.

	Before sleep. Head and Cheeks.	After being mesmerized. Head and Cheeks.	Before Sleep. Hands.	After being mesmerized. Hands.
E. M.	Cold and pale.	Hot and flushed.	Cool and moist.	Very cold and clammy.
S. E.	Natural.	Warm and flushed.	Nat. temperature.	Cold, wet, and livid.
C. E.	Natural.	Perceptibly warmer	Warm and dry.	Cold and clammy.
J. E.	Natural.	Warm and flushed.	Warm and dry.	Cold, wet, and livid.
H. R.	Cool.	Hot and flushed.	Warm and dry.	Deadly cold and wet.
Mrs B.	Natural.	Flushed.	Warm and dry.	Cold and moist.
J. R.	Natural.	Flushed.	Warm and dry.	Cold and moist.
M. H.	Natural.	Head warm, cheeks flushed.	Warm and dry.	Cold and moist.
R. O.	Pale and sallow.	Head very hot.	Cool.	Very cold but dry.
C. E.	Cool.	Head hot, vessels tinged, cheeks flushed.	Cool and dry.	Cold and wet.
M. D.	Natural.	Face flushed, head very warm.	Cool.	Very cold and clammy.
J. H.	Cheeks pale.	Cheeks flushed, head very hot.	Warm and dry.	Cold and moist.
M. H.	Cheeks natural	Very flushed, head hot.	Cool and moist.	Very cold and wet.
L. H.	Natural.	Flushed and hot.	Cool and dry.	Cold and wet.
M. F.	Warm.	Sensibly warmer.	Natural.	Cool and wet.
M. S.	1 No account.	Hot and flushed.	No account.	Warm.
	2 Cold.	Sensibly warmer.	Cold and moist.	Very cold.

Another remarkable condition of the mesmeric state is the ALTERED SENSIBILITY to the action of causes of pain. In this respect it is much like the state of natural somnambulism; and, contrary to what is alleged in the books on this subject, it presents considerable variety in the degree of alteration. Some patients are nearly as sensitive as when awake. This is rare. Others, and they form the largest class, do not suffer from *slight* attempts to annoy, as by tickling, pinching, or even pricking with a pin or needle; while there are a very few in whom the sense of pain appears to be, for the time, nearly if not entirely extinct, and who bear, without seeming to feel, severe surgical operations, such as the actual cautery, extraction of teeth, and extirpation of small tumors.

Admitting the mesmeric somnambulism to exist, we can readily believe in these alterations of sensibility, for observation shows that some natural somnambulists are exceedingly impassive. Thus Sauvage relates the following case:—

"On the 5th of April, 1737," (long before the time of Mesmer,) "visiting the hospital at 10 A.M., I found the patient in bed, which she kept on account of her debility and the pain in her head. The fit of catalepsy had just seized her, and it quitted her after five or six minutes; this was perceived by her yawning and raising herself into a sitting posture, the prelude to the following scene: She began to talk with a degree of animation and *esprit* never observed in her except in this state. Her discourse related to what she had said in the attack of the preceding day. She made pointed applications to persons in the house, whom she designated by invented names, accompanying the whole with gestures, and movements of her eyes, which she kept open. Yet she was all this time in a deep sleep, a fact which was strongly averred, but which I should never have ventured to declare, if I had not obtained satisfactory proof by a series of experiments on the organs of sense. When she began to talk, a blow of the hand inflicted smartly on the face, a finger moved rapidly toward her eyes, a lighted candle brought so near to the organ of vision as even to burn the hair of her eyebrows, a person unseen uttering suddenly a loud cry into her ear, and making a stunning noise with a stone against her bedstead, a solution of ammoniacal salt placed under her eyes and introduced into her mouth, the feather of a pen, and afterwards the finger, projected against the *cornea*, Spanish snuff blown into the nostrils, pricking by pins, twisting her fingers,—all these means were tried without producing the *least sign* of feeling or perception. Soon afterwards she rose, and I expected to see her strike herself against the neighboring beds, but she passed between them and turned corners with the greatest exactness, avoiding chairs and other furniture that happened to be in her way. On awaking, she had not the least idea of what had passed during the paroxysm."

On the very evening on which I copied the above paragraph, I saw a young lady in a somewhat similar state. She was in bed, apparently awake, her eyes being open, brilliant, and expressive, and readily directed to the persons and movements about her. She thought herself awake, but she supposed the persons around her to be strangers, to whom she gave, as in Sauvage's case, fictitious names—most of which names were sounds obtained by spelling the real name backward, thus: instead of Mitchell, she had it Llehctim, and so of others. She laughed at my attempts to pinch her, and thought it singular that I could not produce pain, although I used great force. In this case I attempted for a set time of five minutes, by tickling, shaking, calling aloud, and throwing cold water on her face, to awaken her, but in vain. I then resorted to the gentlest magnetic mode, and in half a minute by watch, she closed her eyes as if for natural sleep, and commenced rubbing her eyelids as children usually do when awaking. She then opened her eyes and looked surprised at my presence. Her whole manner and train of ideas seemed then changed, and she addressed every one properly. Nothing that transpired during her abnormal state seemed to have taken hold of her waking memory. There was a veil of total oblivion dropped over it.*

Artificial somnambulism presents cases of the same kind *in all respects*, but it is to the *lessened sensibility* that your attention is now called.

C. L., a boy of eleven years of age, whose somnambulism was well marked in other respects, such as by a total change of character, by being directed in his dreams, by entire oblivion of passing events, and by marked physical signs, was preternaturally sensitive to pain; his sufferings from being tickled were distressing to view, and yet he remained asleep, and was ignorant of the names of the persons who annoyed him. He felt acutely the puncture of a pin or a pinch, and was startled by even a cautious touch of the finger.

No. 117. M. S. D., aged fourteen, a child at the Orphan Asylum, scarcely noticed the puncture or pinch, but exclaimed vehemently about toothache, just as the unexpectedly applied instrument removed her tooth. She even put her hand to her cheek, but did not touch the operator or instrument. In a moment afterwards she replied to a question, that she "felt better; the '*stoon*' was gone."

No. 46. Mrs. D., aged thirty, a lady of genteel education and association, being too timid to bear the hand

* On another occasion I requested her to remember and state, when she should awake, that I had punctured and pinched her. This she actually did, to the infinite surprise of the family, who had never before heard her make the slightest allusion to any passage of her many paroxysms. In a subsequent part of this essay I shall endeavor to account for this striking phenomenon. I state it here for the sake of greater clearness and a more connected narrative.

of the dentist, desired to be magnetized for the extraction of a tooth. So long as she supposed that the operation would immediately follow the sleep, she, who had been easily composed before, could not be soporized. It was only when the proposed operation was supposed by her to have been abandoned, that she could be mesmerized; and then, unexpectedly to her, the tooth was removed. The operation gave her severe pain, for she cried out, put her hand to her face, and opened her eyes, but almost instantly all signs of suffering were gone; when she was subsequently roused by passes, she had no kind of recollection of either the pain or the operation.

No. 9. Miss E. P., who had been several times magnetized, was put to sleep for a similar operation. In this instance I called in the aid of a fashionable and skillful dentist, and the tooth was extracted, not, however, without the expression of a sense of pain, but without calling the patient out of her sleep. That took place spontaneously some time afterwards, while I and the dentist were discussing the subject.

These cases show that the sense of pain is not uniformly destroyed by the magnetic state, as is alleged in almost all the treatises on the subject. But that the passibility of some mesmerized persons may be nearly if not entirely subverted, is demonstrated by the following case:—

No. 118. E. B., at the Osphan Asylum, being mesmerized, was, *without her knowledge, consent, or expectation*, deprived of a jawtooth considerably decayed, and for the extraction of which she had for nearly a month been endeavoring to summon nerve enough to go to a dentist. The gum was inflamed, and the operation was performed by unpractieed hands, with a dentist's eommon key. Several persons being present, eaeh was desired to watch for any expression or sign of pain. None was detected. The face was ealm and deathlike; the hands lay on her lap without the movement of a finger, and not a fibre of her system gave notiee of any suffering. Yet the tooth resisted strongly, and the patient was, aeeording to the testimony of her physieian, (Dr. Hays,) and her dentist, (Mr. Townsend,) a very sensitive one.

A few days afterwards, at her own request, the same person lost another tooth in the same way, and with exactly similar phenomena.

In both instanees, during her sleep, and soon after the operation, the patient was questioned as to her sufferings, and she replied that she had felt no pain.

Some months afterwards, the same person applied to me, through one of the physicians of the Asylum, to remove, after mesmerizing her, several diseased *teeth.* I promised to do so, provided she would eome to my residenee. There, in the presenee of several of my friends, many of them physicians, the dentist above spoken of removed three teeth, and the stumps of two others, under the following eireumstanees:—

Immediately after the extraetion of the first tooth, so entirely painless did it seem, that an eminent law judge, who was present, observed that the tooth was set in for the oeeasion, that the gum did not bleed, and that I was deceived. The dentist immediately replied, "No, sir, that tooth was as firmly in its soeket as any other similar tooth; and here you see," separating her lips, "is the blood you look for." After the removal of another tooth, it was suggested that the girl was impassive, and that a tooth should be pulled when she was awake, to test her capacity of endurance. Although I had learned from her physician and dentist that she bore pain badly when awake, I thought the suggestion a good one; and accordingly awoke her for the purpose. But it was impossible, by reasoning or entreaty, to obtain her consent, as she alleged that her sufferings when awake were too great for her resolution to support. I then took the dentist aside, and begged him to endeavor to push out one of the stumps with his thumb, or a concealed instrument. He accordingly desired to feel the tooth, as if for observation, and, with much address, suddenly applied his force to it; but a loud cry, and a violent projection of her body backward, gave indication of a suffering beyond her endurance. After a short lapse of time, this subject was remesmerized, and lost the rest of the bad teeth; showing, except in the very last operation, no apparent sensibility. Just as the last tooth, the fifth, was removed, there was a slight expression of pain. But to a question, she replied that she did not suffer. The same patient lately applied to me again, and a medical friend, Dr. S.,* removed a tooth for her when she was mesmerized, without any apparent sense of suffering.

Other cases, showing a nearly total extinction of the sense of pain, might be brought forward, but the phenomena are so much alike as to render the details unnecessary. But such cases are far from being common. They amount to about one in ten of the mesmerized; so that the chance of a patient's being able to avail himself of a loss of sensibility in any given operation, is not

* Doctor Spencer, of Moorestown, New Jersey.

more than one in ninety; for but one in nine can be mesmerized, and but one in ten, when mesmerized, loses his sense of pain. The rest suffer more or less, though all those who have been so treated have subsequently expressed a preference for a mode of operating which does not give them any memory of suffering. If they were not told that they had suffered, they would suppose that no pain had been inflicted.*

While examining this part of my subject, I instituted some experiments to test the degree of sensibility to pain retained by persons in their *natural sleep.* A little reflection shows that slight causes of pain do not produce it in such a state of repose; for sleepers on hard beds, those who are bitten by fleas, bugs, and mosquitoes, and those who are attacked by vampyre bats, and rats, do not seem to feel, or at all events do not awake readily when so tormented. The direct experiment I have often made myself, and caused others to make, and with results such as might have been expected. The sense of pain is greatly impaired in common sleep, so that many persons may be pricked, pinched, or tickled, without being disturbed. Most physicians of matured age have met with cases where causes of severe pain have occurred in the night, which have been *felt* only after the state of sleep has passed away. Perhaps this may account for some of the causes in which persons have been burnt until too much crippled to escape, before they have been roused from slumber. Making, however, every allowance of this kind, the degree of in-

* To the law of obtunded sensibility, we may refer the wonderful impassibility of the convulsionaries of St. Medard, who bore degrees of physical violence such as would have destroyed the human frame in its state of natural sensibility.

sensibility in most of the mesmerized cases is very remarkable; and the persistency of the spell, even when great pain from violent operations is actually felt, is among the most striking phenomena of the magnetic state. While the sensibility to pain is abated, the sense of touch is either unaltered or improved. Almost all the mesmerized are able to determine by the touch, the size, form, and texture of bodies. They excel in this way even their waking powers, and mark with surprising precision the engravings on seals, embossed letters and figures, and other things of that kind, while they seem often totally insensible to a severe pinch, the prick of a pin, or even the action of fire. The patients, by the sense of touch, observe the attempts, but allege that no force is used, because they do not feel pain. Thus a ring containing a gem is placed in the hand. "What is that?" "A ring." "Examine it, and tell what is its setting." The patient examines it, and while so doing, is pinched with a pair of forceps so as to extract blood. No apparent notice is taken of this. "What am I doing?" is asked. "You are pretending to pinch me." "Do I not pinch you?" "No." "Why do you say so?" "Because you don't hurt me." This is a very common result of such experiments.

The sense of touch would appear, from such observations, to be independent of the sense of pain, and *vice versa*. This might have been inferred from other considerations. Many internal parts possess the sense of pain without the slightest sense of touch. The intestines are easily pained, but they do not give us practically any idea of the presence of worms or other contents. The stomach takes no cognizance of the form, size, or roughness of the things we swallow, but our *ingesta* often cause pain. As no two sets of dissimilar nerves perform the same function, so no two dissimilar functions are performed by the same nerves. The nerves of touch are not nerves of motion. The retina does not send filaments to the eyelids, and ocular muscles to the iris or lachrymal gland. Nor is any nerve of sense a nerve of motion. So we might from analogy infer the independence of tact and sense of pain. They are distinct functions, and should therefore be performed by different nerves. The more direct proofs derived from magnetic experiments verify the truth of the analogical position, that *tact and sense of pain are independent faculties, and therefore are probably sustained by separate nerves.* If this be so, the sense of pain is a SIXTH SENSE.

To the obtunded sensibility we may refer some of the phenomena of the mesmeric state, which would otherwise appear difficult of explanation.* A very delicate and feeble girl may be made to appear very strong; for she can extend her arm and leg at length in the air, and keep them there unsupported for a much longer time

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* Thus when a patient, in mesmeric or natural somnambulism, sees very well, even better than those around him, we naturally suppose that we shall find, on forcibly opening the eyes, a painful susceptibility to the influence of a strong light, but we feel surprised at detecting often no uneasiness or impatience. We now perceive that the sense of vision may be highly exalted, while the sense of pain may be greatly depressed, so that the brightest images may excite only pleasurable impressions. Not keeping this distinction in view, we are led to suppose that the eye, which is so insensible, cannot be the medium of vision in such cases, and we therefore too hastily admit the *dreamy* assertion of the somnambulist, that he sees, *as he supposes he does*, with some other part of his surface.

than the strongest man can do under ordinary circumstances. Examining the case closely, we perceive that the one soon feels *the pain* of the protracted effort, and that pain rapidly exhausts the muscular force, and compels the descent of the tired member. The other cannot feel either pain, or, one of its modifications, sense of fatigue, and may therefore appear to possess preternatural strength. It is this which probably exempts sleep-walkers from the exhaustion to which their nocturnal rambles would otherwise expose them; and it may account for the little damage from blows and falls; for the *afflux*, following the dolor, is less likely to occur in mesmerized cases in which the dolor is so seldom felt. The bone-setters, as they are called, probably take, without knowing it, advantage of this law of the animal economy, and, by obtunding the sensibility, enable a patient to make efforts which were prevented before by the pain and stiffness of the limbs. Even without adding the physical force of the bone-setters, the pow-wowers did, in former times, what the former do now under the pretext of replacing limbs which have never been dislocated. The first step in the bone-setter's process is to rub the part for a very considerable time, and then to apply physical force in every direction, so as to break up adhesions; all of which is done with very little, often without any pain.

A medical friend having been a sufferer from neuralgia of the right arm, allowed the elbow-joint to become bent so permanently as to sensibly interfere with his qualities as a driver. At the end of two months he consulted me, and made great complaints of the attempts to put it straight. Finding this, I sat down and for half an hour made gentle downward passes over the sleeves of three garments, which he then wore. I was then able to straighten his arm without any pain, and having moved it freely, I enabled him to retain the entire use of it afterwards.

Another personal friend, who labored under a partial paralysis of the right leg and arm, complained that for more than three months he could not sleep for the first two hours after retiring, on account of a pain and stiffness in the right shoulder. It took him, as he expressed it, two hours to find an easy position for his limb. He had resorted to the usual remedies—sinapisms, frictions, etc., but without effect. At my first visit, under pretext of examining the state of the arm, I used downward passes until I could, without pain, move his arm freely in every way. That night he had no difficulty in disposing himself to sleep; nor did he suffer from the same cause at any subsequent period.

E. D. had suffered for at least two years with chronic neuralgia of the right hip, thigh, and leg, so as to be unable to use his limb without severe suffering. He could give himself only a very limited motion. He fell into the hands of a bone-setter, who in one hour reset, as he said, his limb. In other words, lessened his sensibility, and then gave free motion to the limb. From that time he was able to leave the house and attend to his usual occupations, which had been interrupted for some months. At the end of five or six months he had again an acute attack, followed by a chronic state of pain and lessened locomotion. He thought his hip was again displaced, and would have applied to the empiric a second time for assistance, but I explained the true source of relief, and having put my ideas into use, I also enabled him to resume his business, without professing to set a limb which was not displaced.*

I might here recite a greater variety of truly curious cases of mesmeric relief, where the distress arose chiefly from preternatural sensibility of parts, by which their common movements became painful. I might cite many others in which a sympathetic sensibility was abated, to the great comfort of the patient; but as these may be referred to the influence of moral causes, I will close this part of my subject by observing that I cannot see why the imagination should not aid a blister or a moxa as well as it does a "pass" or a manipulation. The imagination seems to be used by philosophers, as Brydone says the Sicilians do the Devil, to account for everything which they do not understand, or which they are determined not to believe.

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* Miss H., a girl of about twelve years of age, found the frequent occurrence of cephalalgia a serious hindrance, not only to her physical comfort, but her educational improvement. The various means in use for such affections were fruitlessly tried, and each attack caused the loss of a day, spent in bed, with marks of considerable suffering. Having been thrown into the mesmeric sleep for other purposes, while she was in one of her cephalalgic paroxysms, she found, on being awakened, that her headache was gone. This discovery was followed by a repetition of mesmerism whenever the recurrence of pain required it, and from that period she entirely ceased to lose either time or comfort from that cause.

Mrs. E., a lady of middle age, suffered for three days from a neuralgic pain of the right eye, accompanied by a loss of power to lift the eyelid, so that the eye presented the appearance of palpebral paralysis Perhaps, said I to her, you may be relieved by the application of the hand. The hand was accordingly applied in the mesmeric manner for exactly seven minutes, and the experiment was at once followed by an entire removal of the pain and immobility.

Nothing is more striking in this curious subject than the *total obliviousness of the events* of the mesmeric sleep. This is perhaps among the most invariable of the phenomena. Out of a great many subjects soporized magnetically by myself and others, I have seen only three who remembered the events of the sleep well. A few were able to recall a small part of these events; but the large, very large majority, were as ignorant of what transpired as of the contemporary events of the Court of the Grand Llama. Even when dreams of the most exciting character were induced, when the passages of a battle-field, or of a flight to the moon, were vividly depicted by the imagination, not a vestige of them remained in the waking state. The persons whose teeth were extracted, even when purposely alarmed during the operation, did not usually recollect anything that then occurred. This result showed itself alike in the initiated and the uninitiated, in the educated and the illiterate, in the adult and the child. So, natural somnambulists seldom recur *memoriter* when awake, to the actions of the sleep-walking state. We have the story of a miser who robbed himself when asleep, and whose new depository in an old wall was found by following him in one of his nocturnal rambles; of a man who was always found in the morning denuded of his shirt, and covered with soot; whose hiding-place, in a crooked chimney of an adjacent out-house, was found filled with blackened linens. Sauvage's patient, already referred to, "had not the least idea of what had passed in her paroxysm." I myself was once in a large company, late in the evening, at the house of a friend, when being near the door, I discovered descending the illuminated staircase, in her night dress, one of the female children of the family. I immediately passed out of the room, closed the door, and lifted up the child, whose eyes were wide open, and carried her to her chamber. She does not know to this day—and many years have since passed away—that she had placed herself in so critical a position. The disposition to walk in her sleep began on that occasion, continued for some years, and was finally cured apparently by a change of residence.

Cases of insanity and double consciousness are on record in which there remained the memory of the events of the sane and healthful state, while the patients, on recovery, lost all recollection of the diseased or unnatural condition. The following example is full of interest:—

W. M. was admitted into the Pennsylvania Hospital as a lunatic. He had fallen from a horse, injured his head, and lost his reason. In time he became better, and as his health improved his recollection of recent events began to fade. Supposing, therefore, that he would soon be well, to spare him the pain of seeing his misfortune too clearly, I requested his friends to remove him. They did so, and in a very short time he parted with all his malady and the recollection of its incidents. The hospital, the doctor, the treatment, were alike forgotten, and he looked confused when spoken to on the subject of his residence there. I often surprised him by speaking to him in the street. His friends were then told that if he should recover his recollection of the sickness, it would probably mark the onset of a new attack. About a year afterwards, sitting at table, he began to talk, for the first time, about the hospital, and said he would like to see Dr. Mitchell. The fire of insanity blazed anew in his eye, and in a few seconds he threw a fork at the master of the house. He begged to be brought to me, knew me instantly, and recited, with exactness, the events of his former illness. He was again cured, lost a second time the memory of his sickness, recovered it when a third paroxysm assailed him, and, finally, after several repetitions of his mental alienation, became fatuous, and died.

In every important respect, cases of natural and artificial somnambulism may be found to correspond with the case just detailed. In both, the memory of the natural state is carried into the somnambulism, while everything in the latter is forgotten when it passes away; yet, a repetition of the paroxysm brings back the recollections of the former fit. Thus, Sauvage says that the discourse of his patient "had relation to what she had said during her attack on the preceding day."*

In a case of natural somnambulism which I had the good fortune to be permitted to study, the successive fits were marked by a perfect recollection of the events of all preceding ones, while there was a total forgetfulness of them in the ordinary state. So it is usually in artificial somnambulism, there being the same paroxysmal memory, and the same waking forgetfulness.

The most curious part of this subject remains to be told. Magnetizers claim to exercise over the magnetized a supreme sway, so that they can control, as they

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* "I was once concerned," says Darwin, (*Zoonomia*, xxiv., 3, 3,) "for a very elegant and ingenious young lady who had a reverie on alternate days, which continued nearly the whole day; and as in her days of disease she took up the same kind of ideas which she had conversed about on the alternate days before, and could recollect nothing of them on her well days, she appeared to her friends to possess two minds."

allege, the obliviousness itself, and by an order to that effect, compel them to remember particular events to be described after being awakened. This they actually effect; but it is easy, by experiment, to show that the result is not owing to any power of the magnetizer, but to the repetition of the ideas, by which the attention is arrested and the memory aided. Just the same effect is produced by begging a patient to repeat, two or three times, some words or ideas, without giving any *order* to remember. I had an opportunity of observing exactly the same phenomena in a case of natural somnambulism, *where there existed no magnetic* RELATION. Command, or what was the same thing, repetition and directed attention, produced a perfect reminiscency. In this instance, therefore, the supposed power of the will of the magnetizer over the faculties of the patient stands refuted, being replaced by a power recognized in our waking state, that of repetition and attention.*

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* Obliviousness is a condition which no effort of any known faculty can produce. The very attempt, by pointing the attention, but increases the strength of the recollection. It is obvious that no honest person could entertain a forgetfulness, or imagine that he forgot. This phenomenon, therefore, of the mesmeric state, must really exist, or be assumed dishonestly. But as the parties, many of them at least, are of the highest respectability, and entirely without apparent motive to deceive, and the thing alleged is one about which they could not possibly be mistaken, or in which they could not deceive themselves, it follows, if there be any trust to be reposed in human testimony, that there is a state called mesmeric, which is characterized, among other peculiarities, by an oblivion of the thoughts and events which attend it. The very frequent appearance of this phenomenon, in the cases of all classes, ages, and conditions, is among the unanswerable arguments in favor of the existence of the mesmeric state. It is not

One of the most amusing susceptibilities of mesmerized somnambulists is that of being made to dream whatever the magnetizer, or *any other person*, may choose to describe *in a visible or audible manner*. Every absurdity of our ordinary dreams may be imitated in the mesmeric sleep. Nothing is too wild or incongruous for belief, and the astonished patients may be carried, in idea, through the fields of air, over lofty mountains, strange cities, mighty oceans, nay, to distant planets, other systems, and the world of departed spirits. Talk to them of the mightiest efforts, or the most inconsistent phenomena, and they have force for the one and credulity for the other. Here is an example:—

A girl from the country, ten years of age, having been mesmerized for the first time, *soon after breakfast*, was asked, by way of testing her progress toward sleep, what she was then doing. "Eating dinner."

"What is on your plate?"

"Spinage."

"What white thing, I mean?"

"Egg." (*Naturally associated with the idea of spinage.*)

"What is that dish?"

"Mutton."

"Why don't you eat it?"

"I don't like it. I like chicken."

"Who are at table?"

"Brother E., sister," and so on, accurately arranging the family according to the usual order at dinner.

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to be supposed that the rich and poor, young and old, educated and illiterate, without knowing one another, or the laws of mesmerism, have conspired together to disclaim all remembrance of the events of the somnambulous condition.

"Shall we go out?"

"Yes—I want to slide." (It was winter.)

"Take hold of my hands then and let us fly through the air."

"We can't do that."

"Yes we can, if you'll try. Now look down and tell me what you see."

The little girl drew a long breath, as if surprised, and grasped my hand with great force, saying, "Don't let me fall."

"What do you see?"

"Roofs of houses. I see down the chimneys; I see the fires."

"Shall we go down here and slide, or go to the Schuylkill?"

"Go to Schuylkill."

"There it is; what do you see on it?" (It was fast bound in ice.)

"I see a boat."

"Who is in it?"

"Brother A."

"Call to him to come to us."

"Brother! brother! He's coming—there he is."

"Shall we go with him?"

"Yes. Sit here; this is the place for ladies."

"What is that swimming *on* the water?"

"A fish."

"What fish?"

"A shad."

"But what makes it keep on the top of the water?"

"It is sick. Sick fish always do so."

"Listen to it. What does it say?"

"Poh! Fish can't talk."

"Yes, but this one does. Listen: don't you hear it?"

"Oh me!" (Assuming an air of surprise and attention.)

"What does it say?"

"It says A., A." (The name of her brother.)

"Why does it call him?"

"Because it is sick, and afraid he'll hurt it."

"What a strange fish! Look at its head. I don't wonder it is sick."

"I see nothing."

"What! are you blind? Look again! Don't you see that rose-bush growing on its head? See, there are several kinds of roses."

"Oh me! A., catch that fish. It is very strange!"

On being awakened, this little girl resolutely denied the truth of the story told of her, and was exceedingly vexed at the obstinate assertions, as she called them, of her brother and sister. When convinced of the truth by an appeal to me, she burst into tears, and was consoled with great difficulty.

In another case a respectable female was carried back, in a led dream, to a farm in Delaware, the place of her nativity and childhood. It was in the possession of strangers, and her kinsfolks had ceased to exist. The ideal journey was through the air, along the course of the Delaware River. Sho described its scenery, its boats, ships, and the line-of-battle-ship Pennsylvania, supposed to be then at Chester. She saw Wilmington, New Castle, Smyrna, and her native farm beyond it. There was then a long pause, and she sighed deeply.

Tears ran down her cheeks, and, sobbing painfully, she said, "Oh, how changed!"

The real time was ten at night, dark and wintry. The ideal scene was in the morning, and under bright sunshine. She was questioned about the employments, etc. of the people, and assigned them farm tasks well suited to the supposed period of the day. She described their number, dress, and arrangement, with plausibility, and lamented that things were less orderly and in worse repair than formerly. As she described the dwelling, I interfered, and directed her attention to a ladder, curiously colored, placed against the house. She said it belonged to a painter. "What makes him paint the house cross-barred?" "He don't." "Look! are you blind?" "That's odd." "Raise your eyes! You see another house atop of the first, and of a gold color. On that, in a huge tub, grows a tree of a thousand hues. How came it there?" There was a long pause, intense expression of surprise, and a shudder. "Let us get away from this place. It is possessed." And I was compelled by her strong emotion to make her suppose herself in Philadelphia; which I did by saying, "What are you doing in Chestnut Street?" The question seemed at one blow to scatter the fearful vision, and she looked confused. A repetition brought the answer. "Going a shopping." The airy nothings had melted away, and with her new dream came her usual look and tone.

Similar experiments made on a natural somnambulist produced like results. In one such instance, as we flew in imagination through the air, I said, "Take care, or you'll strike your ancles against the State-house steeple. Its point is sharp." The somnambulist drew up her limbs suddenly, and said, "That was a narrow escape."

On the authority of a leading member of this College, I state the following facts. More than forty years ago, when but a lad himself, he discovered in his college chum a disposition to talk and walk while asleep. Some experiments were made on his case, and, among others, this: being led to the side of the bed, he was told that there was water before him, and was advised to plunge in and swim; which he did energetically, to the no small amusement of the by-standers. The gentleman from whom I derived this case is not a magnetizer, and has not seen, I believe, a single case of mesmerism.

Every mesmerized subject cannot be thus led. Some of them, though unable to open the eyes or to move the limbs, and who are even insusceptible of pain, never lose their consciousness of place, time, and position. Such cases are not frequently met with, but they resist all attempts to lead the mind to fancied scenes or places.

Another strong argument in favor of the reality of the mesmeric state, is afforded by the character of the dreams of magnetized persons. Those wild and chimerical scenes, those absurd conversations, those imagined superhuman powers, which are so easily brought with the conviction of reality to the minds of the mesmerized of all ages and classes, afford an undeniable evidence of the existence of a condition which is very peculiar. We know nothing of human nature which entitles us to suppose that so many human beings, untaught in the matter, could or would affect to believe, without any supposable motive, whatever absurdity we might choose to propose, however silly or ridiculous that belief might make them appear to be.

The *time* required to place susceptible persons in the mesmeric state is very various; but, except in a very few cases, is much shorter than is usually supposed, as the following table will verify:—

Table of Time required to cause Mesmeric Sleep and to Awaken.

No. of Case.	To sleep.	To awaken.	Remarks.
2	24 minutes	1½ minutes	It will be seen that the
3	24 "	1½ "	shortest time to mes-
7	15 "	...	merize was 2½ minutes,
10	9½ "	...	the longest 25 minutes,
23	5 "	...	and that the mean time
33	3 "	3 minutes	is between 10 and 11
46	13 "	...	minutes.
56	20 "	...	
59	10 "	...	
60	10½ "	...	The time requisite to dis-
63	5 "	...	solve the mesmeric spell
64	25 "	...	is, when shortest, 1 mi-
65	15 "	...	nute, when longest. 4
70	8½ "	...	minutes. Mean time, 2
71	10 "	...	minutes nearly.
74	7½ "	...	
75	8 "	...	
76	8½ "	...	The proportion of the time
77	5 "	...	to mesmerizing is as 5
78	3½ "	...	to 1 nearly.
80	12½ "	4 minutes	
83	2½ "	...	
84	6 "	...	It may save time to fu-
108	10 "	...	ture observers to re-
117	10 "	...	mark, that when no
118	10 "	...	obvious effect is pro-
119	7 "	1 minute	duced within half an
120	17½ "	2½ minutes	hour, the patient sel-
121	15 "	...	dom falls into the mes-
123	7½ "	2 "	meric sleep by a more
124	11 "	...	prolonged effort.

The uniformity of the time required by some subjects to fall into, or to come out of the mesmeric sleep, is remarkable. Mrs. B., a lady of excellent character and refined manners and education, always, after the second experiment, fell fully asleep in twelve minutes; the time never varying more than half a minute, usually only five or six seconds. The awaking occupied four or five seconds more or less than four minutes. In her case distance produced some effect. Sitting in ordinary position for magnetizing, she awoke in four minutes and four seconds. Sitting back to back, but in contact at the right shoulder, four minutes and thirty seconds. Facing her at six feet distance, seven minutes and thirty seconds. Facing her at twelve feet distance, sixteen minutes and forty-five seconds.

Second experiment.—In position, four minutes and seven seconds; difference three seconds. Back to back, four minutes and thirty seconds; no difference. Six feet off, seven minutes and thirty seconds; no difference. Twelve feet off, sixteen minutes and fifteen seconds; difference, thirty seconds. (Dr. Bridges present during both experiments.)

The following table illustrates another point of interest; the time which is required to terminate the mesmeric sleep when the patient is entirely undisturbed:

Length of the Sleep when undisturbed.

