Observations on the laws governing the communication of contagious diseases, and the means of arresting their progress. [Read before the Literary and Philosophical Society of New-York, on the 9th of June, 1814.] / [David Hosack].

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OBSERVATIONS

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MEANS OF ARRESTING THEIR PROGRESS.

[*Read before the Literary and Philosophical Society of New-York, on the 9th of June*, 1814.]

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The following Observations on Contagion were originally drawn up in June, 1812, in a letter addressed to Dr. Chisholm, and were intended as an answer to his strictures on my proposed classification of contagious diseases: the substance of that communication to Dr. Chisholm has, in every respect, been preserved entire, though, for obvious reasons, in the paper as now printed the epistolary form is laid aside.

New York, January 2, 1815.

OBSERVATIONS, &c.

In July, 1808, I addressed to Dr. Chisholm some observations on contagion, or infection.* The object of that communication was, if possible, to narrow the ground of controversy upon that important and much contested subject. This I endeavoured to do, first, by showing that the distinction which had been proposed by some late writers, between contagion and infection, was unnecessary and fallacious; secondly, by dividing all diseases which are contagious, infectious, or communicable from one person to another, into *different classes*, according to the several laws which appear to govern their communication. These classes are three in number.

The first embracing those diseases which are communicated exclusively by *contact;* as the *itch*, *syphilis*, *hydrophobia*, &c. which are never conveyed through the medium of the atmosphere.

The second including those diseases which are communicable both by *contact*, or the near approach to the sick, and by the *atmosphere*, as *measles*, *small-pox*, *scarlet fever*, &c. which are communicable in every season of the year, and in every climate; in a pure as well as in an impure

* See Edinburgh Med. and Surg. Journal, vol. 5. p. 247. American Medical and Philosophical Register, vol. 2. p. 14.

air, though more readily by means of the latter than the former, and with which persons are rarely infected more than once in their lives.

Under the third class are enumerated those diseases which are only, in general, communicable, or contagious through the medium of an *impure* atmosphere; the air being rendered thus impure by the decomposition of animal and vegetable substances, as in low, marshy countries; or by concentrated human effluvia, as in camps, jails, hospitals, or on ship-board; but the same diseases I alleged, in a pure air, in large and well ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges constantly removed, and attention paid to cleanliness in general, are not usually contagious, or, under such circumstances, are very rarely communicated from one person to another.

In this class I included the *plague*, *dysentery*, *typhus fever*, in its various forms, of jail, ship, hospital, or lake fever, and the *yellow fever*.

I also remarked, that these diseases, like many of the first class, may be repeatedly contracted; but that they are communicable, or otherwise, according to the condition of the air in which they occur, or into which they may be introduced: it was further observed, that the atmosphere thus impregnated by the peculiar virus emanating from the diseased body, becomes *assimilated* to the poison, or ferment introduced, and thereby is rendered capable of reproducing in others the same *specific* disease, whether it be the plague, dysentery, typhus, or yellow fever. Such are the outlines of my first communication to Dr. Chisholm.*

In 1809 Dr. Chisholm did me the honour to reply† to my observations, expressing his entire approbation of the two first classes, but

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* See Note A.

† See American Medical and Philosophical Register, vol. 2. p. 121. See Note B.

objecting to the third. After enumerating his several objections, he requests me to reconsider my third division, which appears to him to be the only objectionable one. This I have done, and now submit to this society the result of a further examination of this subject, and a detail of the facts by which I have been led to my conclusion relating to the laws of communication, which I have more particularly assigned to the febrile diseases enumerated in the third class.

In my first communication, I acknowledge I have stated my observations without so full a detail of the facts themselves whence my conclusions were deduced, as perhaps ought to have been exhibited. To the European reader, unacquainted with the peculiarities of yellow fever, more especially as it has appeared in the cities of the United States, my first statement may perhaps appear defective in that evidence which is so justly exacted upon subjects of this nature. This evidence I shall now endeavour to supply, and thereby to confirm the correctness of the classification which has been proposed. Waiving for the present all inquiry relative to the nature or properties of the contagious principle secreted by the diseased body, or the chemical qualities of the atmosphere deemed necessary for its propagation, or the manner in which the contagion diffuses itself, I proceed to observe, that the history of each disease enumerated in the third class, viz. *plague*, *dysentery*, *typhus*, in all its forms, and *yellow fever*, furnishes evidence of the correctness of the remark, that they are governed by a law peculiar to themselves, that they are contagious or communicable in a *foul* atmosphere, but that they are never or very rarely so in a *pure* air, where the sick enjoy the benefits of cleanliness and ventilation.

The same evidence, I trust, will demonstrate another truth, that these diseases are, in no instances, *epidemic*, as they have been improperly denominated by most practical writers, but that their sphere of operation is, with very few exceptions, confined within the limits to which

the vitiated atmosphere extends, in which they may be engendered, or into which they may be introduced; and that, in this respect, they differ from ordinary epidemics, "which appear in different and distant parts of the same place, and at the same time."

That the plague, when once generated, whatever may be the sources whence it derives its origin, is communicated by a peculiar virus secreted by the diseased body, will not, I trust, be questioned at this day. Independently of the facts contained in the writings of Thucydides, Lucretius, Mead, Dr. Patrick Russell, and others, showing the contagious nature of the plague, the communication of this disease by inoculation, as performed by Matthias Deggio,* Dr. Whyte,† and the Russian surgeon, noticed by Sonnini,‡ have recently established the fact of its propagation by a specific secretion, beyond all possible controversy.

It has been observed, by Assalini, that Dr. Desgenettes, while in Syria, had in vain endeavoured to inoculate himself with the virus of the plague; and by the same writer it is incorrectly added, that Dr. Desgenettes made the experiment under the persuasion that the disease was not contagious; but from the account of the facts as stated by Dr. Desgenettes himself, it appears that the experiment was not made under that persuasion. On the contrary, he expressly declares, that its contagiousness was demonstrated by *a thousand examples*, and observes, contrary to the opinion of many, that the same person was liable to a second attack of it, as was the case with the convalescents whom he employed to attend upon the sick: furthermore it appears, from his own account, that he inoculated himself with matter taken from a person who

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* See Med. Com. vol. 8. p. 349. † See Wilson's Expedition to Egypt, and M'Gregor's Sketches. ‡ See Travels into Greece and Turkey, p. 497.

had the disease in its mildest form, what he denominates the first degree, in which the fever was slight, and the patient easily and promptly cured. Dr. Desgenettes adds, that it was an imperfect experiment, and that it does not disprove the communication of the disease by contagion, and that he made the experiment for the purpose of quieting the fears of the French troops, and of inspiring them with confidence.*

But that the plague, in common with the other diseases I have associated with it, is only communicable through the medium of an impure, or vitiated atmosphere, is an opinion which, although it has never been attended to by physicians, will be found to be verified by every writer on this disease. The plague of Athens, the first of which we have any authentic or satisfactory account, furnishes evidence of this truth. Whether that disease originated in the city of Athens, or was introduced into it from Æthiopia, the fact is established, that the circumstances under which it appeared in that city were peculiarly favourable to its diffusion. It appeared, according to Thucydides, in the beginning of the summer season, and first of all at the Piræus, the port and harbour of Athens, from whence it spread with increasing mortality into the upper part of the city. It appeared, too, at a time when Athens was so crowded with those who had fled thither from the adjacent country of Attica for safety from the invading armies of the Peloponnesians and their allies, that many of them were forced to lodge themselves within the turrets of the walls, or wherever they could find a vacant corner. "The city," says the historian, "was not able to receive so large a conflux of people:" "afterwards the long walls, and a great part of the Piræus, were portioned out to them for little dwellings; at the same time, too, the Athenians were fitting out, at the Piræus, a fleet of one hundred

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* See Note C.

ships to infest Peloponnesus." Even the Pelasgic, a hitherto vacant spot of ground below the citadel, which it was thought profaneness to occupy, and the settlement of which the Pythian oracle had specially prohibited, they were constrained, by urgent necessity, to turn into a dwelling-place. By this influx from the neighbourhood of Athens its number of inhabitants, as stated by a late writer, was suddenly increased from fifty thousand to more than four hundred thousand persons.* In another place Thucydides observes, "Those who had come in from the country had no houses, but dwelled all the summer season in booths, where there was scarcely room to breathe;" he adds, "The pestilence destroyed with the utmost disorder, so that they lay together in heaps, the dying upon the dead, and the dead upon the dying." Even in the public streets some were tumbling one over another, or lay expiring round about every fountain, whither they had crept to assuage their immoderate thirst; the temples, too, in which they had erected tents for their reception, were full of the bodies of those who had expired there. Thucydides proceeds, "In a calamity so outrageously violent, things sacred and holy had quite lost their distinction; all regulations observed before in matters of sepulture were quite confounded, since every one buried wherever he could find a place." He also observes, "it raged the most, and for the longest time, in Athens, but afterwards spread into the other towns, especially in the most populous, but never extended itself to Peloponnesus." We are told by the same historian, that "at the siege of Potidæa, which took place during the same season, the plague followed them even thither, and making grievous havoc among the Athenians, destroyed the army; and that even those soldiers that had been there before, and had, from the be-

* Medical Repository, vol. 1. p. 16.

ginning of the siege, been in perfect health, caught the infection from the troops brought thither by Agnon. After a stay of forty days, having, in that time, lost one thousand and fifty out of four thousand men, he returned with his ships to Athens."*

With these facts before us; the season of the year in which the plague made its first appearance, the part of the city in which it commenced, the multitudes which crowded into it, and those, too, unaccustomed to the air of the town, having been habituated to active employment in the pure air of the country, the impure state of the atmosphere necessarily resulting from this condition of things, combining the evils both of pestilence and war; the disease itself being confined within the walls of the city, while, at the same time, it never extended itself to the neighbouring country, not even to the contiguous towns of Peloponnesus and Bœotia, we are led to the conclusion, that an impure atmosphere is the vehicle or medium by which this disease is propagated.

The circumstances attendant upon the plague, as it has appeared at different periods in the city of Rome, are no less demonstrative of this truth. I will only notice the more remarkable visitation of this disease which took place in the year of Rome 290, and four hundred and sixty-one years before Christ. "This," says Livy, "was a season of great distress; for during this year a pestilential disorder spread itself not only through the city, but over the country, affecting both men and cattle with equal malignity; the violence of the disorder was increased by admitting into the city the cattle and the inhabitants of the country who fled thither for shelter from the enemy's ravages: such a confused collection of animals of every kind suffocated the citizens by the unusual

* Smith's Translation of Thucydides, vol 1. p. 153.

stench, while the country people crowded together in narrow apartments suffered no less from the heat, the want of rest, and their attendance on each other; besides, even contact served to propagate the infection."* Baker's Livy.

Dionysius of Halicarnassus mentions† that the disease seized studs of mares, herds of oxen, and flocks of goats and sheep, doubtless denoting that this disease was remarkably fatal to those animals when collected in numerous bodies. Orosius, in his account of the same pestilence, observes, "Many of the patricians were victims, but it was most fatal to the poor."‡ Livy also has a similar observation, that many illustrious persons died, but that among those of inferior note the virulence of the disorder spread its ravages wide.

The history of the pestilence of modern times, the accounts of which are more minutely and satisfactorily detailed, no less proves that this disease, when once introduced, spreads its devastation by means of a vitiated atmosphere, more especially where such vitiation proceeds from confined human effluvia. Accordingly, in the plague of London, in 1665, at which time nearly one hundred thousand persons perished, we are told by Hodges, that while the better sort of people had various resources to avoid the dreadful consequences of this fatal distemper, it was entirely confined to the poor, insomuch that some gave it the name of the *poor's plague*. Lord Clarendon, in the history of his own life,

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* Grave tempus et forte annus pestilens erat urbi, agrisque, nec hominibus magis, quam pecori; et auxere vim morbi, tenore populationis, pecoribus agrestibusque, in urbem acceptis. Ea colluvio mixtorum omnis generis animantium, et odore insolito urbanos, et agrestem confertum in arcta tecta, æstu ac vigiliis angebat, ministeriaque in vicem ac contagio ipsa vulgabant morbos. Tit. Liv. lib. 3. c. 6.

† Lib. 10.

‡ Lib. 2.

relates that when he and other people of condition, who had fled from the plague, returned to London, they hardly missed one of their friends or acquaintances, the mortality having been confined almost entirely to the lowest orders of the people. "At that time, too, the streets of London," says Thornton, "were narrow, crooked, and incommodious, the buildings chiefly of wood, dark, close, and ill contrived, and by the several stories projecting beyond each other as they rose over the narrow streets, the circulation of the air was almost entirely obstructed. To these inconveniences, he adds, may in some measure be attributed the destruction which had been repeatedly made in the city by the visitation of the plague; for as the air was confined, so the noisome vapours and pestilential atoms were harboured and nourished. Though the destruction of London by the great fire in the succeeding year (1666) occasioned great temporary distress, yet, in the end, it proved of the utmost utility; for, by the rebuilding of the city, and the enlargement of the streets, the free circulation of air was admitted, the offensive vapours expelled, and the city freed from all pestilential disorders."* It is also stated by Dr. Hodges,† that at the breaking out of this plague, the city was unusually full of people: he supposes there must have been upwards of one hundred thousand persons more than usual in the city; and, according to Dr. Baynard, during the progress of this merciless pestilence, there was such a general *calm* and *serenity of weather*, as if both wind and rain had been expelled the kingdom, and that for many weeks together not the least breath of wind could be discovered.

It is also worthy of remark, that the city of Oxford, to which the parliament was removed during the prevalence of the disease, remained

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* Thornton's History of London. † De Peste.

uninfected; which exemption is ascribed by Dr. Plott to the draining and greater cleanliness of that city.*

The great plague with which Marseilles was visited in 1720, and which destroyed upwards of sixty thousand of its inhabitants, presents us with a detail of facts which leads to the same conclusion. This disease, it is well ascertained, was introduced from the Levant by a ship which arrived at Marseilles from the coast of Syria. It appeared first among the sailors of the suspected ship; it was next taken by the porters engaged in opening and airing the merchandise in the Lazaretto; it was then introduced into the city, and spread among the poor, and first of all in a street which was only occupied by the lower class of people.†

In the commencement of the disease, Bertrand remarks, none but children and poor persons were attacked by it.‡ In a short time it extended to the neighbouring streets; it was also conveyed into the Hotel Dieu, by a person received as a patient from the street where the distemper first broke out; two of the nurses and the matron of that institution first died of the disease, when the infection spread with great mortality, destroying the physicians, surgeons, apothecaries, confessors, and all the other officers and servants of the house, with the whole of the poor in the hospital, including above three hundred foundlings.§

Soon after, all intercourse was prohibited between the town and neighbouring country: the scarcity of provisions which ensued, independently of the crowded state of the city, greatly added to the mor-

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* History of Oxfordshire. † Bertrand's Relation Historique, p. 414. ‡ Ibid. p. 50.
§ Ibid. p. 92.

tality of the disease: the number of the sick increasing, an hospital was opened for the reception of the infected, where the disease proved fatal to all the attendants. But the disease was not only propagated in those public institutions, where great numbers were crowded together, and in the confined dwellings of the poor; other circumstances occurred which served greatly to diffuse the poison still more generally throughout the city. According to Bertrand, the streets were crowded with "the sick, the dying, and the dead;" and the vapours which arose from the putrid dead bodies, in every part of the city, served to infect the air and spread the contagion; indeed, it soon extended to places that before this had been inaccessible to it; monasteries, and houses shut up in the most exact manner, were no longer places of security; the whole city became more or less *one infirmary.**

The infection, too, was very much increased from another source not less dangerous. An opinion prevailed that the dogs received the contagion from contact with infected clothes, and thereby became the means of spreading it still more extensively; the consequence was an order to destroy them; in a few days the streets were strewed with their carcasses; a prodigious quantity were thrown into the water; these also were soon cast upon the shore, where, by the action of a hot sun, the air was filled with the most noxious vapours. Infected clothing and furniture were also continually thrown into the street from the windows of the houses in which the disease prevailed, and, if possible, still further to give wings to the poison, fires were injudiciously had recourse to, for the purpose of destroying the infection; "at hours appointed," says Bertrand, "the whole city appeared on fire, and the air became loaded with a thick black smoke, better calculated to retain than to dissipate the

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* Bertrand, p. 145.

contagious vapour."* In fact, these fires, he adds, appeared to relume that of the contagion; "they heated the air, already rendered suffocating by the heat of the season and climate; the pestilential poison became more active, and the disease acquired new force."†

The plague of Aleppo, in 1760, 1761, and 1762, might also be cited upon this occasion, as well as many others, both anterior and subsequent to that period, to show that the epidemic influence of this disease is chiefly dependent upon the atmosphere into which it may be introduced. I cannot, however, pass over, without comment, the plague which the British and French troops suffered during the celebrated expedition to Egypt in 1800 and 1801, inasmuch as it will show that this disease, even in its *native* climate, is governed by the same laws of communication which have been observed when it has been introduced into other countries.