No.	Time to put to sleep.	Time to waken by passes.	Length of sleep when undisturbed.
Mrs. B., (80) aged 21	12½ min.	4 min.	40 and 45 min.
E. M., (82) " 35	10 "	2 "	1 h. and 50 "
E. B., (118) " 22	10 "	2½ "	1 h. and 59 "
S. E., (71) " 21	6 "	1 "	4 hs. and 45 "
M. H., (59) " 21	10 "	3 "	30 minutes.

Of five cases of the most perfect somnambulism, one awoke spontaneously in thirty minutes; one in forty minutes at one time, and forty-five at another; one in one hundred and ten minutes; one in one hundred and nineteen minutes; and one slept for two hundred and eighty-five minutes, or four hours and forty-five minutes. No one of them slept as long as natural sleep usually lasts without interruption in young people. The fears often expressed lest a somnambulist should not awake in the event of the accidental absence of the magnetizer, are therefore not well founded.

I have not been able to perceive that sex exercises any marked influence over the mesmeric susceptibility. Of the number of males subjected to experiment, quite as many, proportionally, as of the females, fell into the state of artificial somnambulism. Thus, of twenty-four males, six were mesmerized; and of eighty-seven females, twenty-one were thrown into an artificial somnambulism; of males, one in four; of females, one in 4·1 nearly.

One of my friends, who made thirty-three experiments, reported that, of eighteen males, three were magnetized; and of fifteen females, three also; being in the proportion, for males, of one in six; and for females, of one in five.

Age is a more modifying agent than sex. Thus, of eighteen males, whose mean age was 24·8, three were magnetized, whose mean age was eighteen; while of fifteen females, whose mean age was 20·8, three were magnetized, whose mean age was 16·3.

Though age, therefore, seems to affect sensibly the results, no age appears to present insurmountable obstacles to the mesmeric influence. I have seen in the mesmeric state a child four years of age, and an old lady of sixty-five. The period of puberty, however, seems to be most favorable to the magnetic influence; the greater number of cases being between the ages of twelve and twenty. Thus, of eighteen males, subjected to experiment, only three were twenty years old or less; fifteen were upwards of twenty; one was forty-six; one thirty-five; one thirty-two; one thirty; and one twenty-eight. The successful cases were twenty-two, sixteen, and fifteen years old respectively.

So of the fifteen female cases, the somnambulists, three in number, were of the ages of thirteen, sixteen, and twenty.

Of thirty-one somnambulists, found in another table, eighteen were under twenty; seven between twenty and thirty; four between thirty and forty; and two over forty.

The *proportional number of persons susceptible of the mesmeric influence* is variously stated by authors. Some writers say one in three, others one in five; but few of them give a less proportion. This discrepancy arises probably from several causes, such as the loose method of keeping the account, the age of the patient subjected to experiment, the difference in the views of the tests of somnambulism, etc. etc. It has not happened to me to meet with any statistical tables on this subject, so that I have no means of making exact comparative estimates; and, therefore, what I offer must be considered only as the first step toward the truth.

At the Orphan Asylum of this city, thirty-six persons were subjected to trial, of whom ten fell asleep, each within a period of ten minutes; that is, one in 3·6. Of these, only two were somnambulists; of the rest, five presented an uncharacterized sleep; while three of them had the same kind of sleep with accelerated pulses, cold clammy hands, and flushed faces. The results, therefore, may be thus stated:—

At one sitting of ten minutes, somnambulism is produced in one person out of eighteen; sleep of an unmarked character, in one in seven; and light sleep, with the mesmeric disturbance of heat and circulation, in only one in twelve. The following table shows the age, sex, etc., of these cases:—

No.	Age.	Sex.	Remarks.
1	11	Male	No other effect than nervous twitches.
2	11	"	Asleep in 2½ minutes; pulse 70 before; 102 during sleep; awoke on being called to; hands cold, clammy, and livid.
3	10	"	Time 6 minutes; pulse 64–112; hands cold and clammy; awoke on being spoken to.
4	11	"	10 minutes; no effect of any kind.
5	10	"	10 " no effect.
6	10	"	10 " "
7	9	"	10 " "
8	9	"	6 minutes; apparently natural sleep; awoke on being spoken to; no physical changes.
9	10	"	No effect in 10 minutes.
10	9	"	" " "
11	10	"	" " "
12	16	Female	10 minutes; sleep apparently natural; no peculiarity.
13	13	"	10 minutes; no effect.
14	12	"	10 " "
15	11	"	10 " "
16	10	"	10 " "
17	10	"	10 " "
18	10	"	10 " "
19	10	"	10 " "
20	10	"	10 " "
21	10	"	10 " "
22	11	"	10 " "
23	11	"	10 minutes; natural sleep, or no peculiarity.
24	10	"	10 " natural sleep, or no peculiarity.
25	9	"	10 " no effect.
26	13	"	10 " natural sleep; second experiment, 5 minutes; same result.
27	10	"	10 minutes; natural sleep.
28	9	"	No effect in 10 minutes.
29	24	"	No effect.
30	10	"	10 minutes; sleep with physical changes; pulse 84–96; awoke on being spoken to.
31	17	"	No effect.
32	17	"	No effect.
33	18	"	No effect.
34	15	"	No effect.
35	14	"	Profound somnambulism; tooth extracted; severe pain; does not awake.
36	22	"	Profound somnambulism; tooth extracted; no apparent consciousness of pain.

An inspection of the foregoing table reveals to us some curious facts. George McKnight, a boy eleven years of age, fell asleep in two and a half minutes, in which time his pulse rose from seventy to one hundred and two, and his hands became cold, clammy, and livid. Immediately on being spoken to he awoke, and his pulse almost instantly fell to seventy-six. Charles Thomas, aged ten, fell asleep in six minutes, and his pulse rose from sixty-four to one hundred and twelve; his hands were cold and clammy, but not livid. He also awoke on being spoken to.

Thos. Turnbull, slept in six minutes, without marked physical changes. He awoke on being spoken to.

Eliza Jane Cooper, aged thirteen, who fell into an apparently natural sleep in ten minutes, at the first experiment, was thrown into the same state in a second experiment, on another day, in five minutes.

Marion S. Davis, aged fourteen, and Ellen Brokers, aged twenty-two, were profoundly somnambulized in ten minutes, and from each a tooth was extracted without the knowledge or consent of either. Marion Davis felt the operation sensibly, while Ellen Brokers manifested no sense of pain whatever.

Persons may easily differ in the statement of results drawn from such a table. That any one should fall asleep within ten minutes, indeed in from two to six minutes, when placed by a stranger, for an unknown purpose, in an upright posture, and subjected to a novel examination, is a fact so curious as to entitle us to supposo the condition unnatural. We might, therefore, without a stretch of credulity, assume that all these sleepers were mesmerized, and that the proportion of susceptibility is of one in 3·6. But for the sake of greater certainty, if we select only the cases in which the physical changes accompanied the sleep, the proportion will be five in thirty-six, or one in 7·14; while if we admit as mesmerized only the cases of profound somnambulism, the proportion will be reduced to two in thirty-six or one in eighteen, in a period of ten minutes, and at the most susceptible age.

In another table of my private experiments on one hundred and eighteen persons—which for obvious reasons cannot be given in detail here—the proportion of mesmerized persons is, to the whole number tried, as one to 7·14, a number corresponding exactly with that at the Orphan Asylum, when the cases marked by physical changes were alone admitted to be mesmeric. In these one hundred and eighteen cases the time was not limited, and the cases stated as mesmerized were all thoroughly somnambulous.

A friend who has pursued this subject with some zeal and success, made out, at my request, from recollection, a table of the cases tried, and the results. The whole number was thirty-three; the somnambulous cases, six, or one in 5·5. Trusting, as he did, to memory, he could not easily forget his successful cases; but he might, and no doubt did, as he himself supposes, forget some of the others, so that his proportion would not probably differ widely from mine.

At the asylum for the deaf and dumb, where experiments were made by a friend, the whole number tried was forty-nine; the number mesmerized more or less completely, was fourteen, or one in 3·5. As the tabular statement at the Deaf and Dumb Asylum was not kept in much detail, the cases profoundly affected have not been carefully distinguished from the rest. It will, however, be perceived that the Orphan Asylum and the Deaf and Dumb Asylum, when all the cases strongly or weakly mesmerized are taken, present nearly the same average, being 3·6 for one, and 3·5 for the other.

A general average gives us one in seven nearly, or exactly one in 7·14.

The Marquis Guibert magnetized, for humane reasons, the diseased paupers in the vicinity of his château of Font-château. The results for six years, from 1834 to 1840, are as follows:—3315 patients, of which 1194 were men and 2121 women. Of the males, 424 were affected to the degree of being unable to open the eyes, while 157 of that number were magnetic somnambulists, or one in 5·04. Of the females, 1279 were affected to the degree of closure of the eyes, while of those, nearly one in three were magnetic somnambulists. Combining the results we have, for both sexes, one in 3·73, or nearly one in four. (*Resultat des Opérations Magnétiques Tarascon*, 1840.) More than one-half of the patients of the amiable and unsuspicious Marquis were cured; many of them of old and obstinate disorders. The experience not only here, but elsewhere, on both sides of the Atlantic, makes the statement of the Marquis rather improbable, and leads to the suspicion that his poor neighbors found in his credulity something more substantial than his cures. For a similar reason we cannot admit the truth of the apparently prodigious numerical ratio of his somnabulous cases.

While the number of persons succeptible to magnetic influence is so small, the proportion of those who can act as successful magnetizers is apparently very great. When a person has been once proved to bo susceptible, it is difficult to find any one who cannot mesmerize him. I have seen the most feeble and delicate females, young children, and aged persons, operate successfully on those whose liability has been tested. But as they who have been mesmerized have the power of aiding the process in subsequent experiments, we may be deceived as to the universality of the magnetic faculty. It is only by seeking new cases that the proportion in this respect can be established. Yet we may reasonably infer its general existence from the fact, that magnetizers, when their products are carefully examined, do not seem to differ in power from one another more than people usually do in sight, hearing, muscularity, or any other physical quality.

Many persons when foiled for several successive times in their earliest attempts to magnetize, usually despair of success, and infer their own incapacity; while those who stumble at first on susceptible cases, are disposed to believe themselves possessed of uncommon powers, and make their boast accordingly. To "cut boldly," as the soothsayer said to the Roman king, seems to be the chief secret of the act of magnetizing. Confidence can alone enable us to fix attention, and persist in the mental effort and self-restraint essential to success. This is the *faith* so often spoken of by the mesmerists; a property not needed by one whose mental discipline enables him to perform the act perfectly, even without confidence in its result.

Neither is faith necessary, or even important, on the part of the subject of a first experiment; since it cannot aid him to reach a state whose conditions and path are yet unknown to him. In subsequent operations such faith is probably a great auxiliary. Sometimes, perhaps, it and habit are the sole agents; as they were probably the active cause of those phenomena which the

Franklin Committee of the French Academy attributed to imagination and imitation.

The writers on animal magnetism seem to have drawn so largely on the imagination as to entitle their works to little respect. Among the other false statements made by them, is that of the greater susceptibility of invalids to the mesmeric influence. That such persons are, for obvious reasons, most frequently mesmerized, is true; but that they yield most readily to that influence is far from being true. Most of the somnambulists whom I have seen, have been in very good health. Thus, of twenty-six somnambulists, nineteen were in good and seven in bad health. In general I have been disappointed in my endeavors to magnetize the sick; the disease appearing to fix the nervous system for the time unalterably. Some of the most readily mesmerized cases were persons of the finest health, of a bright and sanguine complexion and buoyant temperament. It is somewhat curious that all my attempts to magnetize natural somnambulists have utterly failed. The opinion of the mesmerizers, refuted in this paragraph, was convenient for those who asserted the universality of the mesmeric influence, as the failures were attributed to the great health and superior strength of the subjects.

The peculiar relation (*rapport*) supposed to exist between the operator and his magnetized subject, has been one of the most striking, as well as mysterious phenomena of mesmeric exhibitions. The person magnetized is apparently deaf to all other voices, insensible to all other hands, and inattentive to all other questions. He seems rapt in an exclusive abstraction, the subject of which is the magnetizer, to whose will he seems slavishly obedient. This opinion, almost universally received by the believers in animal magnetism, is to them, in public exhibitions, a source of the greatest convenience. According to it, no one can speak to the subject without their consent and aid, and when the privilege is abused, it may be at once withdrawn, and a too rigid scrutiny cut off. It may be used, too, to account for mistakes and failures; for the "rapport" becomes then imperfect; the relation has been by some accident interrupted; and therefore the patient fails in his attempts at miraculous revelations. I had pursued my investigation of animal magnetism long before I met with a single case of this exclusive relation. The phenomenon so invariably found by others, so essential a part of the system, made no part of my magnetism. I saw in the magnetic sleep seventeen somnambulists, not one of whom offered this universal condition. I then taught a patient to believe that such a relation was an essential part of the magnetic system, and she went to sleep with that impression on her mind. To my surprise, I saw in that case the first example of "rapport," and perceived that it was voluntarily imposed on the patient through her belief of its necessity. Soon after reaching this conviction, I was asked to look at a case in the hands of an esteemed medical friend, who promised to prove the absolute incapacity of his subject to hold intercourse with another without his consent and assistance. The patient resisted all ordinary efforts, nay, many extraordinary ones, to make her hear. She who answered the whisper of the magnetizer, seemed not to hear the loudest cries of others directed immediately into her ear. Struck with the peculiarity of the case, I disbelieved that she slept, but, on examining the physical signs, I found her head and face hot, her hands cold and clammy, and her pulse above one hundred. I inquired of the persons around her, how stood her character, and learned that she was entitled to confidence. I could not, therefore, suppose that a mere trick was to be played, but I did suppose that the sleeper might labor under hallucination, the product of nervous disturbance and mental excitement. To test this, I took an opportunity, while the magnetizer's back was toward us, to explode a percussion cap within three inches of her ear. She started violently, but without opening her eyes. The operator, at my request, then questioned the patient thus: "Did you hear any noise?" "No." "Why then did you start?" "Something struck my neck." "But just now you were insensible to touch from others; how is that?" Before she could give an answer to this query, I struck my palms violently and unexpectedly together near her ear, at the noise of which she again started. "You start again." "I did not to my knowledge." "Did you not hear anything?" "No." At this moment she was again startled, and gave the same answer. The magnetizer, who readily admitted the signs given by the patient, explained them by supposing that he unconsciously aided the noise by his volition.

On the following day, after some conversation with the same young lady, whom I found well educated, sensible, and frank, and after having examined her pulse and respiration when awake, I was permitted to somnambulize her myself. She slept in twenty-three minutes, and presented the usual physical and mental signs of being thoroughly mesmerized. As soon as I pronounced her to be asleep, the former experimenter took her by the hand and vainly endeavored to extract from her an answer to his questions. She seemed deaf to him, but answered me readily. This I was told would continue until I willed that she should hear and answer him. Looking at my watch as if to count her pulse, and making no sign of my intentions, I firmly willed that she should hear and answer her questioner, who, at my request, continued to ply her with interrogatories. No answer came although ten minutes were thus spent. He then said, "Will." "I do," replied I, and in an instant she was in communication with him and responded readily. Now, unless she had heard his request to me, she could not have understood my answer, which plainly proves that she heard, but was unwilling to answer until *she supposed she could.* On further inquiry I learned that previous to her first somnambulism she had made herself well acquainted with the theory of mesmerism.

After these experiments I saw several cases of the same kind, in most of which the startled patients resolutely denied having heard any noise. In all of them it was only necessary to assure the subjects that they could hear and answer every one who addressed them, to do away with the whole system of magnetic "rapport." Indeed, my medical friend of the first experiment, told me that by such a course I had totally spoiled his patient for him, the peculiar relation and its striking consequences having disappeared. Of the truth of the opinion for which I now contend, I convinced a sturdy believer in all the art and mystery of magnetism, by making him tell his patient that she could hear and answer if she would, while he willed strongly that she should not. In every instance the result was according to my expectation.*

* A distinguished philosopher, living in this city, was witness

How shall we account for this curious mental state? Is it mere willfulness, or is it a supposed inability to hear and answer? Nothing of the kind presents itself in natural somnambulism, where the patient answers either no one, or every one alike. Two cases of insanity, which fell under my notice, may serve to throw some light on this question. One of these, a female, at the Pennsylvania Hospital, lay in bed for many years, physically able to rise, but kept in bed by the supposition that she was up, but that they who begged her to rise were themselves in bed. Nothing could be done to make her sensible of her true position, and when told to get up, she replied, "Yes, get up—why don't you?" It is not possible, in our sane state, to conceive how she could transpose, in idea, herself and the speaker, and imagine that it was she who spoke when she was spoken to; and that her answers were the words of another person. The hallucination lasted for many years.

The other patient was a male, who imagined that he could hear only three persons in the world, one of whom

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to the following scene:—A young girl, ignorant of the subject of mesmerism, having been mesmerized, was observed to answer readily every questioner. One who knew mesmerism theoretically, asked for an explanation of the supposed anomaly. "She will answer you no longer," I replied, and he found her, to his amazement, deaf to all his cries. I had not willed that she should answer, in the first instance, nor did I exert an opposite volition in the second. It was enough to inform the dreamy being of the expectation, to cause its fulfillment, as if the mere expression formed a law of her being.

On one occasion, when the "rapport" seemed strongest, I shook violently a patient, and said, "Answer me immediately—you can if you choose." To the surprise of her magnetizer she gave me instantly a response, and continued to hold converse with me.

was myself, then his physician. Every artifice was used to extract answers to the interrogatories, or attention to the noise made by others. He seemed stone-deaf to them, and looked as unconscious as if he really heard nothing. Everything was in keeping with his delusion. We cannot suppose that the pulsations of the air did not reach his tympanum alike, from whatever source they came, nor can we think that they did not act alike on his perceptivity; yet, the mental reaction on them was widely different. Either he possessed in his denaturalization a superhuman power over his attention, a preternatural force of abstraction, or he gave attention to all, but a manifestation of it to few. Now, I have observed in most mesmerized persons, a surprising tenacity of impression, and a kind of incapacity to reason against errors which they really perceive. It is only necessary, as I have shown in the remarks on dreaming, to insist strongly, to force the patients to believe anything, however absurd. That they hear what passes around them is obvious, since they may be made to smile or weep by a tale of mirth or sorrow; but, if not told by the magnetizer, it passes through the mind as a dream, and not as a narrative. They seem to believe, with indomitable force, that they can hear only him, and therefore delude themselves as to the source of ideas introduced by the language of others, and, like the lunatic, live only in their impressions. The false confidence reposed in really respectable patients by those who know them well, obtains for the "rapport" a ready belief; by which, these patients unconsciously obtain much of that intelligence, which they as unconsciously retail to the wonder-stricken inquirers after the scenes and events of a distant home. A worthy man or woman falls into the magnetic state and obstinately refuses to answer the by-standers, declaring thus a state of total deafness. Emboldened by belief in this, persons talk freely of the schemes by which they intend to try the powers of the sleeper, and having thus armed her, they start astonished at her prowess. They not only do this, but they offer leading questions, which are answered ambiguously in generals; other questions lead more directly, and thus a strong case is made out. If mistakes arise they are ascribed to some interruption of the "rapport," and a fresh start is taken. As the relation to the magnetizer is presumed to be the most perfect, the questions are generally put through him, and he is thus enabled to lead the patient and mislead the spectators. This is often done, I sincerely believe, without any design to deceive, where the operator is a dupe to his own art, and the patient is in a state of double consciousness, divested of his usual character, and, like all dreamers, ready to believe the most absurd and incongruous things, and to suppose himself capable of the most astonishing feats.

In other cases the magnetizer is doubtless a knave, and the mesmerized person, if really asleep, able to conceive and carry on a scheme of deception. According to their own confession to me, some of the exhibitors present wonders which they admit to be beyond the powers of mesmerism. Take away the supposed "rapport" and cautiously address your questions to one of these clairvoyants, and, as I have invariably done, you will break down his supposed powers.

In some cases the *rapport* is the result of an inability to direct the attention. The subject goes into the mesmeric state with his attention, his whole attention, fixed on the magnetizer, or he goes into a profound stupor, losing even that object of advertency. In the first case without effort, in the other by mesmeric counter-passes, the patient is attentive to the magnetizer. That attention is not under the immediate and ready control of the patient, and therefore waits the direction of the operator, who may place his subject in *rapport* with any one, or with every one at once, by *any expression of his wishes*, by word, or sign, or pass.

The modification of the condition of the senses by the mesmeric state is equally curious and instructive. Sometimes the patient is thrown into a complete coma, a stertorous insensibility. That is, however, rare. More commonly the senses of sight, hearing, and touch, are either unimpaired or improved. Indeed, the light often becomes painful even through the closed eyelids, and intolerable to the opened eye, while the sense of hearing is so acute as to enable a mesmerized subject to perceive sounds totally inaudible to common ears. Thus, a patient could hear, across two long parlors, the scratch of a pin against a door, though inaudible to the individual who applied it. Such exaltation of the senses is not unknown to medical men, who now and then meet with it in certain, so-called, nervous disorders. Most of you have seen patients who, without any apparent affection of the eyes, have been obliged to exclude light from the chamber, and have been able even to read in a room in which others could scarcely discern the largest object. Again, the hearing sometimes becomes so acute as to enable the patient to catch the gentlest whisper from an adjoining room. The late Dr. Parrish once attended a gentleman who could hear the import of a consultation held in the apartment beneath his chamber.

Such phenomena have led to the belief in the existence of a sixth sense; and the opinion has been apparently confirmed by the experiments made on some of the lower animals. Thus, Spallanzani found that the bat pursued his flight as well and as safely after his eyes were extinguished as before, and presumed that he was guided by some unknown sense. Juvine, of Geneva, thought its power depended on the sense of touch, highly exalted by the distribution of numerous nerves to the upper lip, jaw, and external ear; while Cuvier supposed that the bare membrane of its extensive wings, was the seat of the delicate tact by which it received notice of its approach to solid obstacles. Sir A. Carlisle, by the simple experiment of closing its ears, found that the bat owed its power of directing its flight and avoiding impediments, to the refinement of its sense of hearing. That once lost, the little animal struck against everything in its way. Seeing such discrepancy of opinion, I repeated these experiments, and found that Sir A. Carlisle had reached the proper conclusion, for the bat on which I operated flew as well without eyes as with them, but ceased to be able to avoid obstructions when his ears were plugged, or his organ of hearing destroyed by a probe. The exquisite audition of the bat was shown by his easy and safe flight when blinded, even amid the most perplexing labyrinths. When he was in a large room, his circular flight was conformed to its dimensions, and when he entered a long hall, he flew from end to end of it without touching a single obstruction. A hat or even a stick held up in his path, was skillfully avoided, and his flickering flight, on approaching such little impediment, was truly marvelous. In a second suite of experiments I did not disturb the eyes of the bat, but contented myself with destroying his sense of hearing, when, to my great surprise, I saw him lose entirely his power of avoiding the walls and other obstructions of the apartment. His eyes did not seem to aid him, even in a darkened room, for he was then heard to strike against the first obstacle met with in his flight.

I have insisted on this point, because I shall make it curiously applicable to the explanation of some mesmeric pretensions. While the sight, touch, and hearing are often greatly improved, the gustatory and olfactory nerves appear to lose almost universally all their powers. I have seldom met with a somnambulist who could smell or taste. The strongest ammonia, the most pungent pepper, the most exciting snuff, usually fail to produce any impression except that of touch. The bottle is perceived at the nose; the pepper, or salt, or sugar, is taken for sawdust, or coal-ashes, or sand, because of its texture and absence of taste. It is perceived by the sense of touch, but not of taste. When, for the first time, I discovered this distinction, I was of course surprised, for I could not see any reason for the difference of the effect on the different senses. A little reflection, however, pointed out a natural distinction on which the difference is probably founded. The sight, hearing, and touch are purely mechanical senses, and probably vision and audition are mere modifications of touch. But taste and smell are chemical senses, and therefore not likely to be influenced by the same causes as those which are mechanical. To illustrate the distinctions here taken, observe the following case: A negro boy, aged eleven, is placed in the mesmeric state; a sweet cake is given to him. He, at my request, examines its size, shape, color, and the sugar on the surface; he looks at it, feels it and begins to eat it; he spits it out. "What is the matter?" "It has no taste; it is a trick on me; it is made of sawdust, with flour on the top." He says he likes Cologne water; strong ammonia is given in its stead; he applies it to his nose, and declares it to be only water, for it has no smell; to prove which he tastes it before I can hinder him, and persists in his declaration. The experiment made his mouth very sore for several days afterwards.

On these modifications of the senses depend some of the apparently preternatural powers of mesmerized persons. Thus the acumination of the sense of sight enables the somnambulist, natural or artificial, to see under very unfavorable circumstances, as to light and position, while, as in the case of the bat, the extreme delicacy of hearing supplies the absence of sight, when the patient is blindfolded, or has the objects placed behind him. Without knowing the reason, the exhibitors of mesmeric wonders endeavor to aid the supposed clairvoyance by feeling the object to be detected, so that the friction sound suggests, even to an honest subject, images of sight; and the object is thus often guessed at by means undetected by any one of the parties concerned. For this reason the avoidance of whispers or of any other sounds always defeats the experiment. An exhibitor is told in a very low whisper to carry his subject to Paris, or to London, as the case may be. The whisper is heard by the subject, and the work is soon done. A card is lettered and handed up. Those who convey it have shown it to their friends, and its import is whispered, or the magnetizer, to enforce the mental effort, repeats, so as to be inaudible to the by-standers, the words. They are heard, however, and the answer is given. The following scene will illustrate this better. The clairvoyante is of the best kind as to talent and character; no one can suspect her of fraud, for she is very honest and apparently without motive to deceive. She readily tells the names of the articles held behind her while her eyes are well covered. Each person takes his turn in "rapport," and seems convinced that she sees without eyes. My turn comes, and as the family, a private one of high respectability, are full of belief, I am permitted to manage the experiment in my own way. I begin by lifting a stick from a corner of the room; she cannot name it. I run my fingers along it, so as to create a slight sound; "It is a cane." My spectacles are held up noiselessly; she does not form an image. I rub the glasses very cautiously, and go over the other parts in the same way; no image. I give them a shake so as to produce a sound; "They are spectacles." The ornament is brought to me from a mantel-piece; it makes a sound when displaced, and is named at once. I softly lift a pitcher of water; no answer. I drink; "It is a pitcher, and you are drinking."

A public exhibitor permits me to examine the most expert of his subjects, for he does not doubt their powers himself. I place them in the mesmeric state, or they seem to be so placed. I stand behind them and hold up a white *blank* card, on which I had previously pasted a large red wafer. "Do you see this card?" "We do," (two subjects.) "What is its color?" "White." "Very well; what is on it?" "Letters; a name." "What name?" "M-i-t-c-h-e-l." "Very well; what more do you see on it?" "That's all." Laying down that card, I take up one colored red, and say, "This is another card, and it has a color; what color?" One, "Green." "No." The other, "Yes, blue." "No." Both together, "Yellow." I then held up a new *red* spectacle-case; they said it was *yellow*. The other similar experiments met with the same fate; all of them failing utterly; and yet, immediately afterwards, the exhibitor having been for form's sake put into *rapport* with them, made many similar experiments very successfully; *how*, I know not. It may not be from the purpose to say, that I have for five years sought every opportunity of verifying this pretension, but without success, whenever due care was taken to avoid sources of error. During that time I have seen a great many persons who were esteemed good subjects of *clairvoyance*.* I have written to experimenters at a distance, begging them to unite with me in a decisive trial of the powers of their subjects who had acquired great celebrity; but always in vain. They either declined the proffer or failed in the attempt. Among others of this kind, I placed in

* The following anecdote will show how easily they who wish to believe can overlook defects of evidence. An intelligent and most worthy friend of mine had persuaded himself that a very young subject of his could read and describe the figures in engravings with her digital extremities. When I saw her, she was mesmerized, and blindfolded; the Penny Magazine or Cyclopedia was before her. "Read the German text," said he. "Dutch letters," said she. "Yes." She ran her fingers along the margin of the side of the book; then along the top, descended to a level with the letters heading an article, and began to repeat them. I thought I saw the artifice, and, after a moment, inverted the book, and begged her to do it again for me. By a similar movement, she found the *supposed place of the letters*, and tracing them ideally an a *blank margin or footing*, repeated them as before, to the great surprise of some and the infinite amusement of others. In the same way she could be made to describe figures where there was only blank paper. I need scarcely add that all such pretensions or self-delusions fail when scrupulously examined.

nine glass tubes some words covered with two, three, and four folds of paper. These tubes I sealed hermetically, weighed them accurately, took their impression in plaster, and numbered them from one to nine with a diamond pen. They were then sent to a distant place, where several persons were distinguished for *clairvoyance*, with a request for their return with answers as to their contents. *No answer, no tubes were ever returned to me.* I proposed another test to the same parties, begging them to begin on a given day, and tell me for five successive days what object was on each day on my office table, and mail their answers daily. One out of three to whom I wrote made the attempt, and informed me that his subject got into my office, but instantly saw something alarming, and fell into convulsions. A second trial gave the same result. To a famous experimenter I observed, that if his patient could see half as well at a distance as reported, he could easily make a fortune by informing himself of prices in other cities and countries, and buying and selling accordingly. "Oh, sir," said he, "the practice of magnetism makes men too disinterested and benevolent to enable them to profit by the misfortunes of others."

I have seen few mesmerized persons who could not be made to enter upon an ideal journey, and see in imagination men and things at a distance; and still fewer who did not imagine they saw the objects to which their attention was called, whether held in the hand or not; whether really present or not. An example of such a case will perhaps amuse, if not instruct. R. O., an elderly woman, being mesmerized, was asked to look at what I held in my hand. It was a watch; but it did not go. She, having no clue, guessed that it was "a hat."

"No." "Oh, it is a cane." "No." "Why, I must be blind! let me see; it is a tumbler." At that moment some one drank a glass of water. "Yes, it is a tumbler, and you are drinking." "No, it is not a tumbler," said I, and by shaking the watch I caused it to go. She immediately heard it, and exclaimed, "Oh, I see it now! it is a watch." Having laid aside the watch, I held nothing in my hand, but requested her to name what I held. That the images she called up were the result of her hearing, was made evident by this, that she suggested in succession the things which the bystanders moved about or handled audibly. This patient had been long confined to the house, and had not for many years seen the remote part of the city in which she had spent her earlier years; and it had undergone an entire revolution. Being taken ideally to that place, she described it minutely as it *had* been; peopling the streets with ragged children, dogs, and horses, and filling the little shops with old-fashioned articles. We tried in vain afterwards to make her see the *actual* and better buildings of the place. She stuck firmly to her first impressions. Some Spaniards from Cuba were present, who directed her dream toward their tropical home, which they said she described so well as to make them believe she really saw the place. A cool observer, however, could perceive that each idea was sufficiently suggested by the interrogators; and I, to prove this, took up the dream where they left it, and created, by questions, such wild absurdities as left them in no doubt as to the truth. Perhaps there is no better proof of the reality of the somnambulism, than the wild sports and amazing incongruities into which the sleeper's imagination may be led by any one who chooses so to direct it.

If I could rely on the testimony of mere lookers-on, I might collect a volume of evidence in favor of a miraculous lucidity; but such testimony, even under oath, cannot be trusted, as the following circumstances will show: A distinguished divine, who had visited a celebrated clairvoyante at some distance from his place of residence, assured me that she had told him correctly of his house, his furniture, the architecture of his church, and other matters of which he did not believe she could have had any previous knowledge. On a cross-interrogation he admitted that two out of three of her answers were wrong; that she often required several questions to extort an answer, and not unfrequently answered in a general and ambiguous way. Moreover, she had connections who knew him and his affairs intimately, and as she expected a visit from him, might easily have obtained all the knowledge of him necessary for her purpose. But as he was himself without guile, and she had, apparently, the confidence of those whom he held in esteem, he had not thought of either trick or collusion, of the carelessness of the questioner, or of the delusion of the dreamer. A more rigid scrutinizer, who followed him attentively, saw the source of error, and by suffering her, during his interrogation, to follow her own way, without correction, was not able to draw from her any truth whatever as to places and persons at a distance.

But the following case places in strong relief the necessity of extreme caution in admitting the testimony as to the things which excite wonder or surprise. A very respectable publisher having given to one of my friends a marvelous account of a visit which he made to some somnambulist in Rhode Island, I took the trouble to call on him in person, and heard from his own lips, soberly and impressively delivered, the following story: "Having," said he, "some business in Providence, Rhode Island, I took the opportunity to visit a celebrated *clairvoyante*, who created at that time a strong sensation. Her magnetizer, a gentleman of honorable and influential standing, regretted that his patient was too sick for exhibition, but told me, for my satisfaction, of several of her extraordinary feats, one of which, having been performed in the presence of my lawyer, he advised me to learn more particularly from himself. Accordingly I made inquiry of the lawyer, the first of his class, who declared that he had gone to see the lady alluded to, with a determination to try her powers severely, for he had no share of the common credulity. He took with him two friends, and on his way set his watch back an hour and a half. On entering her room he asked the sleeper to tell him by *his* watch, *then in his pocket*, the time of day. She paused and said, 'The watch is wrong, it is an hour and a half too slow.' That might be a mere guess, he thought, so he left the room, set his watch wrong in another direction, wrapped it up in a handkerchief, and, grasping the whole in his hand, came back for an answer. To his surprise it was given correctly. He then begged her to tell him when ten minutes had elapsed. Half an hour passed without an answer. 'Why do you wait?' said the magnetizer, 'it is now half an hour.' 'I wait,' said she, 'because the hands of Mr. ——'s watch have not moved.' It was so! The lawyer, puzzled and confounded, said, 'It is the work of the devil,' and left the room, resolved never again to come so near to a witch." The bookseller then proceeded as follows: "Excited by the strange tale of Mr. ——, which I could not credit, I sought out another magnetizer, who had under his care a girl who had been apparently blind for some years. Having mesmerized her, the operator requested me to take her spirit home, and that she would describe my house, etc. 'No,' said I, 'that has been often done. Can she read?' 'Yes,' said he, 'even if the back of the book is toward her, and at the opposite side of the room.' Accordingly, taking a book from a centre table, and seating myself as far off as possible, near a window, I opened the volume at an unused place, which I knew to be such, for the gilded edges cohered. To my utter amazement she read it well, line after line. I tried another place, with the same effect; another, and another. The lawyer was right. It *is* the work of the devil."

Here was a man of the first character vouching for a fact in detail, in which he could not be mistaken. Now what was the truth? I will answer. The very magnetizer whose feat he related, came to Philadelphia, and told me, when questioned as to the cases cited, that no such occurrences were known to him, and that he did not believe that any *clairvoyant* in the world could perform such achievements; yet that magnetizer was full of the enthusiasm of his art. I can explain this mystery only by supposing that such stories were current in Providence, and generally believed, and that my narrator confiding in this truth, thought it not improper to force the belief of others, by making himself their personal observer. In other matters this is, unfortunately for the cause of truth, a too common error.

In the face of the oft-repeated refutations of the alleged lucidity of magnetized persons, we are again and again called to witness and overthrow this most unfounded of pretensions. The work of detection is apparently endless, for another and yet another *clair-*

voyant challenges us once more to the task. So long as the exhibitor plays the principal part, the performance is highly successful; but the moment that the subject is committed to other hands, the failure is total. This of itself should excite doubt, if not rejection. The success of the experiment in interested hands can offer little to invite our belief, when others as competent fail to discover any traces of such a miraculous power. Most of you must remember the case of the "mysterious lady," who exhibited her feats at the Washington Hall, in Third Street, some ten or twelve years ago. This female was wide awake during the whole evening, and yet with her back to the audience, and at the greatest distance which the apartment would permit, she was able to tell what articles were, in very rapid succession, held in the hand of the assistant. She did not require to be questioned at length, or to be led by several queries to the thing sought for. Her answers were clear, direct, and always successful; yet I could not by the closest observation detect the trick which was played upon us. Gloves, handkerchiefs, keys, knives, watches, scissors, cards, and an endless variety of things, many of them uncommon, were placed in the hands of the manager by the spectators, but they were all detected at once by the mysterious lady. No clairvoyant of the present day, aided by all the power of mesmerism, could compare with that mysterious lady; and yet she did not deny that there was management in the mystery.

In the work of Dubois D'Amiens and Burdin on Animal Magnetism, we find a letter from a physician of Metz, relating that he saw, at Strasbourg, a common Dutch juggler exhibit similar feats on an open stage, where his wife, though wide awake and blindfolded, told readily whatever was going on behind her back. She guessed the age of a person, who communicated it in a low voice to the juggler. She described the appearance and the time of a watch handed up by a spectator. She indicated correctly the value, effigy, and date of a coin presented to the juggler by a by-stander. An observer was requested to mark on a slate some numerals in columnar order; the exact value of which she readily pointed out. Finally, she described the color of the coat of a spectator, its rows of buttons, and the absence of one button of one row. In this case there was no hesitation, no gradual approach to the truth, no army of questions, and no blunders. If, then, such things can be done by a poor, ignorant juggler's wife when wide awake, we are not called on to believe that the clairvoyant succeeds by more miraculous means. That the cases are parallel, may be determined by the fact, that a fair and rigid scrutiny overthrows both. There is, however, this difference, that in many mesmeric cases the subject of experiment is deceived by his dreamy state, while the juggler's agent is always aware of the fraud. All mesmerized subjects, however, are not exempt from the capacity or disposition to deceive. Many of them are more acute in the artificial state, and remember well any concerted plan, so that it is quite possible to rival the mysterious lady or the juggler's wife in the art of seeing without eyes.

Among the supposed powers of somnambulism, is that of being able to travel beyond the precincts of the world we live in, and to witness personally the wonders of the world of spirits. Acute observers cannot fail to perceive that the reports brought back by these adventurers into the place of the departed, are the products of fancy, moulded by prejudice and accident. Thus a Methodist finds in heaven chiefly those of his own sect, and a Catholic discovers that the *other place* is filled with heretics, among whom the Methodists are conspicuous. It is not easy to reconcile these contradictions with belief in "lucidity."

Reasoning once with a pious Catholic against her belief in this mysterious property, she became very warm, and charged me with entertaining an absurd prejudice against a demonstrable principle. Just at the moment of the discussion another believer came in, and took part with her in the argument, alleging his having just seen a very expert *clairvoyante*, who had gone to and described the nether world. "There," said the lady, my opponent, "what can you now say for yourself?" "And pray," said I, "what report does she bring us?" The narrator told many things, among the rest, that the clairvoyante had seen the devil, whom she described. "And what," said the lady, "did she say of purgatory?" "Oh," replied the other, "she said that she had looked in vain for it, and was finally told by a wandering spirit that it was a shrewd invention of the church to entrap honest Baptists,* and lead them to the devil." "Well," said my opponent, "I am now convinced that the whole is a mere trick, and mesmerism a device of the evil one."

That so many "lucidists" should have visited so often the vast regions of space, without bringing back any substantial or verifiable knowledge of a novel character from these hitherto untraveled worlds, seems difficult to reconcile with the proud claims of mesmerism. "Unfortunately," says Gall, the phrenologist, "scientific dis-

* The clairvoyante was a Baptist.

coveries have yet to be made by the long and laborious method of experience, notwithstanding the magnetized see all their internal structure in the clearest manner, and magnetism has been practiced so long."

The writers on mesmerism confidently assert the existence of such *a sympathy* between the operator and subject as enables the former, by simple, unexpressed volition, to affect the thoughts, gestures, and sensibility of the latter. Nay, they go so far as to declare that the actions and involuntary sensations of the mesmerizer are transferred to the patient. They also believe that by volition the operator may occlude or restore any one of the senses, paralyze or make active any part of the body, and, indeed, exercise over the mesmerized sleeper such a control as has hitherto been supposed to belong solely to the GREAT CREATOR.

The identity of natural and artificial somnambulism having been already proved, we are able to see no good reason for supposing that any particular person stands in exclusive sympathetic relation to the artificial somnambulist. There is in the magnetizer no change perceptible either to himself or others to which such a relation could be referred. *Rationally*, then, we perceive no cause for admitting an exclusive individual power. Such is the theory; the practice fairly examined bears us to the same conclusion. I have never been able to move the minds or bodies of my patients, unless by such significant hints as would have been intelligible to those who were awake. It is true, that trained subjects learn to guess shrewdly at the designs of their leader. They interpret his looks and gestures; they understand his silence; they hear his low whisper, and they seem to work miracles. These wonders are never wrought when the patient does not understand by conventional signs or by expressive silence the object of the mesmerizer. On a remarkable occasion, a lady of the purest character, having mesmerized a gentleman of intelligence and refinement, endeavored to prove to me that she could, unknown to him, exert over him a spiritual control. Accordingly, having placed herself in another room, so that a wall intervened between her and the subject, she began to make certain motions with her hands, which were soon followed by similar ones on his part. Having seen him spontaneously make such motions before, I requested her to substitute motions of the feet, which he did not usually move. The experiment then failed, as it always did when rigidly made. The young lady immediately gave up the question, not having before thought of the accidental character of the gestures, and thus showed how unintentional had been her mistake.

Of the sympathetic communication of taste, smell, and sense of pain, exhibitors before the public often make a display; but I need scarcely add that these pretensions have their source in fraud or mistake. A patient who is not trained to this trick, or who is not taught to believe in such a relation, never presents the phenomenon. The sleeper, if asked how a thing tastes, is sure to dream that there is something in his mouth, and he will be directed to its character by what the magnetizer does. Thus, if the latter makes wry faces, he will say, "It is bad—disagreeable;" or, if he smacks his lips, he will say, "It is good—pleasant." He will also listen to the sounds produced by biting, chewing, swallowing, and thus conclude that it is sugar, an apple, water, etc. All this was produced in the following manner:—

An intelligent and honorable medical friend invited me to witness an exhibition of such sympathies, in which he firmly confided. Sugar, salt, Cayenne pepper, water, and vinegar were tasted by the magnetizer, and seemed to be tasted by the patient; but an unprejudiced observer could see that the favorable result was obtained by natural means. The sugar was audibly broken by the teeth, the pepper caused the usual grimaces, and the sound of swallowing when water, and its absence when vinegar was placed in the mouth, gave, one would think, clue enough; but even then the idea came slowly, indirectly, and after a number of questions and answers. On the following day I was permitted to be the actor, and took care to have everything in solution, in covered phials, so as to avoid the direction physically given on the former day. I also took care to keep a composed and uniform countenance, and to let no expression escape me calculated either to lead or mislead the sleeper. I need scarcely say that the experiment was a total failure. Sweet was mistaken for sour, strong for weak, sugar for salt, and brandy for water. It is objected that I wanted faith, or that I was not in full *rapport*, or did not perceive sensations strongly. I reply, that the most incredulous persons have been converted to the magnetic faith *by their own* experiments; that my powers as a magnetizer were highly lauded by most of the *corps*, while they persuaded themselves that I believed, and were proved to be great by extensive comparative trials. In a variety of observations, I always found that the result depended, not on any of the above-mentioned qualities, but upon the conduct of the experiments. Whenever I suffered myself to pursue the beaten path of careless experiment, I heard nothing of want of faith, feeble *rapport*, or defective sensations. In truth, whenever the arrangements were such as to exclude the possibility of error, the patients were either silent or embarrassed in fruitless conjectures. The same fate always awaited the attempt to make the patient feel pain by hurting the operator. They succeeded only when they were carelessly conducted. In fine, after a five years' diligent and faithful search, I have not been able to create, or to see created, the especial sympathy so much vaunted by mesmeric writers and lecturers.

If anything were yet wanted to complete the evidence, it would be found in the fact that any other person, by using the proper mesmeric method, may rouse the somnambulist whom he did not soporize. This fact seemed to be entirely unknown to the most celebrated mesmerists, at the time when I had the pleasure of a visit from one of them, who had acquired, in Rhode Island, some reputation in the art and mystery of magnetism. When I expressed to him my regret at the vast mass of untruth by which an interesting department of knowledge was loaded, and instanced the falsehood as to *rapport* as a part of it, he looked amazed, and with evident good faith challenged me to the proof. I accordingly placed him in the presence of two of my best subjects, each of whom was mesmerized by one of us respectively. I then requested him to awaken my patient. He replied that he feared that convulsions might ensue; but on being entreated, he proceeded to act, and awakened her in a few seconds more than the usual time. As she began to move, he showed unfeigned uneasiness; but so soon as she was wide awake, he charged me with having lent my volition to aid him. This I denied, and to show how unfounded were his suspicions, I begged him to resist, by his utmost force of will, the efforts I was about to make to arouse the subject whom *he* had mesmerized. The position he assumed was amusing. Stooping, as if for a strong effort, he stood with clenched hands, set teeth, and a shaking head, expressive of dissent or opposition. He was at the distance of a few feet from the sleeper, while I took the usual position, and proceeded to use the rapid passes for awakening. In a very few minutes the insensible somnambulist was wide awake, and the disappointed magnetizer exclaimed, "It is not so in Rhode Island."

As the subjects of magnetic processes have the power to resist the magnetic as they can the ordinary sleep, so they can, if they please, oppose the efforts to arouse them. In this way mesmeric adepts refuse to be awakened by any one save the magnetizer, and thus continue to present a rule which uninformed sleepers regularly violate. I have never seen a case of mesmeric sleep in new subjects which could not be broken by any by-stander who used the proper process. Why the exhibited and trained cases do not follow the same law, I may conjecture, but will not now declare.

Within a few months we have heard much of the evidence which phrenology brings to the support of mesmerism, and of the extraordinary light shed by the latter on the former. The discovery of the striking phenomena elicited by applying galvanism and mesmerism to the phrenological organs, is due to Doctor Buchanan, of Louisville, Kentucky. The origin and progress of this new pretension of mesmerism are too well known to you to require here a detail and narrative. I shall confine myself chiefly to an examination of the evidence in the case, and to an endeavor to place the theory of this part of our subject on what I suppose to be its just footing.

Soon after I saw, in western papers, highly colored statements of galvano-phrenological experiments by Dr. Buchanan; I was led, by some mesmeric trials of the organs, to believe that the word mesmeric should be substituted for galvanic, and that the whole subject was another phasis of mesmeric power. To Dr. Buchanan, one of the most respectable of our faculty, and a very useful officer of this Society, I was indebted for an opportunity of examining a case equally curious and instructive. The son of a reputable clergyman, afflicted with hypertrophy of the heart, and intense nervo-cerebral symptoms, had been mesmerized by his father, with a view to the relief of a disease which did not seem to be under the control of medicine. While in the mesmeric state some excitation of certain phrenological organs produced mental expressions so curious as to lead to an invitation to me to witness them. A lad of fifteen years of age, long in bad health, when in the mesmeric sleep, as the finger of the mesmerizer moved from organ to organ, expressed with great force and in rapid succession the most opposite mental emotions. Thus when tune was touched, he was gay and musical; when veneration was covered, he was prayerful and grave. Obstinacy made him endeavor, with marks of displeasure, to keep the front door shut against an ideal intruder; while benevolence suddenly incited him to relent and admit him. Hunger, anger, self-esteem, love of approbation, love of children, were successively displayed, with a force and faithfulness which swept away the incredulity of the observers. Reference to his friends, and an examination of the lad himself when awake, proved that he knew almost nothing of the science of phrenology, and as little of the location of the organs. I could not doubt the truth of these statements. The witnesses were competent, honest, and without any known motive to deceive; and the youth, bad health, and simplicity of character of the lad, were guarantees against any intentional misrepresentation*

Had I not, in the beginning of my attention to mesmerism resolved to believe nothing as settled which could not be demonstrated by several distinct and disconnected subjects, I should not have felt entitled, without subjecting myself to the charge of irrational incredulity, to withhold my full assent to the truth both of mesmerism and phrenology. While, then, I felt deeply impressed with what I saw, I resolved not to come to a final conclusion until I had examined a number of persons, and that critically. Nay, until I could find persons whose positions and want of all education rendered their ignorance certain, I did not feel that an experimentum crucis could be made. As we proceed, it will be seen, that not only from such sources, but from other and unexpected ones of a different character, flowed indisputable facts to settle this question. But let us to our narrative. The next step was an application to a friend for permission to try the experiment on him. I selected him because he was easily magnetized, and because of his intellectual greatness, his unvaried moral worth, and his love of all truth. He consented, expressing at the same time his full persuasion that some error lay in these experiments, and that no such effects would

* Subsequently I learned, what had not suggested itself to any one at first, that an elder brother of this subject had followed the phrenological lectures of Mr. Comlee, and that the science of phrenology had been often, in his presence, the topic of conversation.

appear in his case. All this was, I thought, a favorable preparation for a correct examination. We met in the presence of several friends, as incredulous on this point as himself. At first no mental excitement could be perceived by the subject of experiment, but his gestures were very plainly expressive of the faculties successively appealed to. He was a good phrenologist as he was a good metaphysician, great humorist, and able writer. He perceived his gestures, but declared that they were suggested by no mental state, nor were they the expression of any observable ideas. A little ignorant boy was present whose mesmeric sleep was curious, and who could not wait for me longer. I awakened my friend and put the boy to sleep, that he might see the case. This boy could, by conversation, be excited in the most amusing manner. In particular, by conjuring up the dream of a present and troublesome antagonist, he could be made to fight with bitterness. But all attempts to excite him phrenologically utterly failed. After that, my friend was again mesmerized, and I could wish for this assembly no higher treat than to witness the truth, beauty, and force of his expression, as, in succession, passed over him a dream of the passions and sentiments, and the perceptive and reflective faculties. His rage was terrible, his ideality sublime, his veneration beautifully captivating; but his mirth—I know not how to speak of that mirth! In a musical paroxysm he imagined himself, in rapid succession, the performer on every instrument. Now it was the violin—then the piano—again the kettle-drum, the serpent, the bass viol, the trombone. But no player, in his maddest mood of excitement, could equal him in the intensity of physiognomical expression, or the rapidity and violence of gesticulation. Fearing too great exhaustion I awoke him, after he had been for an hour or more in a phrensy of passion, and found, to my surprise, that he not only did not suffer from his exertion, but that he felt the better for it.

At the close of these experiments we naturally inquired how these phenomena were produced, and several opinions were offered. One, the most obvious, that the effect was phreno-mesmeric; another, *that it depended on phrenological knowledge operating through an excited brain; that the excitement was general, the direction imaginative;* a third, a sturdy mesmerist, that the whole effect was purely mental, the power of the mind of the mesmerizer over the mind of the subject mesmerized; while a sly friend, who loves a practical joke, hinted that he was not entirely satisfied that our estimable subject was not playing upon us. Mr. ——, the sleeper, who should know best, was appealed to as to *his* views; but he declared that he could not tell how the ideas entered his head. He felt an impulse difficult to resist, and although he remembered much of what passed, his recollection was indistinct and broken. He admitted his knowledge of and belief in phrenology, and agreed with me in thinking that the question could be satisfactorily settled only by appealing to more ignorant heads. He was sure that the mesmerizer's finger excited the physical energies of the brain, for the impulse was obvious; but he would not venture to decide that the particular organ was more directly stimulated than the rest, or that the same results could be obtained in one who knew nothing of phrenology.

Shortly after these experiments had been made, a lady of my acquaintance came on a visit to my family, and kindly suffered me to mesmerize her without telling her for what purpose. As she had been mesmerized at a former period, the task was now easy. Nothing was said to give her a clue to my purpose, nor was one word dropped respecting phrenology, until after the close of the experiment. When she fell asleep I placed my finger on the organ of music; she looked pleased, and assumed a listening attitude. "What do you see?" "Nothing." "What do you hear?" "Music." The finger was then placed on mirth—she laughed; on veneration—grave look. "What is it?" "Hush! Dr. T——g* is speaking." On combativeness—a threat to box the ears of one of my children, who was laughing at the time. On philoprogenitiveness—she wondered if her child, left at home, was well covered in bed. On adhesiveness—she reproved her daughter for vexing a pet dog left at home—neither being present. On eventuality—she saw a great many horses in the street, moving about in so confused a manner that though pressed to do so, she could not select one from the crowd. Just at that moment, I removed my finger to veneration, and repeated the request, and asked her how any one of them was caparisoned. "What caparisoned?" said she. "The horse," replied I. "Pshaw! take Dr. T——g for a horse! a pretty story!" She looked really vexed as she said this.

On her being awakened, I began the conversation, by inquiring as to her knowledge of my design in putting her to sleep, of which she professed entire ignorance. I then questioned her as to her recollection of what had passed during her sleep; when she said that she had a

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* Her clergyman.

confused idea of the sudden and rapid passage through her mind of contradictory feelings and incongruous ideas. "It was a wild kind of dream," she said, "of which I cannot name any particulars." Finally, I carelessly talked about phrenology, and asked if she knew much of it. She said, "I know only two or three organs, and them indistinctly." She was then informed of what had passed, at which she expressed great astonishment.

The lady on whom the above experiment was performed, might or might not have seen through my design in placing my finger on different parts of her head. The experiments of Buchanan had been reported by our daily papers, and she had read them; she was partially acquainted with phrenology, and might recollect more when asleep and excited, than when awake and in the ordinary state; a power which is not unfrequently represented by mesmerized persons, and is now and then offered by natural somnambulism. Few persons acquainted with phrenology would fail to see through the design of one who should place his finger over two or three phrenological organs in succession, and inquire as to what the subject was then thinking of. These reflections did not strike me at the time of the observation, and the phenomena not so explained were enough to stagger even the most incredulous; but the experimentum crucis was not yet found. The only totally ignorant person I had as yet tested gave no sign. In the mean time I employed myself in examining the phenomena on the head of the friend to whom I had at first applied; for which purpose I had a cast in plaster made of it, and as he gave expression to the touch, I carefully marked the places on the cast, until I had explored the whole cranium, and not only fixed the old, but marked many new organs on it. This was done that, the same thing being done in other places, we might be able to make fair comparisons, and thus obtain a new form of evidence. The bust I now show you was so constructed. It subdivides some of the old organs, but it does not displace any of them. It places the new organs in the unoccupied interspaces of the old ones, and it nearly doubles their number. As I found the site of all the old organs correctly laid down, according to the new mesmeric observations, it followed that we had a right to expect to find the new ones in similar places in new heads, and might thus possess a good means of verifying or refuting the whole phreno-mesmeric system. The curious may wish to know by what process the new organs were located. After the site of the old ones had been verified and marked, it was observed that there remained large unoccupied spaces, which might possibly represent other faculties. Over these the finger was successively laid, and the effects carefully noted. We were incited also to the task, by learning that in other cities new organs had been thus discovered, and we had, in Philadelphia, seen represented modesty, aquativeness, and curiosity, organs hitherto unknown to the charts of the cranium. There was, of course, great hazard of deceiving ourselves in these experiments, for we were already aware of the extraordinary tendency of mesmerized subjects to convert ideas into dreams, and suggestions into supposed realities, without any corrective capacity. The patient was attentive to, and equally with myself interested in, the inquiry; he was, also, a phrenologist and good thinker, and might, possibly, without discovering the error himself, infer the probable locality of undetected organs, by following the acknowledged principle in the arrangement of the old ones. Whether imaginary or real, the new organs thus placed amounted to nearly as many as had been before known; and several others were rejected, because their excitement gave, on different occasons, contradictory manifestations, or because they were not so distinctly expressed as to deserve a name. Some circumstances in this survey, left us less of certainty than we might have otherwise felt. One or two of the most marked expressions and sentiments were referred, on different occasions, to different parts of the head, and the same part, now and then, gave birth to totally different sets of ideas. We observed that the old and well-known organs never failed us, but that we were not unfrequently disappointed in finding the manifestations of the new ones. Thus I passed my finger many times along the median occipital line, without obtaining any expression of aversion or politeness, though I never failed to detect self-esteem, concentrativeness, and philoprogenitiveness.

When the new organs were sought for on other heads, they were sought for in vain, although, as we found the old ones to correspond on different crania, we had a right to expect the new ones to do so also. It is true, that the two or three which had been displayed by others in public, were sometimes found expressive in new explorations; but those which we detected in private, and kept secret, so far as my observations extended, were never found on other heads, until some intimation of their nature and position was given. After that it was not difficult to find them.

While engaged in our pursuit of new organs, we were favored by a traveler with a chart of the cranium, made by Dr. Buchanan's own hand. Were the two charts, ours and Dr. B.'s, both offered to the reader, he would, no doubt, smile at the extraordinary discrepancies which they present. In the first place, he would observe, that after the most careful and minute survey of the head, we have been able to make but fifty-three organs, while Dr. B.'s offers ninety-four. In the second place, he would perceive, that while we have respected the old site of old organs, Dr. B. has displaced many, and transposed some of them. In the third place, he would remark that many of Dr. B.'s organs are located far below the level of the basis of the brain; and, fourthly, that the doctor's designation and localization of his new organs are equally curious. Thus we have patriotism, conjugality, love of stimulus, muscularity, elective sense, thermal sense, etc. etc. We have benevolence in one place, philanthropy in another, sympathy in a third, and liberality in a fourth. We have filial piety and moral refinement, invention, planning, construction, and system. The location of the old organs is very much disturbed. Tune and construction have changed places; patriotism and inhabitiveness usurp the seat of caution; love of power has fastened on concentrativeness, and that has displaced adhesiveness; while conjugality jostles out inhabitiveness, which has gone to oust circumspection. He divides eventuality into two parts, having regard, the one to new, and the other to old events; while he interposes an organ of motion between eventuality and individuality. Locality he calls distance, and firmness fortitude; while he gives to another, "fortitude," a place within love of approbation, and calls it firmness; and, still another, "obstinacy," is placed at the conjunction of philoprogenitiveness, adhesiveness, and combativeness.

On a comparison of the busts marked at Louisville and Philadelphia, we observe the great extent of discordancy in the alleged new organs, as may be seen by the following table :—

THE PHILADELPHIA BUST.	THE LOUISVILLE BUST.
1. Aquativeness.	1. Mastication.
2. Attention of sight.	2. Love of stimulus.
3. Attention of hearing.	3. Love of life.
4. Curiosity or inquisitiveness.	4. Muscularity.
5. Self-complacency.	5. Love of power.
6. Modesty.	6. Watchfulness.
7. Authoritativeness.	7. Concentrativeness and adhesiveness.
8. Aversion.	8. Conjugality.
9 Affability.	9. Conjugality.
10. Malice.	10. Secretiveness.
11. Envy.	11. Obstinacy, sternness.
12. Ideality.	12. Moral refinement.
13. Space.	13. Period.
14. Constructiveness.	14. Sense of beauty.
15. Tune.	15. System.
16. Number.	16. Sense of force and language.
17. Weight.	17. Size.
18. Imitation.	18. Sympathy.
19. Wonder.	19. Expression and mirth.
20. Acquisitiveness.	20. Excitement, fear, melancholy.
21. Caution and modesty.	21. Inhabitiveness.
22. Love of approbation.	22. Honor and firmness.
23. Form.	23. Light.

I might extend the comparison further, but enough has been shown to prove that we should view with doubt a system which produces such discordant results. The only new organs found in the same position on the two heads are, courage in one, nearly but not quite on the site of firmness in the other; and thirst by the side of hunger, in both. The last is the only exact coincidence; but the Louisville cast carries out a line of organs from hunger, near the ear, through thirst, taste, flavor, to odor, beneath the eye; a manifest suggestion of the fancy.

Advancing from the survey of casts to that of heads, I soon found that persons gave expression to the organs according to their knowledge previously had or acquired from the spectators at the time. A very slight hint was generally sufficient; and the transfer of the finger from one spot to another usually produced from some one the whispered name of the organ whose expression was sought.

There was another mode in which the subject could be sifted, and that consisted in misleading ignorant persons as to the true situation of the organs. This *ruse* almost always produced an expression suited to the name, and not to the place of the organ. Thus, placing the finger on caution, and saying, "Let us try self-esteem," self-esteem was expressed. So, adhesiveness could be enacted on the site of ideality, and acquisitiveness on that of benevolence. Even where no attempt was made to mislead the patient, he often gave an expression inconsistent with the organ pressed upon.

On a recent occasion I mesmerized a very susceptible subject, and after making her exhibit some phrenological phenomena, I requested one of the spectators to try his hand. This I did to take off her attention from me, and while he was operating on the organs of the forehead, very cautiously, applied my finger to the occipital protuberance, and kept it steadily there for some time, after which I applied it to the organs of combativeness, destructiveness, and concentrativeness. Not one of these organs gave any expression, while the organs of music, mirth, veneration, and number, to which the patient's attention was called by the finger of the new operator, acted with considerable vivacity.

If any further elucidation of the true source of these phrenological phenomena be wanted, it is furnished by the following case:—

A. R., a young lady of genteel connections and associations, but whom I knew to be totally ignorant of phrenology, having been mesmerized, displayed, as she always did in this state, a pleasing and rather lively expression. It was usually very fixed. Passing the tip of my finger from organ to organ over the head, I watched for a change of expression. The same fixed look was there. I repeated the experiment, dwelling long on each spot. No effect was apparent. After the lapse of half an hour, I reapplied my hand to the organ of tune, and said, "Do you see anything?" "No." "Do you hear anything?" "No." "I wonder at that, for I am pressing on the part of the head that makes us think of music." "Oh," said she, "that is fine!" "What is fine?" "Why, that trumpet! Oh, there's a whole band!"

If we had no other reason for doubting the reality of phreno-mesmerism, one would be supplied by adverting to the fact that in the mesmeric manipulation, the fingers are often applied to the head of the subject for the purpose of producing the state of sleep some time after the sleep is begun, and yet no one, until after Buchanan's discovery, saw these phenomena; nor do they so now until the patient expects them.*

* I have recently seen a case of natural somnambulism, in most respects analogous to the artificial ones, as, in loss of sensibility,

You must not for a moment suppose that I think there is on the part of the subjects the slightest deception. Most of them are too well known to me to permit such an idea, and the appearance of expression forbids, by its earnestness, such a suspicion. Besides all this, one at least of my mesmerized friends is as earnestly in pursuit of the clue to the mystery of this matter as I am, and would delight in being able to trace it to the true solution, of which he remains yet in doubt.

There is another view of this question equally curious and instructive. A very few observations of the mesmeric sleep are sufficient to show that it has a memory of its own, peculiar and exclusive. The mesmeric sleeper carries into his sleep the recollection not only of all the events which his waking memory recalls, but he can, when so excited, review ideas which had been long and apparently forgotten; just as in common sleep associations are sometimes revived, which had long been dislocated; thus giving to the recollections of a dream the appearance of invention, or even of intuition. The child, too young to voluntarily repeat ideas, sees its parent or grandparent bury some treasure. The scene and its events are gone in a few days; but, in the long progress of years, in sleep, they come back on the mind, and the sought-for treasure being found where so mysteriously suggested, is referred to as evidence of a supernatural revelation.

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obliviousness, facility of being led into dreams, and difficulty of being awakened, save by the magnetic method; yet in this case the most persevering application of the finger to the cranium produced no other effect than to make the patient complain of excitement and confusion. There could not be roused the function of a single organ.

While the mesmerized subject, during his sleep, forgets scarcely any part of his waking life, he remembers, after his sleep is broken, little or nothing of his sleeping hours; but when the charm is renewed there comes with it the memory of his former sleep, with surprising detail and exactness. Whatever is taught in the new state, is knowledge only for that state; and thus, a subject may be taught phrenology or any other science when asleep, of which he can very honestly avow, when awake, his total ignorance. In this way most of the phreno-mesmeric cases are unconsciously made. Let us examine the mode of procedure usually adopted. The patient is mesmerized, his head is examined, the finger is placed on a cerebral organ, and he is asked if he sees or hears anything. The examination, the finger, and the question, suggest at once to a phrenologist the purport of the business, and he is led immediately to the proper answer. That answer he might give even if he had forgotten a system which he once understood. But if he be *totally* ignorant of phrenology, asleep and awake, he usually replies that he sees or hears nothing. "What! do you hear nothing?" If the *hear* is emphasized, he will reply, "Yes, I hear something." "What is it like—is it a tune?" "Oh, yes." "There!" exclaims one of the observers, "he touched the organ of music!" The patient hears that, notes the part touched, and is so far a phrenologist. That spot is ever after associated with the idea of music, at least while he sleeps. In succession several organs are taught; nay, to an acute patient, the whole of them, although he is so totally unacquainted with them when awake as to honestly declare his entire ignorance of the first step in the most elementary department of phrenology. Such a declaration, made by one habitually veracious, coupled with our knowledge of his want of opportunity to learn, seems sufficient to satisfy even the most skeptical inquirer, and he falls at once into the snare set by himself. In this way I have purposely taught phrenology to a very ignorant subject, who still remains, when awake, as ignorant of it as ever.

Morbid cases of an analogous character are on record. One has been given in another part of this paper, where a young lad, subject to periodical insanity, recollected when insane, all the events of his former paroxysms, of which, when sane, the remembrance totally vanished.

The peculiarity of which I now treat has led the phreno-mesmerites to believe that the organs require training, to enable them either to feel the stimulus at all, or to give it its due direction. The training is of the mind, not the brain of the sleeper.

If I had nothing new to offer on the theory of animal magnetism, I might very properly close my paper here, and leave the mass of facts now recorded to produce their proper effect on the mind of my hearers; but as the explanation I mean to propose for your consideration has hitherto escaped notice, it cannot be obvious, and therefore requires an exponent.