We are accordingly told, by the learned Dr. Wittman,‡ "that the disease is more prevalent at Rosetta than in any other town, or part of Egypt; he adds, "the streets of Rosetta are extremely narrow and very dirty. The crowded manner in which the inhabitants live together would appear sufficient, in a stagnant state of the atmosphere, in most of their towns, to generate pestilential or malignant diseases. The very few comforts and conveniences which fall to the lot of the poorer class of the natives in Egypt, by far the most numerous, would lead one naturally to expect great mortality when the plague prevails among them. Dreadful examples are seen annually to happen." In another part of the same work, he is still more explicit on this point, showing that the plague "does not always possess the same activity and force;" and the necessity, as he expresses it, of some "*powerful agent to put the contagion into action, and to give it its full force.*" He then asks,

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* Bertrand, p. 74. † Ibid. p. 75. ‡ Travels in Egypt, p. 525.

"May this agent reside in the atmosphere? Does this peculiar constitution of the air consist in a superabundance, or diminution, of the ordinary proportion of oxygen in the atmosphere? or in the combination of some peculiar gas or gases diffused in it?" He suggests that a series of eudiometrical and other observations, continued for several years, might throw some light on this subject: "Time alone," he adds, "may unfold this mystery."* But when we take into view the facts he has already stated, relative to Rosetta, and are told by the same author, that in Egypt the plague prevails when the Nile is low, and of course the air loaded with the impurities thence arising; that at Constantinople, the cold weather, in winter, puts a period to its progress, and the still more general observation, that the extremes both of heat and cold, are unfavourable to the propagation of plague; these facts, in connexion with those already stated of this disease, as it has appeared at different times, and in different parts of the world, are certainly calculated to dissipate much of the mystery in which this subject has been enveloped. The remarks of Dr. M'Gregor, that the plague varies its type according to the state of the air, and other circumstances, and that by *ventilation*, *fumigation*, and attention to *cleanliness*, the progress of the disease was arrested,† also serve to confirm the correctness of the view which has been taken of this subject. Even the writings of Assalini, who disbelieves the communication of this disease by contagion, furnish additional support to the principle here contended for; for he admits, that when persons are *shut up* and *crowded together*, in infected places, the disease is readily contracted.‡ In another place, he observes, "that if a person be exposed to breathe the infected air in the chamber of a patient, or should he stay too long in the same atmosphere, he

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* Travels in Egypt, p. 533. † Med. Sketches, p. 111. ‡ Observations sur la Peste.

will run a great risk of contracting the prevailing malady."* He moreover proceeds; in order to prevent all suspicion, and avoid all danger of carrying the disease where it has not been before, that they should take nothing with them but the necessaries of life; they should avoid, as much as possible, halting in villages; and each time when they happen to encamp, they should expose their baggage and clothes to the air, which would not fail of dispersing every particle of contagion. As a further evidence, too, of the connexion between the prevalence of the disease and the state of the air, he remarks, that during the epidemic, "the inhabitants residing near the sea were more exposed than those who were at some distance, and that there were several villages situated on the heights which had not even a single sick person." In many other parts of his work, he shows that his mind was not totally devested of belief in the communication of the plague, by contagion; and when danger approaches, like some modern professors in religion, he proves himself to be the practical infidel, by distrusting even his own doctrines; for he takes great pains to inform us of the various means he made use of to protect himself against the disease, and which are both as efficient and judicious as the most sturdy contagionist could possibly have employed. Imlac, in Rasselas, speaking of the appearance of departed spirits, says, "Some who deny it with their tongues, confess it by their fears." So with Assalini, and, indeed, the same may be said of many others who *affect* to disbelieve the doctrine of contagion.

In addition to the details cited from Thucydides, Livy, and from the writers of modern times, I might here introduce similar facts recorded of the plague of Florence, which appeared in that city in 1348.†

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* Observations, &c.

† See Introduction to Boccacio's Decameron.

But to conclude upon this part of the subject, and in the language of Dr. Chisholm himself, "Every physician who has delivered his opinion of the origin of the plague maintains, that a peculiar state of the air is absolutely necessary to establish the powers of contagion, and give circulation to the imported infection."*

Another disease which I have placed in the same class with the plague, and have considered as governed by the same laws of communication, is *dysentery*. By this disease I mean not that *local* affection of the bowels which is frequently symptomatic of diarrhoea, and unaccompanied with fever, but that form of it which has been described by Pringle, Blane, and other practical writers, under the title of epidemic dysentery, or the dysentery of camps.

This disease, like the plague, appears also to derive much of its infectious character from the condition of the atmosphere in which it takes place: in pure air, where cleanliness and ventilation are attended to, it rarely extends beyond the individual in whom it first originates; but in a vitiated atmosphere, loaded with moisture, marsh effluvia, or the perspirable matter, and other excretions of the human body, especially where many persons are crowded together and in small apartments, dysentery communicates itself to the greater part of those who may be exposed to its influence. Zimmerman remarks, that "in general it appears to him that dysentery became contagious purely through nastiness and the crowding many people together in a small space, but was by no means so of itself."†

And as a further evidence that the disease was derived not from the noxious qualities of the atmosphere alone, but from contagion communi-

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* Essay on the Malignant Pestilential Fever, vol. 1. p. 286.

† Zimmerman on Dysentery, p. 20.

cated through that vitiated medium, he also observes of the dysentery which occurred at Dettingen, in 1743, that such of the officers, among whom it was not so general as among the soldiers, as had lain wet at Dettingen were first attacked by it; the rest received it by contagion; but a regiment that had not lain in the damp, nor been exposed to the rain, remained perfectly free from it, at a small distance from the camp; though, excepting that they were not subject to the contagious effluvia of the rest, "they breathed the same air, ate the same provisions, and drank the same water."* And in the hospital in the village of Feckenheim, about a league from the camp, the dysentery being introduced, "the air became infected to such a degree that not only the rest of the patients, but even the apothecary, nurses, and the other servants, with most of the inhabitants of the village, were infected."†

Dr. Donald Munro, who, as an army physician, had frequent opportunities of observing the character and progress of dysentery, ascribes the greater violence of this disease to obstructed perspiration, moist and putrid vapours, the putrid steams of dead horses, of the privies, excrements not covered with earth, or to the unwholesome, moist, putrescent vapours of marshy or wet grounds, or pools of stagnating water acted upon by the heat of summer, and of other corrupted animal or vegetable substances, all which served to increase the infection. Hence he observes that in camps the more hot and rainy the season, the more wet and marshy the ground, and the more the air is replete with putrid vapours, the more frequent and the more fatal is the dysentery.‡

The remarks of Sir John Pringle are also in point on this subject. "Some dysenteries," he observes, "appear upon first taking the field,

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* Zimmerman on Dysentery, p. 26. † Ibid. 139.

‡ Diseases of the Army, v. 1. p. 314—316.

but the cases are never so bad, nor nearly so frequent, as towards the end of summer; they then become epidemic and contagious. They have always been most numerous and worst after hot and close summers, especially in fixed camps, or when the men lay wet after a march in warm weather."* "In general the contagion does not suddenly spread; for whole towns and camps are never seized at once from the impurities of the atmosphere; but the infection is carried from one to another by the effluvia, or clothes and bedding, &c. as in the plague." "In camps the contagion passes from one who is ill to his companions in the same tent, and from thence, perhaps, to the next." "The foul straw," he adds, "becomes infectious, but the greatest sources of infection are the privies, after they have received the dysenteric excrements of those who first sicken. The hospitals likewise spread it, since those who were admitted with the flux not only gave it to the rest of the patients, but to the nurses and other attendants of the sick."† And to show that this disease is not dependent on a general constitution of the atmosphere, but upon that which is impure, and to which the dysenteric taint has been communicated, he observes of the epidemic which raged at Nimeguen, in 1736, "that none of the neighbouring towns suffered, unless by their communication with the place infected."‡ Similar facts, illustrative of the rapid extension of this disease, when introduced into ships of war, are recorded by Dr. Blane, in his valuable work on the Diseases of Seamen.

That the contagiousness of *typhus fever* is, also, in a great degree, ascribable to a similar condition of atmosphere as its pabulum, is demonstrated by facts recorded in almost every book of practice, more especially in those relating to the diseases of the army and navy, which have ever been found to be nurseries of this disease. The observations made

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* Diseases of the Army, p. 218. ed. 7th. † Ibid. 254. ‡ Ibid. 252.

upon this subject by the Linds, Pringle, Blane, Percival, Smyth, Trotter, Haygarth, Ferriar, Currie, and others, relating to the spread of this disease, when introduced into hospitals and ships of war; its prevalence and diffusion among the poor of London, Edinburgh, Liverpool, and the manufacturing towns of Great Britain; the beneficial effects which have been derived from the establishment of fever wards, and houses of recovery, the advantages which have been experienced from the fumigating or oxygenating processes introduced by Dr. Johnstone of Worcester, Guyton De Morveau, and Carmichael Smyth, in arresting the progress of the typhus fever, all irresistibly lead to the conclusion, that the impurities of the air constitute the fuel of this disease; and, to use the expressive language of Dr. Ferriar of Manchester, in a late communication which I have received from that learned physician, that "dilution with atmospheric air is now ascertained to be the most effectual mean of destroying contagion, and of controlling the ravages of this disease."*

Were it necessary, I might adduce a volume of additional testimony on this subject. I cannot, however, omit the following pertinent remark of Dr. Haygarth, who, like another Howard, has devoted his life to the investigation of this interesting subject; and to whom Great Britain is indebted for the first establishment of institutions specially devoted to the important purpose of arresting the progress of contagious diseases. In his remarks on the nature of the contagion which produces putrid fevers, he observes, "I soon discovered that their infectious atmosphere was limited to much narrower extent than even the small pox. So manifestly I observed this to be the case, that in a clean, well-aired room, of a moderate size, the contagious poison is so much diluted with fresh air, that it very rarely produces the distemper, even in nurses exposed to all the putrid miasms of the breath, perspiration,

* See American Med. and Phil. Register, vol. 2.

fæces, &c. whereas, in the close, dirty, and small rooms of the poor, the whole family, generally, caught the fever. Hence we may conclude, that in well-aired and clean apartments, the air is seldom so fully impregnated with the poison as to acquire an infectious quality."*

The observations of the late Dr. Willan are also in point on this subject. "Formerly," says that accurate observer, "the typhus, with petechiæ, &c. often occurred in our prisons, and proved fatal to those who were under confinement in close cells, or who lodged in crowded apartments. Mr. Box, surgeon of Newgate, informs me that the fever has been rendered less frequent there, and less virulent, by removing the persons first affected, into airy rooms, or wards, and by a general attention to ventilation, cleanliness, &c. so that, at present, petechiæ do not appear in more than one case in thirty."† And of three hundred and seventy-nine patients committed into the London House of Recovery, says Dr. T. Bateman, nine only, or about one in forty-two, were affected with petechiæ.‡

The facts which have been ascertained relative to the communication of *yellow fever*, furnish no less conclusive evidence that this disease, like those already noticed, is, or is not, generally contagious, depending on the qualities of the air to which it may be communicated. The history of every visitation of this disease, in the United States, establishes this truth. It has not only regularly made its first appearance in our sea-port towns, and in those places where the air is most impure; at that season of the year, and in those seasons when such impurities acquire their greatest virulence; in those houses which are most crowded with inhabitants, and where there is the least attention paid to cleanliness; but, wherever

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* Proceedings of the Board of Health in Manchester.—Letter from Dr. Haygarth to Dr. Percival, p. 8. † Willan on Cutaneous Diseases, p. 469. ‡ Ibid.

the same disease has been thence conveyed to other parts of the same city, or town, or into the country, it either was propagated or extinguished, according to the *local* circumstances of the place to which it was so conveyed.

Dr. Lining, in his description of the yellow fever which was introduced into the city of Charleston in 1732, 1739, 1745, and in 1748, observes, that, although the infection was spread with great celerity through the town, yet, if any from the country received it in town, and sickened on their return home, the infection spread no further, not even so much as to one in the same house. He remarks, that the disease was generally more fatal to those who lay in small chambers not conveniently situated for the admission of fresh air.* The yellow fever with which the city of New-York was visited in 1791, and which was introduced by a vessel from the West Indies, and rendered memorable by the death of one of our most respected citizens, General Malcolm, who was the first victim to the epidemic of that season, is thus recorded by Dr. Jonas Addoms, in his excellent dissertation on that disease:

"About the middle of August, 1791, a contagious fever appeared in the city of New-York, which first discovered itself near Peck-slip, a part of the city thickly inhabited, its houses generally small, and badly ventilated; many of the inhabitants were in indigent circumstances, which is a frequent cause of the want of cleanliness. Here it raged a considerable time; it then began to spread, as some attendants on the sick became infected who lived in other neighbourhoods. By this means it was carried to other families, and most generally could be traced to this source. It likewise proved more particularly fatal near the place where it first appeared, than in any other part. Thus at length it

* Edin. Phys. and Lit. Essay, vol. 2. p. 403. 427.

spread through the city, until about the middle of October, when the weather growing a little cooler, the disease greatly abated, and in a short time disappeared."*

Dr. Addoms, the author of that dissertation, since that time resided many years in St. Croix, and being associated with a celebrated physician of that island, the late Dr. Gordon, had ample opportunities of seeing the yellow fever in all its forms. During his last visit to this city, not long before his death, he informed me that the disease which he had seen in New-York in 1791, was precisely the same which he afterwards saw in St. Croix, and which frequently prevailed during his residence there, more especially among Europeans newly arrived within the tropics. He also remarked, at the same time, that this disease always acquired new virulence, and was rendered highly contagious, when introduced among soldiers crowded in barracks, or on shipboard.

In the yellow fever of 1793, which was introduced into the city of Philadelphia from the West Indies, it is conceded, on all sides, that the disease made its first appearance in Water-street, and that all the cases of this fever were, for two or three weeks, evidently traced to that particular spot. It is also a fact well ascertained, that in the vicinity of the place where the infection was first received, the air was, at the same time, in a very offensive condition from a quantity of damaged coffee which was exposed upon the dock, and under circumstances favourable to its putrefaction and exhalation. From that place the disease gradually infected a considerable part of the city, the Northern Liberties, and district of Southwark, and did not subside until terminated by frost, after having proved fatal to nearly five thousand persons.

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* Inaugural Dissertation on Yellow Fever, p. 7.

It is also to be remarked, that its ravages were chiefly confined to the poor, and to those parts of the city where the houses were small, and the least attention given to cleanliness and ventilation. In the language of Mr. Carey, "it was dreadfully destructive among the poor. It is very probable that at least seven eighths of the number of the dead were of that class; the inhabitants of dirty houses have severely expiated their neglect of cleanliness and decency by the number of them that have fallen sacrifices. Whole families, in such houses, have sunk into one silent undistinguishing grave. The mortality in confined streets, small alleys, and close houses, debarred the free circulation of air, has exceeded, in a great proportion, that in the large streets, and well-aired houses. In some of the alleys a third or fourth of the whole of the inhabitants are no more. The streets in the suburbs that had the benefit of the country air have suffered little. It is to be particularly remarked that, in general, the more remote the streets were from Water-street, the less of the calamity they experienced."*

Though the disease," says Dr. William Currie, "was highly contagious, the influence of the contagion was circumscribed to a narrow sphere."†

As a further evidence that it did not depend on a general condition of atmosphere, the same author remarks, "that while this formidable disease was making such ravages in the city, the country, for some miles around, was never more healthy."‡ In another work Dr. Currie has very explicitly admitted the *qualified* contagiousness of yellow fever, observing, "that it is only contagious in situations where the air is confined, and the exhalations of the sick are permitted to accumulate,

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* Carey's Account, 4th edit. p. 61, 62. † Treatise on the Synochus Icteroides, p. 8.

‡ Ibid. p. 11.

through neglect of frequently changing the bed and body linen of the patient."*

Similar facts are recorded of the visitation which New-York experienced of the same disease in 1795. Upon another occasion I shall make public the evidence which is in my possession, indisputably proving the importation of the yellow fever of that season from Port-au-Prince. In that year the disease appeared upon the east side of the city, first affecting some seamen who had received the infection from a brig directly from Port-au-Prince; from thence it spread in the vicinity from Dover-street to Peck-slip; but throughout that season it was confined, in a great degree, to that part of the town where the local condition of the atmosphere was peculiarly favourable to its diffusion; for not only an unusual quantity of filth was accumulated in Peck-slip, but at that very time a great number of emigrant poor had arrived from England, Ireland, and Scotland, so that the numerous lodging houses, especially in that neighbourhood, were unusually crowded; add to this, that the weather was uncommonly moist, and thereby peculiarly calculated to spread the infection. According to the statement made by Dr. Bayley it was particularly fatal to the emigrants of that very summer; for "out of nearly eight hundred persons who died," he observes, "not more than one hundred and fifty were citizens of New-York."†

In another part of the same statement he remarks; "so limited was the operation of the contagion that the number of those taken sick in low situations, compared with those residing in more elevated parts of the city, may be computed as twenty to one."‡

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* See Observations on the Yellow Fever, in the Philadelphia Med. and Phys. Journal, vol. 2. part 1.