The theory of Mesmer has long been abandoned; but the idea of a peculiar vital fluid, of which the nerves are the conductors, susceptible of being collected and discharged, of being withdrawn from, or directed into, the bodies of others by expert magnetizers, is at present the predominant principle of the mesmeric school. The passes, the contact, the manipulation of the fingers, are founded on this notion. The mesmerizer believes that he draws the nervous fluid from part to part of his subject, carries it out of the body, receives it himself, and shakes it from the ends of his fingers. He also supposes that this fluid is subject to the control of the will, and that through it, as a channel of communication, the ideas and wishes of the magnetizer are conveyed to the subject. There are a few transcendental mesmerizers who discard all physical sources of power, and attribute the whole of the phenomena to a spiritual communication.

The existence of a fluid of any kind, universal as supposed by Mesmer, or vital as now usually alleged by modern mesmerizers, has not been made even probable by any experiments or observations, of which in a very careful survey of the subject I can find the record. On the other hand, the authenticated phenomena seem to prove the contrary. The overthrow of the doctrine of "rapport," or communication, deprives the magnetizer of his principal argument in favor of a fluid of any kind. If there exists no relation of a peculiar kind between actor and subject, the vital fluid is no longer necessary to its explanation. The following experiment will place the question in a strong light:—

Three persons were mesmerized in close succession. They were left asleep, and a friend, whom I met in the street, was requested to call and release them, which he did easily.

Now I, who magnetized these three persons, must have been, according to the theory, greatly plus or minus, with respect to the vital fluid, of which, however, I felt neither the redundancy nor deficiency; while my friend, who aroused them, must have been in an opposite condition; yet, when we subsequently met, neither was conscious of any change. If I had taken away enough of vital fluid to make three persons sleep, he must have restored the whole quantity at his own expense; and yet, after three times the loss sustained by each, he felt no kind of drowsiness; or, what would make three persons sleep, made on him no impression. I have no doubt that a dozen persons might be soporized by one, and awakened by another, without any drowsiness being felt by the latter. On the supposition that the magnetizer conveys the fluid *to* his patients instead of *from* them, the same difficulty will present itself, for then the restorer would have to receive as much fluid as made three persons sleep.

The effect of the direction of the passes goes distinctly to disprove the emission or absorption of a fluid. In exactly the same mood of mind the operator moves his hand along the arm of the patient, toward or from the body, and yet the effect is to paralyze the arm, or to stiffen it by the one motion, and to restore its power by the other, exactly opposite effects. Here the action is uniform, for the change of the position of the patient will direct the motion to or from the body at pleasure, and yet the results are diametrically opposite. Blindfolding the operator, so that he cannot tell how his gestures are inclined, will not alter the effects, which may be made to depend on the changing positions of the patient.

The theory of a pure spiritual influence alleged by certain German writers, is even less tenable than that of a vital fluid. I have never been able to convey my ideas to my subjects except by means of words or signs. It is true that after a number of sittings, patients learn to interpret even one's silence, and to guess with great sagacity the meaning of pauses, movements, and looks. Whenever the ideas are such as have not been already practieed upon, and the mesmerizer makes no sign, the experiment fails. Nay, if he give such an expression as is not comformable to his mental state, the expression, and not the thought, will be attended to and obeyed. So when testing in a phrenological subject phreno-mesmerism, if the mesmerizer mistake the organ, and expect, for example, an impression of circumspection while his finger is on acquisitiveness, he will see his patient pocketing ideal money, while he thinks he should show signs of fear. For these, among many reasons, I cannot believe in the government by the unexpressed will.

The electrical theory of mesmerism has been with the mesmerizers a favorite speculation. Desirous of accommodating it to the prejudices of society, some of the advocates of mesmerism have pretended to account for its influence by reference to the probable movement, accumulation, and dissipation of the electric fluid in the nervous system. That electricity is not a cause of the mesmeric state seems to be proved by the fact, that hitherto no obvious electric means or apparatus has produced any of the mesmeric phenomena. A stronger argument is derived from the failure of the most delicate tests of the presence of electricity, during magnetic experiments. Armed with the beautiful galvanoscope of Mr. Sexton, I endeavored in vain to excite its movements by putting patients connected with it in the mesmeric state. The method of soporizing, and particularly of awaking subjects of experiment, goes far also to prove the absence of electrical influence, since no known laws of electricity could explain the results. The intervention of a plate of glass did not sensibly affect the influence of passes made along the arm of a sensitive patient, nor had charged conductors the slightest power of controlling the phenomena.

The term magnetism, while it proclaims the origin of Mesmer's alleged discovery, carries yet to the mind of the public the idea of some, at least occult, relation of terrestrial to animal magnetism. Even practiced magnetizers, if they do not see any such connection, are thorough believers in the disturbing effects of the presence of metals, and particularly of iron, of which magnetized iron is supposed to be the most potent. I once saw one of the illuminati remove an iron poker from beneath the rockers of a chair, where it had been placed to steady it, while the mesmeric process was applied to its occupant, saying that no success could attend me while that was there. It was in vain that I assured him of the constant presence of the bar of iron in former and most felicitous operations. It was not by him considered possible; and, strange to say, having replaced it, he saw me fail, with a fine susceptible subject, who had often gone to sleep before while it was there, but who could not do so after the doubt was started. She then thought she felt the bar acting constantly in opposition

I made, subsequently, many experiments on the supposed influence of metals and magnets, and found that when their presence was not suspected they exerted no power, and that even when visible they produced effects solely on those who had been thought to expect them. In one case I magnetized, without any unusual phenomena, a young and very susceptible subject, while I had three strong horseshoe magnets in my pockets, and another one under the cushion of the patient's chair. It is true, that when manipulation is made, magnet in hand, results are obtained such as the hand would produce, but that only proves the power of the hand and not of the magnet. I have seen a patient violently affected by the supposed proximity of a distant magnet, while its approach, when supposed to be distant, did not elicit any sensible effects. Indeed I have several times tried the sensibility of a sleeping subject by directing the electro-magnetic current through every part of the body, without any other disturbance than the violent muscular action usually produced by that agent. Even when the current from a strong apparatus belonging to Professor Hare was directed through the face and lower jaw, there was no expression of sense of pain, although no other person present could bear for an instant the same application, nor could the patient bear it when awake. A magnet, therefore, of the greatest power, aided by a strong current of electricity, exerted no peculiar influence on a mesmerized subject. Nothing, therefore, seems clearer than the conclusion at which we arrive, that *electricity and magnetism, singly or combined, do not cause, and cannot control, the mesmeric influence.*

The charging of metals and other bodies, such as water, with magnetic power, is universally practiced by mesmerizers. Elliotson, following some of the writers of the German school, not only believes that metals may be so charged, and made potent for magnetic purposes, but that some metals possess very high and some very low powers, while a few, iron being one, have no such susceptibility. Experiments entirely satisfactory on this subject, are not easy to make, as the influence of the mesmerizer cannot, if he be present, be excluded, and it is almost impossible to keep the patients ignorant of our design. Thus, therefore, there commonly mingles with the effort, the influence of both operator and subject. When, however, pains are taken to guard against all known sources of error, the imaginary potency ceases, and, as might have been expected, *the patients experience no effects from contact with the mesmerized metals.*

If metals, especially those of the currency, were so susceptible of being imbued with mesmeric force, the business of a money-changer, shopkeeper or banker, would be fraught with inconveniences. A nervous lady would scarcely dare to receive a guinea or eagle from other hands lest she should fall asleep in the shop or the street. It is vain to contend that the metal must be voluntarily impregnated, since Dr. Elliotson and others have shown that an uncharged sovereign, being placed by a magnetized person in the hand of one who is concealed by the bedclothes, acquires, by that step alone, a power proportionate to the strength of the patient, as manifested on that delicate dynamometer, the system of the mesmerized. "The effects," says Elliotson, "were in every instance precisely proportionate to the strength of the patient in whose hand the sovereign had been placed." (*Physiol.*, p. 1175.) The only case in which I ever saw a metal produce a mesmeric effect, which could not be mistaken, was when a sleeping subject was offered half a dollar to instantly awaken himself. I never saw eyelids so suddenly unfolded as in that case.

So long as patients do not expect an effect from metals, and the operator is kept at a distance, their influence is entirely negative. Nay, more, whatever the subject is taught to expect from such a contact is almost always produced, whether that effect be conformable to authority or in opposition to it.

Very recently, (*Neurypnology*, London, 1843, James Braid, M.R.C.S., etc.,) a physician of Manchester, in

England, has suggested the novel idea, that the whole of the mesmeric phenomena arose from the fixed attention of the subjects of experiments to any object placed before the eye, such as the head of a cane, the cork in a bottle, etc. Were this the case, should we not have cases of mesmerism daily, for every day persons perform the act to which such mesmerism is referred. The experiment on the blind, who cannot look, and do not see, and those at the Orphan Asylum, where the patients were told to keep the eyes closed during the experiments, sufficiently refute the Manchester theory. In fact, the experiment succeeds as well in trained subjects, when the object usually looked at for the effect is placed behind the back, or even carried away, provided it is supposed by the patient to be there, and he is made to believe that it can exert mesmeric influence. Throughout, the experimenter was deceived by his own act; for he was operating on his new subjects at the time, perhaps, unconsciously; and when trained subjects were used, they placed themselves in the magnetic state by habit and volition. Whenever *new* subjects are employed, and the operator keeps at a distance, the experiment always fails, or, at least, it has done so in my hands. This is one of the many errors into which mesmeric observers very honestly fall, through ignorance of the influence of habit, volition, and expectation.

The theory of imagination, started by the Franklin Committee, and adopted in our own day by Bertrand, having been shown, in an early part of this essay, to be inadequate to account for the first mesmeric paroxysm, though it may explain those that follow, there remains but one mode of explanation, which, under some form or other, must, I think, be ultimately adopted. It has, too, the merit of taking nothing for granted, of asking for the admission of no new principles, of violating none of the known laws of nature, and of finding at each step the support of well-marked analogies, in the better known and more exact sciences.

In the first place, then, I believe, that when the mesmerizer endeavors to magnetize a patient, he, *unconsciously, acts on his own nervous system.* While he directs his attention to the single purpose of closing the eyes of his subject, he arrests the current of his ideas, and thus disturbs both his mental and physical system. He puts *himself* in a certain state. That such is the fact, is proved by this, that the postures and relative positions of the parties remaining the same, any single prolonged idea of slow and labored motion, will answer the purpose of the magnetizer as well as that of closing the eyes. Thus let him, while seated before his subject, think of constant, but imperfect efforts to pull down a branch of a tree in a distant field, and he will find himself as successful as when he endeavors, in idea, to close the eyelids. The first step in the process of mesmerizing is, therefore, *the action of the mesmerizer on his own system;* which must, unquestionably, be a fixed alteration of his innervation.

There is nothing very remarkable or questionable in all this. The whole world knows that a man's volition may affect his own nervous system, and that any disturbance of nervous equilibrium, is followed by more or less vascular and calorific alteration.

But how is this change influential on the nervous system of another? I answer, by INDUCTION. It is a well-known law of several departments of science, that the mere disturbance of the equilibrium of certain imponderable substances will produce a loss of the normal distribution of that substance in surrounding bodies. Thus, if we increase or diminish the quantity of electricity within a Leyden jar, or if we even *alter its distribution* there, there will be produced on the exterior of the jar a commensurate disturbance of the electrical power; but not of the same kind. The electricity, which cannot either pass through or run over the glass partition, nevertheless exerts on the opposite side of it a decided and obvious influence. The interior, if positive, will cause the exterior to be negative, and *vice versa.*

This is termed *induction.* Or, when an excited body, without transmission of any known fluid in either direction, disturbs the electrical repose of adjacent bodies, the phenomenon is called induction. In the same way, a nail or screw may be made temporarily magnetic, by the mere approach of a magnetized iron bar, while its newly acquired power is instantly lost by its withdrawal. This is now termed the effect of *presence*, and the principle is often beautifully illustrated in chemical experiments. For example, hydrogen and oxygen repose quietly together, until a piece of cold, pure platinum is introduced, when they sustain a disturbance of some unknown character, and explode violently. On examination, the platinum is found unchanged, showing that it acted solely by its presence, and not chemically.

To this *principle of induction*, to this *doctrine of presence*, well known to philosophy, and daily observed in imponderable substances, I refer the productions of the magnetic sleep. The manipulation, though auxiliary, is not essential. The essentials are relative position, contact, at some point, and a fixed idea of slow and labored motion. The hand placed on the brow seems to aid in producing the effect; but patients usually sleep well without touching more than the hand. All the motions indicative of the extraction or immission of a subtle *aura* are useful only when they act on the imagination of one who has been taught to expect them. To uninstructed patients they bring no assistance.

The more direct arguments in favor of the doctrine of induction are numerous. When the hand of the magnetizer passes along the arms of certain subjects their muscles become rigid or paralyzed, or are restored to use, solely according to the *direction* of the motion. This well-authenticated experiment is remarkably like that of giving or taking away the magnetism of a bar of steel, which is placed in new circumstances by one movement and restored to its ordinary state by another. Very sensitive patients are affected oppositely by the palm and the back of the hand, the phenomena produced by either being made to disappear by the other. Thus if we stiffen an arm by laying the palm on any of its flexor muscles, the biceps for instance, that rigidity is quickly removed by the pressure of the opposite side of the hand. If we retain the hand long in the latter position, the arm will again become rigid, when the laying on of the palm will render it flaccid. Although I have repeated this experiment a great many times I do not yet consider it as a settled point. It is the only principle which, because of having recently discovered it, I cannot fully avouch.

It is probable that the strange fact which I am now about to produce is not unknown to some of you. There is in this city a seamstress of a healthy appearance and rather robust frame, who circulates in the course of a year through a large business acquaintance. Most of the ladies for whom she works have observed that her presence disposes them to drowsiness, although she is neither taciturn nor talkative, and the work at which they sew in common is well calculated to keep them amused and awake. Some of them having discovered the source of the drowsiness, took their seats at a greater distance, and found that when not in contact they felt little of this effect, and that at a distance of five or six feet it totally disappeared.

I have met with more than one curious case of nervous disorder, in which the patient felt oppressed by the near approach of certain persons, while the presence of others was positively agreeable. Both of these phenomena were apparently without any moral cause; nor could the most critical investigation discover any other than a physical influence. In one of these cases, attended by medical friends in consultation, the near approach of one of the physicians arrested the circulation and suppressed the respiration, while no inconvenience was felt at the presence of the other. The patient was unconscious of any difference of feeling toward them, and as her health improved and her nerves became less sensitive, she gradually lost the morbid impressibility, and knew finally no distinction. The approach of the physician was regarded with fear, but never with aversion, and the fear arose solely from the discovery of his untoward influence, an influence not suspected until it had been several times exerted. The terrific paroxysms were supposed to be accidental, until their uniform coincidence with the presence of the doctor led to the discovery of their true source.

When the induced sleep is light, as happens in many cases, the mere withdrawal from the side of the patient will cause him to awaken. When the induction is more complete the time for recovery is longer. In one case it was nearly five hours. But when the time for spontaneous restoration is even very short, as, for example, half an hour, the presence of persons who constantly attract the attention, and tease the patient, has the singular effect of prolonging the magnetic state for hours. As such a prolongation is not designed by them, it seems probable that the disturbance prevents the subsidence of the artificial or induced state, and thus tends to postpone the recurrence of the normal condition. I have observed also that the restoration is much more speedy and complete when the magnetizer leaves the room or the house. A patient who never awoke spontaneously while I remained in the room where I had soporized her, awoke in half an hour when on one occasion I was compelled to leave her for that period of time. No one disturbed her in my absence. For these reasons I am disposed to believe that when one magnetized person seems to put another to sleep, the latter falls voluntarily into the mesmeric state. The proof of this is found in the fact that such a feat is never performed on one who has not been mesmerized before, and who has not therefore learned how to mesmerize himself.

A curious inquirer might here raise the question, how far is it possible for this inductive power to act in detail, or how far can we succeed in influencing on this principle the power or locomotion of one or more limbs? Another might ask whether the thoughts, so much under the influence of nervous motion, might not thus be produced by reflection from the mind of the operator? As I have never been able to produce in this manner thought or motion, I scarcely deem the question of much importance, and place the queries here simply because they were asked by an ingenious friend to whom I explained my theory of induction.

Much has been said of the *medical intuition* of mesmerized patients. They have been supposed to be able to declare the nature not only of their own cases, but of those of others, and to point out, with unerring certainty, the remedies for both. The physician who takes the trouble to read the reports of these *pathologists by intuition*, cannot fail to smile at the ridiculous absurdities into which they fall, and when he hears them prescribe! No man of common sense could credit this pretension, especially after being told that these sleeping Galens prescribe exactly according to knowledge previously acquired. Thus, a French girl recommends ptisans and mercurialized goat's milk, for what an American would suggest Indian gruel and butternut tea. The same patient is ordered by different somnambulists very different and sometimes opposite remedies. Thus, one says, "Never bleed that patient;" another, "If you do not bleed, she must die." I have known of these sleeping oracles place the heart in the right hypochondriac region, the kidney in the anterior part of the chest, and the spleen in the position of the bladder. In *every case* where I have been able clearly to diagnosticate the pathological state, I have found my somnambulists mistaken in their decision. As to their practice, it was usually too absurd to be followed; and any confidence in it which might possibly have been otherwise obtained, was destroyed by the ludicrous pathology and the clumsy pharmacy.

Persons in the most perfect health, being produced as invalids, have been treated, by the somnambulists, pathologically and practically, as affected with grave maladies, while others, suffering with severe diseases, have been supposed to be lightly afflicted. In fine, if the somnambulist is ignorant when awake, he is, unless otherwise *taught* at the time, no less so when asleep; and whatever may be his sagacity when his eyes are open, he is, when mesmerized, susceptible of the belief of any possible assertion or absurdity.

Another remark: The delicacy of patients is not lost in the mesmerized state; so that any observation calculated to suffuse the cheek, produces its wonted blush. I saw a young lady color deeply when asked whose was the miniature she wore in her bosom. Now, let me inquire, what would be the effect on a modest man or woman, of giving to either the alleged power of seeing through opaque barriers—a power which must be possessed by those who describe, as if seen, the interior of the human body? If the innocency of Paradise be not added to the faculties of the *clairvoyante*, she must feel the absence of the fig-leaves, and turn from the painful exposure of her acquaintance.

Unless medical lore have reached its millennium of perfection, there is yet something to learn both pathologically and therapeutically. Most physicians think our useful art has scarcely passed the vestibule of truth, and that its palmy state is in the years of a distant future. If so, how is it that medicine, by mesmeric divination, has not added a single remedy to the catalogues of the schools, nor detected the true nature of one ambiguous disease? We are, no doubt, surrounded by the useful unknown, the, as yet, unavailable treasures of nature. Has mesmerism, after sixty-eight years of zealous labor, presented to science or art one single trophy? It has not, and it remains for the advocates of its intuition to tell us the wherefore.

Mesmeric writers and charlatans speak loudly in favor of the remedial uses of magnetism in the treatment of diseases, particularly those of a nervous character. In a former part of this paper I have already recited some cases which have an interesting bearing on this portion of our subject. But a reference to them will show that the effects were, in most cases, very transitory, and that in no case did the processes used do more than afford temporary alleviation. What may be effected by further experimentation remains to be shown. At present we see nothing for felicitation on this subject, since in a five years' scrutiny I have not found any good reason for preferring the mesmeric to the usual medical means in any single case. That mesmerism is sometimes an agreeable and useful auxiliary, the cases cited clearly prove; but that its effects are more than palliative and temporary cannot be admitted. A case of any importance has not, so far as I know, been cured by mesmerism. During a suite of experiments, a few persons of delicate constitution and disturbed nervous functions have been restored to health and good spirits, but whether through mesmeric potency, or the mere agency of time and events, cannot be known. The infrequent occurrence of such results leaves the question doubtful and unimportant; and, as yet, no mesmerizer, as such, has been able to establish, in our country at least, such evidence of the utility of his art as to attract to himself a profitable business. With very moderate success, and a mystery to boot, our credulous and wonder-loving age would send its golden offerings to the shrine of mesmerism, but even the little merit for its inception is wanting; and the few half false, half true certificates on which empiricism builds its golden structures, are not given to the successors of Mesmer and Perkins.

Let me not, however, be understood as denying to mesmeric processes all medical value. Cases occur in which ordinary means fail and mesmeric action is useful. But they are few and far between. They are, as far as I can now judge, functional affections of the nervous system of a temporary character, or sympathetic irritations of a weak affinity. Exalted sensibility may be softened or abated in a few persons by such means, and the ordinary feeling of a part may be so lessened in some cases as to admit of painless operations. The fewness of the "susceptibles," and the time and trouble necessary to find them, must ever render mesmerism of little *general* value. To those who feel readily its influence, it often proves a most valuable privilege, when it is used to soothe the disturbed nervous system, to procure sleep, vainly sought by other means, and to abate the sensibility of parts already in pain, or about to suffer from surgical operations.

Rare as are such cases, the good physician will always be glad to meet with them; and if he wishes to defeat the empirics, he must not entirely overlook this and every other proper means of relief. It may not, therefore, be from the purpose to give a few simple rules for the application of mesmerism to the cases which demand its use.

The great general rule for the remedial application of mesmerism is founded on the fact that the nerves of sensation are obtunded by the mesmeric sleep, and that when there is no such sleep, a part of the body may be deprived of its feeling in a greater or less degree, by the mere application of the palm of the hand, or, still better, by moving the hand *slowly* over the part, in the peripheral direction, always taking care to carry it back to the point of departure, at a distance from the surface to be acted on. When a part is defective in sensibility or power of motion, it may be sometimes restored to its usual state by centripetal movements; the passes directed toward the great nervous centre being always restorative. The time required for a full effect seldom exceeds a quarter of an hour. In this way by attending to the direction of the movements of the hand, the pain of neuralgia, of burns, and of bruises, may be sometimes greatly relieved; and parts stiffened by long disuse may be restored to locomotive power by an almost painless experiment.

But on such applications no certain reliance can be placed; while mesmerism labors also under the disadvantage of sometimes producing disastrous results. Frightful disorders of the nervous system and extraordinary perversions of the mental state, follow now and then the imprudent use of mesmeric processes.

I myself once produced in a lady, apparently in good health, the most alarming convulsions, from the sinister effects of which her constitution did not entirely recover for two years. It is true, that she resisted the mesmeric influence, and may have been thus more gravely affected; but the resistance itself cannot always be guarded against, as proved by this very case; for, observing her efforts, I warned her of her danger, and proposed to suspend the experiment, but she herself insisted on my persistency.

On another occasion, I was engaged in magnetizing a fine, tall, stout young gentleman, in full health, when his circulation was suddenly arrested, and even his heart ceased to beat. He fell as if dead on the floor, but a few reversed passes restored him to his usual state, without leaving behind any known evils. But he might not have been relieved.

I have been, not unfrequently, called up in the dead of night, to see persons who had been thrown, by imprudent and ignorant mesmerists, into singular nervous affections. Sometimes, without any signs of somnolency, the subjects of mesmeric action become faint, paralytic, or maniacal. In other cases they fall asleep, become alarmed, fancy that they cannot be awakened, and betray, by the most violent moral and physical expression, the intensity of their agony. The mesmerist is himself sometimes the cause of the terror; for he imagines that he has not the power to awaken his patient, or perhaps the knowledge of the process proper for restoration; and thus, becoming alarmed himself, communicates his fear to the subject and the spectators.

At 4 o'clock A.M., one tempestuous winter morning, a parent, in the agony of his apprehension for the safety of his only child, called me to assist in rescuing her from a mesmeric sleep as deep as ordinary coma. The mesmerizer, young and inexperienced, losing hope, had toiled vainly from 10 o'clock P.M., and was himself in a state bordering on phrensy. A calm deportment, choice of position, and quiet passes, sufficed in half an hour to restore this child to the arms of her parents.

In another instance I could not rectify the condition by any mesmeric means, but was compelled to lay the patient in bed, watch beside her couch, and leave her system to the slow process of spontaneous recovery.

But the most distressing case was that of a young lady of fifteen, who, living at a boarding-house in the city, at a distance from home, was, for the amusement of a company of young people, mesmerized by a gentleman who had seen but never used the processes of magnetism. The sleep came, the experiments went off well, and everything augured favorably, until attempts were made to arouse her. Finding the time longer than he had anticipated, the mesmerizer became alarmed, called for water, sank into a chair, and fainted. The patient, half awake, partook of the alarm, opened her eyes, screamed in an agony of fear, and soon losing sight of the true cause of her apprehension, imagined herself surrounded by robbers. Fleeing, with frantic gestures and disheveled hair, from the room, she treaded, with amazing skill and rapidity, the darkest intricacies of the mansion, and found her way even into the street. It was just as they forced her back into the house, that I arrived. I took her hand, told her I was her friend, led her to a seat, and endeavored to awaken her mesmerically. My gestures alarmed her again, and it was only after replacing her in the mesmeric slumber that I succeeded in bringing her back to natural consciousness. She remained ill for several days, having a sense of great prostration, palpitation of the heart, and a severe headache. Notwithstanding such a warning, the poor girl was again mesmerized, became violently enraged, fought with the fury of a tigress, and after her restoration, required local depletion, and the medical attendance of a week to confirm her injured health.

I saw one other case of a nearly similar character, in which I had myself the curiosity to *repeat* the experiment, believing that the untoward phenomena arose probably from the peculiar influence of the first experimenter. The patient did not, it is true, suffer as at first; but I saw enough to entitle me to forbid the repetition of the mesmerism; and I was compelled to watch the case for several days afterwards, fearing, continually, the recurrence of a nervous paroxysm.

Such cases, though they occur rarely, demonstrate forcibly the hazardous nature of the mesmeric processes, when applied for the purpose of producing sleep. When directed merely to the obtunding of nervous sensibility, or the removal of local pain, they seldom do harm; yet, even then, they should be intrusted only to those whose studies and professional experience enable them to judge of the propriety of the operation, and of the limits of perfect safety. Even they should resort to this extraordinary mode of relief, solely when better established and more fully understood methods of cure have been tried in vain. Though upwards of seventy years in use, mesmerism has been studied to little advantage. The whole field of its remedial applicability is yet uncultivated. Whether weeds or flowers may grow there, remains to be tried. That it is an agent of great power over the nervous system has been, I think, plainly proven. But it will require a long, patient, and exact investigation, such as I have attempted, with respect to its general laws, ere its medical value can be satisfactorily settled. The way to such an examination is now open; for, as the *opprobrium* of the subject vanishes before a rigorous investigation, we shall be embarrassed no longer by extrinsic and artificial difficulties.

If no other good has resulted from the labors narrated in this paper, the analogies pointed out experimentally between natural and artificial nervous disorders, and the introduction of a new process for relief in the former, assure me that I have not worked in vain. I may venture to hope, too, that many symptoms of nervous disorders will now be better understood than formerly, and that some peculiar forms of insanity may meet with a more philosophical treatment.

In any event, the collection of curious and digested phenomena will aid some more fortunate inquirer, in his way to new and just principles, on the very difficult subjects of nervous and moral action. Should I have done no more than that, it would be labor well bestowed. But the research has been itself a most agreeable, though arduous undertaking. The field is so new, the phenomena are so startling, and the subjects involved so important, that I feel willing to continue the eventful investigation, hoping that, in the future pursuit, I shall have many companions on the way, so as to enliven the chase and multiply the chances of discovery.

THE RECAPITULATION.

1. The investigations into the claims of mesmerism have been hitherto imperfect, because they have been conducted either by interested partisans or prejudiced opponents.

2. All previous examinations of this difficult subject have been directed rather to its undue pretensions than to its less obtrusive foundations.

3. The researches of the committees, detailed by learned societies, have been contradictory and unfruitful, chiefly because they examined the trained subjects of the mesmerizers, rather than those among their own friends and acquaintance, on whom they could rely for the unsophisticated representation of the natural phe-

nomena of mesmerism. They invited deception, and either implicitly confided in it, or, having detected the attempt to mislead, condemned the whole system as one of fraud and imposture. Hence, they were always in those extremes which border on truth, but are never within its confines. Astronomy is not the less true, because the ignorant believe that the stars are holes through which the light of heaven breaks, or because astrologers pretend to see the fates of humanity registered in the conjunction and disseveranee of the planets.

4. Imagination and imitation cannot account for the uniformity of the phenomena of the mesmeric state in persons, of all ages and conditions, who are totally ignorant not only of the symptoms to be produced, but of the design of the mesmerizer.

5. Neither will they explain the analogies found to exist between natural and artificial somnambulism.

6. Nor can we, by any rational view of their causes, ascribe to anything but a physical influence, the effect of passes on the diseased condition of certain patients, some of whom did not observe the manipulation, and none of whom understood its objects.

7. Admitting that the mesmeric sleep may be, and is produced solely by mental means, the methods, as well as the phenomena of restoration, both in natural and artificial somnambulism, forbid us to believe that the patients are usually conscious either of tho act or the intention. Many of them showed plainly their ignorance, by their conversation at the time, and others were totally incapacitated for observation.

8. If we admit the awakening without the aid of the patient's mental co-operation, we ean find no reasonable

difficulty in believing that the mesmeric sleep is producible also without that co-operation.

9. The phenomena of artificial somnambulism are,—

First. An exaltation of the circulation, without a corresponding increase of the respiration.

Secondly. An obtunded sensibility to causes of pain, and sometimes, though rarely, its total obliteration.

Thirdly. The more or less complete obliviousness of the thoughts and events of the mesmeric state, while awake, although the memory of the events of the natural state is strong in the artificial state.

Fourthly. The retention of locomotion, and the facility of being led into suggested dreams, are also curious effects of the mesmeric action. Nothing is too high for the daring, or too absurd for the belief of the dreamer. But all the mesmerized patients are not susceptible of this influence. A few subjects resist, even when asleep, all attempts to mislead them; although they present most of the peculiarities of somnambulism.

10. To this property of artificial dreaming may be referred the alleged miracles of clairvoyance, intuition, and prevision. The subject dreams that he sees, and the questioner is deceived by his confidence, his plausibility, and his ordinary character. He knows him to be honest, and he does not perceive that he is himself led astray by his uncorrected imagination. There is all the effect of a fraud, without intention to mislead, and without blame.

11. The mesmeric effect is usually producible within ten minutes, and at the first sitting; but some persons have yielded only after long and repeated trials. In general, unless very marked effects are exhibited within

half an hour, all subsequent attempts to mesmerize are fruitless.

12. The mesmeric sleep may be dissolved by time alone, the natural duration of the paroxysms lasting from thirty minutes to nearly five hours. The fear of not escaping from the spell, in the event of the death, or absence, or loss of power of the magnetizer, is therefore not well founded.

13. The artificial solution of the mesmeric sleep acquires sometimes only a single wave of the hand, sometimes many. The mean time is about two minutes.

14. Independently of the voluntary aid of the mesmerized subject, the time taken to dissolve the sleep is very sensibly affected by the distance from him. Thus, in contact, a case consumed 4′ 4″; at two yards, 7′ 30″; at four yards, 16′ 45″.

15. Sex does not appear to exercise any very marked influence on the mesmeric susceptibility.

16. Age is a more modifying cause than sex. Though no age is exempted, the very young and old seem least susceptible; and the period of life between twelve and twenty is that most favorable to the mesmeric influence.

17. Of the temperament, the nervo-sanguineous seems most liable to mesmeric action. The magnetizing power appears to be very generally possessed, but the susceptibility to soporose mesmeric impression is confined to a few individuals, being about one in seven or eight of those subjected to trial.

18. Although without an exception, so far as I can discover, mesmerists agree in believing that a sound state of health is unfavorable to the success of their operations, I have found it most conducive to well-marked

magnetic results. Of twenty-six somnambulists, nineteen were in good, and seven in bad health.

19. The *rapport, relation or communication*, supposed to have an absolute existence, dependent on the mesmeric fluid, seems to be entirely voluntary on the part of the patient, and to rest on his knowledge of its supposed necessity. It is therefore a delusion, but one of the greatest convenience to the public exhibitors of mesmeric wonders.

20. The delusion as to the "rapport," is one of the many hallucinations of the mesmeric state, for which the subject of it is no more answerable than for any of the wild and monstrous dreams to which the disordered fancy may be led in that unnatural condition both of mind and body. This truth is clearly proved by analogical cases of insanity where similar delusions continue for years.

21. The mesmeric state curiously modifies the condition of the senses. Sight, hearing, and touch are usually improved; taste, smell, and sense of pain as commonly impaired.

22. As the sense of touch and of pain are so diversely affected by mesmerism, we are led to regard them as independent senses, probably therefore supplied by separate nervous fibres. Such an inference ought to have been made before, for many organs have the sense of pain but not the sense of touch. The presence of a poison will give pain to the stomach or intestines which do not perceive the motions of the worms that infest them. If this view be correct, the sense of pain is a *sixth sense.*

23. *Many* of the feats of the *clairvoyants* are the result of the sharpened hearing which enables them to de-

tect objects by the sounds they make. They really believe they see them, and so does the exhibitor, although he aids them by handling audibly the various objects. Thus he opens and shuts a pencil, a penknife, or a spectacle-case, and rubs a stick, or a sheet of pasteboard. He always makes as much noise as possible with everything, and he generally asks the producer of a marked card to explain the words or device to him.

24. As we cannot believe in mesmeric *rapport*, so we are not able to credit the existence of any peculiar sympathy between operator and subject. Untrained or ignorant patients never show sympathetic phenomena. I have been pinched, and hurt otherwise, a great many times, without observing any suffering on the part of my subjects, until they were taught to believe that such a relation existed, and then they very honestly felt hurt, as people do in dreams, a kind of imaginary suffering.

25. The phrenological phenomena of mesmerism, when rigidly examined, are found to consist, as do most of the mesmeric wonders, of "such stuff as dreams are made of." *The excitement of the brain is general, the direction of that excitement is given by the mesmerized person's knowledge of phrenology*, but the patient is not in any case aware of his mental co-operation. This singular delusion or misapprehension runs through nearly the entire subject of mesmerism, most of the phenomena of which are a strange mixture of physical impulse and mental hallucination. Phrenologists alone feel the phreno-mesmeric excitement. Persons partially acquainted with phrenology, experience it only as to the organs known to them; while those who are totally ignorant of the subject, present no local manifestations until they are taught, either awake or asleep, what they should

know, and what they should do. The displacement of old organs in one city, their retention of location in another, and the adherence of the patients to the peculiar and dissimilar systems of phrenology which they have been respectively taught, show clearly that the direction of the cerebral excitement is personal and arbitrary, while the new maps of the cranium, so widely different from each other, leave us no longer in the least doubt as to the delusive source of the compound science of phreno-mesmerism.

26. The mesmeric influence is the effect of what the natural philosophers call induction. The will of the operator acts solely on himself; his altered system reacts by proximity on the subject of the experiment, by an unexplained power, analogous to the equally inexplicable induction of the mechanicians and the presence of the chemist.

27. Mesmerism may be sometimes usefully employed to allay nervous irritation, procure sleep, and obtund nervous sensibility during surgical operations; but from the fewness of susceptible persons it can be used very seldom for such purposes. In all other cases it appears to be of little use, and, so far as I know, has never cured any serious disease. On the other hand, it sometimes, especially in unpracticed hands, produces frightful disorders both of mind and body, and should, therefore, be resorted to solely for proper and important purposes, and then only with due precaution.

28. The cases of natural somnambulism, so like those of the mesmeric state, the permanent magnetic power of some individuals, the relief afforded to paralysis and stupor, and the restoration from natural somnambulism by mesmeric passes, go far to show that the disturbance of

the nervous system, which is produced by mesmerism, may, and does, occur in certain stages of disease, and is not unfrequently present in nervous affections, where we have not, hitherto, suspected its coincidence.

29. Mesmerism may, for the above reasons, be employed to relieve, temporarily, affections of a nervous character, when the usual means fail. But it should be used always with caution, and only when the failure of all ordinary measures render its application a matter of necessity.

30. The claim to a peculiar medical intuition, set up by magnetized persons or their exhibitors, is destitute of foundation. The pathology is usually absurd; the prescriptions are inefficient, dangerous, or ridiculous; and, after sixty-eight years' mesmerism has not detected a new theory of disease, or suggested one useful remedy.

Doubtless the mesmerists will say, that I pay too little attention to the testimony of others on many of the points in which I differ from them; and others may allege, that for all that part of the subject which I admit to be true, I give too much weight to my unsustained personal labors and observations. In both I may with truth, and without undue pretension, reply, that I did not expect to settle any question *definitely* by these researches. They were made carefully and honestly, and the results set down without exaggeration or extenuation, for the purpose of making as close an approximation to an obscure truth as the time and opportunity would permit. Others, following in the same exact path, may enforce or weaken my conclusions, but sure I am, that it is only thus that we shall finally settle these vexed questions, and not by opinions founded on unrecorded observations, or vague generalities derived from loosely kept records. While I find volumes of conclusions, I discover no tables to which I can refer for support or refutation. I see many edifices, but I discover no foundations for them, and naturally infer that as they rest on no solid basis, they are without weight, and made of imagination.

As to the charge of refusing the testimony of others, I answer, that their evidence is so conflicting as to destroy itself. The most substantial proof, that of distinguished medical men, is usually on my side, and if I have not availed myself of that, how can those complain who give opinion on the other side? Few are competent to observe in a question involving medical knowledge and scientific attainment. He who would truly understand such phenomena, must know all that is known of the nervous system, and much that is taught as physical science. He must have studied also the human mind in health and disease, and have examined the kindred complaints of somnambulism and catalepsy. Now, it is not a little remarkable that the authors who have written in favor of the higher claims of mesmerism have not been thus prepared, while the more accomplished observers have decided against these claims.

Let me illustrate this further. Phenomena are observed in the heavens—among the stars. Every one sees them—but to whom do we look for the explanation by which these phenomena are fashioned into facts? For how many thousand years did the constellations glide across the zenith in mighty brilliancy, observed by millions of eyes, before the splendid phenomenon assumed, to the human understanding, the shape of a fact? Until explained by Copernicus, it was a bright illusion, the very opposite of that which it seemed. If this illustration does not lessen the confidence of ignorant observers in their powers of discrimination, I am at loss for means to teach them the humility which can alone give much to the observations of any one, however otherwise prepared for investigation. That sleepers often describe well distant places and events, is true; but does it follow that they obtain the knowledge by spiritual inspection? or are they indebted to other and more intelligible means of discrimination? It is not less true that there is sometimes the manifestation of strong personal sympathy between mesmerizer and subject, but are there not unexamined sources of error in the most obvious explanation of this phenomenon? The dispute is less as to the appearances than the view to be justly taken of them. The vast and airy beings that darkened for ages the skies of the Brocken Mountains were the wonder and terror of the ignorant peasantry, until more competent observers proved them to be the shadows of human beings, cast by the rising or setting sun in exaggerated volume, on a screen of clouds. That which had been a frightful phenomenon became an agreeable fact. The shadowy things of artificial somnambulism have long enough displayed their visionary forms on the sky of human wonder. It is time to give them that true import which will take them from the mountebank and pretender, and place them in the hands of philosophy. If I can believe that I have done only so much as to bring philosophy to the task, free from prejudice and restraint, I shall be satisfied that my labor has not been in vain.

CONCLUSION.

I may be, perhaps, justly charged with giving to the subject of mesmerism an undue importance, and bestowing on it a disproportionate share of time and attention. The results, being chiefly negative, add almost nothing to our stock of knowledge, and the pretensions now *demonstratively* overthrown, being discarded already by common sense, and the antecedent labors of others, scarcely deserve, in the opinion of the world, a passing notice. But I think I am justified in my laborious investigation by the interest still felt in the subject over a large part of the civilized world, by the want of digested and comprehensive facts, and by the bearing of the phenomena on the practice of medicine and on the physiology of the nervous system,

Perhaps, too, it may not be unimportant to the guardians of public and private morals, the administrators of justice, and the conservators of family and educational discipline, to learn what unsuspected physical agents are at work on the human frame at all times and in all places. They may thus be enabled not only to guard against abuses, but to make indulgent and charitable estimates of the character and extent of crime and error.

ON THE

PENETRATIVENESS

OF

FLUIDS.

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[Extracted from the American Journal of the Medical Sciences, for November, 1830.]

In 1829 I read before the Philosophical Society a short memoir on a new method of forming gum-elastic into thin plates, sheets, and bags. In some instances the balloons formed by the process then described, had, when filled with hydrogen gas, the power of ascending to a considerable height in the atmosphere. Those which were confined to the atmosphere of my lecture-room, at the Medical Institute, descended again after a period of time varying from an hour to two days. The cause of the descent, which did not seem of easy explanation, became a subject of investigation.

The gas might have escaped from the balloons at the ligature, or by permeating the dense wall of gum-elastic, or by uniting chemically with the internal surface of the latter. To free the gas from the compression to which it is subjected in a balloon, I confined it in a wide-mouthed bottle, over the aperture of which I tied very firmly a thin sheet of the elastic membrane. In a few hours the descent of the cover into the cavity of the bottle gave evidence of a diminution of the contained gas, and finally the cover was burst inward by the pressure of the atmosphere, so great had been the rarefaction of that which remained in the bottle. On weighing the membranous cover, no gain in weight could be perceived, and thus I presumed that the gas had escaped. By repeating the experiment, and covering the bottle with a small bell-glass holding atmospheric air, I found, after a time, in the latter vessel, an explosive mixture, while the contents of the bottle itself were found to be pure or nearly pure hydrogen. Evidence was thus afforded that hydrogen penetrated the membrane not by any *vis a tergo*, for no pressure was applied, but by some inherent power of considerable amount. The facility of permeation appeared also much greater in the hydrogen than in the atmospheric air, which, if it entered at all into the bottle, did not penetrate in any appreciable quantity, when fully one-half of the hydrogen had made its escape.

In the next experiment the arrangement of the gases was altered: common air was inclosed in the bottle, and a bell-glass confined around it an atmosphere of hydrogen. As was expected, the hydrogen entered the bottle rapidly, raised up the tense membrane, formed it into a globe, and finally burst through it, and thus made its escape from the confinement to which it had been spontaneously subjected.

The minuteness of the atom of hydrogen might readily enough account for the greater facility with which it penetrated the membrane, but could not be considered a good reason for the *energy* with which the penetration was accomplished. A gas having a heavy atom was, therefore, selected for further experiment, and carbonic acid, subjected to the same sort of confinement, was found to permeate the membrane with as great power, and very much greater facility. In succession, most of the gases were submitted to the same ordeal, and all of them found, except nitrogen gas, to exercise the same power, but with very different degrees of rapidity. The *power* was ascertained by comparison with common air, and the *rate of action* both in that mode and by comparison with each other. The depression or elevation of the membranous cover clearly indicated the escape or entrance of a gas, and when two active gases were placed one on each side of it, its rise or fall expressed the difference of rate, because each was, at the same moment, in the act of permeation, as proved by many examinations of the contents of the bottle and bell-glass.

Having once ascertained the rate of action of each gas relative to air, a prediction could be made as to their rate in reference to each other. Hence, gases which operated on air with nearly equal velocity, affected the *horizontality* of the membrane very little when placed on opposite sides of it. Thus carbonic acid and nitrous oxide act with great facility on common air, and in nearly equal degree; and when placed on opposite sides of the membrane, penetrate it rapidly, but cause a very slow change in its position. The facts here presented warrant the conclusion, that if two gases, equally penetrant *exactly*, could be found, they would, under the above described arrangement, mix uniformly, without in the slightest degree altering the state of the membrane.*

* Subsequently having discovered that olefiant gas and arseniuretted hydrogen have, with reference to common air, exactly equal rates, they were placed on opposite sides of a membrane, with the full expectation of sustained horizontality on the part of the membrane; which was confirmed by the result.

The greatest possible degree of effect on the membrane arises when we place on opposite sides of it the slowest and most speedy penetrator; for instance, nitrogen and sulphuretted hydrogen. In that case the change is immediately visible.

As in all the previous experiments different gases were placed in comparison, I placed the same gas on both sides, and expected, for the "sufficient reason," no change. The experiment accorded with expectation. The membrane remained stationary.

The circumstances *essential* to the transmission of gases through the membrane, formed an interesting subject of inquiry.

My first attempt was to produce a vacuum, by placing the gas in a bottle, and exhausting, by means of the air-pump, the bell-glass which covered it. The gases effected their escape from the bottles thus treated, with a velocity proportioned to the rate of permeation already ascertained; sulphuretted hydrogen passed out more rapidly than carbonic acid, and that than hydrogen. Still as *some* air is always found in an exhausted receiver on the finest air-pump, I passed a tube containing carbonic acid into a Torricellian vacuum, where it very speedily escaped and caused the descent of the mercury. Even this experiment could not prove perfectly satisfactory, as mercurial vapor occupies the barometric vacuum. A perfectly empty bag carefully closed was placed in carbonic acid and nitrous oxide successively, without undergoing the slightest inflation. If a very small portion of *any kind* of air remained in the bag, inflation followed, provided the bag were exposed to a *different* gas.

By another arrangement I obtained my object more unexceptionably. Having found by inverting a bottle holding confined gas, and thus plunging it into mercury, that no gas escaped, and that consequently mercury could not promote or sustain the permeation of the gas, I reached my object by the following means. Closing a tall cylindrical lamp-glass at *one* end with gum-elastic, and filling it with mercury, it was placed, so filled, on the shelf of the mercurial trough, having the end closed by the membrane uppermost. Through this fine film the mercury could be plainly seen in close contact with its under surface, while the deep depression of the membrane showed the power of the column of mercury by which it was drawn down. By leaving it in the air, or by placing over it a bell-glass of any gas, more slowly, but at their settled rates, the gases penetrated the membrane and accumulated in the cylinder, thus permitting the descent of the mercury. The process continued long after the mercury had abandoned the surface of the membrane, and the space was occupied by the gas, in, of course, a rarefied state.

It became, then, evident, that anything which could remove the gas from the surface of the membrane at which it had arrived by penetration, would continue its transmission. Of course, then, agents chemically attractive of a particular gas, when placed beneath the membrane would promote its permeation. In fact, lime-water and solution of baryta were rapidly carbonated by the transmission of carbonic acid, and sulphuretted hydrogen almost instantly precipitated the lead of the acetate placed in solution on the opposite side of the membrane, which became black on the side of the solution. A neater mode of performing this experiment is the following. Inject by means of a gum-elastic bottle and pipe, into a very small bag of gum-elastic, stretched until fully transparent, a solution of the substance to be acted on. Carefully tied, washed, and dried, the bag is to be passed up through mercury into a receiver holding the gas, which for solution of baryta should be carbonic acid, and for that of acetate of lead, hydrosulphuric acid. In a few moments, in the former case, a white coat is seen to completely line the internal surface of the bag, and in a few minutes to fall down and accumulate at the bottom of it. In the latter case, the inner coat is dyed indelibly black. In either case, if water be alone placed in the bag, it will absorb a considerable quantity of either of these gases, and their presence may be ascertained by the usual tests.

If any suspicion had arisen that the gases escaped or entered by the route of the space included under ligature, it was dissipated by all the experiments mentioned in the last section; inasmuch as in the first experiment, that with the lamp-glass, the gas was seen to stud beautifully the under-surface of the membrane, standing on it in minute drops or bubbles, mistaken at first for water. In the experiments with baryta and lead in bags, the whole surface was covered, the precipitation taking place *only* there. Especially was it manifest in the last experiment, where the inner surface was stained black, while the solution remained clear and colorless. The gas, therefore, penetrates through *every part* of the membrane.

Being desirous of ascertaining more accurately the relative facilities of transmission, I solicited the assistance of my friend and pupil, Professor J. K. Finley, to whose patience, skill, and delicate manipulation, I owe much of the certainty of the following experiments.

Having constructed a syphon of glass with one limb three inches long and the other ten or twelve inches, the open end of the short leg was enlarged and formed into the shape of a funnel, over which finally was firmly tied a piece of thin gum-elastic. By inverting this syphon and pouring into its longer limb some clean mercury, a portion of common air was shut up in the short leg, and was in communication with the membrane. Over this end, in the mercurial trough, was placed the vessel containing the gas to be tried, and its velocity of penetration measured by the time occupied in elevating to a given degree the mercurial column in the other limb. Having thus compared the gases with common air, and, subsequently, by the same instrument, and in bottles, with each other, I was able to arrange the following gases according to their relative facility of transmission, beginning with the most powerful:—ammonia, sulphuretted hydrogen, cyanogen, carbonic acid, nitrous oxide, arsenuretted hydrogen, olefiant gas, hydrogen, oxygen, carbonic oxide, and nitrogen.

Ammonia transmitted in 1 minute as much in volume as sulphuretted hydrogen in $2\frac{1}{2}$ minutes; cyanogen, $3\frac{1}{4}$; carbonic acid, $5\frac{1}{2}$; nitrous acid, $6\frac{1}{2}$; arsenuretted hydrogen, $27\frac{1}{2}$; olefiant gas, 28; hydrogen, $37\frac{1}{2}$; oxygen, 1 hour and 53 minutes; carbonic oxide, 2 hours and 40 minutes.

Nitrogen has a rate of penetration so low as to be difficult to ascertain, because there is no gas of a lower rate with which to compare it. Only by causing it to pass through a membrane by means of a column of mercury, is the fact of its transmission known. In that way, the quantity being compared with that of carbonic acid, its rate was found to be about three hours and a quarter.* This experiment, made but once, is not confidently relied on; but the rate of nitrogen is unquestionably less than that of carbonic oxide.

Chlorine immediately altered the texture of the membrane, as did muriatic acid gas, sulphurous acid, nitric oxide, and some others, so that it was impossible to reach, for their rate of penetration, accurate results.

In every case the movement of the gas through the membrane became progressively slower, until it totally ceased; and finally, but more slowly, the mixed gas returned, as indicated by the descent of the column of mercury. The retrogradation ceased only when the two columns came to equilibrium, or failing the possibility of that, when the mercury in the shorter limb had reached the membrane, through which mercury has not been found able to penetrate.

Acquainted with the *fact*, and the relative *rate* of the penetrativeness of gases, the *degree of force* became the next subject of inquiry. That it was considerable, could be seen by looking at the stout membranes broken by it.

By greatly increasing the length of the taller limb of an inverted syphon, similar to the one already described,

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* A vessel filled with atmospheric air and closed by gum-elastic was submerged under water for two weeks, when it was found to contain only nitrogen gas. Possibly this arrangement may furnish a new eudiometer. It offers a new mode of obtaining nitrogen gas.

A phial containing atmospheric air, after being closed by a membrane, was placed in a receiver holding nitrous oxide. In about two weeks only nitrogen was found in the phial. These facts show the mechanically sluggish character of nitrogen *gas:* with its chemical inactivity we have been long acquainted.

I was able to bring to bear on the common air imprisoned in the shorter limb, a very considerable column of mercury. Up to a pressure of sixty-three inches of mercury, or rather more, equal to more than the power of two atmospheres, the penetrative action was found capable of conveying the gases, the subject of the experiment, into the short leg, through the gum-elastic membrane. The entrance of the gas into the short leg was expressed by the ascent of the long column of mercury in the other, which, as it entered, it was compelled to heave up. At the height of sixty-three inches, the membrane, though supported by cloth, could scarcely sustain the weight, and would not bear any increase of height. Although, therefore, at present, I do not know the limit of this power, I believe it will be found very much greater, because the power of the column which was tried did not, until a leak was sprung, seem to very sensibly affect the rate of entrance.

To the mind of a physician, the repetition of the foregoing experiments, substituting animal membranes for gum-elastic, would naturally suggest itself. Should animal membranes present the same phenomena, the interest of the investigation would be vastly enhanced, and a very important service done to the cause of "Physiological Medicine." That animal membranes would act in the same manner, was rendered probable by the well-known experiments of Priestley, who affected by means of oxygen the color of blood confined in a bladder. It had also been observed by him that a closely tied bladder, containing hydrogen gas, is found after a considerable lapse of time to contain only atmospheric air, and that, in quantity perhaps, equal to the hydrogen lost. Several other facts of the same kind are detailed by him.

Finally, in the *Journal of the Royal Institution*, I find the following notice of the singular inflation of a bladder, by Thomas Graham, A.M., F.R.S.E., Lecturer on Chemistry, Glasgow:—

"In the course of an investigation of mixed gases through capillary openings, the following singular observation was made:

"A sound bladder with stop-cock, was filled about two-thirds with coal gas, and the stop-cock shut; the bladder was passed up in this flaccid state, into a bell-jar receiver, filled with carbonic acid gas over water. The bladder was thus introduced into an atmosphere of carbonic acid gas. In the course of twelve hours, instead of being in the flaccid state in which it was left, the bladder was found distended to the utmost, and on the very point of bursting, while most of the carbonic acid gas in the receiver had disappeared. The bladder actually burst in the neck in withdrawing it from under the receiver. It was found to contain thirty-five parts carbonic acid gas by volume in one hundred. The substance of the bladder was quite fresh to the smell, and appeared to have undergone no change. The carbonic acid gas remaining without in the bell-jar, had acquired a very little coal gas.

"The conclusion is unavoidable, that the close bladder was inflated by the insinuation of carbonic acid gas from without.

"In a second experiment, a bladder containing rather less coal gas, and similarly placed in an atmosphere of carbonic acid gas, being fully inflated in fifteen hours, was found to have acquired forty parts in one hundred of this latter gas, a small portion of coal gas having left the bladder as before.

"A close bladder, half filled with common air, was fully inflated in like manner, in the course of twenty-four hours. The entrance of carbonic acid gas into the bladder depends, therefore, upon no peculiar property of coal gas. The bladder partially filled with coal gas, did not expand at all in the same jar containing common air or water only.

"M. Dutrochet will probably view, in these experiments, the discovery of *endosmose* acting upon aëriform matter, as he observed it to act upon bodies in the liquid state. Unaware of the speculations of that philosopher, at the time the experiments were made, I fabricated the following theory to account for them, to which I am still disposed to adhere, although it does not involve the new power.

"The jar of carbonic acid gas standing over water, the bladder was moist, and we know it to be porous. Between the air in the bladder and the carbonic acid gas without, there existed CAPILLARY CANALS through the substance of the bladder filled with water. The surface of water at the outer extremity of these canals being exposed to carbonic acid gas, a gas soluble in water would necessarily absorb it. But the gas in solution, when permeating through a canal it arrived at the surface of the inner extremity, would *rise* as necessarily into the air in the bladder and expand it. Nothing but the presence of carbonic acid gas within could prevent the disengagement of that gas. The force by which water is held in minute *capillary tubes*, might retain that liquid in the pores of the bladder, and enable it to act in the transit of the gas, even after the pressure within the bladder had become considerable."

A careful perusal of Mr. Graham's notice will excite, in every one who knows the value of experimental interrogation, an expression of surprise at the failure, on the part of that intelligent and ingenious chemist, to pursue in the only true spirit of science the investigation of a principle, one of the most striking manifestations of which had thus been placed conspicuously before him. Content with a single additional experiment, he comes, *in the ancient method*, to immediate conjectural explanation, and has thus lost an easy opportunity of making a beautiful, and perhaps extensively useful discovery. Made at an earlier period, his observation was published in the Journal for October, 1829, and has since attracted apparently no scientific attention. Such is usually the fate of the most pregnant facts which are not perceived to bear on some *generality*. This one passed from *my* mind along with all the other isolated phenomena of that number of the Journal, and only shone importantly when illuminated by the reflected light of an extensive principle, subsequently developed. These remarks are made, not to throw any discredit on the character of the accomplished gentleman to whom they refer, but to correct the baneful error of ancient dogmatism, which yet weighs so heavily on the cause of nature and truth. It was true that the carbonic acid entered a closed bladder, and that too *with power*, and it was equally true, that oxygen had done the same thing in the experiment of Priestley, and that, in his hands, even common air had penetrated to replace hydrogen in a similar viscus, and yet he ascribed the phenomenon observed by him to the *capillaries* and the conducting power of *aqueous canals.*

In what manner the *power* of "rising into the air" was given, and whether it was dependent on the force of water, or some other cause, does not and could not be made to appear from the single fact as presented by Mr. Graham. A very little practical interrogation, following the *word* just uttered by nature, would have obtained an answer fraught with new and important truth.

But to return to the immediate subject of this essay: Analogy, the experiments of M. Dutrochet, and the observations of Priestley and Graham, gave me almost the certainty of finding animal membranes performing relatively to the gases the same function which belongs to those formed of the inspissated juice of the *Jatropa elastica.* Accordingly, each gas was subjected to the action of animal membranes, which replaced the gum-elastic at the mouth of the short limb of an inverted syphon. Dried bladder, and gold-beater's skin, moistened to cause an approach to a normal state, and sections of various recent tissues, were successively tried, and found to act on the gases in the manner and order in which they were affected by gum-elastic. The more *recent* the membrane, the more rapid and extensive the effect produced; and in *living animals* the transmission was very rapid.

Besides the estimates of comparative movement made with the syphon, experiments in a different manner were resorted to, more clearly to show the general truth. Thus a piece of the strong intestine of a goose connected with the œsophagus and gizzard, being partially inflated with common air, and firmly tied, was left in an atmosphere of carbonic acid, where *in less than ten minutes* the inflation caused it to burst. On repetition of this experiment and examination before fracture, a very large quantity of carbonic acid gas was discovered to have entered the intestine. Crop, bladder, etc. etc. of recently killed animals produced exactly similar results. Perhaps the following experiment will be esteemed even more satisfactory: Carefully removed from the chest of a snapper, (*Testudo serpentaria,*) its lung was partially inflated with common air, and confined there by a ligature on the tracheal tube. Exposed in this state to an atmosphere of carbonic acid, or nitrous oxide, it became very soon fully inflated by the gas to which exposed, as subsequently proved by chemical examination. Less than half an hour of exposure sufficed for the full inflation of the lung, which was removed only when it threatened to burst. Containing a portion of nitrogen, it was left exposed all night to an atmosphero of oxygen, yet scarcely enough entered to signify its presence; in quantity superior to that which is held in atmospheric air. A taper appeared in it somewhat brighter than beforo its immersion.

In a subsequent experiment, the two lungs of a snapper having been extracted, were inflated, respectively, with common air, and carbonic acid gas. So prepared, each lung was surrounded by a bell-glass, containing an atmosphere of the other gas, so that common air surrounded the carbonic acid and *vice versa.* That lung which contained common air soon burst by the infiltration of carbonic acid, while the other collapsed by its escape.

In concluding the series of experiments, on *the question of fact,* some were made on *living animals.* A quantity of solution of acetate of lead having been thrown into the peritoneal cavity of a young cat, sulphuretted hydrogen was discharged from the pipe of the generating retort, directly into the rectum. In four minutes the poisonous gas killed the animal, giving to it, because of enormously dilated pupils, a very wild aspect. Instantly on its death, which was itself an affair of a moment, the peritoneal coat of the intestines, and the walls of the cavity in contact with them, were found lined with a metallic-looking precipitate, adherent to the surface, and susceptible of removal by nitric acid moderately diluted. It was the characteristic precipitate of sulphuretted hydrogen when acting on lead. When in another experiment, the abdominal cavity was almost instantly opened, only the intestines and stomach presented the bronzed aspect; the peritoneum of other parts, and the bladder, appeared of their natural color, thus proving that the gas had infiltrated, and not passed through any rent or fracture, an event which would have stained the whole of the lining membrane of the cavity, and dyed the bladder. This experiment forcibly reminded us of that where the internal surface of a gum-elastic bag holding lead-water, was stained black by sulphuretted hydrogen, while the solution continued pellucid.

In another experiment on a cat, a solution of acetate of lead was placed in the thorax, and sulphuretted hydrogen in the abdomen. Almost immediately, on the entrance of the sulphuretted hydrogen into the abdominal cavity, death ensued, with the same dilatation of pupil as before. On inspecting the thoracic side of the diaphragm, which was done as quickly as possible, the tendinous part of it displayed the leaden aspect of the precipitate by sulphuretted hydrogen. Many years ago, in 1823, while engaged in investigating Magendie's theory of venous absorption, I colored the diaphragm of a living cat blue, by placing a solution of prussiate of potash on one side, and that of sulphate of iron on the other. At that time I supposed the effect to be *vascular*, but the experiments on membranes of gum-elastic afford an explanation which more rationally refers it to *organic molecular infiltration;* for, in such membranes, vessels cannot possibly exist at all; and as animal membranes act in a manner so perfectly accordant with that of the coagulated vegetable juice, it would be judging against evidence, to refer their agency to widely different causes. At the same relative rates, with the same power, and that a great one, they could scarcely act, in obedience to causes so dissimilar as those alluded to.

Every one who has read the beautiful memoir of Dutrochet, on "*L'agent Immediat du Mouvement Vital, etc.*," and who has, as nearly all have, suffered their belief to be swayed by his eloquence of fact, method and style, will on a cursory glance at the experiments detailed in *this* paper, refer them to the "NEW POWER" so ably contended for by the French naturalist. That they depend on the *same power* cannot be reasonably questioned, whether that power be one long known, or recently discovered. In his experiments made exclusively on liquids, and developed with surpassing good fortune and sagacity, he proved the transmission of liquids through animal membranes, and saw them penetrating, too, *at different rates*, some solutions passing rapidly, some with greater slowness, some in scarcely appreciable quantity, and some never passing at all. Their *force*, too, he found to be of estimable amount. In fact, every aspect of the two sets of experiments tends more and more clearly to induce a reference of them to one and the same cause, whatever that cause may be. Although the facts presented by him demonstrate all this, yet M. Dutrochet did not perceive it, as is evident from his reference of the phenomena to a source to which, in latter years, the French naturalists and philosophers have been accustomed to look with almost superstitious reverence. Electricity is the great key of scientific explanation; and the theory of Du Fay is relied on, though badly itself sustained, as the *point d'appui* of almost all other theories. M. Dutrochet has accordingly ascribed the transmissions to that power, and supposed, in the very teeth of some of his most striking facts, that the current was from a less dense to a more dense fluid; or from positive to negative, dependent not on an inherent power of infiltration, and of course for the same membrane always the same, but varied or even inverted at pleasure, by arrangements productive of supposed electrical powers. He says, p. 129:—

"Ces resultats nous font déjà pressentir que l'impulsion qu' éprouvent les liquides dans ces expériences, dépend d'un courant électrique déterminé par le voisinage de deux fluides de densité ou de nature chimique différentes, fluides que sépare imparfaitement une membrane perméable. *Cette membrane ne joue évidemment aucun rôle propre dans cette circonstance; elle ne fait fonction que de moyen de séparation entre les deux fluides* auxquels elle est cependant perméable: *les liquides la traversent, soit dans un sens, soit dans l'autre, au gré de l'action réciproque des deux fluides qui baignent ses parois opposées.*"

As he used water and solutions in water, by which the former became denser, he found, as might be expected, that it infiltrated the tissue more readily than most of its solutions: hence, in such cases, the water penetrated more quickly than they, and the current usually set most rapidly from less dense to more dense. But when he used essentially different liquids, he yet found the water going through at its *high rate*, as we perceived to be the case with sulphuretted hydrogen and ammonia. Water traversed the animal membrane rapidly, to join *alcohol*, which, according to his electrical theory, should not have been the case, as the alcohol is less dense than water. For this and some other exceptions, Dutrochet attempts to account, by reference to influence derived from *chemical qualities.*

If, however, as in the case of the gases, two *liquids* of different rates of penetrativeness be placed on opposite sides of an animal membrane, they will in time present the greater accumulation on the side of the less penetrant liquid, *whether more or less dense*, but will finally thoroughly and uniformly mix on both sides, and at length if any pressure exist on either side, yield to that and pass to the other side.

As some substances have no penetrativeness, such as milk or blood, or at least their solid parts, the water placed on the opposite side of the membrane alone moves, and it is only after the decomposition by putrefaction, and consequent formation of a *new fluid having penetrant properties*, that any current sets in the direction opposite to that of the water. To prove this, it is only necessary to show that *alcohol* penetrates gum-elastic much more rapidly than *water;* and that, therefore, when that kind of membrane is interposed between them, the greater current is from alcohol to water, and not from water to alcohol.

A hollow glass cylinder, open at both ends, was closed completely by two membranes of gum-elastic, having been previously perfectly filled with alcohol. It was then sunk in the large pneumatic trough of my laboratory, where it remained one week. At that time it presented a concavity at each end, of decided depth, proving the escape of a considerable quantity of alcohol. On the other hand a similarly prepared vessel filled with water and submerged in alcohol, presented at the end of a week well-marked *convexities*, demonstrating the insinuation of alcohol. If it be contended that the nature of the membrane affects and even reverses the electrical state, it may be well said in reply, that there is no analogy for that, and moreover the same membrane acts under the movement of *gases* precisely as an animal membrane. The supposition would invest it with a most Protean character.

In making experiments for the preparation of gum-elastic by ether, that liquid was found to readily infiltrate its tissue. Alcohol has been already shown to penetrate it better than water, and water enters its substance so slowly, that a bag of a thinness productive of almost perfect transparency, and containing four ounces two drachms fifty-seven grains, lost by evaporation but eight grains in the first period of twenty-four hours, and fifteen grains during the next three days. Viewing these facts, a prediction was founded on them relative to the effect of placing ether in contact with one surface of such a membrane, while alcohol or water occupied the opposite surface. As was expected, the greater quantity accumulated on the side of the less penetrative substance, and the ether always caused by its transmission an augmentation of liquid on the side of the alcohol or water. Using *animal membranes*, facts of a similar kind, previously ascertained, led us to anticipate the *opposite* result. According to expectation, water being most penetrative, passed through so much more rapidly than ether or alcohol as to swell the amount of liquid on their side.