† See Bayley on the Epidemic of 1795, p. 90.

‡ Ibid. p. 86.—See also Letters to Dr. Buel by E. H. Smith.

In 1798 New-York was again visited with this scourge of our seaport towns: during the months of August, September, and October, about two thousand persons fell victims to this disease, at the end of which time a keen frost put an almost instantaneous termination to its progress. The disease of that season first appeared at the shipyards, in the neighbourhood of New-slip, and, as in former years, was introduced from the West Indies.* After cutting off several persons in the neighbourhood in which it commenced, the same vessel was removed to another slip, also on the east side of the city; from thence the disease was communicated by those who worked on board to a thickly-settled part of the city, where the houses are small, the streets narrow, and chiefly occupied by the poor; viz. Cliff-street, John-street, Ann-street, Fair-street, Eden's-alley, and Rider-street; at the same time, however, it still continued to extend its ravages in the vicinity of the place to which the poison had been first communicated; and to some other thickly-settled parts of the town, to which it was subsequently conveyed. In a short time afterwards it was introduced into Pearl-street, and in that part of it between Burling and Peck-slips, where it spread very extensively. In that season a number of circumstances concurred to diffuse the contagion in that part of the city: a great quantity of rain had fallen, so as to overflow the cellars in Pearl-street, which were, at the same time, stored with salted provisions; these were soon afterwards spoiled, and loaded the atmosphere with a highly offensive vapour; the disease raging at that time in that neighbourhood, acquired new virulence, and, for the most part, followed the course of the vitiated atmosphere, "beyond the limits of which," says Mr. Webster, "the disease exhibited

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* See statement of facts on this subject by the Rev. Dr. M'Knight, in the Amer. Med. and Phil. Reg. vol. 3.

little infection:" indeed, the extension of this disease, as has already been frequently observed, was so circumscribed within the limits of this impure air that it became very generally believed that, in that season, whatever may have been the case in former years, the disease exclusively arose from those domestic sources, more especially from the putrid provisions. But that the yellow fever of that season did not derive its origin from the spoiled beef is evident, not only from the fact that the disease had already previously appeared in other parts of the town, and even in that very neighbourhood, before those heavy rains had fallen, and their pernicious effects were perceived; but also, that those tainted provisions, unaccompanied with the specific poison of the disease, did not of themselves communicate infection to those who were constantly exposed to their effluvia.

Mr. Edmund Prior, the inspector-general of beef at that time, informed me, that of forty persons whom he had employed in examining the beef, and in removing and emptying such barrels as were found in a putrid state, not one was taken ill of the yellow fever.* But Dr. Chisholm and Dr. Stewart have abundantly shown, that decomposed animal or vegetable matters will not, of themselves, produce the pestilence;† and that this disease is generated in the human system, and communicated from one person to another, by a peculiar secretion from the morbid body. My object is to show that when such virus is introduced into a certain state of atmosphere, the disease is readily contracted, but that beyond that atmosphere it is rarely infectious.

Although the diseases which have been noticed are rarely communicable in pure air, and are not generally contagious in the country, it is not less true, that in some few instances it appears, either that the virus,

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* See Note D.

† See Note E.

as secreted from the diseased body, is alone, in sufficient quantity, or possesses a sufficient degree of virulence, to reproduce such diseases; or, that by means of the impurities collected about the diseased individual, occasioned by inattention to cleanliness and change of clothing, the retention of his excretions, or the confined air of his apartment, the virus itself becomes multiplied, and thereby the means of communicating the disease from one to another are in the same degree increased: for it is a fact not to be questioned, that instances of yellow fever, as well as of the plague, dysentery, and typhus fever, have been occasionally infectious, even in the more pure air of the country, though it must be acknowledged that such cases are of rare occurrence.

It is observed by Dr. Rush, whose records of the several visitations of the yellow fever in the city of Philadelphia will be lasting monuments of the facts which they contain, as well as of the impressive and eloquent manner in which they are related, "that out of upwards of one thousand persons who have carried this disease into the country from our cities, there are not more than three or four instances to be met with of its having been propagated by contagion."* Such instances, however, have occurred in New Hampshire, as related by Dr. Spalding;† in Connecticut, as stated by Dr. William Moore of this city;‡ on Staten-Island, in 1793, as recorded by Dr. R. C. Moore;§ at Huntingdon, on Long Island, in 1795 and 1798;‖ and at Germantown, in the vicinity of Philadelphia, as related by Dr. Wistar.¶ But these very exceptions, if they can with propriety be denominated exceptions,

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* Observations on the Origin of the Yellow Fever of 1799, p. 12.

† Med. Repos. vol. 3. p. 8. ‡ Addoms' Disser. p. 7. American Med. and Phil. Reg. vol. 2. p. 177. § Ibid. vol. 2. p. 22. ‖ Ibid. vol. 3. p. 191.

¶ Additional Facts and Observations by the College of Physicians of Philadelphia, p. 36.—See Note F.

manifestly prove the specific character of those diseases, and that they are propagated by a specific secretion peculiar to each disease, whether it be plague, dysentery, typhus, or yellow fever. Indeed, to use the emphatic expression of the Edinburgh Reviewers on this subject, "In the present state of medical knowledge, it would not be at all more absurd to deny the existence of fever altogether, than to maintain that it is not propagated by contagion."* But, in the language which Dr. Mead has applied to the plague, we may say of all the diseases of this class, "that a corrupted state of the air is, without doubt, necessary to give these contagious atoms their full force."†

If it were necessary I might go on to cite every return of the yellow fever with which the United States have been visited to show, that the progress of the pestilential poison has ever been commensurate with the impurities of the atmosphere, and that when sufficiently diluted with pure air, it ceases to propagate itself.

It is probably owing to this impure condition of the atmosphere that the various fevers, and the greater mortality of diseases in general, are to be ascribed, which physicians have frequently observed to precede the appearance of pestilential disorders, and to announce their approach, and which have led many to conclude that the pestilence itself was thus engendered by local circumstances, and not imported. Facts of this nature have served to mislead the editors of the Medical Repository, and many other late writers, who thus confound the *exciting* and *predisposing* causes of disease; who do not discriminate between the inflammable materials, and the spark which lights the flame; but have identified the domestic circumstances which have served to diffuse

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* Edinburgh Review, vol. 1. p. 246. † Mead's Medical Works.

the poison of yellow fever, with the peculiar virus itself, by which that disease has been introduced into the various cities of the United States.

The same local circumstances, I believe, will go far in accounting for the "pestilential state of the air," the "secret constitution of atmosphere," so often recorded by writers on epidemics; at the same time that they teach us that the diseases now under consideration are only epidemic in as far as the vitiated state of the air is itself epidemic.

I, however, wish it to be understood, that I do not exclude the influence of bodily predisposition, the passions of the mind, and many other circumstances, in aiding the propagation of pestilential diseases.

Having, as I trust, shown, by the facts that have been adduced, that the plague, dysentery, typhus, and yellow fever, constituting the third class of contagious diseases, require an impure state of the air to diffuse and multiply them, the question next presents itself, in what manner does such impure air operate in spreading those diseases? Upon this part of the subject I have the misfortune to differ from Dr. Chisholm no less than I do as to the necessity of such an atmosphere to propagate the peculiar poison of each of those diseases. Dr. Chisholm observes, that if the proposition had been advanced, "that those diseases, particularly the pestilential yellow fever, are rendered more violent in their action under the circumstances stated, of an impure atmosphere, that no possible objection could be made to it, inasmuch as it is supported and proved by all experience;" and he proceeds to express the opinion that such an atmosphere may have an effect "by rendering the system of the healthy person, who receives the poison from the sick, more susceptible at the moment of its introduction, of its peculiar action;" but that this multiplying power does not proceed from any action

of the air upon the peculiar virus of those diseases; that "it does not proceed from the impure atmosphere becoming assimilated to the poison introduced."

That air, deprived of its due proportion of oxygen, and loaded with mephitic materials, especially the confined excretions of the human body, will vitiate the mass of circulating fluids, and impair the functions of the nervous system, cannot be denied; that the febrile diseases with which the system may be affected while in this state, will acquire an extraordinary degree of malignancy, will also be readily conceded; but that such condition, either of the atmosphere, or of the human system, increases its susceptibility to be acted upon by the virus of those contagious diseases, composing the third class, does not correspond either with the facts which have fallen under my own observation, or with those I have been enabled to obtain from the writings and observations of others.

The well known facts relative to the communication of *jail fever* to the judges presiding at the Black Assizes, in 1577,* and a similar infection being communicated to the judges on the bench, and other persons present, at the sessions held at the Old Bailey, in 1750, while the prisoners themselves remained in health, insensible to infection, furnish incontestible evidence of the effects of habit in diminishing the sensibility to the poison of fever: and with regard to the *yellow fever*, it assuredly has not been the case in the United States, that those who were most accustomed to the impure air of the place in which the disease prevailed, were more susceptible of the disorder than those who had recently arrived from the pure air of the country, or from the more elevated parts of the town. On the contrary, those who were least accustomed to the

* Bacon's Works, vol. 2. Stowe's Chronicle.—See Note G.

impure air of the city, or of the infected spot, were uniformly observed to be most susceptible of the contagion. Those, too, who enjoyed the most vigorous health, and the most robust constitutions, the reverse of that condition of body which would be the effect of a residence in impure air, were more readily infected upon coming into the atmosphere impregnated with the contagion, than those who had remained constantly exposed to its influence. Whatever differences of opinion have existed among the physicians of the United States, as to the origin of the disease, they are all perfectly agreed relative to the facts which I have just stated. Indeed, Dr. Chisholm himself inadvertently admits the same to be true; for he observes, that, in the West Indies, sailors, soldiers, and young men, especially those who have recently arrived from Europe, and are least accustomed to the climate, were more obnoxious to it than others.*

Dr. Gordon, and, indeed, all the most distinguished practical writers on this subject, concur in the same observation. "New comers," says Dr. Gordon, in the appendix to Dr. Chisholm's late valuable letter to Dr. Haygarth, "were infected with the pestilential fever, while the old seasoned soldiers had only the common tropical remittents; and this was universally the case whenever both diseases were at the same time epidemic."† A similar, and still more general, observation on the predisposition of those who are unaccustomed to impure air, is made by Dr. Blane. "Infection," says he, "like some other poisons, does not so readily affect those who are accustomed to it, and therefore those who are in the habit of being exposed to it, frequently escape its bad effects." For the like reason, he adds, "physicians and nurses are less susceptible than others; and strangers, who are accustomed to a pure air, are

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* Chisholm's Letter to Haygarth, p. 182, &c.

† Ibid. 220.

the most susceptible of any."* With these facts and observations before us, we are compelled to conclude, that the impure air necessary to propagate the contagion does not operate in the manner Dr. Chisholm supposes, by "increasing the susceptibility of the system to the action of the poison introduced." On the contrary, I believe that it produces its effects by some chemical combination with the peculiar virus secreted from the diseased body, and that thereby the contagion becomes more or less extensively multiplied, according to the extent and virulence of such vitiated atmosphere.

I shall not attempt to define the precise nature of the chemical union which takes place under such circumstances. But I wish it to be distinctly understood, that in such combination, I do not believe with those writers who contend that a *tertium quid* is produced, or, as Dr. Adams of London, in his late publication on Epidemics, has reiterated the same idea, "that a new kind of air is generated."† On the contrary, as far as I am enabled to view the subject in connexion with the facts usually observed during the prevalence of the diseases which have been noticed, I am inclined to believe, that in this combination the peculiar virus of those diseases is in no way changed, but multiplied; and that this multiplying power is a process very analogous to that which we observe to take place in the assimilation of the fluids of the human body to the peculiar taint which may be introduced into the system, as, for instance, in small pox and syphilis; or, perhaps, that it more nearly resembles the process of fermentation, as it occurs in inanimate matter. By both these processes such an assimilation takes place in the fluids acted upon, whether of the living body or in dead matter, that they partake of the same properties with the virus or ferment introduced, and

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* Diseases of Seamen, p. 223.

† Adams on Epidemics, p. 11.

are thereby rendered capable of renewing the same process in other bodies under similar circumstances. This process has very properly been denominated by Dr. Walker, the *assimilating fermentation*,* and has been no less successfully employed both by him and by Mr. Cruikshank,† as well as by Dr. Cullen, to explain the changes which take place in the living system, acted upon by small pox, and the virus of other contagious diseases, than it has been by Sir J. Pringle,‡ Macbride,§ and Alexander,‖ to the phenomena of fermentation, as it occurs out of the body.¶ The history of plague, dysentery, and typhus fever, as well as the recent observations in animal chemistry, furnish a variety of facts which may be adduced in illustration of such fermentative process taking place in the atmosphere, and in watery fluids loaded with the excretions of the human body, or the vapours of vegetable and animal substances in a state of putrefaction.

Similar facts, illustrative of the fermentative process contended for, have been observed whenever the yellow fever has prevailed in any of the cities or towns of the United States. I have already stated that this disease has always prevailed in proportion to the presence of such fermentable materials. It is no less, true that whenever the disease has been introduced it has spread in the greatest degree in those seasons when the air was unusually moist: this was remarkably the case in New-York, in 1795** and 1798,†† and in Philadelphia, in 1793 and 1798:‡‡ and that the yellow fever has prevailed in the United States in those seasons when the heat, combined with moisture, was most favourable to such assimilating or fermentative process

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* Walker's Inquiry into the Small pox. † Anatomy of the Absorbing Vessels. ‡ Diseases of the Army, Appendix. § Experimental Essays. ‖ Experimental Essays and Experimental Inquiry. ¶ See Note H. ** See Bayley on the Yellow Fever of 1795. †† Hardie on the Yellow Fever of 1798. ‡‡ See Rush and Currie.

is also proverbially true. It is also to be observed, as universally admitted, that the same disease has uniformly been extinguished by the approach of frost, which destroys such fermentative process.

Another argument in favour of this explanation is derived from the fact that this disease has, in several instances, been introduced into our cities, without extending beyond the individuals who have introduced it; manifestly owing to the active exertions of a vigilant police, at the same time that every attention was paid in preserving cleanliness about the persons of the sick. This was remarkably the case in the year 1804, when the yellow fever was introduced at the Wallabout, on Long Island, and in 1809, when the same disease prevailed at Brooklyn. In each of those years the fever was introduced into this city by persons who had received the infection on Long Island, but, owing to the circumstances just mentioned, it was not communicated to others; while the same disorder, owing to local circumstances, spread in the vicinity of those places on Long Island where it had first appeared.*

During the year 1811, the yellow fever was also introduced into the city of Amboy, New-Jersey, from the Havanna, but did not spread beyond those persons who were first attacked in consequence of their immediate exposure to the air of the infected vessel. The local circumstances of Amboy, its elevated situation, its dry and sandy soil, its wide streets and spacious houses, their distance from each other, and the remarkable cleanliness of the town, most satisfactorily account for the sudden extinction of the disease, while the evidence of its importation must be admitted to be conclusive.†

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* American Med. and Phil. Reg. vol. 2. p. 95, &c. † See American Med. and Phil. Reg. vol. 3.; also Edinburgh Med. and Surg. Journal, and the Med. and Phys. Journal of London. —See Note I.

But there is another circumstance which particularly merits attention: in every epidemic visitation of the yellow fever, several days, viz. from eight to twelve, or fourteen, have generally elapsed between the first cases that appeared, and the communication of it to other persons, even in the same neighbourhood; insomuch that not only our citizens, but our physicians themselves have been led to doubt the existence of the disease, and to stigmatize as alarmists those who first announced the deadly visitor.* I can never forget the occasion, in 1795, when that venerable and experienced physician, the late Dr. John Bard, assembled the physicians of this city to announce to them the first cases of this disease which he had observed in the family of his friend, Mr. Jenkins. The physicians met, but declared they had seen no other fevers than what they had been accustomed to observe every year, and even doubted, on that occasion, the correctness of Dr. Bard's observations, relative to the nature and character of the disease to which he called their attention: but that accurate observer had been too familiarly conversant with the yellow fever as it appeared in New-York in 1743 and 1762, and too well knew the pathognomonic symptoms of that disease to confound it with the fevers of our own climate: he, accordingly, in the most emphatic language, replied to their doubts, "Gentlemen, within a fortnight you will all see and acknowledge the West-India yellow fever to exist in our city." The event is well known.† The same interval between the first cases of the disease and its subsequent diffusion in the neighbourhood where it first made its appearance is noticed by almost every writer who has recorded the yellow fever in the United States.

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* See Note K. † See Bayley and Hardie on the Epidemic of 1795.—See also Currie on the Fever of 1793.