When alcohol is largely diluted with water, it penetrates an animal membrane more easily itself, and offers to the pure water which reaches it from the opposite side less invitation to infiltrate it, according to a law of *progressive diminution*, pointed out by our experiments on gases. Such a diluted portion of alcohol placed by M. Dutrochet in his endosmometer, and raised above the level of the pure water on its outside, found, in the force of the higher column, sufficient cause for its escape, which continued until the level was reached, when action apparently ceased. If the level be obtained at the commencement of the experiment, either no appreciable change is observed, or the movement is unquestionably in a direction contrary to that stated by Dutrochet. So, when gases are permeating in opposite directions any interposed membrane, the penetration soon begins to lessen, because there is on either side less porosity unoccupied, and there is also in them the repellent character of their gaseous state. M. Dutrochet reconciles these apparently contradictory facts to his system, by supposing chemical influence to produce the first, and electricity the second. In either case he does not appear to dream of independent and original powers of penetration, by which the liquid comes through to the opposite side of the membrane, *remaining* in *its* tissue, or passing on by a similar power of infiltration into new matter, or such matter being absent, accumulating on that side by the influence of mechanical power, or electrical excitement, or chemical combination,—truths adequately demonstrated by my experiments on gases.

The blinding effect of preconception on the most philosophic and candid mind can, perhaps, have no better exemplification than is afforded by what M. Dutrochet says relative to the point of accumulation, when a *diluted acid* and water were placed on opposite sides of an animal membrane. As alkalies produced toward them a current, for the support of his electrical theory, acids should be found to set the current toward water, and *he found it so.* In my experiments, the greater current was *always* toward the acid and not from it; and I find that Dr. Wedemeyer (Untersuchungen über der Kreislauf des Bluts,* etc.) has made the experiment with a like result. On reference to Dr. Togno's experiments, (*Amer. Journ. of Med. Sci.*,) which were chiefly repetitions of those of Dutrochet, we perceive that he does not seem to be satisfied perfectly with the report of Dutrochet on *this* subject. Let any one desirous of testing this matter, tie a piece of animal membrane over the end of a hollow glass cylinder, partially fill it with

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* See this Journal, vol. v. p. 199.

diluted sulphuric acid, and place it in a vessel of clean water, so as to bring the two columns to a level. In a few hours the column holding the acid will rise considerably above that of the clean water, proving the greater current to set from water to acid, and not from acid to water. Tests, however, show that *some* acid does pass the membrane.*

To feel *assured* of the error of Dutrochet, I repeated the experiment in another form. A tube of five-sixteenths of an inch in diameter, ending in a funnel-like extremity of an inch and a quarter, was covered at its broad end by animal membrane, then partially filled with diluted acid, and placed, membrane downward, in clean water, so as to bring both columns to a level. INSTANTLY the rise in the narrow tube was perceptible, and amounted to nearly half an inch in half an hour. Reversing the order, by placing the clean water in the tube and the diluted acid without, as sudden and progressive a descent of the column of clean water was observable. Tests, after a short time, betrayed the percolation of some acid, and finally, in every case the liquid became uniformly acidulous throughout, and the two columns fell to a common level, an event which may always be expected, unless the combination produced by transmission is not penetrant.

Water may be removed from the surface of a membrane at which it has arrived, in many and various methods. Invitation may be given to it by a column of mercury, contained in a hollow cylinder closed above by animal membrane. Water readily passes through, may be seen studding in drops the surface of mercury, gradually

* This fact I demonstrated to Dr. Togno.

covering the under side of the membrane, causing at length the separation and descent of the mercury, and continuing to enter the cylinder, until the mercurial column sinks to the level of the general contents of the trough. There the action ceases; but if the water placed *above* the membrane be now removed, the mercurial column will again rise, and all the water having escaped through the membrane by the process of infiltration into the atmosphere, the mercury will be finally seen in close contact with the membrane from which it had receded. Sometimes before the completion of the process, a change takes place in the condition of the animal matter, and some gas being introduced below, suspends the ascent of the mercury.*

A sponge *slightly* moistened, or dry oatmeal, or any other absorbent, placed, by means of a moderate weight, closely in contact with the membrane, will, by absorbing the water, cause its continued permeation.

Even *vis a tergo*, as in the instance of the gases, will produce infiltration where there exists no other cause of penetration. Over the end of the short limb of an inverted syphon was tied a piece of bladder, and over that, and *in close contact with it*, was also secured a piece of sheet caoutchouc. Water was then placed in the short limb, in communication with the bladder, and thus left for a few hours without compression. No appreciable amount of infiltration ensued. But, in a short time after a column of mercury had been placed in the long limb, water was plainly seen to insinuate itself through the bladder, and to raise up and separate from it the more

* A new hygrometer was suggested by this experiment, of which I purpose giving an account to the Philosophical Society.

elastic membrane which surmounted it. After all the water had passed into the space between the two membranes, the syphon was placed in its ordinary position, the end of the long limb resting in the mercury of the trough. Soon, the water repassed the bladder, ascended through the short column of mercury lying above it, and collected in the curve which then formed the pinnacle of the apparatus.

Another fact, in itself important, bears forcibly against the electrical theory of Dutrochet. To try the absorbent power of the dermoid tissue, pieces of it in a recent state, were tied, cuticle outward, over bottles which contained common air or carbonic acid gas. Over the bottle which held carbonic acid, was inverted a jar of common air, and over that holding air was placed a jar of carbonic acid. The more penetrating gas was in the one case in contact with the cuticle, and in the other with the dissected under surface of the skin. A trial of the contents, after twenty-four hours, showed that much more carbonic acid had penetrated in that apparatus where it was applied to the cuticle, than in the other. As in that case it had gone from the jar into the bottle of common air, while in the other case very little carbonic acid gas had escaped from its receptacle, I filled it again, and tied over it a piece of skin with its cuticle looking inward. In twenty-four hours the carbonic acid was equally diffused through both bottle and jar. Two similar sections of intestine were slightly inflated with common air, one of them being turned inside out. Both having been carefully tied at the ends, were placed in identically the same carbonic acid, in vessels of equal size. It was soon apparent that the one which had been inverted filled itself most rapidly, and although rather less than the other, soon greatly exceeded it in size and hardness. After remaining so exposed for eighteen hours, vessels of common air were placed over the distended bags, when a diminution of volume became in time apparent, and was more rapid considerably in the specimen which had not been inverted. It appears, then, that the transmission of a gas is easiest where it is placed on the cuticular or mucous surface of an animal membrane, rather than on its cellular or peritoneal surface—a fact to be kept in view in rating the transmissibility of the different gases or liquids. The fluids should be compared under exactly similar circumstances, standing in the same relation to the surfaces of the membrane used.

In the following experiment, made with great precaution, we perceive a result distinctly indicative of the superior penetrability of the cuticular surface. Over the mouths of two phials, accurately filled with alcohol, weighing, according to a *Pesé-Ether*, thirty-five and a quarter degrees, were tied two pieces of human skin. In one the raw side presented, in the other the cuticular side. Both were placed mouth downward in similar specimens of water, with columns of equal altitude. After the lapse of twenty-four hours, the alcohol was examined, and found to weigh more, by at least one degree, in the phial which presented the cuticle to the water. In it the etherometer sunk to thirty-three and a half, while in that which presented the dissected surface to the water it fell only to thirty-four and a half. The one had been reduced by the water one degree and three-fourths, and the other only three-fourths of a degree.

In all these cutaneous experiments, we perceive not only the agency of the membrane itself, but even that of its respective surfaces, so that we are not at liberty to admit the assertion respecting the action of the liquids, as independent of the influence of the intervening membrane.

In truth, it is now manifest that the liquid, if penetrative, permeates a given tissue at a rate dependent on the character of tissue and power of penetration. If on the opposite side there exist a substance or power capable of occupying or removing it as fast as, or faster than, the membrane delivers it, the actual rate of transmission will be as high as is possible; but if not so capable the accumulation will be at a lesser rate, and will represent the degree of permeability of the inviting substance alone. Thus, for illustration, if ether can convey away water as fast as, or faster than, the membrane can transmit it, the rate of penetration will be the greatest possible, and will represent the full penetrability of that membrane by water. But if ether is less penetrable than membrane, the rate of accumulation will not represent the power of the animal tissue, but that of the ethereal interstices, which, on the supposition, is less.

The *power* of this process in liquids, like that of the gases, is not *yet* measured. It is the *power of infiltration* in all such cases, and must be eminently great. Like all processes having dependence on molecular action, this one is influenced by electricity, when that is brought to bear on it, but we can scarcely, after a fair estimate of the value of facts, see anything more in the power than that of common interstitial infiltration, a power marvelously great, but insusceptible of demonstrative reference at present to any known cause.

The amount of force having been shown to greatly exceed that of atmospheric pressure, we feel assured that the interstices are penetrated not by any *vis a tergo*.

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It must, therefore, be attributed to some species of *attraction*, the force of which, as shown by the condensation of some gases by charcoal, sometimes equals a power of forty atmospheres, or nearly six hundred pounds on the square inch, a power amounting nearly to that of steam, *at its maximum density.** It is not chemical, because the quantity absorbed bears no relation to known affinities; it is not homogeneous attraction, for it takes place *solely* among dissimilar substances, and often subverts the condition produced by that power as in some cases of solution.

After having proceeded thus far with my argument and experiments, I felt as if it were important, if not essential to my positions, to test the power of gum-elastic as an absorber of gases independently of the artificial arrangements which brought different gases to the opposite sides of it. For that purpose I selected a hollow cylinder of gum-elastic, with thick parietes about an inch in length. This specimen was placed in a cylindrical graduated test-glass, filled with carbonic acid gas and placed over mercury. In less than one minute the mercury began to rise, and in eight hours, during which the observer was absent, it had risen to a considerable height. A rough attempt to measure the bulk of mercury raised, and of gum-elastic used, showed that nearly an equal volume of carbonic acid had been absorbed by the caoutchouc. A piece of dry bladder was subjected to the same treatment, and produced a similar rise of the column of mercury. Macerated in water for an hour, and then wiped well with a dry towel, so as to

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* Found by comparing the experiments of Cagnard de la Tour with those of the Committee of the Institute of France.

obtain dry surfaces, the same piece of bladder was again placed in the gas over mercury, and produced a diminution apparently equal in quantity to that which, when dry, it occasioned.

The bulk of the gum-elastic was considerably increased by the infiltration, so that although easily placed in the glass vessel, it was of difficult removal. This fact, added to that of the thorough penetration by water of an animal membrane macerated in it, shows how much of the phenomena described in this paper is attributable to the organic molecular infiltration. The remainder of the effect is dependent on the moleculo-porous relation of the gas or liquid to the substance beyond, into which infiltration carries the permeating substance. If the recipient beyond the membrane be as active as the membrane, or more so, all that the membrane brings to its surface will be transmitted as fast as it arrives; but if that recipient be of inferior penetrability, less will pass on than the membrane could carry through, and in that case the rate of penetrativeness of the substance relative to the membrane is inappreciable. Any gas penetrates another gas better than it does any solid, hence we obtain for *them* the true rate. But liquids penetrate each other sometimes less rapidly than at the rate of the transmission through the membrane. Such cases do not show the rate of transmissibility by the membrane, but of reception beyond.

Having completed the *first* series of experiments on molecular infiltration, before entering upon an account of the second, reserved for the next number of the Journal, it may be refreshing both to experimenter and reader, in a very toilsome investigation, to pass in cursory review some of the almost infinite theoretic and practical suggestions which flow from the facts before us.

The most striking generality, is that of the *high power* of penetrativeness of *gases* for *organic molecular tissue*, long known to be infiltrable by liquids, but until now not generally known to admit of any permeation, by at least *insoluble* aëriform substances.

Secondly. We are struck with an unexpected result, the *great* POWER of gases to infiltrate *each other*. It has been long known that aëriform substances, confined in the same apartment, finally mingle uniformly, and that, even if the lighter one be placed above the other. To account for this, and some other facts of the same class, Mr. Dalton supposed that each gas, in reference to the vertical relation of its particles, stood in an attitude of independence of any other gas present, as much as if no such gas were confined along with it, no particle of one gas being supposed to rest on any particle of the other, the interstitial cavities of one gas being in fact a *vacuum* for the reception of the molecules of the other, each for each.

The *power*, however, of this infiltration being known, we are entitled to conclude that the interspaces of gases are reciprocally occupied with a force similar, and probably equal to that which causes the imbibition of liquids by solids, and produces solutions of substances, even of the highest cohesive attraction. Solutions may now be esteemed infiltrations by solids and liquids of the tissues of each other, *requiring*, perhaps, only a *fitness in size*, rather than a chemical or cohesive attraction, for we see it subverting even the greatest cohesive power, and holding no apparent relation with known chemical affinities.

The atmosphere cannot any longer be considered as a mixture in the common acceptation of the term. Its gases penetrate each other interstitially with great mechanical force, so great as to defy all mere mechanical means of separation. It is an exemplification of solution.

When the particles of a solid separate and enter the tissue of a liquid, it is termed solution, when the liquid, penetrates the solid, and the latter maintains its solidness, it is usually called infiltration, imbibition, absorption, etc. etc. The processes are perfectly alike in principle, the different names being expressive of that and of certain accompaniments or effects also.

By means of our second generality, we are enabled satisfactorily to explain many phenomena not heretofore easily accounted for. Thus we understand how a gas or odor flows so rapidly through the whole tissue of a still atmosphere, and why some gases do so more speedily than others. An explanation is also given of the diffusion of odors, even against a draught or current, and it accounts for this fact, among others, that brimstone thrown into the fire, is perceived by the olfactories, when the draught of the chimney is even perfect.

As proved in some experiments, already detailed, many solids are dependent on water for the power of penetrating tissues, or gases, etc., and it appears probable that many odorous solids, in particular, enter the atmosphere, solely by penetrating its hygrometric constituent. Thus, in solution, coloring matters readily, in certain cases, pass through membranes impenetrable without such aid, and every one has perceived the singular smell of a dusty road, after a shower, even at a very considerable distance. In a damp day, or immediately after rain, we more distinctly and vividly enjoy the fragrance of the parterre. Malaria seems to be dependent on the same cause for its penetration into the atmosphere, for every one knows the greater hazard of a residence in low damp situations, and the general unhealthiness of a damp summer or autumn. As electricity is a great hydragogue, and substances in a negative state forcibly attract moisture, we might expect to find that season most damp and unwholesome in which the atmosphere maintained an electro-negative condition, and that driest and most healthful when it was electro-positive. Facts on this subject are yet to be created; but this one presents an aspect german to the subject. Mr. William Mason, of Philadelphia, a philosophical instrument-maker, respected both for his ingenuity and correct moral character, informed me, that when, in 1820, the yellow fever existed here as an epidemic, he could not excite an electrical machine at his residence, in the infected district, although at his shop, which lay at some distance from it, the operation of the machine was sufficiently powerful.*

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* Aqueous gas penetrates the air more or less rapidly according to the temperature and moisture of the atmosphere. According to our law of *progressive diminution*, evaporation is slower in a moister atmosphere, and *vice versa*. The following experiment shows that aqueous gas has also *its rate* of penetration. A long tube, surmounted by bladder, held water and mercury; the former of which being above, was in contact with the membrane. Although the mercury rose gradually as water escaped, yet some air found its way through the bladder, and, occupying the upper part of the tube, separated the liquid and bladder from each other. Under such circumstances only, air and aqueous gas could reach its lower surface. Notwithstanding this, and the gradual increase of the quantity of air, the mercurial column continued to rise, showing that the rate of the penetration of aqueous

There exists, between the lower surface of air and the upper surface of water, a space possessed of powers analogous to those of the interspaces of substances in general. Along this plane certain substances dart with surprising facility, losing, as their particles separate, all cohesion, and acting repulsively. The oils are remarkable in this regard, and camphor exhibits, because of it, curious and agreeable movements, when thrown upon perfectly clean water.*

But it is chiefly with reference to physiology, pa-

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gas is greater than that of atmospheric air, by which it could not be counterbalanced. Curious to see the effect, I tied over the summit of the tube, a bag, holding carbonic acid, which thus replaced the atmosphere. Almost immediately, the mercury gave intimation of descent, by losing its convex summit. It did fall, and carbonic acid entered through the membrane, faster than the moisture had at any time escaped.

* The best mode of examining this property of camphor is the following:—Take a piece of cork, a flat four-sided prism, and attach to its narrow sides, close to the ends, and diagonally opposite to each other, two small pieces of camphor. Resting with its broad surface upon a considerable plane of *quite clean water*, the apparatus will regularly rotate, and that either until the camphor is consumed or the interspace is filled with that substance, or an emanation from it. Oil, by filling the space, immediately suspends the motion. If a cork be greased slightly, or camphorated at the end, it will move in a direction from that end, and with considerable velocity. The same thing happens when fine dry flour is attached, or when the but-end of the cork is dipped into ether or alcohol. A cavity being made in the upper surface of a floating cork, near the end, filled with ether, and connected by a cotton filament with the water, it will sail about a pneumatic trough for a considerable time, always moving toward the solid end. A little rudder being attached to the cork, and slightly inflected, the vessel may be made to sail entirely round a circular tub.

thology, and practical medicine, that we see in the foregoing experiments things of much real value. They throw a particular light on the functions of respiration and cuticular absorption, and will probably lead to the employment of gaseous agents of cure with confidence and certainty.

The experiments on the mutual action of gases and liquids, show that although a gas may, when *alone* presented to a liquid for which it has no chemical affinity, penetrate its molecular cavities, yet it will again leave it to join any gas whatever which is brought into communication with the liquid. Thus carbonic acid or nitrous oxide readily penetrates blood or water, but returns from either into the air or any other gaseous substance which contains no carbonic acid or nitrous oxide. It is in this way, probably, that the oxygen disappears, and an exactly equal quantity of carbonic acid replaces it in the bronchial cells. Oxygen penetrates slowly the membranous tissue, to infiltrate and brighten the blood; carbonic acid is immediately formed, and being a gas differing from the remainder of the air yet in the air-cells, its tendency is to return, to penetrate that air, and thus escapes through the trachea along with it. The oxygen enters, because there is oxygen enough behind to permit that, and it is also an observed fact. The carbonic acid formed, makes its escape, because invited by the molecular tissue of atmospheric air. Keeping up any reference to known facts, we can scarcely doubt the truth of our explanation, or venture to adopt any other. The investigations of John Davy, and our careful repetition of his experiments, with others fully as conclusive, leave no doubt of the entire absence of carbonic acid in the blood.*

It must, therefore, be produced in one of two modes, either by the penetration of oxygen into the blood, and its union there with carbon, or the exit of carbon from the blood, to unite with oxygen in the air-cells. Now, as carbon is one of the most fixed substances in nature, and has not been proved capable of such transmission, we are, if *facts* be our guide, compelled to adopt the other theory, which is perfectly in accordance with the laws of gaseous infiltration. If it be asked how the carbonic acid is formed in the blood, at so low a temperature, we reply, that carbonic acid is actually created *at a lower temperature*, by the agency of infiltration, when oxygen gas is imbibed by a piece of fresh cold charcoal. The difference in the rate of permeation is quite sufficient to account for the escape of all the carbonic acid formed by the infiltrated oxygen.

Our theory does not account for the production of animal heat, but it is presumed that no well-informed physiologist now seeks for it in the action of the lungs, or the process of decarbonization. The simple fact, that cold-blooded animals breathe without *any* increase of temperature, proves that mere breathing to *any amount*

* Having filled a phial with hydrogen gas, blood was received into it from a vein, so as to exclude the agency of oxygen. When completely full of blood, the phial was closed by sheet gum-elastic, and immediately subjected to the action of the air-pump. Under such circumstances, no gas of any kind could be immediately separated from the blood; but after coagulation was completed, a bubble of air, about the size of a pin's head, was perceived beneath the membrane, and that that was atmospheric air, or nitrogen, was proved by its long continuance there, without apparent diminution or escape.

will not produce heat. Like all the other animal functions, that productive of heat is dependent on a normal condition of blood, and is thus *indirectly* governed by the act of respiration. As in cold-blooded animals there is no apparatus for producing heat, respiration does not in any way influence *their* temperature. So in some of the cases quoted by John Hunter, where blue-boys maintained a temperature preternaturally great, the blood was very imperfectly decarbonized. In such cases the calorific function found some novel stimulant.

Our experiments afford ready explanations of the effect of the various gases when respired. Carbonic acid not only cuts off the necessary supply of oxygen, but also penetrates into the blood, and passes through the route of the circulation.

We perceive why nitrous oxide, so identical with oxygen in all its chemical habitudes, should act so differently on the human system. It penetrates at least sixteen times as rapidly, and probably acts then solely as oxygen would do. Hence, we see why it does not exhaust us; for it not only acts upon excitability, but creates a fresh supply of it, so that its consumption is not felt. We can also easily see why an animal was destroyed in ten minutes by breathing hydrogen, while carbonic acid produced the same effect in two minutes. In section i. article ix. of his Physiological Researches, Bichat relates some curious exemplifications of the passage of gases into the blood-vessels through the lungs of living animals. For instance, hydrogen gas could be set on fire, as in bubbles it escaped from a remotely situated *blood-vessel*. As he had used some force by means of a stop-cock and syringe adapted to the trachea, to throw in, and retain the gas, he ascribes its entrance to that cause. We see, however, that though impulsion augments the effect, yet that it is existent independently of any *vis a tergo*. Gases not *at all* soluble in blood, will not pass without force, but that force is, in some degree, applied in every act of expiration. Those soluble in blood, find ready entrance when not held back by the interstitial molecular power of the other gases with which they enter the bronchiæ.

The emptiness of the blood-vessels after death, or rather their fullness of gaseous matter, is no longer a case of difficult solution. Always present in the air-cells after death, air and carbonic acid gas must find a ready entrance into the emptied capillaries of the lungs, always prompt to dilate through the influence of the elastic matter which exists in and around them in the lungs. As any kind of air acts as a stimulant to the heart's cavities,* a gaseous circulation is kept up, and the aëriform matter passes into the great channels of circulation.

It does not appear difficult to understand why so

* In 1823, being engaged in dissecting a sturgeon, (*Acipenser brevirostrum?*) its heart was taken out and laid on the ground, and after a time, having ceased to beat, was inflated by mouth for the purpose of drying it. Hung up in this state it began again to move, and continued for ten hours to pulsate regularly, though more and more slowly. Left at one o'clock A.M. in slow motion, it was found next morning still and hard. When last observed in motion, the auricles had become so dry as to rustle as they contracted and dilated.

With the heart of a *Testudo serpentaria*, (Snapper,) I lately repeated the experiment, and found it beat well under the influence of oxygen, hydrogen, carbonic acid, and nitrogen, successively thrown into it. Water also stimulated it perhaps more strongly, but made its substance look pale and hydropic, and in *one minute* destroyed action beyond all known means of restoration.

penetrating and poisonous a gas as sulphuretted hydrogen should often exist in the intestines without injury; for, being mixed up with other gases, its tendency to infiltration is greatly restrained. When undiluted, its diffusion through the whole system is fearfully rapid.

"Of all the gases," says Dr. Ure, "sulphuretted hydrogen is the most deleterious to animal life. A greenfinch plunged into air which contains only $\frac{1}{1500}$ of its volume, perishes instantly. A dog of middle size is destroyed in air that contains $\frac{1}{800}$, and a horse would fall a victim to an atmosphere containing $\frac{1}{250}$.

"Dr. Chaussier proves, that to kill an animal it is sufficient to make the sulphuretted hydrogen gas act on the surface of its body, *when it is absorbed by the inhalents.*"

One of the objections to the belief in aërial poisons most confidently urged by *anti-miasmatists,* is the absence of all proof of absorption of gaseous matter, and indeed this was the sole difficulty of any real moment in the way of the triumphant establishment of the theory of miasm. Will it now be going too far to say, that this difficulty is removed, and that we can explain why miasmata affect persons so differently who reside in different apartments of the same house, or who lived on opposite sides of the same street? Although being a very little nearer to the source or to the ground may not appear important, yet the difference of a few yards makes in either case a momentous distinction. Very near to its source a gaseous substance occupies a larger portion of the atmospheric space, and presents not only more matter, but matter less restrained by the molecular power of the air with which it is mingled. Not only is a greater quantity presented, but it is withheld from admission into the tissues by a slighter restraint.

As pressure unquestionably affects the rate of gaseous infiltration, *a difference in the amount of atmospheric pressure* will perhaps be considered of some importance, and *assist* in accounting for the general unhealthiness of low situations, and inter-tropical latitudes.

Spontaneous evaporation has been long a subject of interest to the philosopher, and has not hitherto admitted of adequate explanation. Now we perceive that in elevating moisture into the atmosphere a very powerful agent is at work, one capable of subverting the cohesion even of solids, and of producing the continued infiltration of the atmosphere. Heat being also capable of destroying the attraction of aggregation, augments evaporation and interstitial infiltration. On this (I speak it hesitatingly) depends the power of steam. Caloric penetrates gases as they do each other, and escapes from them in exactly the same manner when substances which contain less of it invite its penetrant power in a new direction. Thus, for illustration, carbonic acid penetrates common air, and, so far as we know, will expand it, if constantly supplied, to an amount of power not yet measured. But so soon as another gas or penetrable substance is presented, it begins to withdraw from the air and to penetrate that. The hollow intestine used in one of our experiments was powerfully inflated by its entrance, and yet as rapidly collapsed when the gas was invited outward by the presence of another gas on its exterior. The resemblance of phenomena does not end here. Each penetrates different substances with different degrees of facility, and the *quality of the surface* is often to both as influential as the character of the substance which affords it. The *fact*, the *force*, the *enlargement of bulk*, the *penetrativeness varying usually with the sub-*

stance and surface to be acted on, being however uniform relative to all gases, the *constantly diminishing rate of progression*, the *issuing out again when invited by new substances, or a vacuum, or when mechanical compression is applied*, all afford evidence of analogy as perfect as is perhaps ever offered to the view of philosophy.

We are struck with its resemblance *to water* in one respect. Highly concentrated calorie invites the penetration of all liquids, and perhaps of all solids, and thus, while held in solution by it, they obtain a penetrativeness themselves which does not naturally belong to them, and are elevated into the atmosphere in spite of specific gravity, however high, or of atomic weight, however considerable. Some facts not yet sufficiently studied, lead me to the perhaps hasty conjecture, that even the *decomposing* influence of caloric is owing to this power. Water exercises it in that way in some cases, such as that of acetate of lead.

The great length to which my remarks have unexpectedly extended, and the call of the printer, prevent me from going fully into the consideration of the connection of our experiments with pathology and therapeutics. Their bearing on these departments of medical science will furnish subject-matter for a future essay. In the mean time, we feel entitled to believe that we better comprehend some of the phenomena of colic, tympanitis, and emphysema, and see more clearly the cause of the value of certain methods of cure.

Bichat was among the first to produce the passage of air of various kinds into the blood-vessels and cellular tissue of the lungs, by forcing it into the air-cells and there confining it. Even when the blood-vessels were full of froth, and emphysema became extensive, he could perceive not the slightest laceration of the bronchiæ. When the impulsion was moderate, the air passed only into the blood-vessels; when more violent, its presence became manifest in the cellular tissue. In certain cases referred to by authors, violent exertion, laborious respiration, and severe flatulency of the intestines, have forced air into the blood-vessels and cellular tissue. Colic has produced also tympanitis, and few practiced physicians are ignorant of the fact, that great gaseous distension of the abdomen has disappeared without the *apparent* escape of any wind. When we consider attentively the laws by which are regulated the entrance and exit of gases under the action of their penetrativeness, we feel scarcely at a loss to understand these phenomena.

The prodigious accumulation of gas in the stomach and bowels, in hysteria and epilepsy, may be explained, by supposing the air, which exists by infiltration in every part of the animal economy, to be forced by the violent compression of spasmodic action into the hollow viscera, where already existent gases invite its entrance. In some experiments on the effect of certain gases on living cavities, made by my ingenious friend Dr. Finley, their escape was so rapid as to create surprise.*

The establishment of the fact of the penetration of liquids, each according to its peculiar rate, and the modifications of that rate dependent on extrinsic force, such as impulsion or invitation, electricity, etc. teach us many valuable lessons both in philosophy and medicine. Especially I would invite attention to tho cause of the remedial influence of *pressure*, as auxiliary to other means of cure.

* North Amer. Med. and Surg. Journ., No. vi., 1857.

RECAPITULATION.

1. Substances formed of organic matter are generally penetrable by gases of all kinds, and by several, if not by all, liquids.

2. Each animal or vegetable tissue is differently penetrable as to time by different fluids.

3. But all fluids penetrate any particular substance at rates susceptible of being ascertained. The gases retain the relation observed by reference to one substance in all other cases. Whatever may be the greater or less penetrability of any given tissue, the gases penetrate it, relatively to each other, according to the ratio observed in experiments on other tissues.

4. The *ratio* is not so uniform in the instance of denser fluids. Liquids, though ratable with regard to permeation of any given substance, do not act similarly on different organic substances. Thus water penetrates most, if not all, animal tissues, better than any other liquid whatever, and consequently passes through them to accumulate in *any of its own solutions*, and in alcohol or ether; while these two latter substances penetrate gum-elastic with more facility than either water or its solutions. Therefore, with regard to gases, the *ratio* of penetration depends on them alone, while, in the case of liquids, it depends on the joint agency of both liquids and tissues.

5. When the quantity of the fluids is limited, there is a gradually diminished rate of progression as the infiltration proceeds. It is proportional to the state of dilution, and ceases when the substances have become, on

both sides of the membrane, of uniform condition, unless some extrinsic power is then operative.

6. The power of the penetrativeness is very considerable, being *certainly* superior to that of two, and *possibly* equal to more than that of forty, atmospheres.

7. Penetrativeness acts not only on organic tissues, but also on gases and liquids, and with apparently equal power on all. For, after permeating a membrane, the gas or liquid goes on into the molecular tissue of the gas or liquid beyond, and no pressure which the membrane can bear, acts as a restraint on the progression.

8. Although of such high mechanical power, the penetration can be, to a certain degree, affected by extrinsic agency. Thus, pressure or attraction will cause permeation, where it would not otherwise take place, as when a single gas or liquid travels not only through, but beyond a membrane, where there exists nothing to imbibe it, which it would not do, unless subjected to propulsion. Electricity, possessed of hydragogue powers, acts on water in a similar manner, causing it to collect on either side of an animal membrane, at pleasure, although no other liquid is there to receive it.

9. The penetrativeness of gases for each other seems to vary in velocity, but not in force.

10. Reference to the above-mentioned laws and modifying agencies enable us to explain many phenomena hitherto imperfectly understood. We, by means of them, comprehend the uniform constitution of mixed gases in any vessel, or in the atmosphere, notwithstanding the greatest difference in specific gravity. It explains the diffusion of odors, the nature and power of spontaneous evaporation, and the probable nature and progression of caloric under slow conduction. It affords us new views

of the theory of respiration, and accounts, in that process, for some well-ascertained facts, for which there previously existed no adequate explanation.

It shows us how emphysema and tympanitis may happen without secretion of gases or lesion of tissue, and how a spontaneous cure may be produced. It leads to the probability of the existence of gaseous matter of very various kinds, in almost every part of the animal frame, resident there molecularly, and not *en masse*, but susceptible of being collected into mass in the great cavities or the cells of the tissue, or the blood-vessels, by mechanical or electrical influence, or the attractive interstitial agency of other masses of air.

It teaches the important truth, that water is the great general infiltrator and diluent, a knowledge of whose habitudes will be thus rendered both clearer and more useful.

Before closing my remarks, I am happy to be enabled to say, that a considerable number of my medical friends visited my laboratory, and saw for themselves the verifications of my statements. I solicited their observation both for the confirmation of my own impressions, and for the greater readiness of reception which the public always affords to facts which have appeared in a similar light to several different individuals of adequate judgment.

In my next I hope to present a table of the rates of penetrativeness of liquids for animal membranes. I hope also to ascertain the amount of force. On the relation of the respirable gases to the blood and other liquids, I possess already many interesting facts, which will be then promulged.

PHILADELPHIA, *September 15th*, 1830.

Since the foregoing paper was sent to the editor of this Journal, I have had an opportunity of reading M. Dutrochet's short essay, entitled "*Nouvelles Recherches, etc.*" In it I find that the author has discovered his mistake relative to the action of acids in general, but has fallen into one quite as important respecting the agency of diluted sulphuric acid. He *now* considers it a *nullifier* of endosmose, instead of a promoter of exosmose, being not only itself inactive, but the cause of inactivity in other solutions. Feeling confident of the power of diluted sulphuric acid to receive as much water as the animal membrane could convey, I, in conjunction with Professor Finley, carefully repeated our experiments on that substance. In every case, where the solution exceeded 1°, (*Baumé*,) it was adequate to the occupation of as much water as could be presented by the membrane. At 2°, 11°, and 25°, the acidulous liquid gave the same *rate* of aqueous infiltration as did alcohol, ether, etc. A solution of sulphate of soda at 11°, and at 3° Beaumé, and a solution of ammonia at 40° centesimal alcometre being infiltrable by water, at a rate not less than that of the animal membrane, of course afforded, when compared with that liquid, exactly the same results. Although all these substances gave evidence of having been contemporaneously transmitted through the membrane, yet the quantity, easily appreciated chemically, was not so great as to make a sensible difference in the altitude of the column, whose rise represented the transmission of water. When, by the entrance of a considerable quantity of water, the acid was so far diluted as to intermingle with it more slowly than the membrane could present it, a rapid diminution of ascent ensued. At length, so little was received as to barely compensate for the effect of gravitation. Finally, the diminished power of reception being below the effect of gravitation, the liquid descended again, and the two columns reached a common level. Seeing these causes of change, we can estimate the rate solely by observing the time taken to traverse a *short space*, and that immediately at the commencement of the experiment. Unless the less penetrant liquid be of much more power of reception than is actually necessary, its dilution soon destroys its adequacy, and lessens the apparent rate, just as in forming solutions, we perceive a great diminution of solvent power, as the point of saturation is approached. In addition, when both liquids are traversing the membrane at the same time, there is a progressive approach to a common state, favorable to repose. M. Dutrochet, therefore, by observing the effect of solutions of different strength, in a considerable length of time (an hour and a half) obtained results, not the act of the membrane, but of the solution—not the maximum effect of the tissue, but the constantly diminishing action on water of a gradually diluted solution. His results might, therefore, have been anticipated by calculation; for, as water dissolves less and less, in a given time, of any soluble substance, so a soluble substance acts on water presented to it in a steadily declining ratio. When the demand for water is above the powers of supply through the membrane, the rate will be regulated solely by the water and membrane, and is the same for a great variety of substances. When the demand becomes less than the supply, the case is one of simple solution, with which the membrane may be supposed to have no connection. It is then acting the part of a still surface of water.

The following facts, ascertained at an early period of this investigation, will place this principle in a strong light. An inverted syphon, such as already described, was filled with atmospheric air, a portion of which, by placing thirty-four inches of mercury in the long limb, was confined in the shorter one. There being here the same gas on both sides of the membrane, the current set in the direction given by impulsion and the long column fell—

$\frac{3}{8}$ths of an inch	in 2 hours and 30 minutes,	or 50 min. per $\frac{1}{8}$th.
$\frac{3}{8}$ths more	in 2 hours and 39 minutes,	or 53 " per $\frac{1}{8}$th.
$\frac{3}{8}$ths "	in 2 hours and 26 minutes,	or $48\frac{2}{3}$ " per $\frac{1}{8}$th.
$\frac{1}{8}$th "	in 1 hour and 1 minute,	or 61 " per $\frac{1}{8}$th.
$1\frac{1}{4}$ inch in all	in 8 hours and 36 minutes.	

At this period of the experiment, when the mercurial column stood two inches and a half lower, *proportionally*, than at the commencement, a vessel containing carbonic acid gas was placed over the shorter limb. Immediately the long column began to rise—

$\frac{2}{8}$ths of an inch	in 20	minutes,	or 10	minutes	per $\frac{1}{8}$th.
$\frac{1}{8}$th more	in 10	"	or 10	"	per $\frac{1}{8}$th.
$\frac{1}{8}$th "	in $12\frac{1}{2}$	"	or $12\frac{1}{2}$	"	per $\frac{1}{8}$th.
$\frac{1}{8}$th "	in $37\frac{1}{2}$	"	or $37\frac{1}{2}$	"	per $\frac{1}{8}$th.
$\frac{1}{16}$th "	in 60	"	or 120	"	per $\frac{1}{8}$th.

The column appearing stationary, was left nine hours unobserved, at the end of that time—

$\frac{13}{8}$ths were lost in 9 hours,		or $41\frac{1}{2}$ min. per $\frac{1}{8}$th.
$\frac{5}{8}$ths " "	in 3 hours 21 minutes,	or $40\frac{2}{10}$ " per $\frac{1}{8}$th.
$\frac{2}{8}$ths " "	in 1 hour 24 minutes,	or 42 " per $\frac{1}{8}$th.

At this moment the mercury came into contact with the membrane, all the gas being excluded.

The uniformity of descent, and the progressively diminishing rise, are striking facts. It will also be observed, that the carbonic acid *seemed* to cease action, because of a weight of nearly thirty inches of mercury, whereas, in another experiment, sixty-three inches were readily driven upward. We therefore easily perceived the cause of Dutrochet's mistake.

One other nullifier of endosmose is thought by Dutrochet to exist. A solution of hydro-sulphuret of ammonia at first quickened, and then totally arrested the motion of the fluid in the stem of his endosmometer; for which he accounts by supposing the final production of sulphuretted hydrogen in the solution, and the extinctive agency of *that.*

The great activity of gaseous sulphuretted hydrogen, on which Dutrochet made no experiments, led me to suspect that its solution was gifted with considerable penetrant power, and by thus counterbalancing the amount of penetrating water, appeared to act in arrest of motion, presenting just such a case as we witnessed when comparing together olefiant gas and arsenuretted hydrogen. For verification, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with water, and scarcely any motion observed. A similar solution, inclosed by an animal membrane in a wide-mouthed bottle, was placed in a vessel of pure water, mouth downward. In this instance the membrane gave no sign of inflection at first, but after several hours showed a slight bend inwardly. In both these cases the portion of liquid, originally clean water, when tested by acetate of lead, afforded the deep black precipitate, indicative of the presence, abundantly, of sulphuretted hydrogen.

In a second experiment, with a solution of sulphuretted hydrogen inclosed in a bottle, the water placed in the outer vessel contained the slightest trace of acetate of lead. Scarcely was the bottle immersed before the precipitation of the lead commenced. Finally, a solution of sulphuretted hydrogen in water was, by means of the inverted syphon, compared with alcohol confined in its shorter limb. In this instance, and in every repetition, the movement was manifested toward the alcohol, the rise of which showed that the penetrative power of liquid sulphuretted hydrogen is somewhat greater than that of water, and of course much greater than that of alcohol. These experiments were made with extraordinary care, because by them seemed to hang the fate of this whole question of principle. The whole doctrine of regular rate of penetration, etc. must fall to the ground if my trials had been confirmatory of the observations of M. Dutrochet.

The totally different results, as to the force of penetration, at which M. Dutrochet and myself have arrived, render necessary a few words of explanation.

It will be conceded that the fairest mode of estimating the force is when the liquid is fresh and the process just well begun. The altitude of the highest column of mercury which it can raise will represent its power, and that column should, if possible, be laid on it at once. In this manner I proceeded, and found that both bladder and gum-elastic were broken by a column higher than sixty-three inches, although just before giving way the column was rising. It could rise solely by the power of penetration, no other known agent of motion being present. But M. Dutrochet laying on a column less than sufficient, left his apparatus to raise that column for a day or two, until the process of elevation ceased. The height then reached he considered as representing the power of *endosmose.* The attentive reader will readily perceive in this plausible experiment the same error which deprived the facts, as to time, of value. The solution had become diluted, and the water on the other side had become impregnated, and, independently in a great measure of the weight of the column, the causes of production of penetrating currents had ceased, and these beautiful experiments reported, not the weight which could be raised, but the time required by such a solution to distribute its qualities uniformly, or nearly so, on both sides of the membrane. Left in that state the column descends, thus evincing the cessation of penetration, not its *forcible* repression. This is well proved by his latest experiment, in which, having raised a column of mercury by the penetration of water into a solution of gum Arabic to twenty-eight inches, and while still rising, he replaced the external water by a solution of gum Arabic, when an immediate descent was observed. The substitution of clean water again caused an elevation of the column.

On the whole, captivating as is the method, and elegant as are the experiments of this little volume of M. Dutrochet, it does not bring additional support to his doctrine of *endosmosis.* Yet whatever may be the issue of the experimental investigation to whose rigid scrutiny this most important subject is committed, the philosopher and physician can scarcely find language adequately to express the obligation, the high obligation, under which science has been laid by the elegant labors of M. H. Dutrochet. In him we discover the *punctum saliens* of a principle which is the master-spirit of animal and vegetable motion, the ruling power of chemical science, the governing influence of atmospheric composition, the presiding genius of respiration, circulation, and nutrition, the cause of disease, and the restorer of health. But whatever may be *now* his fame, how little is it, compared to that which may be anticipated for him by one who takes even a careless view of the mighty field of novel observation, just redeemed from the rich wilderness of nature! This tribute is paid the more unhesitatingly because it is due, and because I have so freely criticized and censured where the cause of science and truth demanded severity. It is in great men and in great discoveries that blemishes are most ungraceful and most injurious. The very magnitude and extent of the principle for whose detection we must thank Dutrochet, give a fearful importance to the slightest co-extensive errors.

September 18th, 1830.

ON THE

PENETRATION

OF

GASES.

ON THE

PENETRATION OF GASES.

[Extracted from the American Journal of the Medical Sciences for November, 1833.]

In November of 1830, I published a paper, on the force and ratio of transmission of gases through membranes. The subject has since that period attracted much attention, both here and in Europe, and the experiments then made have become the basis of reasoning on many morbid phenomena, and some physiological functions, particularly that of respiration. These considerations have led me again to review the experiments made at that time, and to make others, which seemed necessary to the full elucidation of the subject. This labor became the more necessary, since Mr. Graham, of Glasgow, a chemist of growing reputation, has, in the course of an experimental investigation of the transmission of gases through stucco plugs and other inorganic substances, confounded together two very different actions, and thus thrown some obscurity over the whole subject.

In 1829, I believe, Mr. Graham attempted to ascertain with accuracy the law by which the gases intermingle, or are diffused through one another. Mr. Dalton had previously shown, that when two gases of different specific gravities are placed in contact, so that the heavier gas shall be beneath the other, they, notwithstanding, gradually commingle, even if a *long narrow tube* be the only connecting medium between their respective reservoirs. These and other facts led Mr. Dalton to conclude that each gas is repulsive only of itself, and that its interstices are a *vacuum* for the reception of any other gas whatever. The fallacy of that view of the subject might have been made out by the fact, that the commingled gases occupy as much space as when existing separately, and, therefore, cannot be supposed to enter the interspaces of each other as into a *vacuum*. The first *experiment*, however, which domonstrated the error, was that recited in my first paper, showing that the force of "diffusion," as well as of "penetration," exceeded by an unknown quantity the pressure of two atmospheres. Mr. Graham, by confining gases in vessels communicating with the air by narrow apertures, found that the ratio of diffusion varied inversely as the square root of the density. In a paper read before the Royal Society of Edinburgh, on the 19th of December, 1831, more than a year after the publication of my paper, Mr. Graham describes a series of experiments on "diffusion," made by the intervention not of artificial apertures, but of plugs of stucco in which the pores are minute enough to oppose a *slight* resistance to the mechanical escape of aërial fluids. By placing various gases in a glass vessel closed at one end by a stucco plug, and resting on mercury or water, and observing the time taken to escape, and the volume of re-entered atmospheric air, he formed the following table:—

Table of Equivalent Diffusion Volumes of Gases, Air being 1 or Unity.

		Specific gravity.
Hydrogen	3·83	0·694
Carburetted hydrogen	1·344	0·555
Olefiant gas	1·0191	0·972
Carbonic oxide	1·0149	0·972
Nitrogen	1·0143	0·972
Oxygen	0·9487	0·111
Sulphuretted hydrogen	0·95	1·1805
Protoxide of nitrogen	0·82	1·527
Carbonic acid	0·812	1·527
Sulphurous acid	0·68	2·222

Thus his former conjecture was confirmed, and he appears to have proved that "diffusion" is inversely as the square root of density. As the pores of stucco, charcoal, paper, etc. are penetrated with great ease by the gases, the quantity constantly presented at the surfaces of the stucco is greater than the diffusive power is capable of conveying away, hence only the "diffusion" can be thus exhibited, since the amount of "penetration" is limited to the quantity removed by the air. The removal more rapidly by a current or a *vacuum* greatly increases the amount effused, showing the much greater "penetration." Substantially in the former paper, I stated that when the "penetration" exceeded the "diffusibility," only the latter could be measured, whereas, when the diffusion is greater than tho penetration, it is the action of the barrier which is estimated. Overlooking this distinction, Mr. Graham has confounded together all kinds of penetrable barriers, and asserts that "dried bladder answers for showing the *diffusion* of hydrogen when stretched over the open end of the tube receiver; the diffusion, however, through a single thickness of bladder is effected at least twenty times more slowly than through a thickness of one inch of stucco; while, on the other hand, either air or hydrogen, under mechanical pressure passes more rapidly through bladder than through a great thickness of stucco. Gold-beaters' skin is even more permeable by gases under slight pressure than bladder, and less suitable for diffusion." (*Pages* 240, 241.) That Mr. Graham has fallen into unaccountable error in this particular, is demonstrable by the following simple experiment:—

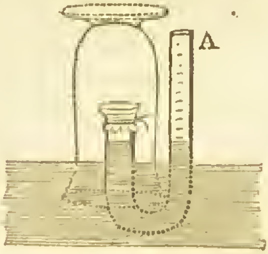
Over the end of a tube eight inches long was tied a piece of moistened bladder, which was subsequently dried. The tube was then filled with mercury, and placed erect on the mercurial pneumatic shelf, by the side of a similar and similarly treated tube closed with stucco. In less than three minutes the air had passed through the stucco, and followed the mercurial column down to the level of that in the trough. In the other, in *twelve hours*, enough of air had not entered through the bladder to disengage the mercury from its contact with it. It amounted to a small bubble floating about against the under surface of the bladder. To secure to the bladder complete contact with the glass, it must be tied on when wet, with a dry *waxed* string, and then left some hours to dry. Any tube thus treated will admit air by *mechanical* impulsion *many thousand* times more slowly than stucco of an inch in thickness. When it does otherwise it is unsound or imperfectly tied on. Whenever the hydrogen finds its way out by a passage between the glass and bladder, it diffuses more rapidly than carbonic acid, and only then.

Another experiment made with a different object demonstrates the same fact. Three tubes each six inches in height and capable of holding two cubic inches of air were closed with bladder at one end, and filled half full of nitrogen, hydrogen, and oxygen, respectively, so that each tube being placed over water, held a column of that liquid nearly three inches high. Notwithstanding the pressure inward, the tubes all of them fully supported the columns, and at the end of five days they all contained less air and more water than at the beginning. No stucco plug would support a three-inch column of water for five minutes.

Notwithstanding these discrepancies, I thought it important to go again into an examination of the "penetration" of gases, and to write on the subject a series of papers, of which this, the first, will contain little more than an experimental review of ground formerly, but hastily traversed. The great importance of the subject seems to me to justify the repetition of the experiments, which will be reported more in detail than the former ones.

The annexed wood-cut represents the inverted syphon with which a great many of the following experiments were made. Enlarged at one end into a kind of funnel or inverted cone, seven-tenths of an inch in diameter, over which the membrane is tied, its other limb two-tenths wide, is graduated into divisions of equal lengths, of the capacity of 0·003 of a cubic inch. Under the membrane were placed 0·075 of a cubic inch of atmospheric air, which was confined there by mercury having the same level in both limbs of the syphon. Thus

Fig. 1.



prepared, the funnel of the syphon was pressed under mercury and brought up into a bell-glass, holding two and a half cubic inches of the gas to be tested. The rise of the column of mercury in the outer limb indicated the velocity of influx, and the quantity. In the first series of experiments exhibited in the following table, gum-elastic was used as the barrier. Its flexibility when dry, its refractory character, its feeble hygrometric power, well fit it for a standard of comparison.* The whole of the experiments arranged in the first table, were made with the same membrane, so that, making allowance for the effect of dust, mercury, and the irregular manner in which quicksilver moves in a narrow glass tube, the various results by the same gas are very uniform.

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* In a very thin, transparent bag of gum-elastic, were placed ℥iv ʒij lvii grs. of water, which lost in weight per day, as follows:—8 grs., 4⅔, 6, 3⅗, 4¼, 3⅛, 3½, 3½, 2, 0, 2⅓, 2, 2, 1¾, etc. The average loss for one hundred and twenty-nine days was 2·367 grains per day.

A section of bladder tied up in the form of a bag, and holding ℥iv, ʒvij, grs. ii of water, lost per day 259 grains, 117, 303, 621, when it became offensive.

TABLE I.

Of the Rate of Entrance of Various Gases through Gum-elastic into Atmospheric Air confined in the Inverted Syphon, Fig. 1.

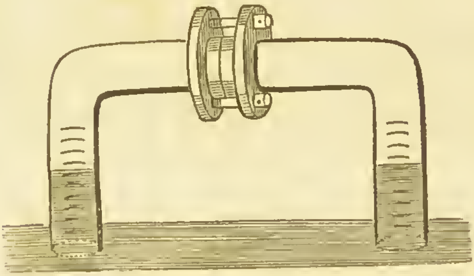
	Sulphuretted Hydrogen.		Cyanogen.		Cyanogen.		Cyanogen.		Ammonia.		Ammonia.		A mmonia.		Carbonic acid.		Carbonic acid.		Hydrogen.		Hydrogen.		Oxygen of nitre.		Oxygen of Chl. of pot.		Oxygen to nitrogen.	
No. of spaces.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.	Temperature.	Time required.
	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′ ″	°	′
1	80 F.	1 47	70	1 45	71	2 45	73	1	68	2 15	68	2	68	1 60	68	6 15	68	5 45	70	25 15	70	25	74	32 45	75	42	79	40
2		1 14		1		1 3		1 35		1 15		1 45		1 35		5		4 30		15		19		32		42		40
3		1 8		1		1 12		1 15		1 30		1 45		1 50		4 23		4 45		16		17 30		45		44 30		45
4		0 51		1 10		1 4		1 3		1 30		1 45		1 45		4 14		5 45		14 45		15 30	76	45				50
5		1 7				1 23		1 13		1 45		2		1 45		5 15		5 45		16 45		19		57 30				70
6		1 5				1 17		1 16		2 30		2 15		1 30		5 30		6		20		22		56				
7		1 26				1 28		1 22		1 45		2 30		1 45		6 14		5 45		22 45		22 15		76 30				
8		1 24				1 48		1 30		2		3 15		1 30		7 15		8 10		24 30		25 15		93 30				
9		1 41				1 42		1 47		2		3		2 15		7 38				25				96 30				
10		1 20				1 20		1 40		2 30		3 15		1 50		6 45				25 40				107				
11		1 44				1 31		1 35		1 46				2 5		7				28 50				155				
12		1 51				1 53		1 29		2 45				2 30						36 30								
13		1 55				1 36		1 56		2 15										41								
14		1 50				2 13		1 42		3																		
15		1 33				1 49		1 50		2 30																		
16		2 24				2 25				2 45																		
17		2 16																										
18		2 6																										
19		2																										

The average rate of penetration is *inversely* as the following numbers.

Sulphuretted hydrogen, 1; cyanogen, 1·166; ammonia, 1·75; carbonic acid, 4·50; hydrogen, 15; oxygen, 35; or taking the least number of each column, we have—sulphuretted hydrogen, 0·85; cyanogen, 1; ammonia, 1·25; carbonic acid, 4·233; hydrogen, 14·75; oxygen, 32. As these numbers are also inversely representatives of the quantities of the gases admitted in equal times, it follows that nearly fifteen times as much cyanogen enters in a given time as of hydrogen, whereas, according to Graham, hydrogen should enter in about four times the measure of cyanogen.

In the instrument, Fig. 2, some of the gases were compared, so as to verify the results of the first table, the intervening membrane being also gum-elastic. The instrument consisted of two hollow cylinders of iron or brass with flanges and screws so that they might be forced into powerful contact. To accomplish this the better, the screws had perforated heads through which a lever could be passed. Between the shoulders of the cylinders, was placed the membrane, and by means of the screws and levers, the shoulders of the cylinders tightly compressed the membrane, so as to entirely cut off communication with the air at the point of contact. Into the cylinders were fixed by sealing-wax curved glass limbs, of equal length and diameter, so that when put together the whole represented an inverted syphon

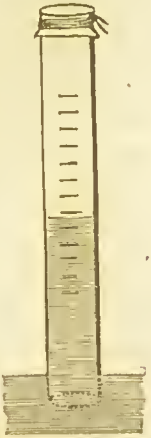
Fig. 2.



open at each end, but separated into two compartments by the membrane at its middle point. By placing measured quantities of the gases to be compared, on opposite sides of the membrane, the experiment commenced under perfectly equal circumstances. The quantities were alike, they were on the same level at the membrane, and were subject to equal causes of tension. But few observations were thus made on gum-elastic, and these merely to verify the results presented by the funnel-syphon. Carbonic acid was found to be more penetrant than hydrogen, hydrogen than oxygen, and oxygen than nitrogen, made both by phosphorus and hydrogen. The exact degree was, for want of time, overlooked.

Another general verification was made in simple tubes standing erect over mercury or water, and covered with the same kind of membrane. In these, cyanogen was more penetrant than carbonic acid, that than hydrogen, that than oxygen, and nitrogen scarcely ever suffered any other change than that produced by alteration of temperature. In the next paper more exact results will be given.

Fig. 3.



Before proceeding to the action of gases on wet animal membranes and recent animal tissues, some observations were made on dry bladder. A tube, Fig. 3, was filled to the top with mercury; after standing over the mercurial trough all night, a very small bubble of air was observed among the mercury yet in contact with the membrane. A similar tube closed with a plug of stucco an inch in depth suffered the air to enter so rapidly as to let the mercury fall to the level of that in the trough in a few minutes. Having ascertained in this manner the tightness of the membranous cover of three equal tubes, a cubic ineh of hydrogen, oxygen, and nitrogen rcspeetively, was plaeed in them. By transfer to water that liquid took the plaec of the mercury without wetting the membranes. Aftcr fivc days thc hydrogen was less by 0·38 of a eubie ineh, thc oxygen by 0·08, and the nitrogen 0·015. Stating the rate of hydrogen as in the first table at 15, that of oxygen will be 3·158, and of nitrogen 0·58, or hydrogen penetrates nearly five times as rapidly as oxygen, and that nearly six times as rapidly as nitrogen, whcn dry bladder is used for the cover, and the gascs stand over water.

A syphon, Fig. 2, contained in its limbs 125 parts of hydrogen and earbonic acid respectively. A pcrfeetly dry bladdcr intcrvened, and was compressed by the shoulders of the iron as foreibly as possible. At the cnd of twenty-four hours, of the 125 parts of hydrogen 2 passed to the carbonie acid *through* the membrane, 51½ by the space bctween the iron shoulder and bladder to the air, and by the same way thcre re-entered 30⅔ of common air.

Of the 125 parts of carbonic acid, 5 passed through to thc hydrogen, 30½ passed by the sidc of it to the air, and thc rc-enterd air measured 25½.

Penetration through dry bladder—hydrogen 2, earbonic aeid 5, or 1 to 2·5.

Diffusion by thc side of thc membrane—hydrogen 51·5, air 30·666, or 1·68 to 1; carbonic aeid 30·5, air 25·5, or 1·196 to 1.

Experiments on the penetration of gases through wet and recent animal tissues.

Thc syphon, Fig. 1, uscd for thc cxperiments arranged in Table I. containing the same quantity of atmospheric air, was covered with bladder soaked in water until perfectly infiltrated, and then wiped dry on the surface. It then, by immersion in the following gases, gave the results stated in Table II.

TABLE II.

Rates of Penetration through Wet Bladder in Syphon, Fig. 1.

Nos.	Ammonia.		Ammonia.		Cyanogen.		Cyanogen.		Carbonic acid.		Carbonic acid.		Hydrogen.		Hydrogen.	
	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Min.	Temp.	Min.
1	72°	8	°72	11	71°	49	69°	50	69°	224	71°	228	72°	38	70°	37
2		12		10		66		62		214		194		30		34
3		11		9		55		48		193		192		31		31
4		8		8		46		44		158		193		37		
5		10		10		54		51		228		225				

Ratio—1 to 5·5—20·7—600.

TABLE III.

Rate of Penetration through the Crop just taken from a Chicken, and tied over the end of the Syphon, Fig. 1.

Nos.	Ammonia.		Ammonia.		Ammonia.		Cyanogen.		Cyanogen.		Carbonic acid.	
	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.	Temp.	Sec.
1	72°	8	71°	8	73°	8	71°	45	69°	55		156
2		10		10		6		43		46		139
3		14		14		5		41		36		95
4		18		18		5		27		28		115
5						8*		47		42		137

Ratio—1 to 4—12·3.

* Immediately after an experiment with any gas, a repetition with the same membrane and gas usually showed acceleration.

The piece of paper containing the account of the experiments on oxygen for Table II. and of oxygen and hydrogen for table III. having been mislaid, I am able to recollect merely the general agreement exhibited by the other gases. Oxygen penetrated somewhat more slowly than hydrogen.

Experiments on the penetration of gases through wet and fresh animal tissues in the inverted syphon, Fig 2.

1. Over mercury 0·85 of a cubic inch of carbonic acid and oxygen from nitre were placed in the opposite limbs of syphon, Fig. 2, separated from each other by wet bladder. In twenty-five hours the carbonic acid was lessened to 0·355, which lost by washing 0·330, leaving behind 0·025 of oxygen. The contents of the other limb were lost without admeasurement, but were greater than at the beginning of the experiment.

2. Repeated. 1·10 parts of carbonic acid, 0·85 parts of oxygen, after twenty-one hours found the carbonic acid reduced to 0·63, which lost by washing 0·605, leaving 0·025 for the oxygen sent through.

The other limb contained 0·965, lost by washing 0·30; 0·30 of carbonic acid and ·025 of oxygen went through the membrane, which held in its pores 0·195 carbonic acid and 0·155 oxygen. Or carbonic acid is ten times as penetrant as oxygen.

3. Repeated. Carbonic acid 1·35, oxygen 0·85; after a lapse of nineteen hours and fifteen minutes the gas in the carbonic limb was 0·88, lessened by washing to 0·05; while the other limb contained 1·125, reduced by washing to 0·785—so that 0·34 of carbonic acid penetrated, and 0·18 were absorbed; while 0·05 oxygen penetrated,

and 0·015 were absorbed; or carbonic acid is more than six times as penetrant as oxygen.*

4. Repeated. Substituting *fresh* chicken *crop* for the wet bladder—1·35 of carbonic acid, and 0·85 of oxygen—time, twenty-seven hours and thirty minutes. The carbonic acid limb contained 0·35, which by washing was reduced to 0·05; the other limb held 1·55, reduced by washing to 0·80—so that 0·75 of carbonic acid and 0·05 of oxygen penetrated, while the membrane held 0·50 of acid, and none of the oxygen. According to this experiment, carbonic acid penetrates fifteen times as rapidly as oxygen.

In the same syphon, Fig. 2, by the intervention of wet bladder, hydrogen and cyanogen were compared. In these experiments equal quantities were used. The column of mercury rose in the limb containing cyanogen, and fell in that holding hydrogen, thus indicating the superior penetrancy of the heavier gas.

2. Repeated. The hydrogen received twenty parts of cyanogen, and transmitted only one part.

3. Repeated. Twenty-eight parts of cyanogen, and nearly one and a half of hydrogen permeated the wet bladder.

1. Hydrogen and nitrogen compared over water by wet bladder. Time, nine or ten days; instrument, Fig. 2; quantity, 2·55 of each; temperature ranged between 69° and 78° Fahr. By Hare's eudiometer the hydrogen was shown to hold 0·289 of nitrogen, the nitrogen 0·50

* After observing the great amount of absorption by the wet membrane, I increased the proportional quantity of the more absorbable gas.

hydrogen; hydrogen therefore penetrated 1·724, nitrogen 1·0.

1. Comparing oxygen and hydrogen in the same manner with intervention of fresh crop—after two days the greater penetrancy of hydrogen could be seen by the change of volume, which was not, however, very considerable.*

Experiments to ascertain the full result of penetrant action through wet bladder.

To effect this object a syphon, similar to that represented in Fig. 1, was so adjusted that, its outer limb at A being made short, it could *discharge* the mercury in proportion to the influx of gas at the other end without *very materially* altering the pressure. The mercury discharged represented the quantity of aërial influx through the membrane, or rather the difference between the penetrant action of air and the gas. The wide end of the syphon contained 0·25 parts of a cubic inch of air, the bell-glass nearly a pint of gas. The first experiment was made with ammonia at the temperature of 69° Fahr. After a lapse of between thirty minutes and an hour in all cases the ammonia seemed to cease action, and the quantity of mercury thrown out was, in three different cases, 1·025, 0·90, and 0·975, respectively.

Cyanogen under the same circumstances took at least eight hours to complete its action, and the quantity of mercury thrown out varied from 0·90 to 1·00.

Carbonic acid did not cease action for nearly three

* In the last two experiments nothing is certain but the greater penetrancy of hydrogen over either oxygen or nitrogen, for the motion in the limbs of the syphon showed that. In both, the bladder began to spoil at the end of the experiment.

days, when it was found that 1·125 parts of mercury were discharged.

In a second experiment ·555 parts were discharged in twenty-five hours; in the next twenty hours and fifteen minutes, 0·15; in the next twenty-four hours and ten minutes, 0·075; and in the nineteen hours and thirty minutes immediatly preceding the cessation of action, 0·02. Total, with correction for temperature and pressure, 0·98 in about three and a half days.

Hydrogen under a similar arrangement discharged 0·045 in eleven hours and fifteen minutes; 0·025 in seven hours and fifteen minutes; 0·045 in sixteen hours; 0·07 in twenty-five hours and fifteen minutes. Total, after correction, 0·205 in fifty-nine hours and forty-five minutes, at which time an accident put an end to the experiment while it was yet in action. From the experiments immediately antecedent, it is probable that the amount would finally have reached about 0·95 to 1·00, or a cubic inch; or, as in the other cases, 0·25 of air would mingle with 1·00 of gas, the proportion being about 4 to 1, as in the mixture of nitrogen and oxygen in the atmosphere. The penetration of *air* through wet membranes is so slow as to leave scarcely a trace of such action by analysis, even in the most protracted experiments. I have therefore made my calculations without introducing it.

From these experiments it appears that the rate of influx was very great or rapid for ammonia, much slower for cyanogen, still slower for carbonic acid, and very slow indeed for hydrogen. Ammonia completed in half an hour the penetration which required eight hours for cyanogen, three days for carbonic acid, and an unknown but much longer time for hydrogen. The latter in two days and a half had done about the one-fiftieth of the work executed by ammonia in half an hour. It is, however, to be observed, that the exact time for ammonia was not well observed, and it may have been mis-stated, but not to an extent exceeding half an hour.

In the attempt to discover the cause of the difference of velocity of the transmission of gases through porous bodies, I examined experimentally the passage of compressed aëriform fluids through visible apertures. Condensed to an equal degree in the same reservoir, air, hydrogen, and carbonic acid were successively allowed to escape through an aperture distinctly visible, and the times of the descent of the mercury of an air-gauge noted in *seconds* as it passed over equal spaces, falling from nearly four atmospheres to the usual barometric level:—

Hydrogen—11, 10, 7, 6, 7, 5, 5, 5, 5, 4, 5, 3½, 4, 5, 6, 8, 22.

Carbonic acid—36, 33, 25, 24, 21, 17, 18, 16, 13, 14, 14, 12, 14, 14, 16, 22, 29.

Ratio on the whole, 1 to 3·163.

Through a smaller aperture just visible the times of escape were, in seconds:—

Hydrogen—19, 16, 15½, 14, 14½, 12, 12½, 10½, 14½, 14, 17, 27.

Carbonic acid—51, 45, 47, 41, 36, 34, 35½, 31½, 38, 37, 42½, 61½.

Common air—43, 40, 40, 35, 32, 31, 31, 26, 33, 32, 38, 55.

Ratio—1 to 2·688—2·344.

A repetition gave very similar results. The air escaped more rapidly than carbonic acid.

A stucco plug a quarter of an inch in length was next used under like circumstances, care being taken not to compress it. The results were, in seconds:—

Hydrogen—74, 76, 78, 79, 80, 85, 90, 93, 98.

Carbonic acid—158, 155, 151, 159, 149, 174, 181, 180, 203.

Common air—173, 163, 169, 172, 167, 188, 200, 198, 215.