A similar interval has been frequently noticed in the history of the plague. Dr. Russell, in his account of the plague of Marseilles, in 1720, observes, "that from the 12th of July to the 23d there was a deceitful pause, during which the popular apprehensions began to subside. The physicians were reproached with ignorance in having mistaken ordinary fevers for the plague. The disease, however, in this interval, had continued to spread in the street, Rue de l'escale, where it made its first appearance."*

It has also been remarked of the plague, as well as of the yellow fever, that the infection spread most rapidly when the atmosphere was not only heated and loaded with moisture, but when it was least agitated by wind or thunder storms. During those calms when the air may be said to be relatively at rest, it has been uniformly remarked, that the contagion of the yellow fever has multiplied itself most extensively, as was always very apparent by the greater number that were seized within five or six days after such close weather had been observed, all which circumstances certainly conspire to promote the fermentative process that has been contended for.

This is not all: whenever the yellow fever has been introduced into the cities of the United States, its first extension has always been slow and gradual. Upon several occasions its boundaries have been accurately defined by our board of health. This, as I have stated on a former occasion, was remarkably the case in this city in 1805. The disease, in that year, was confined, for some weeks, to a small portion of the eastern side of the city, and, as stated by the board of health, "not a case occurred, in any part of the town, that was not referable to that as its source."† This fact being ascertained, the board accordingly for-

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* History of the Plague.

† Hardie's Account of the Malignant Fever of 1805.

bade intercourse with the infected portion of our city, and ordered an abandonment of that part of the town, threatening violent measures if their orders were not immediately complied with. In a short time after, the infection extended a few streets further; the board of health again defined its limits, and again declared that still not a case had occurred that could not be traced to this part of the city as its source.*

Will not the same assimilating or fermentative process furnish the most satisfactory solution of the fact noticed by Boerhaave, Cullen, Lind, Russell, and many others, that *fomites* are more to be dreaded than the excretions alone proceeding from the diseased body? Not, however, in the manner those authors suppose, that such fomites acquire greater virulence; but, that by the same process the specific poison has been more extensively multiplied by means of the atmosphere and foul excretions which are involved in the clothing worn by the sick; and that by the same means the danger of the infection has been increased in the same degree that the poison has been multiplied. As a further evidence, too, that the contagion is *multiplied*, but not more concentrated, as those writers have imagined, it is a fact established by every writer on those contagious diseases, that the first cases of every epidemic are uniformly the most fatal; but that, as the season advances, the danger of taking the disease is increased, while the disease itself has, perhaps, become even milder than it was in the commencement.

Let me further ask, do not the processes lately introduced for disinfecting the air by means of the fumes of the acetic acid, the oxygenated muriatic acid gas, the nitric, and sulphuric acid vapours, operate by making new combinations with some of the ingredients constituting the

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* Chisholm's Letter to Haygarth.—See Note L.

tainted atmosphere, and thereby decomposing the morbid compound? According to Dr. Crawford, "the fluids which destroy the fœtid odours most speedily are those which are acknowledged to contain the greater portion of oxygen, and it is, therefore, extremely probable that this change depends on the union of the oxygen with animal hepatic gas, or some one of its constituent parts." But the explanation which has been offered by the late Dr. Garnett, of the manner in which the oxygen thus employed combines with the hydrogen gas which holds the morbid secretions in solution, appears to me the most satisfactory explanation that has been given of those phenomena.*

From these facts, I have been led to conclude,

1st. That an impure atmosphere is indispensably necessary to multiply and extend the specific poison constituting *plague*, *dysentery*, *typhus*, and *yellow fever*.

2dly. That the impurities of the atmosphere do not produce their effects, in the manner suggested by Dr. Chisholm, by increasing the susceptibility of the system to be acted upon by the peculiar virus of those diseases.

3dly. That, instead of predisposing the body to be thus acted upon, the reverse is the fact; that the predisposition of those who are most exposed to such impure air is less, while those who reside in the pure air of the country are most, liable to be infected when exposed to the contagion.

4thly. That the impurities of the atmosphere are fermentable materials, to be called into action by the specific ferment of those diseases, aided by heat, moisture, and a calm state of the atmosphere, and that

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* Proceedings of the Board of Health of Manchester, p. 40—42.—Robertson's Treatise on Medical Police, vol. 2. p. 127.—Robertson's Natural History of the Atmosphere, vol. 2. p. 352. —See Note M.

as far as such atmosphere extends, and the circumstances favourable to such fermentative or assimilating process continue, so far those diseases become epidemic, but no further.

The same idea of an assimilating process appears to be expressed by Lucretius, when, speaking of the contagiousness of the plague, he observes,

> "Proinde, ubi se cœlum, quod nobis forte venenum,
> Conmovet, atque aër inimicus serpere cœpit;
> Ut nebula ac nubes paullatim repit, et omne,
> Quâ graditur, conturbat, et immutare coactat.
> Fit quoque, ut in nostrum quom venit denique cœlum
> Conrumpat, reddatque sui simile, atque alienum."
>
> LUCRETIUS, de Nat. Rerum, lib. VI.

Or, as it has been rendered by that learned surgeon and accomplished scholar, John Mason Good, Esq.

> "But when the heaven, of poisonous power to us,
> First moves remote, its hostile effluence creeps
> Slow, like a mist or vapour; all around
> Transforming as it passes, till, at length,
> Reach'd our own region, it the total scene
> Taints, and assimilates, and loads with death."

If the view which has been taken of this subject be correct, a still more important truth is the result; that, while by a rigid and well-executed system of quarantine laws we have it in our power to guard against the introduction of the spark that kindles the flame, we are also enabled, by means of domestic cleanliness and ventilation, to extinguish it when introduced. For this purpose our magistrates and guardians of the public health cannot be too attentive in their police regulations to have all noxious materials removed from our streets and

our dwellings; and, at the same time that they are ornamenting our cities by the erection of magnificent buildings, and the introduction of other important improvements, they should also avail themselves of every opportunity which may present of widening our streets, and of reserving squares and other pieces of ground to be ever kept vacant, as among the most effectual means of preserving the health of our citizens, and guarding against the propagation of contagious diseases.

——τὸ Πελασγικὸν, ἀρχὸν ἄμεινον.

——"Best is Pelasgic empty"

was wisely expressed by the Pythian oracle; thereby denoting that every large and populous city, as well as Athens, should have its *pelasgics*, or vacant pieces of ground, as so many reservoirs of pure air, for the purpose of counteracting the effects of contagion when introduced.

DAVID HOSACK.

ADDITIONAL NOTES

ON

CONTAGION, &c.

NOTE A. (*See page* 6.)

Observations on Contagion. Communicated in a Letter to Dr. Chisholm, of (Clifton) England; dated New-York, July 16, 1803.

Dear Sir,

Agreeably to my promise in a former communication, I shall now state to you the result of my observations on contagion, a subject which has created so much dispute in the medical world, and which divides our profession in the western as well as in the eastern hemisphere. As far as I have examined this subject, it appears to me to be more a dispute about *words* than *facts*. The abuse of the terms *contagion* and *infection*, and the neglect of writers in not annexing to them a precise definition of the manner in which they severally employ them, have, I believe, been the source of our medical warfare, relative to the contagiousness of yellow fever, and some other diseases: *e. g.* the greater number of medical writers enumerate, in the list of contagious diseases, all those which are in any way communicable from one person to another, whether by *contact, fomites, atmosphere*, &c. without designating the *circumstances* attending these several modes of communication.

Lind, in his papers on contagion and infection, (which he considers as synonymous terms,) is guilty of this error, in which he has been followed by most writers upon the subject of fever, &c. The late Dr. Bayley, in his account of the yellow fever which

prevailed in New-York in 1795, proposed a distinction between *contagious* and *infectious* diseases. He made use of the first term to denote such as are communicated under any circumstances of atmosphere, whether pure or impure, as small pox, measles, &c. *Infectious* diseases he denominated those which are communicated in consequence of an impure or vitiated state of the atmosphere, *i. e.* that the *impurities* of the atmosphere communicate the disease, not that the air contains any *specific material* derived from the patient, except such as may be occasioned by want of cleanliness. This distinction, proposed by Dr. Bayley, is, in my opinion, an approach nearer the truth than any of his predecessors have advanced, but it does not present us with a view of the *whole truth*, upon the subject. The visiter or attendant contracts disease from one of *two sources*, either from the filth of the sick room, or from a *specific something* issuing from the body of the sick, the consequence of the peculiar disease under which he labours. If a person visiting another ill of the *yellow fever* or *plague*, derives his disease from the *impure atmosphere* of the apartment, I ask how it happens, that in all instances he contracts the *same disease* with that of the person whom he visits? Why is his disorder not an *intermittent*, a *remittent*, *jail fever*, or *dysentery*, which are considered the usual produce of filth? If he derives any thing specific from the sick, his disease is then assuredly not to be considered as occasioned by the *atmosphere*, but depending on the *peculiar condition of the fluids*, or state of the system, induced by the action of a specific poison; in other words, it is to be considered a *contagious* disease. The distinction proposed by Dr. Bayley, inasmuch as it does not account for the communication of the *peculiar form* of fever or disease which is thus propagated, I, therefore, consider to be insufficient to account for the circumstances attending the communication of those diseases to which it is applied. That I may not be misunderstood, I will suppose A to be ill of *dysentery*, a disease well known to be attended with a *peculiar train of symptoms;* he is in a small, confined apartment, his person is neglected, the atmosphere around him is rendered impure and offensive; under these circumstances B visits him, and a few days after is also taken sick with the *same disease*, attended in all respects with the *same* dangerous symptoms which characterize the disorder of A. Dr. Bayley, and those who adopt the doctrine of *infection* as opposed to *contagion*, consider the disease of B to proceed from the *impurities of the chamber*, and not from any thing *peculiar* emanating or secreted from the body of A. But as we may, without hazard, visit an equally filthy chamber where C lies ill of *cholera morbus*, or D with a *broken limb*, I therefore ascribe the disease of B to something more than the *impure air* of the chamber of A. I ascribe it to a *peculiar virus* generated in his system by the disease under which he labours, and communicated

by his excretions to the surrounding atmosphere, rendering it thus capable of producing the same disease in those who may be exposed to its influence.

The communication of this virus from the sick to the well, in whatever form it may be conveyed, as uniformly produces the *same disease*, as *inoculation* excites the *small pox*, or *vaccination* conveys the *vaccine virus*. So far, then, there is something in *common* in the communication of contagious or infectious diseases, which should be accordingly expressed in the language we employ—some of those diseases are conveyed in one form, others in a different; we should then be equally careful to mark those circumstances in which they *differ*, as well as those which they possess in *common*.

Such an arrangement appears to me not only practicable, but, at the same time, calculated, in some degree, to harmonize the differences of opinion which now separate the contagionists and non-contagionists. Under these impressions, I propose to arrange those diseases which are communicable from one to another under three heads. First, those which are communicated *exclusively* by *contact*. In this class I enumerate

The Itch,
Syphilis,
The Sibbens of Scotland,
The Laanda of Africa,
Frambæsia, or Yaws,
Elephantiasis, or Leprosy,
Hydrophobia, and
The Vaccine virus.

Neither of those diseases can be communicated in any other way than by *contact;* they are, therefore, *contagious* diseases, in the strict etymological sense of the term. It is also to be remarked, that these diseases are never conveyed through the medium of the *atmosphere;* actual contact alone can communicate them from one person to another.

These diseases, acknowledged by all to be contagious, and so denominated by all writers, have a law of communication peculiar to themselves. But there is a second class of diseases also considered as contagious, which are communicated under different circumstances, governed, in this respect, by different laws of communication.

Those to which I now allude are such as are communicated both by *contact* and by the *atmosphere*. In this class I arrange

Small pox,
Measles,
Chicken pox,
Hooping cough,

Scarlet fever, and
Cynanche maligna.

Contact, or the *close approach* to the sick, labouring under these diseases, will communicate them to those who are susceptible of their influence—but they are no less communicable through the *medium of the atmosphere.* A *second law*, which governs the communication of this class of contagious diseases, is, that they are communicable in *every season*, in the heat of *summer*, as well as in the cold of *winter*—in a *pure* as well as in an *impure* air, though more readily by the latter than the former. A *third law* of communication in this class of diseases is, that the persons afflicted with them are not generally susceptible of a second attack. I say *generally*, because exceptions are related upon very respectable authority.

This second class of contagious diseases is, therefore, abundantly distinguished from the first; but they are still associated by most medical writers under the same head of contagious diseases, without assigning to each class its discriminating characters.

The same want of discrimination has, in my opinion, occasioned the numerous disputes among physicians relative to the contagiousness and non-contagiousness of those fevers which I enumerate as the *third* class of diseases that are communicable from one person to another. Under this head I arrange

Plague,
Yellow fever,
Typhus, *jail*, *ship*, *hospital*, or *lake fever*, and
Dysentery.

These diseases are only, in general, communicable through the medium of an *impure* atmosphere: in a pure air, in large and well ventilated apartments, when the dress of the patient is frequently changed, all excrementitious discharges immediately removed, and attention paid to cleanliness in general, these diseases are not communicated, or very rarely so, from one to another. But in an *impure* air, rendered so by the decomposition of animal and vegetable substances, as takes place in low marshy countries, or by concentrated human effluvia, as in camps, jails, hospitals, or on shipboard, they are rendered not only extremely malignant and mortal in themselves, but become communicable to others who approach the sick, or breathe the same atmosphere, which has become *assimilated* to the poison introduced, insomuch that *the same specific disease* is communicated, whether it be the *plague*, *yellow fever*, *typhus*, or *dysentery*.

Hence we account for the fact stated by Sydenham, and other writers on epidemics, that the prevailing disease swallows up all other disorders; *i. e.* that during the prevalence of an epidemic plague, typhus, dysentery, or other diseases of this class, every indisposi-

tion of a febrile sort readily assumes the character of the prevailing disorder. We know this to be experienced in the diseases of other countries, and we see it daily exemplified in our own: both in our cities and in the country towns, when, after heavy showers of rain, and the action of a hot sun, a decomposition of vegetable and animal substances takes place, and dysentery or typhus fever is produced, it assimilates the air to itself, whatever may be the acting poison. But under other circumstances of weather and season, the disease thus originating from some local circumstances, or from a peculiar habit of body in the person so affected, does not extend beyond the family in which it first occurred, or, perhaps, the individual in whom it originated.

This class of diseases, therefore, like the former, has a law peculiar to itself; *i. e.* the diseases composing it are communicable, or otherwise, depending upon the *condition of atmosphere* in which they occur or are introduced—whereas those of the second class are conveyed from person to person, through a *pure* as well as an *impure* medium: but they also are rendered more virulent and malignant in an atmosphere charged with miasmata than in that which is free from such ingredients.

It is also, I believe, generally true of the diseases of the *third class*, not perhaps excepting the *plague* and *yellow fever*, that they may be taken a *second time*. This has been advanced by the advocates for the domestic origin of yellow fever, as an argument against the contagiousness of this disease.

But, upon the same principle, they must deny the contagiousness of all those disorders which I have enumerated in the *first* class, as *itch*, *syphilis*, &c.; for most of them are also to be taken a second time: yet they are acknowledged by all to be *contagious diseases*. In the same manner, many persons make the *small pox* a standard, and conclude that yellow fever is not contagious, because it is not communicated under the same circumstances of atmosphere and season, and governed by the same laws with that disease.

They might with the same propriety conclude, that the *scarlet fever* is not contagious, because it is not attended with the pustules of small pox. This teaches us the importance of correct language to convey the several degrees of contagion which have been noticed; and that, while we may make use of the terms now in use, we should annex to them such explanations as will convey those different laws of communication which have been enumerated. With those precautions in the use of the language we employ, I believe, the contagionists and non-contagionists will find themselves very much in the situation of those theologians of whom Pascal speaks, and ready to adopt the expression of one of them, when he observes,

"La difference qui est entre nous est si subtile, qu'à peine pouvons nous la marquer nous mêmes."

We would then be ready to admit, that the yellow fever is a contagious or communicable disease, in an *impure* atmosphere; but not generally so where the air is preserved pure and free from noxious materials.

This doctrine, too, I believe, will better account for the apparently contradictory facts, which have been urged by the advocates of the two opposing opinions, than any system that has been adopted.

It will also lead to a system of police regulations, which will best insure us against the ravages of yellow fever when introduced, at the same time it will teach us carefully to guard against the introduction of it from abroad.

I shall treat this subject more at length upon another occasion, in connexion with the evidences of the importation of the yellow fever into the United States.

I am, sir, with sentiments of high respect, your's,

Dr. Chisholm. DAVID HOSACK.

NOTE B. (*See page 6.*)

Strictures on the classification of Contagious Diseases proposed in the preceding communication, contained in a letter from Colin Chisholm, M. D., F. R. S., &c. &c.

Clifton, (England,) October 14, 1809.

Dear Sir,

You will find in the October number of the Edinburgh Medical and Surgical Journal the observations on contagion you favoured me with some time ago.

I approve highly of the first part of your paper; but on reading the third class of your division of contagious diseases, I find myself obliged to dissent, or, at least, to hesitate in affixing entire approbation to it: and, indeed, I have reason to think, that when you have reconsidered the admissions and concessions you have there made, and the tendency which they have to throw all fevers, however arising, except those which are symptomatic of what are called specific contagions, and which comprise the whole of your second class, into an undefined state of anomaly, you may see cause for retracting them, or giving them more limitation. For my own part, I feel more inclined to divide all diseases of contagion into two classes, which may be denominated apyrexious and pyrexious; and as the latter are distinguished, mediately or immediately, by specific causes, they may be divided into two sections, the symptomatic and the ideopathic.

If a febrile disease has for its cause, an aura, a gaseous fluid, emanating from the body of a man labouring under that fever, which will manifest activity only within a certain, generally definable, distance from that body, how can we with any propriety of conception or language, say, that such a disease is only communicable through the medium of an impure atmosphere? Again, is not a fever proceeding from the action of marsh miasmata different from the fever we have just been considering, inasmuch as the one is *not* communicable—the other *is certainly* communicable from the subject of it to be a healthy subject? How then can we admit such a proposition as the following: "In an impure air, rendered so by the decomposition of animal and vegetable substances, as takes place in *marshy countries*, or by concentrated human effluvia, as in camps, jails, hospitals, or on shipboard, they (your third class) are rendered, *not only extremely malignant and mortal in themselves, but become communicable to others who approach the sick, or breathe the same atmosphere which has become assimilated to the poison introduced, insomuch, that the same specific disease is communicated, whether it be plague, yellow fever, typhus, or dysentery?* Pardon me when I say I do not understand this; when I say I perceive a marked inconsistency in it; several causes of an opposite nature producing effects all partaking of one general principle, contagion. Is not this a very near approach to the heterogeneity I have apprehended? I suspect that dysentery does not come with marked propriety into the same class with plague, yellow fever, (malignant pestilential,) and typhus, for it is certainly oftener the product of marshes than morbid animal effluvia, and partakes very often of the type of intermittents. When I thus object to your classification as it relates to pyrexious contagions, I fully admit the occurrence of hybrid fevers, that is, contagious fevers with the types of those we know to proceed from the action of marsh miasmata. But on these occasions, all the conditions which constitute pyrexious contagions are present; their form is only a little changed; their nature is unaltered: they are communicable only within the radius of contagion. That plague, or yellow fever (by which I understand the fever to which I have given the name malignant pestilential,) more especially, "are *only* communicable through the medium of an impure atmosphere;" that in a pure air, in large and well ventilated apartments, &c. they are not communicated, or very rarely, are propositions which I much doubt, because they are most certainly in direct contradiction to my own experience. I speak more immediately concerning the malignant pestilential fever. Were this a fact sanctioned by universal experience, (forgive the pointed expression, it is only for your own eye,) the extirpation of this fever might, in general, be a very easy operation. But I have seen too many proofs of its contagion communicating itself to healthy persons in a pure atmosphere, to admit the validity of your definition of it. If healthy persons, or persons possessing every internal

feeling and external appearance of health, passing to leeward, within six feet of other persons just recovered from the malignant pestilential fever, and wearing the clothes they had on their persons during the presence of the fever, have received the contagion of the fever from them, and have perished under its action; if this has happened in the open air (wherein nothing generally conceived to be impure, could be perceived) surely your definition cannot be said to accord with experience. Again, if in a chamber kept as clean and as pure as it is possible to keep a sick chamber, the disease has been in many hundred, I might safely say thousand, instances, communicated from the persons afflicted with it, to others in a state of apparent health, when the latter have approached the former to within the radius of contagion; if this is true, and the proofs of its being so are innumerable, I must again say, that surely your definition cannot be said to accord with experience. Had it been advanced, indeed, that all pyrexious contagions, but particularly that of the malignant pestilential (yellow) fever, are rendered more violent in their action under the circumstances you have stated, no possible objection could be made to the proposition, because it is supported and proved by all experience; and the reason is, perhaps, as obvious as the fact itself is generally admitted. It does not proceed from the impure atmosphere becoming assimilated to the poison introduced, but from rendering the system of the healthy person, who receives the poison by approximation to the sick, more susceptible, at the moment of its introduction, of its peculiar action; or the chemical physician may say, had it been advanced that the atmosphere of the sick chamber, being confined and close, as it too often is, is rendered obnoxious to health, by having its oxygen diminished by the respiration of the persons, sick and well, inhabiting it, and that thereby the effluvia of contagion become more concentred by being less subjected to decomposition or solution, whichever you will, the proposition would be less objectionable. But in either case, the laws of the existing contagion are not affected; in the one, the healthy person is only made more susceptible of its impression; in the other, it becomes less volatile, and therefore more virulent, and more certain of infecting. But the atmosphere is not assimilated to its principle. And, indeed, it must be evident, that were this not the fact, we should have no right to expect a specific disease produced by a specific contagion—plague and yellow (malignant pestilential) fever produced by the contagions peculiar to them.

But all fevers appearing in the circumstances you mention, would necessarily have an undefined character; they would be universally anomalies; a result so inconsistent with the regularity of nature, I imagine, has never been met with. There are, indeed, seeming anomalies produced by a combination of morbid causes and effects. But as these are well known to depend on the irregular performance of some particular functions, occa-

sioned generally by morbid depositions on the organs of these functions, they lose their anomalous form in the mind of the judicious physician, and are cured by the adoption of appropriate means. Now, as the diseases which you class under your third head, have each a peculiar and well-defined character, so must they severally have a peculiar contagion; and the circumstances under which you represent them, must be considered as purely adventitious, and aiding only inasmuch as they may predispose the healthy body to be acted on by the peculiar contagion it is exposed to, or as they may render the basis of that contagion less decomposable. One word more, and I am done with my impertinent critique: are not all contagious diseases capable of communication by *fomites?* (a word which, by the by, does not seem to be well understood. Servius's comment on a passage in the first Æneid of Virgil, gives a more distinct idea of the word *fomes* than any I have met with. You know it is received by our medical lexicographers as derived from the verb *foveo*, a derivation I doubt much the correctness of. The passage I allude to you will find at verse 178, the last member of which is "rapuitque in fomite flammam;" on which Servius says, "Fomes sunt assulæ quæ ab arboribus cadunt, cûm inciduntur, et igni concipiendo commodæ sunt." Now, were we to substitute words in the following manner, we should have, I imagine, a correct idea of fomites: Fomes sunt indusiâ, panni aliaque vestimenta quæ corporibus ægrorum peste aliave febre contagiosa laborantium, cadunt; cûm inciduntur et contagionè concipiendæ commodæ sunt.) If they are, what has impure atmosphere, so rendered by the decomposition of animal and vegetable substances, to do with their origin? The purest atmosphere conceivable, that is, an atmosphere in all respects well conditioned to support animal life in a perfect state, cannot prevent an attack of the malignant pestilential (yellow) fever, if a healthy person is exposed to the fomes of its contagion, and is predisposed to be acted upon by it. The remarkable proof of this which occurred at New-Haven, and which has been so ingenuously related by Dr. Munson, must be perfectly in your recollection. The present inquiry has nothing to do with those singular ideosyncrasies which resist contagion, although exposed to that of variola, rubeola, &c. as well as that of malignant pestilential fever, &c.

As the foregoing is not entirely my own opinion relative to your classification of contagious diseases, I hope, and indeed feel convinced, you will forgive the freedom I have taken in so candidly giving it. You will also, I am satisfied, receive in good part, in the spirit of true philosophy, my request, that you will reconsider particularly your third head, which I imagine is the only objectionable one. I am aware of the inconsistency which may be attributed to me in stating a proposition so opposite to that which may be found in my Essay on the Malignant Pestilential Fever, vol. 1. p. 281, and in my letter to Dr. Haygarth, p. 142. But herein, I trust, I manifest that disposition which should be

paramount in the minds of those whose object, in all discussions, is truth. Subsequent inquiry and reflection have convinced me that I stood not on solid ground, when I stated, that among the causes of pestilence is the product of animal substances of every description, deprived of life, and in a state of putrefaction: and I cordially agree with the critic on my letter to Haygarth, when he says, "We are more inclined to think that there is a specific contagion distinct from mere putrefaction, and which, perhaps, is not cognizable by any of our senses."—*Crit. Rev. July*, 1809.

It may be asked, as all contagious diseases proceed from certain specific or peculiar causes, what are those causes, and how arises that diversity of character assumed by contagions? To this question we must be silent; for no pathologist, however experienced, and however deep his research into nature may be, can give a satisfactory answer. We know that there is such a diversity, and, in general, we have some knowledge of the treatment they severally require. Perhaps this is as much as we ought to know. It is a subject which must for ever elude human research. Neither anatomy nor chemistry give any aid here. Exuberant fancy may, indeed, wanton in theory, and may conceive every different form of contagion as proceeding from a different combination of the same chemical principles acting differently on the organs of our frame, or on the fluids which they secrete. But strip these notions of their fancy-dress; let them stand naked before us, and they become mere phantasms. Par levibus ventis, volucrique simillima somno.

Believe me to be,

Dear sir, very faithfully your's,

Dr. Hosack. C. CHISHOLM.

NOTE C. (*See page* 9.)

The American publishers of the New-York edition of Neale's translation of the work of Assalini on the plague of Egypt, in their introductory observations, have, among others, the following singular remark:

"The existence of such contagion [of the plague] has never been proved by the evidence of one of the senses. The contagion is a mere conceit of the mind; and all reasoning upon such a visionary and fancied agent can be but hypothesis, and have no better claim to our assent than the fluid of magnetism, and the ether of gravitation; an assumption not near so worthy of our assent, as if it had been said, that the invisible angel of de-

struction had literally and bodily descended, sword in hand, to execute a heavy judgment of his Lord."

In refutation of these extraordinary and unwarrantable assertions, it were an easy matter to cite a volume of evidence. The following facts, selected from others of a similar kind, will, I trust, be sufficient for the purpose of demonstrating the fallacy of the doctrine of the American publishers referred to, and the correctness of the view that has been contended for, relative to the contagious character of the plague.

Dr. Guthrie, a celebrated physician of St. Petersburgh, in his inquiries relative to the contagiousness of the plague, observes:

"Chance threw in my way an excellent opportunity of examining into the merits of this opinion, by consulting the physicians and surgeons of the Russian army, lying in the conquered Turkish provinces of Moldavia and Walachia, where the plague ever obtained more or less during the whole war, and at the beginning raged with destructive violence; and on addressing myself to Baron Ash, physician general, I had an answer that met my ideas on the subject.

"The difficulty of ascertaining the exact time when the infection is received, and, consequently, the interval that takes place before the attack of the pestilential fever, must appear upon the face of the case, except experiments were to be made on purpose, which is scarcely to be expected. However, the intrepidity of a single man has thrown some light upon the matter: this was Mathias Deggio, one of the surgeons of the hospital at Buckarest, a building appropriated to the cure of the plague in the Russian army.

"He, perceiving the gentlemen of his profession condemned, in a manner, to death, if punctual in their duty, had the resolution to inoculate himself for the plague, in the full confidence of its efficacy, and ever afterwards found himself invulnerable, whilst his companions around him were falling victims to its fury. He produced the disease by inserting, with the point of a lancet, under the epidermis of his arm, matter from a pestiferous abscess, and followed the cold regimen observed in the small pox, as he had imitated its mode of inoculation; on the fourth day of the puncture the fever declared itself."

Dr. Guthrie adds "the Baron then said a great deal in favour of inoculation of the plague, and supported his opinion with the case of Mathias Deggio given above; and the analogy that has been observed between the plague, small pox," &c. (*Medical Commentaries Edin. vol.* 8.)

Another fact of a similar nature is recorded by Sir Robert Wilson: "That daring spirit of investigation into the causes and effects of those diseases, whose principles are yet unknown, and which has so much distinguished the profession, was not to be intimidat-

ed by the menacing consequences of a bold examination into the powers and properties of the plague.

"Dr. Whyte, an English physician, determined to discover if this malady, so destructive to a large portion of the globe, and which filled with apprehension the remainder, could not be checked, or rendered less virulent, by the introduction of inoculation.

"Resolved to become the patient of his own speculation, during the time the plague raged again at Rosetta, (which it did towards the fall of the year, when numbers of Sepoys died,) he inoculated himself with matter taken from the buboes of an infected person. The attempt failed twice; the third proved fatal; in three days after the symptoms appeared, he died, falling a much-to-be-lamented victim to a disinterested zeal, benevolently and intrepidly directed for the benefit and happiness of the community." *Wilson's Hist. of the British Expedition to Egypt, 4to, page* 257.

The highly respectable Dr. M'Gregor, (*see Medical Sketches of the Expedition to Egypt from India*) relates the same fact more circumstantially as follows: "I know not that I can better describe the disease, than by a short statement of the cases of some of the medical gentlemen who had the disease, most of whom wrote me accurately every thing that they felt. Dr. Whyte entered the pest-house at El Hammed, on the evening of the 2d of January, 1802. In a letter of that date he writes to me, '*I just now inoculated myself, by friction, with bubonic matter on the left thigh;*' on the 3d, he says, '*I have this morning inoculated myself, by incision, on the right fore arm.*' Mr. Rice, then doing duty in the pest-house at El Hammed, gives the whole of the case. In a letter on the 3d of January, he writes to me, '*Dr. Whyte came here last night; soon after he came in, he rubbed some matter from the bubo of a woman, on the inside of his thighs. The next morning, he inoculated himself in the wrist with a lancet, with matter taken from the running bubo of a Sepoy:*' In subsequent letters Mr. Rice says '*that Dr. Whyte continued in good health on the 5th, and all day on the 6th, till the evening, when he was attacked with rigors and other febrile symptoms. After sweating profusely, he was better in the morning of the 7th, but in the afternoon the shivering returned; and after it had continued 30 minutes, a severe hot stage came on, then a profuse sweating followed, but with it much affection of the head, tremor of the limbs, particularly of the upper extremities, tongue black and dry, skin hot, pulse full, hard, and irregular, thirst, great prostration of strength, and anxiety. The head was the only place he complained of, and it seemed to be the principal seat of his disease: On the 8th, these symptoms continued, and there was some delirium; he begged to be removed from the pest-house at El Hammed, to the old pest-house at Rosetta, under the charge of the Arabs. He was removed on the morning of the 9th, and died in the afternoon of that day very delirious.*'"

Another instance of the same intrepid, but benevolent effort to diminish the sufferings of humanity, is recorded by Sonnini, (*Travels into Greece and Turkey, London ed. p.* 497.) "Every physician," says that writer, "has not the courage, or, to speak more correctly, the madness of that Russian surgeon, prisoner at Constantinople with a number of his countrymen, who took it into his head to inoculate these unfortunate beings with the plague, in order to render the contagion less destructive: by this means he killed two hundred of these prisoners; and, fortunately for the rest, the inoculator, after having performed the operation on himself, died of his own treatment."

M. Assalini has stated, in corroboration of his own opinion of the non-contagiousness of the plague, that M. Desgenettes, the chief physician to the army of the East, while in Syria, inoculated himself with the matter of the plague, without taking the disease, and that he made this inoculation under the persuasion that the plague was not contagious. The following observations, taken from the work of M. Desgenettes himself, will clearly show the incorrect statement of M. Assalini on this subject, and that Dr. Desgenettes considered the contagiousness of the plague as absolutely demonstrated.

"Ce fut pour rassurer les imaginations et le courage ébranlé de l'armée, qu'au milieu de l'hôpital je trempai une lancette dans le pus d'un bubon, appartenant à un convalescent de la maladie au premier degré, et que je me fis une légere piquure dans l'aîne et au voisinage de l'aisselle, sans prendre d'autres précautions que celles de me laver avec de l'eau et du savon qui me furent offerts. J'eus pendant plus de trois semaines deux petits points d'inflammation correspondants aux deux piquures, et ils étoient encore très sensibles lorsqu'au retour d'Acre je me baignai en présence d'une partie de l'armée dans la baie de Césarée. Cette expérience incomplete, et sur laquelle je me suis vu obligé de donner quelques détails à cause du bruit qu'elle a fait, prouve peu de chose pour l'art; elle n'infirme point la transmission de la contagion, démontrée par mille exemples; elle fait seulement voir que les conditions nécessaires pour qu'elle ait lieu ne sont pas bien déterminées: And, as a further evidence that this experiment, if it deserves the name of an experiment, was made for the purpose, as Dr. Desgenettes declares, of inspiring the troops with confidence, he adds, "Je crois avoir couru plus de danger avec un but d'utilité moins grand, lorsqu'invité par le quartier-maître de la soixante-quinzieme demi-brigade, une heure avant sa mort, à boire dans son verre une portion de son breuvage, je n'hésitai pas à lui donner cet encouragement." (*Histoire Médicale de l'Armée d'Orient, par le Medecin en Chef, R. Desgenettes, a Paris, an* x. 1802.)

In addition to the testimony of Dr. Desgenettes, I subjoin the following interesting letter from Alire R. Delile, M. D., who accompanied the French army in their expedition to

Egypt, and who, as a member of the Institute, had the most ample opportunity of obtaining correct information relative to the opinions of the medical staff on this subject.

Dear Sir,

I am happy of any opportunity which reminds you of me, and on the subject upon which you desired an answer, I am ready to give it to you.