Ratio—1 to 2·005—2·18. The air penetrates with less facility than carbonic acid.

Through a *compressed* stucco plug rather more than an inch in length the times of escape were, in minutes, as follows:—

1. Hydrogen—$12\frac{1}{3}$, 11, $13\frac{1}{3}$, $14\frac{1}{2}$, $13\frac{1}{2}$, $15\frac{1}{4}$, 18, $18\frac{1}{4}$.
2. " $10\frac{1}{2}$, 11, $12\frac{1}{2}$, $13\frac{2}{3}$, $12\frac{2}{3}$, $13\frac{1}{3}$.
1. Carbonic acid—17, $18\frac{1}{3}$, $21\frac{1}{3}$, $22\frac{1}{2}$, 22, $25\frac{1}{2}$, 29, $29\frac{2}{3}$.
2. " " 17, $18\frac{1}{3}$, $20\frac{1}{4}$, $20\frac{3}{4}$.
1. Common air—$20\frac{3}{8}$, 21, $23\frac{1}{2}$, 25, $24\frac{1}{4}$, $27\frac{1}{4}$.
2. " " $19\frac{1}{4}$, 20.

Ratio—1 to 1·626—1·727. The air penetrates with less facility than carbonic acid.

RECAPITULATION.

	Hydrogen.	Carbonic acid.	Air.
Ratio of times—visible hole.....	1	3·163	......
Lesser aperture......................	1	2·688	2·344
Thin plug of stucco...............	1	2·005	2·18
Thick dense plug..................	1	1·626	1·727

It seems, if these experiments can be trusted without frequent repetition, that the larger the orifice the more nearly the proportional quantities of gases given out in a certain time approach to Graham's law of diffusion; and that the more minute the apertures the less the proportional facility of the escape of the lighter gas. Thus air passes through a visible aperture faster than carbonic acid, but not so fast through the pores of stucco; while the proportional rapidity of the escape of hydrogen is greatest through the widest aperture, and though greater in the smallest, yet progressively lessened. It passes through a large opening 3·163 times as fast as carbonic acid; through a smaller one 2·688 times as fast; through a thin plug 2·005 times as fast, and through a thick one only 1·626 times as fast. By still more minute channels it is possible that these two gases may pass with about equal facility, and pores may exist so minute as to reverse the order of penetration, as is demonstrated with respect to carbonic acid and common air in these very experiments. The experiments to bear on this question are yet in progress, but those which exhibit the transmission through gum-elastic, wet bladder, and recent animal membrane, demonstrate, if not the cause, at least the fact of a reversal of the order of penetration; for through such in all instances the heavier gas penetrates much the more rapidly. If the size of orifice determine the penetration, philosophy may yet, by experimental investigation of the effects of apertures of visible but varied size, learn the law of alteration, and thus finally, by observing the penetration through invisible pores, calculate their size, even determine the volume of the penetrant atoms, and detect many mysteries of physiology and pathology, by a process which at first seemed to promise no contribution to the stores of useful science.

Other and indispensable engagements forbid my entering at present on a record of the many very interesting suggestions forced on us by the facts which have been here recorded. But they will have more weight when they follow the whole series, which I hope to lay soon before the readers of this Journal. For the imperfection of very many of my experiments I must be indulged, since the calls of an arduous and imperative profession often interrupted, and sometimes destroyed, the most promising phenomena. To present them as they are, it was necessary to work during the night, and sometimes all night, a period not very favorable to nice observation and delicate manipulation. But though imperfectly, they are honestly made, and to avoid obscurity the prolix details have been stated at length, and the reader has thus the opportunity of judging of the correctness of both deductions and calculations. By reference to my former paper, it will be perceived that the ratio of penetration of some of the gases as there stated, is erroneous, although not to an extent subversive of the great general truths there set forth. Cyanogen, ammonia, and sulphuretted hydrogen, are less unequally penetrant than I had supposed, and the extraordinary alteration of rate occasioned by imbuing a dry membrane with water, was not then clearly enough expressed. The relation of oxygen and carbonic acid to each other, being that which is to the physiologist much the most interesting and important, has been studied in a greater number and variety of experiments, and so far as they are connected with the subject, it has been fully elucidated. For the rest, much remains to be done; and after I have examined in detail tho relations of a similar kind between liquids, and the relation of both kinds of fluids to the blood, it is not improbable that new light will be shed on the dark subject of respiration.

Enough has now been done to show that the law of diffusion through stucco, established so well by Mr. Graham's very philosophical labors and reasoning, does not apply to substances of a closer texture, and that physiology must depend on the experiments on animal membranes themselves for the elucidation of the many important difficulties in the way of a satisfactory explanation of the functions connected with aëriform fluids.

ON A NEW PRACTICE

IN

ACUTE AND CHRONIC RHEUMATISM.

30

ON A NEW PRACTICE

IN

ACUTE AND CHRONIC RHEUMATISM.

[Extracted from the American Journal of the Medical Sciences, Vol. viii. p. 55.]

In the autumn of 1827, a patient affected with caries of the spine, was suddenly attacked with all the usual symptoms of acute rheumatism of the lower extremities. One ankle and the knee of the opposite leg tumefied, red, hot, and painful, affording as fair a specimen of that disease as is usually met with. The usual treatment by leeches, purgatives, and cooling diaphoretics, with evaporating lotions, had the effect of transferring the symptoms to the other ankle and knee, and finally to the hip. Disappointed in the treatment, I began to suspect that the cause of the irritation might be in the affected spine. The difficulty of cure, the transfer of irritation from one part of the lower extremities to another, without any sensible diminution of disease, and the fact of the existence of caries in the lumbar vertebræ, which lie near the origin of the nerves of the lower extremities, rendered probable the opinion, that in the spinal marrow lay the cause of this apparently indomitable and migratory inflammation. Under this impression, I caused leeches to be applied to the lumbar curve, and followed them by a blister placed on the same spot. Relief promptly followed these remedies, and the pain ceasing to be felt in the limbs, was perceived only in the immediate vicinity of the spinal curve. After the blistered surface had recovered its cuticle, a few leeches placed over the diseased spine removed the pain, and left the patient in the usual state of indifferent health attendant on such forms of spinal disease.

Striking as were the benefits of the application made to the spine in a case of apparent inflammatory rheumatism, they did not lead my mind at the time to the general conclusions, which, viewing the case as I now do, they ought to have suggested.

In the beginning of the ensuing winter, another case of a similar kind presented itself. A little female patient, having curvature of the cervical vertebræ, was attacked in the night with severe pain in the wrist, attended with redness, tumefaction, and heat. As, on the appearance of these symptoms, the pain in the neck, to which she was accustomed, subsided, I easily persuaded myself of the spinal origin of this inflammation, and accordingly applied leeches to the cervical spine, with the effect of procuring a prompt solution of the disease of the wrist.

On the 5th of October, 1831, as Dr. Parker, of Elkton, Maryland, was driving his carriage down a slight declivity, his horse fell, and he was thrown out in such a manner as to alight on the back of the neck and shoulders. He was instantly paralyzed, *partially* in the hands and arms, and *totally* in the lower extremities. On the following morning, there was great pain in the *hands* and *wrists*, with swelling, heat, and redness, exactly such as is observed in acute rheumatism.

The pain in the hands and wrists was always abated by remedies applied to the affected part of the spine, and aggravated by pressure or rough friction there. The doctor had been subject, some years before, to rheumatism in the arms, side, and hip, and was able to recognize in this attack the same phenomena. While the rheumatic affection after the fall existed, it exhibited the translatable quality so characteristic of rheumatism, attacking sometimes one arm, sometimes the other, but never passing to *any other part* of the body.

Remarks.—In this case, reported by my friend Dr. Evans, of Elkton, with the approbation and concurrence of Dr. Parker, we have the highly respectable testimony of both to the opinion, that the injury of the spine produced the affection of the upper extremities, and that that affection was in all respects similar to rheumatism.

It is objected, that the hands and wrists might have received simultaneous injury by the fall; the objection is obviated by the fact that, at any subsequent period, pressure or rough friction at *the spinal seat* of injury aggravated or reproduced the tumefaction, pain, and redness of the extremities.

In this case, then, we have an obvious spinal cause producing rheumatism in the upper extremities, a rheumatism characterized by pain, heat, redness, tumefaction and translation.

These cases led me very naturally to the reflection, that perhaps other cases of rheumatism might have an origination in the medulla spinalis, and depend on an irritation of that important organ. In the following spring an opportunity of testing by practice the truth of this opinion presented itself. William Curran, a respectable livery-stable keeper in Marshall's Court, had been for upwards of two years afflicted with a rheumatism of the lower extremities, which gradually deprived him of the use of his limbs, and finally confined him to his chamber. Regular medical aid, and many empirical remedies, had been procured, without an abatement of the pain, which became at length almost intolerable.

On my first visit I found him in his room in a paroxysm of pain. His legs were swollen from knee to ankle, and the enlargement of the periosteum and integuments gave to the anterior face of the tibia an unnatural prominence. In that place the pain and tenderness on pressure were particularly developed. He was also suffering severely from pain in the scalp, which had existed for a short time previously, and was at length almost insupportable. Along with these symptoms appeared the usual febrile action, with its concomitants.

Notwithstanding the significant hints given by the spine cases referred to, I treated this case for a time in the usual manner—depleted freely, purged actively, blistered the head; and, having caused an abatement of fever, administered corrosive sublimate and decoction of sarsaparilla. Defeated in all my efforts, I at length suggested to my patient the possibility that his disease was so unmanageable because we had not applied our remedies to the true seat of disease, and that by addressing our measures to the spine, success might yet be found. Accordingly on the 16th of February, 1828, nine days after my first visit, I had him *cupped at the back of the neck*, and as he could not bear any more direct depletion, inserted a large seton over the *lumbar spinal region*. The cupping, followed by blisters to the back of the neck, relieved his head, and as soon as the seton began to suppurate freely, his legs became more comfortable. From the 25th, nine days after the insertion of the seton, I visited him but seldom, although I had seen him once or twice a day until that period. Indeed, I paid him but seven visits after the 25th. The last was on the 30th of March. Soon afterwards he resumed his usual pursuits, and about the beginning of June the seton was removed. Since that time he has not had a return of his complaint, and is, at the date of this paper, in the full and vigorous exercise of all his physical faculties.

I could scarcely doubt as to the cause of the cure in this case, because the treatment applied to the spine was that alone which had not been fully and fairly tried, either by me, or those who had preceded me. Indeed, the last applications were made with some hope of success, and the grounds of that hope were expressed to the patient, who was fully persuaded that the spinal treatment was the chief, if not the sole agent of restoration.

Robert Gordon, well-known as the carrier of *Poulson's Daily Advertiser*, fifty-six years of age, of vigorous constitution and active habits, was the subject of the following attack. Observing a severe pain in his right heel and ankle, immediately followed by redness, heat, and tumefaction, he caused himself to be largely bled, and took some salts and magnesia. On the following day the pain and swelling increased, and the ankle and knee of the opposite limb becoming similarly affected, he was confined to bed.

On the third day my first visit was made. The patient had then a full, strong, frequent pulse, flushed face, dry skin, whitened tongue, and complained much of the severity of the pain in his legs, and his incapacity to endure the slightest pressure or motion. As he had already been purged and used a lotion, I directed the application of seventeen cups to the lumbar region, so as to abstract twelve or sixteen ounces of blood.

Next morning found the pain almost entirely gone; does not complain of moderate pressure, and is able to move his legs without inconvenience. Ordered a draught of salts and magnesia, with an evaporating lotion of camphor in alcohol.

Third day. Pain in legs scarcely perceptible, but the shoulders, elbows, and wrists are beginning to exhibit marks of severe inflammation, expressed by pain, tumefaction, heat, and redness. Ordered twelve cups to the cervical spine.

Fourth day. The patient sits up; complains of stiffness, but no pain except in one wrist, and that very slight. Directed Epsom salt and magnesia.

Fifth day. Finding nothing for which to prescribe, arranged the patient's diet, recommended the occasional use of aperients, and took leave of the case.

Called on the tenth to inquire into results, and found there had been no return of the disease.

Since that time a very severe winter has passed, during which the subject of this report has continued in his customary health, and in the pursuit of his usual employments.

The reader will in the above case perceive that the general bleeding, though very copious, proved of no service, and that the large local depletion of the lumbar region benefited solely that part of the disease which lay at the peripheral extremities of the nerves supplied by the lower end of the spinal marrow. The inflammation in the upper extremities continued afterwards in progress, and was arrested only when cups were placed over the cervical end of the spinal column. The whole case exhibits a fine exemplication of the difference in the character and extent of the influence of general and topical depletion, and proves that *local blood-letting is most potent when applied to that part of the spine which supplies with nerves the parts in a state of active inflammation.*

As I feel, in common with the profession, a greater confidence in hospital reports, especially when made by those who are not by interest or reputation blinded or misled, I shall present the history of some cases treated after the new method, as drawn up at my request by Dr. Stewardson and Dr. Norris, the resident-physicians of the Pennsylvania Hospital.

The following case, reported by Dr. Thomas Stewardson, is peculiarly interesting, because of its evident dependence on *irritation of nervous masses*, and the immediate and perfect remedial action of the local applications.

"CASE I.—William Anderson, colored man, a seaman, aged fifty, was admitted into the Pennsylvania Hospital on the 31st of December, for a chronic rheumatism of upwards of five years' duration. Occasionally the disease intermitted, but generally continued to affect him during the cold season. The pain affected at one or at various times almost every part of his *right* side from head to heel, but had in no case at any period crossed to the opposite side. Like other cases of chronic rheumatism, it was most severe in cold weather, and when warm in bed. According to his statement, he seldom suffered from a winter attack for a less period than three or four months, and the existing exacerbation had lasted only a few weeks.

"On the 2d of January, two days after his admission, eight cups were applied to the *back of the neck* and *left side of the head*, and a powder was taken, consisting of guiaicum and nitrate of potassa, of each ten grains, with directions to repeat it three times a day.

"On the 3d. Pain in the *head* and *arm* completely gone; *leg* no better.

"On the 4th as on the 3d. A blister to the nape of the neck, and eight cups over the lumbar spine.

"On the 5th. Says the cups almost immediately relieved the pain in his *leg*. He now feels perfectly well. On account of the extreme rigor of the season, the patient was not discharged until the latter part of February, during which period he remained entirely free from disease."

"CASE II.—Jane Black, aged sixteen, was admitted into the Hospital on the 9th of March, 1831. About four weeks antecedently, she perceived pain, tumefaction, and a sense of *numbness* in her feet and ankles, which gradually deprived her of locomotion, and on the third or fourth day confined her to bed. On the second day after the attack, her wrists and hands were similarly affected. In the course of a week her wrists, fingers, and ankles, became flexed and rigid, feeling pain from every attempt to straighten them. Such was her condition when admitted. She states that she is of a costive habit, and had been amenorrhagic for two or three months before the appearance of rheumatism. The previous treatment consisted, as she said, of a blister to the *umbilical region*, and some powders and drops. On her admission, Dr. Norris applied six cups to the cervical, and six to the lumbar spine, which 'took away entirely the *pain*.'

"On the following day Dr. Otto saw her, and recommended continuance of the treatment, and accordingly four cups were applied to the upper, and four to the lower part of the spine, with the effect of enabling her to extend her wrists, and to grasp, though imperfectly, with her hands.

"On the 11th. Took Epsom salt.

"On the 13th. Spine cupped as before, and a dose of magnesia directed. After the cupping to-day, she begins to observe a 'pricking sensation, as if her feet and hands were *asleep*.'

"On the 16th. Cups as before.

"On the 18th. Find her free from pain and tumefaction, recovering gradually the use of her hands, experiencing no uneasiness on motion or pressure. She is unable to stand, because 'her feet slide from under her,' but the attempt gives no pain. Besides the remedies already mentioned, soap liniment was applied twice a day to her wrists and ankles."

Remarks.—In this highly interesting case, the complication of rheumatic irritation, with *numbness*, and enfeebled condition of the extensors of the hand, and the congeneric flexors of the feet, amounting almost to paralysis, emphatically directs us to the centrally nervous origin of this disease.

"Case III.—William White, seaman, aged fifty-two, was admitted, November 27th, for rheumatism. He stated that he had an attack the preceding winter, which had confined him to bed for five months, and that the present affection had commenced with equal severity. On admission, his wrists and arms were tumid and painful, and he complained also of pain in the *lumbar region* and lower extremities. Cups were applied to his spine, and repeated, at proper intervals, two or three times, without the use of any auxiliary remedies. The relief was almost complete, when, in consequence of some accident, he was affected with fever and pain in his head, for which he was cupped and blistered at the nape of the neck, and a saline purgative given. Being relieved from the cephalic irritation, he began in a few days to complain again of pain in the feet and ankles, which appeared hot and tumid. Cups having been applied to the base of the spine, entire exemption from pain ensued. The severity of the season prevented his discharge until the 26th of February; but for more than a month before, he had ceased to feel any other inconvenience than a very slight soreness on the top of his feet, and that only when walking. That pain left him previous to his discharge." This case is reported by Dr. Stewardson.

"CASE IV.—William King, a seaman, was admitted for a surgical disease, for which he used venesection and low diet; followed by balsam of copaiba and cubebs.

"On the 24th. He was seized with severe rheumatic pain in his left side and shoulder. For this he was twice bled largely, and put under the use of sarsaparilla and nitrous powders, and afterwards of Dover's powders. A stimulant liniment was also applied to the affected part. Under this treatment he remained until the 6th of February, when the pain appeared to be fixed in both the side and shoulder, and he had not been benefited in any way by the remedies employed.

"On the 7th of February. All other remedies being discarded, twelve cups were applied to the spine.

"On the 8th. Pain relieved, cups to be reapplied.

"On the 11th. Patient states that the last cupping has almost entirely removed the pain from his shoulder, but has not benefited that of his side. Ordered eight cups to the dorsal spine.

"On the 13th. No change after last cupping. Cups to be again applied.

"On the 16th. The pain in the shoulder left the patient soon after the application of cups on the 13th, and has not returned.

"As the pain in the side was confined at last to a small surface, and had been constant for some time, a few cups were applied immediately over it, with a beneficial effect. Their repetition at length entirely removed it." This case is reported by Dr. G. Norris.

Remarks.—The practical interest of this case consists in the total failure of the most judiciously selected remedies of the current practice, and the facility with which the disease, so obstinate before, began to yield to the *very first* application of cups. To those who still maintain the identity of the effect of general and topical depletion, this case presents a striking difficulty.

"CASE V.—William Brown, seaman, was admitted March 5th, 1831, for rheumatism. Three months ago, he was exposed at sea to great hardships in an open boat. On the day after he was picked up, he felt pain in his shoulders and elbows, which remained until after his arrival in port, and then suddenly attacked his lower extremities, while entire exemption from pain was experienced in the upper ones. On admission, he complained of pain in the whole course of his legs, but particularly severe in his knees and ankles. The right ankle is swollen, hot, and very painful. Directed the application of ten cups to the *small of the back.*

"March 6th. Is no better. On examination, I found that the cups *had not been placed on the part as ordered*, but had been extended to the top of the spine. Therefore ordered another cupping to the loins.

"On the 7th. Was relieved by the cups for a time, but the pains returned. Cups to be repeated.

"On the 8th. Has had very little pain since the last scarification. The tumefaction of the right ankle has disappeared, and the heat and pain have entirely gone from it.

"On the 11th and 13th. In consequence of the reappearance of slight symptoms of the disease, cups were ordered. Their application in both instances afforded relief." Reported by Dr. G. Norris.

Remarks.—In the case just recited, the attention of the reader is called to the fact, that the cups produced no relief whatever when applied over that part of the spine which did not transmit nerves to the seat of inflammation, thus verifying the important doctrine that the most potent influence is exerted when our depletory remedies are addressed as nearly as possible to the *disease-exciting agent.*

"Case VI.—Thomas Gordon, a man of color, a seaman, aged thirty-four, was admitted on the 15th of February, for *rheumatic fever*. The pain is confined *chiefly to his limbs;* and his pulse, although excited, is not very active. Ordered ten cups to spine.

"On the 17th. No improvement. It is discovered that the cups had not been placed near the spine, but at a considerable distance on each side of it. Ordered ten cups to *dorsal spine.*

"On the 18th. The pain in his body and arms diminished, but no improvement observable in his lower extremities, in consequence of which eight cups were applied to the *lumbar* portion of the spine. For a slight cough some mucilage was ordered. The patient was relieved by the last cupping, and the pain *almost entirely* left him. For stiffness in his legs a stimulant liniment was finally directed.

"On the 1st of March. Having been previously apparently cured, the disease suddenly returned. As he had, along with other symptoms of fever, a strong and frequent pulse, sixteen ounces of blood were abstracted, and nitrous powders administered; but as on the following day no abatement of the pain of the lower extremities appeared, and though the fever was reduced, eight cups were applied to the lumbar spinal region, which *entirely relieved him.*

"On the 9th of March. He was discharged cured. After the last scarification, he used, for stiffness and weakness of his joints, a stimulating liniment." Reported by Dr. Stewardson.

Remarks.—In this case several facts are worthy of notice. *Twice* the cups failed to relieve the *lower extremities*, once because they were not applied to any part of the spine, and once because they were applied on the *dorsal region.* The very first application to the lumbar region afforded the expected benefit. In the relapse a large bleeding and nitrous powder sustained a total failure, while a very moderate quantity of blood, drawn from the lumbar region by cups, produced an immediate and final solution of the disease.

"Case VII.—William Richardson, a seaman, was admitted on the 11th of February for rheumatism. His attack commenced two weeks before, with pain in the dorsal region and occiput, followed by a *sense of numb-*

ness, with pain in almost every part of his body. On admission his skin felt cold, his pulse was frequent, tongue slightly coated, and his bowels regular.

"On the 12th of February. Twelve cups were applied along the spine.

"On the 13th. *Has no pain;* slight numbness of the legs; no appetite; slightly vertiginous; directed him an ounce of sulphate of magnesia.

"On the 14th. Nausea, for which ordered effervescing draught. For the numbness, directed soap liniment.

"On the 15th. No improvement; the numbness of his hands being especially disagreeable, a few cups were applied to the nape of his neck.

"On the 17th. *Find the patient free from pain and numbness.*

"For an enlargement of the spleen this patient remains in the Hospital, but has not had any relapse." Reported by Dr. Stewardson.

Remarks.—The most remarkable feature in this case is the concomitant numbness, and the greater difficulty of removing that than the pain, a fact which is not unfrequently observed in cases of rheumatism. The vertiginous affection, too, is interesting as significant of the irritation of central nervous masses.

"Case VIII.—Rebecca Leshler, affected by rheumatism of two weeks' duration, exhibited a swollen arm and shoulder, attended with pain and redness. She could elevate her arm only when firmly grasped by the hand of an assistant, when the motion became comparatively easy. In the evening of the 5th of March ten cups were applied, so as to extend from the top of the neck downward, immediately over the spine. On the following morning the pain was gone, and on the subsequent day every vestige of redness and swelling disappeared. *No other treatment was used.*" Reported by Dr. Stewardson.

Although other cases might be cited, in confirmation of the views here taken, I have not leisure, at this time, to digest and arrange them. I may observe, in general, that as far as I now recollect, only two cases of apparent rheumatism have, in my hands, either in private practice or in the Pennsylvania Hospital, resisted the treatment recommended in this paper, and both of these were in reality *neuralgia*, and exhibited no traces of inflammation. One of them was an affection, severely painful, located in the bottom of the heel, the other was gastric and intercostal.

The preference given to local depletion over other local measures, arose from the greater apparent success of its action, which scarcely left anything to be desired; but cases will occur in which other measures must be used, and in which, perhaps, all measures will fail. We are warranted, however, in declaring our conviction, that few failures will happen in thus treating *acute* rheumatism, and that success will diminish, as passing through *chronic* rheumatism, we may enter on the ground of *neuralgia*, a disease which sometimes spontaneously disappears; but is scarcely ever, in this city, cured by *medical* means. The art of the surgeon occasionally subdues it, and the physician often allays, but seldom removes it. Being paroxysmal, and often slumbering for weeks and months, it is not unfrequently mastered in appearance, though seldom cured in reality.

[The following observations, which seem required to complete the subject, are extracted from a second paper upon the same subject.—Ed.]

In reviewing the thirty-five cases now presented to the readers of the Journal, several general truths seem worthy of particular notice. Among these not the least important is the bearing on the question of *pathological seat.* The first case, in conjunction with the cases of rheumatism produced by carious spine, reported in my first paper on this subject, leaves no doubt in my mind that a *condition of parts*, exactly the same as in rheumatism, may and does exist as an effect of irritation of the great nervous masses at the centre. It remains to be proved by the opponents of this system, by equally conclusive facts, that a genuine translatable rheumatism is ever found to be independent of such a cause, or that the disease *properly called rheumatism* is ever a primary affection of the limbs or joints. The extraordinary facility with which most of the recited cases were cured by exclusive spinal treatment goes far to establish concurrently the same truth. For when a true inflammation of a joint has a purely local character, as in Case XXXIII., the spinal treatment is altogether useless, while the more direct application of the same means meets with prompt success. If inflammation alone of the tissues ordinarily attacked by rheumatism constituted that disease, inflammation from a sprain or blow affecting the same parts should have the same translatable character, which is so characteristic of true rheumatic inflammation, and should also be capable of passing not only to similar parts, but to the viscera, as not unfrequently happens in rheumatic cases.

The great value of the opiate practice in rheumatism, is no small auxiliary to the arguments in favor of the central origin, because it is only by lessening nervous irritation that such a medicine can prove useful. It follows as a corollary, that the best possible practice in these cases consists in spinal depletion and counter-stimulation, combined with the judicious use of opiates. That such a practice has not been more generally followed in the public duty at the hospital, arose from a desire to obtain the more conclusive evidence of the truth afforded by the avoidance of obscuring complication. That being now attained, the practitioner will no longer feel justified in leaving out of his system of cure any one of the useful auxiliaries called for by the varying contingencies of his cases.

Another general fact is, that rheumatism attacks those parts only whose nerves come out at or near to the part of the *medulla spinalis* which is in an irritated state. This was the fact in the carious cases, and in the case of Dr. Parker, of Elkton. Although all the nerves in descending along the spinal canal must have been subjected to diseased influence, only those which *took their departure* from that part of the spine were influential in producing rheumatism, which consequently appeared at *their* sentient extremity exclusively.* The same point is sustained strongly by the effect of the remedial measures, which were scarcely ever of any use when

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* In my letter to Dr. Evans, March 14th, 1832, making inquiry respecting Dr. Parker's case, I find the following :—" Was there (I should suppose not) any rheumatic pain of the legs during the present illness?" Answer : " Has not had any rheumatism in the legs since the injury."

applied to remote parts of the spine, and were generally promptly beneficial when directed to the origin of the nerves of the part inflamed.

As far as we could ascertain, the remedial effect of the spinal treatment was most potent when applied exactly over the place of exit from the spine, of the nerves which supplied the inflamed part. It is also remarkable that the affection of the upper extremities was almost always the most easily remedied. This may be ascribed with probability to the greater effect of the cups on those parts of the spine which lie least imbedded in cellular tissue and muscular fibre.

In conclusion, I may be permitted to advert to the close connection between common rheumatism and certain diseases of mucous and fibrous tissues in the eyes, nose, mouth, alimentary canal, bladder, and urethra. In many cases diarrhœa and dysentery are found to alternate with rheumatism of the extremities, and particularly of the lower limbs. Wherever such cases happen, they are found to yield more readily to spinal treatment than to any other mode of cure, thus affording another proof of the spinal origin of such cases.

On the whole, it seems to me that the evidence for the pathology contended for is much more conclusive in the present instance than that on which the profession is accustomed to rest for much of its accredited science. As far as I can comprehend the objectors, their main difficulty lies in abandoning an unquestioned opinion, founded on prescriptive learning, and the more obvious phenomena of the disease. But even they will find it impossible to explain away the facts brought forward in favor of the new doctrine, unless they can show that in original inflammations of the periphery, the most judicious treatment consists in applications to the centre, a position fully overthrown by every abortive attempt to remedy sprains or bruises of the limbs by medicaments to the spine. Another objection lies in the absence of tenderness of such parts of the spine as are here presumed to be under irritation. If, however, that irritation transmits pain to a remote part, we ought not to expect to find it tender on pressure, but we ought to look for aggravation of the peripheral disease, an event actually produced in Dr. Parker's case. In a very few persons the spine was found tender on pressure, but these were patients who belonged to a debilitated class, in whom certain parts of the spine are almost always found too susceptible, whether they have or have not rheumatism, hysteria, or any fibrous or any uterine irritation. That tenderness so much insisted on, is but proof of an irritated condition of the *spinal braces*, the effect of weakness too highly tasked, and is totally independent of disease of the *medulla spinalis*.

So far as this case has been discussed, the *facts* have been adduced solely on the side of the new doctrine, while its opponents have contented themselves with irrational and supposititious objections, according to a system which, long since driven from other sciences, finds yet, I am sorry to say, a refuge in the temple of medical dogmatism. They admit the remarkable cures, the potent practice. They do not deny that rheumatism is produced by caries and injury of the spine, but as they do not see or feel the spinal irritation in the rest of the cases, the treatment, the relief, the victory over this obstinate malady, carry no argument to them, though sustained by analogies so striking and so profitable.

Tabular Abstract of Cases.

Name.	Date of admission.	Date of Cure.	Duration of treatment.	Remarks.
W. W.............	1 Sept.		1 day	
Eliz. Turner.....	14 Sept.		4 days	
Geo. Steiner.....	13 May	18 May	5 do.	
Chas. Wilson....	21 Jan.	9 Feb.	19 do.	Spinal treatment failed.
W. Barnard......	15 Mar.	20 Mar.	5 do.	
Chas. Schrœder	30 Mar.	1 April	2 do.	
W. Bowes.........	1 Nov.	6 Nov.	5 do.	
Anthony Della..	20 Dec.	25 Dec.	5 do.	
Christ. Rudolph	23 Dec.	25 Dec.	2 do.	
Daniel Wilson...	30 Dec.	2 Jan.	3 do.	
John Morgan...	30 Dec.	2 Jan.	3 do.	
Charles Slater...	3 Jan.	11 Jan.	8 do.	
And'w Franklin	3 Dec.	11 Feb.	70 do.	Four relapses.
W. Carter.........	16 Feb.	22 Feb.	6 do.	
Joseph Pratt....	4 Oct.	6 Oct.	2 do.	Several other relapses; discharged on 24th Nov. Total, 59 days
The same......	15 Oct.	17 Oct.	2 do.	
Jos. L. Baker...	29 Nov.	3 Dec.	4 do.	
John Charles....	1 Dec.	4 Dec.	3 do.	
John Wilson.....	3 Dec.	4 Dec.	1 day	
J. M. Grezinger	24 Oct.	19 Dec.	56 days	Salivated.
Jas. Carpenter..	7 Dec.	15 Dec.	8 do.	
Marg. Poston...	24 Nov.	30 Jan.	67 do.	Blisters and splints.
W. White.........	23 Nov.	27 Nov.	4 do.	Total, 74 days; other treatment used.
Relapsed......	1 Dec.	26 Dec.	26 do.	
Relapsed......	2 Jan.	16 Feb.	34 do.	
John Cave........	28 Dec.	2 Feb.	36 do.	Splints.
John Henry......	2 Jan.	22 Jan.	20 do.	
W. Goldsmith...	15 Jan.	19 Jan.	4 do.	
Capt. P...........	10 Feb.	13 Feb.	3 do.	
G. W. Peirce....	13 Feb.	15 Feb.	2 do.	
M. Donnelly.....	13 Feb.	27 Feb.	14 do.	Total 22 days.
Relapsed......	2 Mar.	10 Mar.	8 do.	
P. McCoy.........	2 Feb.	10 Feb.	8 do.	
J. Parsonage....	23 Feb.	2 Mar.	7 do.	
W. Woodruff.....	7 Jan.	26 Feb.	50 do.	
S. Summers......	25 Feb.	3 Mar.	7 do.	
R. McDonald....	16 Feb.	6 Mar.	18 do.	

Twenty-two of these cases were cured within eight days; and of the remaining ten cases, four were instances of frequent relapses through imprudent exposure during convalescence. At least two were supposed to complain for the purpose of remaining in the hospital, an event which not unfrequently exhibits hospital practice in a disadvantageous light. Only four cases therefore required any other than spinal treatment.

Although cupping is the most potent mode of spinal treatment, I have, in private practice, not unfrequently found that a good rubefacient, such as a sinapism, produced great relief, and sometimes effected a cure. At the very commencement of an attack it is often adequate to the entire removal of the pain, and consequent prevention of greater severity of symptoms.

THE END.