Comparisons have been made between contagious diseases, viz. the plague of the east, and the yellow fever of America. Opinions have been deduced from the examination of facts in different cases; but facts have not always been sufficiently ascertained for us to rest our opinions indiscriminately upon them.

I was in Egypt a witness to many facts which I recollect perfectly. A report that the inoculation of the plague had been attempted in vain must have originated from letters sent from Syria to France, and notes spread afterwards through Europe and America.

I was every day in the company of medical men, in Cairo, and of people of learning, who devoted a great part of their time to study and observation, and I have heard the report of the inoculation of the plague always contradicted; nor did I ever hear M. Desgenettes relate, as attributed to him in publications, that he had tried to inoculate himself with the venom of the plague. He could not wish for any additional honour which the boldness of an attempt of that sort had, in the opinion of many, reflected upon him: any degree of honour which could gratify him, he had acquired by his frequent attendance upon the sick, by his conferring all kind of comforts upon them, and by his daring to feel their pulse, their tumours at their bed, though fatal experience had taught him that most practitioners had fallen victims to their resolution in performing such an office. But among professional men, the importance of the inoculation of the plague had induced M. Desgenettes to authenticate a fact which I consider a mere rumour has circulated.

From several of my friends and colleagues of the commission of arts, namely, M. Coquebert and M. Champy, who died of the plague, M. Pothier, who recovered, having been attended by M. Desgenettes, I have had strong reasons to convince myself of the contagiousness of the plague. The quarantine laws established of old, with a great deal of severity in France, after many distressful events, the preventive means used by the Europeans settled in the cities of the east, who, by shutting themselves and their families in houses, and by avoiding contact with people, and with every thing from out of doors, except when well washed, and who are never attacked with the disease; while the loss of those who have remained exposed to the contagion is often lamented, have been sufficient

examples to convince me early of the truth of that opinion. Further I cannot say any thing from my own experience. The French physicians differed in their opinions whether the plague was indigenous or accidentally imported in Egypt, *but none of them denied its contagiousness.* Owing, perhaps, to the same opinion that I had, and to the natural propensity to imitate what others did with success, in avoiding, as much as they could, communication and contact, I escaped the danger, and I am happily placed in the situation to express to you the sentiments of esteem with which I remain,

Sir, your most obedient,

humble servant,

Dr. Hosack. ALIRE R. DELILE.

Without further enlarging on this head, we are, I believe, prepared to conclude, with the learned Dr. Parr, "That those who have suggested and disseminated doubts of the contagiousness of plague, are answerable for the lives of thousands, and, in some instances, have paid the forfeit with their own." *London Medical Dictionary.*

NOTE D. (*See page* 23.)

I have remarked in the text as an evidence, among others, that the yellow fever which prevailed in New-York in the year 1798, was not produced by putrid beef, that those persons who were employed in removing such putrid provisions from the city escaped the yellow fever. The same respectable gentleman, Mr. Edmund Prior, who furnished me with that fact, has also informed me, that of *forty* persons who were engaged in that particular service, under his immediate inspection, *thirty-eight* were attacked with a complaint of a very different character, the *dysentery;* which disease it is well known to all practical writers, frequently arises from decomposed animal and vegetable matter, as its exciting cause.

The remaining *two* persons who had been thus employed by Mr. Prior, escaped the dysentery, with which their comrades were afflicted; but upon leaving that service, and being afterwards exposed to the contagion of *yellow fever,* while working on board of ships, they fell victims to that disease; a circumstance which clearly shows that dysentery and yellow fever derive their origin from different sources: for additional particulars on this head, see Sketch of the Rise and Progress of the Yellow Fever, &c. by William Currie, Philadelphia, 1800.

That the yellow fever of 1798, did not proceed from the putrid beef, to which many

physicians ascribed its origin in that year, is also evident from the facts stated in the following extract from a letter which I received from the Rev. Dr. M'Knight of New York, dated November 6th, 1809.

"In the year 1798, Melancthon Smith, and another man whose name I do not recollect, were the two first instances of the yellow fever in New-York. They had both been on board the ship Fame, as she came up to the New-Slip in the East River, where she unloaded her ballast, and emptied her bilge water. The stench occasioned thereby was so intolerable, that, the wind being south, the inhabitants on the north side of the dock had to shut up their doors and windows, and some of them left their houses. To my knowledge, as many as twelve or fifteen died in that neighbourhood, in the course of two weeks, several of whose funerals I attended. This same vessel was afterwards removed to a dock in the vicinity of the Coffee-House: there several persons, who wrought on board of her, who resided in Eden's Alley, took the fever, and carried it up into that part of the town. Just at this time, there was a very heavy rain, which filled most of the cellars, in the lower part of the city. The fever raged with alarming violence on Golden Hill. It assumed the appearance of the plague."

Thus, then, it appears that many cases of the fever of that year, existed in the city, before the heavy fall of rain took place, and its effects upon the stored provisions were perceived; consequently, the yellow fever of that season did not proceed from those putrid materials as its source. That the foul state of the air which ensued lent wings to the contagion, I trust will by all be admitted.

NOTE E. (*See page* 29.)

To employ the language of that distinguished medical philosopher, Dr. Chisholm, it has become a kind of axiom in medical physics, that the effluvia of decomposed animal bodies are the most certain and frequent cause of malignant and pestilential fevers.

This opinion, which has been promulgated from age to age, and which has been so generally received, because it has not been investigated, has, at length, been most ably examined and refuted by Dr. Chisholm.

From his learned and elaborate essay on this subject, I have taken the following statement of facts, as illustrative of the doctrine which has been advanced in the text.

"There are grounds of belief, that even the concentration of the miasms of putrid animal substances, does not give rise to fever, and seldom, if ever, to disease of any description. The following facts certainly militate against a contrary conclusion.

"1. In the neighbourhood of Bitton, in Gloucestershire, about a mile from Willsbridge, which was my residence for nearly four years, there is what is called, 'a bone manufactory,' in which animal bones, after the extraction of their medullary oil by boiling, are distilled, and yield the usual products, muriate of ammonia and sulphate of soda. From this manufactory, a fœtor of the most offensive nauseating nature proceeds, and fills the atmosphere for nearly a mile around, diminishing in strength as it recedes from its source, and in proportion to its dilution or decomposition. The country is thickly inhabited, and near the manufacture itself is the village of Oldland, the population of which is very considerable; yet, in not one instance has this manufacture proved, in the smallest degree, injurious to health. I have frequently visited it with the most complete impunity. For several years the superintendant, Mr. Henderson, his wife, and family, lived in a house, having on one side, connected with it, that in which the retorts are placed, and on the other, that in which the bones are boiled:—yet they had every appearance of health, and they assured me they enjoyed it. A more convenient house, on a neighbouring hill, becoming vacant, Mr. Henderson rented it for the accommodation of his family. Soon after they began to reside in it, they lost their health, and were, when I last saw them, much inclined to return to their old and stinking habitation. This exemption from disease in the manufactory of sal ammoniac, &c. has been noticed by Morveau and Chaptal. *Edin. Med. and Surg. Journ. vol.* 2. *p.* 295.

"2. Between Bristol and Hanham, on the banks of the Avon, is Conham, remarkable for nothing but its having been chosen for the site of an extensive manufactory for the conversion of the flesh of dead animals into a substance resembling spermaceti—a project which has been relinquished several years ago. This being also not very distant from Willsbridge, I made a good deal of inquiry into the result, as far as it affected the health of those immediately engaged in the process, and of the inhabitants of its thickly-peopled neighbourhood. The foreman, or superintendant, Richard Bolston, residing now at Jeffries-hill near Hanham, has been my principal informant: and his account was confirmed by that of Mr. Thomas Pearsall, of Willsbridge, and other respectable persons. Bolston was two years employed constantly in this business; and during that time resided in the midst of dead animal bodies, horses, asses, and dogs, many of which were left to pass through the natural process of putrefaction. He had three labourers under him, and he declares that neither himself nor any of these men suffered from a moment's sickness, or, indeed, experienced the smallest inconvenience. Their business was to cut up the carcasses, to strip the muscular flesh from the bones, and to dispose of it first in boxes, perforated for the admission of water, which were afterwards laid in pits filled with water. The entrails, and every part not useful to them, were left to putrefy on the sur-

face. The pits prepared for the animal matter thus disposed were seven feet deep, and four broad and long, and each calculated to contain the flesh of fifty horses, beside asses and dogs. An idea may, therefore, be formed of the immense volume of putrid animal effluvia, enveloping continually the persons of Bolston and the labourers, by being informed that there were six of these pits, and, consequently, three hundred carcasses of horses, and as many of asses and dogs, exhaling, in greater or less abundance, their offensive miasms. Nothwithstanding this Bolston declares, that although the stench was offensive in the highest degree, yet he and those with him sustained no injury—and to this the inhabitants of the country around bear ample and angry testimony, both in relation to Bolston and themselves.

"3. Another remarkable fact, well known where the manufactory of refined sugar is extensively carried on—butchers preserve the blood of the slaughtered animals in open tubs, kept in close, small, shut-up houses, sometimes for several weeks, until the quantity required is completed, or until there is a demand from the sugar-bakers for it. It is then, in a putrid state, conveyed through the public streets, in carts or drays, to the sugar-houses, emitting the most offensive effluvia, and extremely annoying to all those who pass it. It is seldom immediately used by the sugar-bakers, but kept by them in casks, in a putrid state, filling the air of the manufactory, and frequently of the vicinity, with its putrid miasms, or what Galen and his followers would call σπερματα λοιμου γινεσθαι, the seeds of pestilence. But what is the result to the workmen, or to the inhabitants of the surrounding houses?—nothing inimical to health. This fact exists constantly in the city of Bristol, where, in general, the streets are extremely narrow, and the houses excessively crowded, and ill ventilated—and yet the harmless nature of these exhalations may be daily verified—I speak from my own observation, and the experience of the most respectable sugar-bakers. In summer it is more remarkable than in winter.

"4. Mr. Newman, surgeon in Stokes Croft, Bristol, a gentleman of great worth and professional skill, procured for me from his friend, Mr. Bevington, and his brother, Mr. Newman, of Bermondsey, in Southwark, the following interesting particulars respecting the leather-dressing business.

"'I have just received your letter of the 20th inst. (January, 1810,) making inquiry respecting putrid, contagious, and low fevers, as affecting the workmen employed by leather-dressers, to which I can give you a pretty clear reply. Our men are generally healthy, and the most so of the labouring poor—many have been in our service and knowledge fifteen and twenty years, and I do not recollect one case of the kind occurring (in our establishment) in London. The first process in dressing is to put the skins into a

pit of water to soften them, which is often used two or three times, that is, for two or three parcels, before it is changed, until the stench is intolerable. After this process the skins are struck out over a beam, and hung up, side by side, as close as possible, in a small room excluded from external air, which we term a stove: in this state they remain until they heat and slime, so that we can pull off the wool. The process of putrefaction is here so rapid as to disengage large quantities of volatile ammoniac, affecting the eyes of strangers with tears, and their noses with the most offensive smell. Our men always pull the skins in the stove in cold weather from preference, and are occupied in it a whole day at a time without injury.' Another gentleman, a brother of Mr. Newman's, concerned in the leather-dressing trade, but not in the same house, in Bermondsey, informs him, 'that so far from our workmen being unhealthy, or particularly subject to fevers, the reverse is the fact—the men employed look generally robust and healthy. In a concern in this line of business of fifty years standing, in which fifty men are constantly employed, the men have been uniformly healthy; and in this a circumstance is deserving of notice, viz. the men who work upon the raw skins, from which there is a constant and profuse exhalation of putrid steams, and those employed at the lime and tan-pits are equally healthy.' Mr. Newman, the writer of the above, says there are about sixty leather-dressers' and tanners' yards in Bermondsey, and in them about seven hundred men are constantly employed.

"It may, perhaps, be objected to this account, that the business of leather-dressers, in other countries, had been represented as extremely unhealthy. Hippocrates is supposed to have meant something of this kind as the cause, when he mentions the case of a person, Philiscus, *residing near the wall*, who died on the 6th day of a malignant fever; (Epidemic. lib. 1. s. 3.;) for anciently, and now, indeed, offensive trades of this kind were carried on in the suburbs, παρα το τειχος of cities. This was the case at Rome, beyond the Tiber, and some of the Latin poets have exercised their wit in allusions to it. It is highly probable, however, that the real cause naturally existed in the spot itself set apart for the 'sordidiores artes,' and that what was attributed to them proceeded from the marshy nature of the soil. Certain it is, without recurring to this explanation, we cannot reconcile Mr. Bevington's and Mr. Newman's, two respectable living witnesses, with the testimony of Ramazzini and Mercurialis, Martial and Juvenal, as quoted by him; and there is sufficient evidence that the Transtiberina Regio of Rome, and the Paduano (once, 17th century, male sanus, bestiis quam hominibus aptior) were proverbially unhealthy from their marshes, and that Bermondsey is not. *See Ramazzini, De Morb. Artific. cap.*

15.; *and Annotat. in Lib. Lud. Cornelii, Veneti de Vit. Sobr. Commodis; and de Virg. Vestal. Valetud. tuend. Dissertat.**

"5. I borrow the following singular fact from the ingenuous and experienced Ramazzini. 'In hac civitate, (Modena,) quæ pro suo ambitu satis populosa est, ideoque domos confertas habet ac prælatas, mos est ut tertio quoque anno in singulis domibus cloacæ expurgentur, quæ per vicos discurrunt. Cùm ergo domi meæ id opus fieret, contemplatus unum ex operariis istis in antro illo Charonæo magnâ anxietate ac sollicitudine opus suum peragentem, miseratus tam improbi laboris, ipsum interrogavi, cur tam sollicité laboraret, et non pacatius id ageret, ne ex nimio labore in multam lassitudinem incideret, tunc miser ex antro illo oculos attollens, meque intuitus: nemo, inquit, nisi expertus, imaginari potest, quanti constet, plus quàm quatuor horis in hoc loco morari, idem enim est cæcus fieri.—Rursus ab eodem quæsivi, num in faucibus ardorem ullum persentiant, difficultatem aliquam respirandi patiantur, capitis dolore tententur, num odor ille nares percellat; nauseam pariat; nihil horum respondit ille, neque pars ulla in hoc opere mulctatur, præter oculos.'—This account was afterwards confirmed by his observing a number of these people reduced to blindness and beggary. 'Oculis tamen solummodo, bellum tam atrox indicunt fœtidæ exhalationes istæ, ac illos acutissimis spiculis sic feriunt, ut illis vitam, id est lumen, eripiant.' Thus, as certain acrid substances seem exclusively to affect different and distinct parts of the body, as cantharides the bladder, the torpedo the nerves—'sic halitus illi ex humanis fæcibus per varios corruptiones gradus trium annorum spatio, talem adsciscant naturam, ut oculos tantùm lacessant, cæteris vero partibus ignoscant.' (*De Morb. Artific. cap.* 13.) This fact is no less important than curious, as it tends to show the inconsiderate conclusions of some eminent writers respecting the influence of the exhalations of privies on the health of men. Sir John Pringle often attributes the epidemics of camps to this as a cause; but it is fair to believe that he did so, without allowing himself sufficiently to investigate the subject. The tendency of this fact, too, goes to the overthrow of some of the bold, and, I am inclined to think, hasty, assertions of Dr. Miller, relative to the locality of the cause of the pestilential fever of New York, in 1805; for what is 'the blast of putrid exhalations from the sewer of Burling-slip' to the 'halitus ex humanis fæcibus per varios corruptiones gradus trium annorum spatio' of Modena? (*See Edinburgh Med. and Surg. Journal, vol.* 3. *p.* 252.) Now, whether the effect of these exhalations is asphyxia at Paris, according to Sauvages, (*Nos. Meth.*

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* My Essay on the Malignant Pestilential Fever may also be consulted with illustrative effect under the article of Martinico, vol. 2. p. 120---128.; under St. Lucia, ibid. p. 183.; under Demerara, ibid. p. 200.

vol. 1. p. 820.) or amaurosis at Modena, according to Ramazzini, in either case there is ample proof that they cannot be productive of putrid or pestilential fevers."

"Clavigero, on the authority of Torquemado, says, that at the dedication of the great temple of Mexico, anno 1486, 72,344 human beings, prisoners taken in war for the purpose, were sacrificed to the Mexican gods; and that a petty king, or lord, about the same time, in imitation of his master, the emperor, sacrificed many thousands on a similar occasion. On the erection of the great altar at Mexico, more than 12,000 were offered up:—and the annual average of human creatures thus disposed of, amounted to 20,000, beside a prodigious number of quadrupeds and birds. Notwithstanding this dreadful waste of human blood—and notwithstanding the horrible stench always present in this quarter of Mexico, the diseases, among an immense population, some say six millions in the city alone, were trifling, and proceeded almost altogether from marsh miasmata. The bodies of the victims were precipitated to the bottom of the steps of the altar, there to putrefy; or were sometimes ate by the Mexicans; and a pond of water, situated close to the great temple, was continually tinged by the blood of the sacrifices. (*See History of Mexico, vol.* 1. *p.* 201. 232. 281. 426. *See, also, Herrara, decade* 3. *c.* 16.; *Prevost's Voyages, &c.*) The prodigious sacrifice of peace offerings, made by Solomon at the dedication of the temple of Jerusalem, may be compared, 1 Kings, viii. 63.

"The annals of Dahomy furnish numerous illustrations of the foregoing remark; a nation whose kings delighted in blood, who wanted heads, not slaves, to garnish their palaces continually stained with human gore, and whose 'annual customs' presented to the terrified European many thousand human beings, sacrificed to the manes of their ancestors—a barbarous oblation, founded on the wildest and most savage superstition, denominated by them, 'the watering the graves of the deceased royal family.' There are some remarkable instances of the savage cruelty of these natives of Guinea, given by Governor Dalzel, in his History of Dahomy, in which, if pestilence could be the produce of the putrefaction of animal bodies, we should expect to hear of the most direful pestilential epidemics; but in which no such result is even noticed. 'It being now noon, they sat down to dinner on the ham and fowls they had brought with them; but were so annoyed by flies, they could scarce put a morsel into their mouths, without taking in some of these vermin with it. They little thought whence this nuisance proceeded, else they would have made a much shorter dinner; nor was it till about 3 o'clock, when, being desired by a messenger from the great captain, to come to the king's gate, that on their way they perceived, with no small degree of disgust and horror, two heaps of dead mens' heads, piled up on two large stages, and covered with swarms of their late visiters, the flies. The interpreter told them, 'they were *the heads of four thousand* of the Why-

dahs, who had been sacrificed by the Dahomans to their god, *about three weeks before*, as an acknowledgment of the great conquest they had obtained.' The king of Abomey, Ahadee, lived in a kind of charnel-house, yet was healthy, and seventy years old when he died. Dalzel thus describes this singular palace: 'The author had once an occasion to pass the limits of the courts already described, when King Ahadee was sick, and would see him in his bed-chamber. This was a detached circular room, of about eighteen feet diameter. It had a thatched conical roof; the walls were of clay, and whitewashed within. There was a small area before it, formed of a wall about three feet high, the top of which was stuck full of *human jawbones;* and the path leading to the door was paved with *human skulls;* the area within was also paved with skulls, which, I understood, were those of neighbouring kings, and other persons of eminence and distinction, whom, having taken prisoners in the course of his wars, he had placed there, that he might, literally, enjoy the savage gratification of trampling on the heads of his enemies.' " *Edin. Med. and Surg. Journ. vol.* 6.

In another work of Dr. Chisholm, (*see his Letter to Dr. Haygarth, 8vo. London*, 1809,) he has the following important statement on the same subject.

"Among a number of facts to prove that putrefaction and filth did not give rise to the pestilence, I shall select the following. In the year 1781, a gentleman who managed the mercantile concerns of John Brown and Son, of Copenhagen, in St. Croix, received from them a cargo of provisions, which he, in expectation of higher prices, kept in his storehouses, until the stench from them incommoded the neighbourhood, and could be perceived in the street. A complaint was made to the master of police, who ordered the provisions, consisting of beef and pork, to be examined; and they were found in such a putrid state, that upwards of a thousand barrels were ordered instantly to be carried outside of the harbour, and sunk in the sea. No appearance of infection or of sickness of any kind appeared in the house, in the neighbourhood, or among the people employed in transporting the provisions. In the year 1786, I think, a Swedish brig came into the harbour of Christianstaëdt, in distress: she had suffered much at sea, was leaky, and in coming in struck on a reef at the entrance of the harbour, from being unmanageable. She was got off with difficulty, and brought into the carenage to be hove down and repaired. Her cargo, which consisted of coffee in bulk was in such a state as to be condemned as useless, and thrown on shore, where it rotted and formed a dunghill. This lay for several weeks near the carenage, which was frequented by sailors and people from different nations; and yet, though it was in the hot months, no sickness or infection was occasioned by it. It was finally removed through my remonstrances. Let this important fact, which, indeed, is confirmed by what happens every year on coffee estates in the West Indies, be compared to the singular assignment of the pestilence of 1793, by Dr. Rush, to the effluvia

of damaged coffee. How baseless the fabric of his theory! About fifteen years ago a large whale, in a state of putrefaction, was driven on shore at the Estate Longford, in St. Croix. It was in summer, when the crop was over. The proprietor ordered large quantities of the blubber to be cut off, and boiled in his sugar coppers, to supply his numerous concerns with oil, and for sale. The negroes of the adjoining estates, likewise, supplied themselves with it. Yet none of them or of the white inhabitants were affected with any sickness, though the stench from the whale could be perceived at a quarter of a mile distant. To these I may add this general fact, that Danish ships have seldom arrived at St. Croix from Denmark, without having their provisions in a greater or less putrid state; but sickness has never been occasioned thereby." P. 252, 253.

It is also a proof of the innocence of dead animal matter and of the communication of the plague by a specific secretion from the living body, that the corpses of those who die of plague do not convey the disease.

"It is remarkable" says Mr. Howard, "that when the corpse is cold of a person dead of the plague, it does not infect the air by any noxious exhalations. This is so much believed in Turkey, that the people there are not afraid to handle such corpses. The governor at the French hospital in Smyrna told me, that in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent, (especially if he opened any of those windows which looked towards the great burying ground, where numbers every day where left unburied;) but that it had no effect upon the health either of himself or his family. An opulent merchant in this city likewise told me, that he and his family had felt the same inconvenience, without any bad consequences." *Howard on Lazarettos, 2d ed. Lond. 4to, page* 25.

Rondeletius, as quoted by Sennertus, asserted that he had dissected bodies dead of the plague in presence of many of his pupils with perfect safety. Much interesting information on this subject will be found in the first volume of Dr. Ferriar's Medical Histories and Reflections. Dr. Ferriar remarks,

"It is a general opinion, that pestilential disorders are occasioned by the effluvia of dead bodies, but there is reason to question the truth of this. When plague has appeared, in the neighbourhood of places where many bodies had remained unburied, after general engagements, other causes can be pointed out as more likely to have produced it. But many instances can be produced in which thousands of dead bodies have been left to putrefy on the field of battle, without causing pestilential distempers. This was not unnoticed by the attentive Diemerbroek. 'Cadavera, sive hominum,' says he, "sive aliorum animalium putrescentia pestem non generare, docent multæ magnæ strages, in quibus talis cadaverum inhumatorum putrefactio nullas pestes induxit. Anno 1642 in

agro Juliacensi, maxima strages facta est, et ad minimum 8000 militum, occisa fuerunt præter majorem adhuc famulorum, rusticorum, aurigarum, puerorum & mulierum numerum, atque equorum copiam innumerabilem; corpora inhumata sub dio computruerunt, nulla tamen pestis insecuta est. Hic in Germania, durantibus his nostri ævi crudelissimis bellis, etiam plurimæ maximæ strages factæ sunt, post multas tamen illarum nulla peste subsequente.' P. 31.) These facts are strengthened by a well-known circumstance, that in no case could the origin of a putrid fever be ever traced to the effluvia of dead bodies in a dissecting room. Nor have fevers been observed to originate, or to rage more severely in houses surrounding church-yards, in the middle of large towns, though the stench of the putrid bodies, over-heaped in such receptacles, is often insufferably offensive."

It has also been stated, that decomposed *vegetable* matter is not, as contended for by many physicians, the cause of yellow fever. The following fact, stated by Dr. John Stewart, of Grenada, abundantly proves that *vegetable* filth has no more agency in the production of that peculiar type of fever than putrid animal substances. "That vegetable and animal matters in a state of putrefaction do produce disease is not to be denied; but that vegetable matter in a state of corruption is, on many occasions, harmless, is evident, from the very offensive heaps of cotton-seed, and the pulpy covering of the coffee-berry, which are daily to be met with in Demerara, without being considered as a cause of fever; nor should this circumstance be omitted, that when fever does prevail, it is at a season when those causes do not act powerfully." *See Observations on the Nature and Treatment of the Malignant or Yellow Fever which prevailed in the island of Grenada, (W. I.) in the years* 1793, 1794, *and* 1795, *in a Letter to David Hosack, from John Stewart, M. D., &c. &c. Amer. Med. and Phil. Register, vol.* 3. *p.* 183.

It may also be remarked as an additional testimony to that stated by Dr. Stewart, to prove that the yellow fever does not derive its origin from decomposed vegetable matter, that whenever that disease has prevailed in the United States, it has not appeared in the country where such vegetable matter is most abundant; but has been chiefly confined to our largest cities, and those towns which are situated on the seaboard; a fact totally inexplicable upon the principle that the yellow fever is the product of vegetable putrefaction. I am fully aware the opinion has been entertained that this form of fever prevails in the interior of our country, and especially in the vicinity of the lakes; but whoever will consult the statements furnished by physicians residing there, and who have had the best means of obtaining correct information, will find ample refutation of that opinion. *See Frisbre's Sketch of the Medical Topography of the Military Tract of the State of New-York; and Brown's Sketch of the Country watered by the Mohawk River, &c.*

Amer. Med. and Phil. Register, vol. 4.; Needham's Sketch of the Medical Topography of Onondaga, state of New-York. See Barton's Med. and Phys. Journ. 1st supplement.

But we need not confine ourselves to European writers for information on this subject. Similar facts have been observed in our own country, and in this city in particular, and equally show the absurdity of resorting to vegetable or animal putrefaction as the source of the malignant fever with which the United States have been recently visited.

Adverting to the condition of the city of New-York anterior to the American revolution, and before a regular system of police regulations was adopted; to the offensive state of the town during the revolutionary war, when inhabited by the British troops; to the immense collection of foul materials of every sort in the cellars of the numerous buildings destroyed by the great fire of 1776, during the whole of which period this city enjoyed a total exemption from the pestilential fever, we must be convinced of the limited and incorrect views of those who look no further for the origin of this evil. In like manner, the offensive state of our slips, our wharves, and our market-places, until within a very few years; the putrefactive processes attendant upon our tanneries, morocco, starch, and glue manufactories, slaughter-houses, tallow-chandleries, sugar-houses, &c. &c. the filthy and neglected condition of our streets, and, we may add, of many of our burial grounds, furnish incontestible evidence that these are innocent when considered as the *primary* causes of the mortal epidemics which have desolated our cities. The influence of such an impure state of the atmosphere, resulting from these various causes, as a *secondary* agent in multiplying and diffusing the poison of fever when introduced, has already, I trust, been made sufficiently manifest.

Mr. Howard, in his remarks on the plague, (*see History of Lazarettos, p. 43.*) adverting to the opinions of those who questioned the contagiousness of that disease, asks, "Have not some of our professors sullied their names with such dangerous doctrines? From no other cause," he continues, "than the error of physicians, who constantly maintained that the disease, then epidemic, was not contagious, happened that terrible visitation which, in 1743, ravaged the city of Messina and its vicinity, with the loss of above forty-three thousand individuals, in the short space of only three months."

May it not with equal propriety be asked, can the physicians of the United States, who shall carefully have reflected upon the facts which have just been stated, and others of a similar nature which might easily be adduced, persist in the belief they have expressed, that the pestilential fever that has appeared in our cities and seaport towns is the product of decomposed animal and vegetable matter?

NOTE F. (*See page* 30.)

The following cases, selected from those referred to in the text, illustrate, in the most satisfactory manner, the specific nature of the contagion of the yellow fever, at the same time that they show that this disease is occasionally communicated from person to person, even in the pure air of the country.

Facts relative to the contagious nature of Yellow Fever, in the pure air of the country. In a letter addressed to David Hosack, M. D. from the right Rev. Dr. Richard Channing Moore.

Staten-Island, October 20, 1806.

DEAR SIR,

The discordant opinions which are held by physicians of the first reputation, upon the subject of yellow fever, have prevented me from replying to your letter of January last, lest the information which I may offer should give rise to such observations as would necessarily involve me in a medical controversy. From frequent conversations with my worthy preceptor, the late Mr. Richard Bayley, as well as from the perusal of those tracts which had fallen under my notice, I for many years entertained the opinion, that the yellow fever, which has proved a scourge to our cities, originated exclusively within their enclosures, and was confined to the impurity of their immediate atmosphere.

One of the first circumstances which excited in my mind an impression of the infectious nature of the disease, and which induced an alteration in my views, was the illness and subsequent death of Dr. Wynant and his wife. This gentleman had been called to take the charge of a man from New-York, ill with yellow fever, upon the north side of this island. The doctor, after an examination of the case, judged it expedient to bleed his patient, and while engaged in the performance of that operation, the man was seized with violent puking, and discharged the contents of his stomach upon his physician's clothes.

From the appearance of the matter so discharged, Dr. Wynant expressed his apprehensions with respect to his own safety; he continued, however, his attendance faithfully until the patient expired. A few days after the death of the person alluded to, Dr. Wynant was taken seriously ill; the usual remedies were applied, from the use of which he imagined himself relieved, and expressed a conviction of his recovery. At this moment he was visited by Dr. Henderson and myself. When we entered his room, which was a

fine, airy, comfortable apartment, he declared to us his expectation of being restored in a little time; the danger of the disease he concluded to be completely removed, and he was then in the use of bark and wine. His wife, an amiable woman, was sitting at his bed side, to all appearance in full health, elated with the prospect of her husband's recovery. She, however, soon discovered that her hopes were premature: the next day the companion of her bosom was wrested from her arms by that fatal disease, the force of which she had flattered herself was subdued.

Upon the day in which the doctor died, which was the 13th of October, '99, Mrs. Wynant was attacked with the same fever which had terminated the life of her husband, and within the space of five days from its commencement she fell a victim to its malignant, deadly influence.

About three years have elapsed since I was called to visit, in consultation with Dr. Halsey, the son of a favourite parishioner, who was ill with yellow fever, which disease he had contracted in the city of New-York. The day preceding the dissolution of the young gentleman in question, it was necessary, in consequence of a copious involuntary discharge of urine, to change the sheets upon his bed, and whilst the disconsolate father raised the body of his son, I supported his drooping head upon my shoulder. The patient died; I performed his funeral rites, and in a few days was attacked with the disease myself. During my confinement and convalescence, I was visited by several of the faculty. Dr. Bainbridge of New-York was at my house, who expressed his conviction of the serious nature of the disease, from which I was then recovering. It must be remembered that I had not been in the city for *many weeks* preceding my indisposition, and that the young gentleman from whom I received the complaint was confined in an airy, well ventilated chamber, surrounded with every comfort which the tenderness and opulence of his parents could procure.

In addition to the instances above mentioned, I have been informed by Mr. Abraham Banker, a gentleman of learning and intelligence, of the death of Mr. Degroat and his wife. It appears that a person from the city of New-York, by the name of Oswald, had engaged lodgings for himself and family at the house of Mr. Degroat, an opulent farmer upon the north side of this island. Mr. Degroat went to the ferry with his wagon to escort the strangers to his house; and upon his return home, rode next to Mr. Oswald, with whom he was engaged in close conversation. A few days after the arrival of Mr. Oswald, Mr. Degroat was seized with the yellow fever, to which he very soon fell a victim. Mrs. Degroat, who had affectionately attended her husband, was also attacked, and after struggling with the disease a few days, she followed her late companion to the tomb. Mr. Oswald, from whom, Mr. Banker is convinced, the contagious effects of the

disease were received, added another to the list of mortality in that family; so that in the space of ten or twelve days, from the arrival of those fugitives from the city, the house was swept of three of its inhabitants.

In the preceding narrative, I have related such facts as have occurred in the circle of a few miles, and I leave you at full liberty to make such use of them as you may deem expedient for the public good. To resist the force of truth cannot be the object of any individual of the faculty of medicine: the learning and the talents of our medical brethren must render them superior to every sinister consideration, and I feel persuaded that their united wish is to advance the health and happiness of their fellow creatures, and to diffuse a light upon a subject, of which, to say the least, the views of physicians are extremely imperfect.

Accept, dear, Sir, the assurances of my regard, with which my mind is impressed toward you, and believe me to be your friend and humble servant.

DR. D. HOSACK. RICHARD CHANNING MOORE.

Statement of Facts tending to prove the contagious nature of the Yellow Fever, at Germantown, in the year 1798, *by C. Wistar, M. D. Professor of Anatomy in the University of Pennsylvania, &c.*

The disease which produced the fatal effects now to be related, commenced in the family of Elizabeth Johnson, a widow who lived in the main street of the village of Germantown, about six and a half miles from Philadelphia.

The person first affected was her child, Betsey Johnson, who had been in Philadelphia from the third to the seventh of August, in a neighbourhood where several cases of the fever had already appeared. She returned home the seventh, and on the ninth of the same month was attacked with the yellow fever, which terminated fatally in four days.

Fourteen days after her death, viz. August 27th, Mrs. Duy, the next neighbour of Mrs. Johnson, who had visited Betsey several times during her illness, was attacked with a fever supposed to be of the same kind, and died at the end of four days.

On the thirtieth of August, the wife of Charles Hubbs, who also lived near to Mrs. Johnson, and had visited both Betsey and Mrs. Duy, once at least during their respective indispositions, *but had not been in Philadelphia for many months*, was attacked with unequivocal symptoms of the yellow fever, in its most malignant form, and died the 2d of September.

Mr. Duy, husband of the above-mentioned Mrs. Duy, was attacked sixteen days after the death of his wife, viz. September 18, and died also, after an illness of six days.

A few days after the death of Mrs. Duy, an English gentleman and his wife, of the name of Fisher, who had fled from Philadelphia on account of the fever, went to board with Mr. Duy, and were placed in the chamber occupied by his late wife during her illness: they were also attacked with fever. Mrs. Fisher was taken, September 19th, and recovered in a few days, but Mr. Fisher, who was attacked four days after his wife, died with the black vomit, the 27th of September.

At the same time, the disease re-appeared in Mrs. Johnson's family, in a young female servant, who was very ill, but recovered. Soon after the attack of this girl, Mrs. Johnson herself was taken ill with the same disease: she had visited both of her neighbours, Mrs. Duy and Mrs. Hubbs, while they were sick, she also had assorted the clothes of her deceased daughter, four or five days before her own attack commenced, but had not been in Philadelphia for a month. Her disorder continued eight days, and terminated the 28th of September, with convulsions and the black vomit.

A few days before the death of Mrs. Johnson, Elizabeth Stern, a woman who lived in the family, was attacked with fever, and became very yellow. Her symptoms appeared moderate at first, but after lingering a fortnight she also died.—The wife of a tenant of Mrs. Johnson, who lived in a separate part of the house, but used the same yard, was attacked before the death of Elizabeth Stern, and recovered with great difficulty.

The last victim to be mentioned, was one Stephen Post, an old man, who lived at a distance, but worked in Mr. Duy's barn, while the bed was there on which Mrs. Duy died. He was also attacked with fever, and died in a few days.

These melancholy circumstances occurred in a village which has long been remarkable for its salubrity, at a time when the other inhabitants enjoyed their usual health. In most of the cases, the disease appears to have been contracted at the house of Mrs. Johnson, which, before this distressing period, had been eminently distinguished by the health and longevity of its inhabitants. The family were extremely neat, and it may be asserted with confidence, that the premises were never more clean than they were at the time of this truly affecting catastrophe. What cause but contagion is adequate to the production of such a disease among persons so situated?

Philadelphia, Dec. 15, 1805.

NOTE G. (*See page* 33.)

The following statement fully illustrates the principle, that those who are accustomed to breathe an impure air frequently escape disease, while those who from their better condition in life enjoy the more pure atmosphere, are readily attacked with contagious fever when exposed to its exciting cause.

"The most pernicious infection," says Lord Bacon, "next the plague, is the smell of the jail, when the prisoners have been long, and close, and nastily kept; whereof we have had in our time experience twice or thrice, when both the judges that sat upon the jail, and numbers of those who attended the business, or were present, sickened upon it and died. Therefore, it were good wisdom that in such cases the jail were aired before they be brought forth." *Nat. Hist. exp.* DCCCCXIV.

In Stowe's Chronicle a more particular account is given of the same circumstances, as occurring at the fatal assizes held in the year 1577, in the following words: "On the 4th, 5th, and 6th days of July were the assizes held at Oxon, where was arraigned and condemned Rowland Jenkins for a seditious tongue; at which time there arose amidst the people such a damp, that almost all were smothered. Very few escaped that were not taken. Here died in Oxon three hundred persons; and sickened there, but died in other places, two hundred and odd." "Of the same kind of infection," says Sir John Pringle, "we have an unhappy instance so fresh in our memory, that I needed not to have mentioned it here, had it not been for such as live at a distance, or those who are to come after us. In the year 1750, on the 11th of May, the sessions began at the Old Bailey, and continued for some days; in which time there were more criminals tried, and a greater multitude was present in the court, than usual. The hall in the Old Bailey was a room of only about thirty feet square." "The bench consisted of six persons, viz. the lord mayor, three of the judges, one of the aldermen, and the recorder, whereof four died, together with two or three of the counsel, one of the under-sheriffs, several of the Middlesex jury, and others present, to the amount of above forty; without making allowance for those of a lower rank, whose death may not have been heard of; and without including any that did not sicken within a fortnight after the sessions."

Similar instances of infection are related to have taken place at the Black assizes, at Taunton, and at those of Exeter, in 1586. *See Rees' Cyclopædia*, art. *Contagion.*

Facts of a like nature have fallen under the notice of that accurate observer, Sir Gilbert Blane. An extract from his letter on this subject is here inserted.

Extract of a Letter from Sir Gilbert Blane, M. D. &c. to the Hon. Rufus King, Esq. Minister at the Court of St. James from the United States of America, dated

London, 26th November, 1798.

Sir,

I sit down to perform the promise I made you this morning, of putting on paper some remarks on the nature of the yellow fever, and the means of preventing it.

In doing this I shall chiefly confine myself to those views of it in which the magistrate is concerned. The adopting of measures for the prevention of disease is one of the most important duties of a wise and patriotic government; and the discovery of these means, as well as the efficiency of the steps to be taken, must depend on a thorough knowledge of the causes by which it is excited and influenced. My opportunities upon actual service in the West Indies in the late war, when physician to the fleet under the command of Lord Rodney and Admiral Pigot, and my present official duty as a member of the Medical Board of the Navy, have necessarily brought to my knowledge a number of facts relating to this subject, and I shall be extremely happy if the communication of some of the most important of them can throw any light, which may prove useful to the American government in checking an evil so afflicting and calamitous.

The first question that occurs with a view to preventive measures is, whether this disease be infectious, and under what circumstances it is so?

In those situations in which I observed it in the West Indies it was evidently so. There was the most incontestible evidence of this, both on board of ships and at hospitals, and the doubts which have been started on this point, seem to have arisen from the operation of infection being blended with that of other causes, which must concur with it in order to give it effect.

But whatever doubts there may be on this subject in the West Indies, there can be none in the climate of North America. This will be best proved and illustrated by an example.

On the 16th of May, 1795, the Thetis and Hussar frigates captured two French armed ships from Guadaloupe on the coast of America. One of these had the yellow fever on board, and out of fourteen men sent from the Hussar to take care of her, nine died of this fever before she reached Halifax on the 28th of the same month, and the five others were sent to the hospital sick of the same distemper. Part of the prisoners were removed on board of the Hussar, and though care was taken to select those

seemingly in perfect health, the disease spread rapidly in that ship, so that near one third of the whole crew was more or less affected by it.

This fact carries a conviction of the reality of infection, as irresistible as volumes of argument; and it further affords matter of important and instructive information, by proving that the infection may be conveyed by the persons or clothes of men in health. *Blane's Diseases of Seamen, p.* 605—607. 3*d edit.*

The principle which we have endeavoured to illustrate relative to the operation of febrile infection upon individuals unaccustomed to its influence has, in several instances, been strikingly exemplified by circumstances which have occurred in the debtor's prison, and in the bridewell of New-York. The following memorandum, made at my request by Dr. J. W. Francis, is in point on this subject.

"In the month of September, 1811, a febrile disorder of the typhoid character made its appearance in the debtor's prison of this city: its origin was owing to causes similar to those which usually produce a vitiated state of the atmosphere in confined apartments, the want of pure air, and the crowding of a large number of persons together, &c. &c. The contagion thus engendered was observed to operate with peculiar severity upon those individuals who were suddenly introduced into this vitiated air. About the 16th of July, 1814, several cases of the *typhus carcerum* occurred in the bridewell of New-York. The disease was first observed to exist in an apartment of the institution commonly called the eastern wing, a room about fifty feet long and twenty-five broad. Within a very few days after, the complaint became more general; and out of eighty-five individuals at that time confined in this part of the building, nearly forty were taken ill with symptoms characteristic of typhoid fever. The disease, in this instance, as in the former, was produced from the local circumstances of the place; the crowded condition of the ward, the want of cleanliness about the persons and in the clothing of the prisoners, and the neglect of free ventilation. The increased impurities of the atmosphere of the apartment seemed to give additional activity to the virulence of the disease: of the persons thus affected a large majority were those who had come from a pure air, and were but recently subjected to the noxious air of the place, several not more than thirty or forty hours, and many not more than three or four days. The infection was more readily communicated to those, too, who were naturally possessed of vigorous and robust constitutions, and its effects were, in most cases, more violent upon persons of this description than upon others. That the sphere of the infection was confined within certain limits, as affirmed by Dr. Lind of the malignant fever at the naval hospital at Haslar, and lately most satisfactorily shown by induction from well-attested facts concerning typhoid infection in general, (*see Letter of Dr. Haygarth to Dr. Percival,*) was abundantly manifest from the healthy condition of the

prisoners in other apartments of the bridewell. The progress of the disease, which I have thus briefly noticed, was arrested by the removal of the sick, and the materials impregnated with the morbific poison; by the introduction of pure air, by ablution, and by other means now most generally had recourse to for the purification of fever-wards and houses of recovery, as recommended by Dr. Ferriar.

"It has again and again been observed, by the keepers of our prisons, that those criminals who have long been accustomed to breathe the vitiated air of the crowded apartments of these institutions very frequently escape febrile contagion: the poison itself might, at first view, appear to have lost a portion of its virulence; but it has with more propriety been maintained that the individuals thus inured to its influence are, from habit, exposed to it with impunity; while, on the other hand, the reverse is the case with the unassimilated, when subjected to the operation of an impure air, deriving its noxious properties from concentrated human effluvia. It is a remark of most writers, that the infection of typhus, whether occurring in jails, ships, or elsewhere, becomes concentrated, and, consequently, more active by the cold of winter. This opinion is strengthened by the well-known fact, that a greater number of deaths takes place from fevers of this nature, in our prisons, during the winter than in the summer season."

NOTE H. (*See page* 36.)

I HAVE expressed the belief that the changes which the atmosphere undergoes during the prevalence of certain pestilential disorders are ascribable to a fermentative or assimilating process. I am strengthened in this opinion, which I have for many years maintained, by the recent and elaborate investigations of MM. Gay-Lussac, De Saussure, and other distinguished chemists, which have thrown much light upon the subject of fermentation, and also by some interesting observations lately published in the Edinburgh Review. *See Edinburgh Review for April*, 1814.

For a perspicuous elucidation of the manner in which the poison of diseases of specific contagion operates upon the human constitution, in assimilating the mass of fluids to its own peculiar nature, the reader may consult an Essay lately published by Dr. J. W. Francis, Professor of Materia Medica in the University of the State of New-York, and inserted in the American Medical and Philosophical Register, (*vol.* 4. *p.* 474—519:) also a valuable Dissertation on the Pathology of the Human Fluids, by Dr. Dyckman, of New-York.

NOTE I. (*See page* 37.)

THERE is scarcely to be found on record a series of facts which so conclusively prove the specific character of the pestilential or yellow fever, or points out the circumstances under which it is propagated or extinguished, as those referred to in the text: *e. g.* we have seen this disease introduced at the Wallabout on Long Island in the summer of 1804, and the poison diffusing itself in that neighbourhood as far as the impurity of the air extended; while the same disease conveyed, from the Wallabout into the city of New-York, was instantly extinguished by means of the relative purity of the atmosphere, the effect of the rigid system of police then observed. *See American Med. and Phil. Register, vol.* 2.

In like manner, in the summer of 1803, (*see Statement by Dr. Gillespie, Amer. Med. and Phil. Register, vol.* 1.) the yellow fever was introduced and spread in the village of Brooklyn, Long Island; at the same time that the city of New-York, within eight hundred yards' distance, enjoyed the most perfect exemption from it; a fact which at once disproves the dependence of yellow fever upon a general constitution of atmosphere which many physicians believe to be necessary both for the origin and propagation of this form of fever. We are led to the same conclusion by a perusal of the report relative to the introduction of the yellow fever into the city of Amboy, New Jersey, in the summer of 1811. *See Amer. Med. and Phil. Register, vol.* 3.

NOTE K. (*See page* 38.)

THE following extract of a letter from the Rev. Dr. Samuel S. Smith, the late president of Princeton College, shows that the fact I have stated of an interval taking place between the first and the subsequent cases of fever is of so frequent occurrence, that it even attracts the notice of those who are unconnected with the medical profession.

Princeton, July 24th, 1808.

DEAR SIR,

I have not any doubt but that yellow fever contains a specific contagion, essentially variant from that of small pox, with which it has so often been compared in order to deny contagion being incident to it. It requires a certain putrid state of the atmosphere as a conductor, in order to impart it; and it may inoculate and assimilate any confined

portion of atmosphere which has been exposed to the requisite causes of contamination, so that every part of it shall have power to communicate the poison. In the pure atmosphere of the country the poison is commonly so diluted, that it is too weak to excite the fever, except under peculiar circumstances.

In every instance in which yellow fever has been introduced into Philadelphia one circumstance has invariably taken place: after the persons who have first taken the disease have either died, or recovered, there has been *an interval of health* for several days, usually from ten to thirteen or fourteen, before the alarm has been renewed. From this circumstance, I have concluded, that during that space of time the infection secretly works in the blood before it appears in fever. Often there is a second interval of apparent health, but not of so long continuance. I hope that your view of the subject will carry more conviction with it than we have hitherto perceived; and I hope it will even contribute, among the more sensible part of the profession, who do not think merely by authority, to unite jarring opinions and to settle common principles.

SAMUEL S. SMITH.

By physicians who have recorded our epidemics such interval has been repeatedly observed. In Dr. Caldwell's Essay on the Yellow Fever of Philadelphia of 1805, it is very circumstantially noticed by that ingenious and able writer.

NOTE L. (*See page* 40.)

In the official document of the Board of Health of New-York, published on the 14th of September, 1805, they thus addressed the inhabitants of this city: "The board have formed a decided opinion, that the principal seat of the prevailing disease [the malignant epidemic fever] is that part of the city included between Burling-slip and Old-slip as far west as Pearl-street. Almost all the cases of disease which have occurred, can be distinctly traced to a communication with that part of the city. It is a matter of extreme regret, that the repeated admonitions of the board, to remove from this quarter, have been disregarded by a number of individuals who have remained the self-devoted victims of disease and death. They conceive it their duty again to enjoin it upon their fellow citizens, who have continued there, to remove immediately." Again, and in the same address: "All persons who do not comply forthwith with this advice of the board, to remove from the above described part of the city, which is deemed the principal seat of the disease, and which does not contain more than thirty-three acres, will be considered guilty of a wanton exposure of their lives, and will justify the board in resorting to com-

pulsory measures." The epidemic fever which prevailed in Philadelphia in 1793, spread in a similar manner, according to Dr. Rush and others. "For awhile," says Dr. Rush, "this fever was confined to the above-mentioned part of the city, but the disorder is spreading, and now appears in other places, so that several are affected in other parts of Water-street; some in Second-street; some in Vine-street; some in Carter's-alley; some in other streets; but, in most cases, the *contagion can be traced to Water-street.*" Proofs of the same kind might be taken from the most authentic accounts of the yellow fever as it has prevailed at other seasons, and in other cities and seaports of the United States: proofs wholly irreconcilable with the assertions of those who have declared that the malignant yellow fever arises at "distant and unconnected points;" that "no relation is observed between the source of the supposed contagion and the spreading of the disease to individuals or families," and who have maintained that there "never was any successful attempt to trace, in regular series, the propagation of it to any number of persons from the first case, or from any single point of infection." *See Rush's Account of the Bilious Remitting Yellow Fever as it appeared in Philadelphia in* 1793; *Account of the Yellow Fever of New London in* 1798; *Hardie on the Malignant Fever of New-York in* 1805; *Chisholm's Letter to Haygarth; Official Documents published by the Board of Health of New-York; Amer. Med. and Phil. Register, &c. &c.*

NOTE M. (*See page* 41.)

Hints on Purifying the Air of Infected Apartments, by the late Dr. T. Garnett, Professor in the Royal Institution, &c.

"If the air contained in a phial, be rendered offensive by putrid animal and vegetable substances, it may almost instantly be made sweet by dropping into the phial a few drops of *oxygenated muriatic acid;* or more effectually still, by introducing into it a small quantity of oxygenated muriatic gas.

"This experiment may be easily made; and it will be found that the air will, in this way, be deprived of the most putrid taint possible. Morveau and Berthollet have found, that if oxygenated muriatic gas be disengaged in a dissecting room, the bad smell from the subject will be corrected for a time; and that if the subject be washed with oxygenated muriatic acid, it will exhale no bad smell for a considerable time. These curious, but

well-ascertained, facts, naturally lead us to inquire into the action of the oxygenated muriatic acid, in correcting the putrid effluvia.

"It is now well known, that almost all the putrid smells disengaged by putrifying substances, are owing to the extrication of inflammable air, or hydrogen gas, loaded with some or all of the three following substances, sulphur, phosphorus, or ammonia; and these substances do not give out any very disagreeable smell, except when dissolved in hydrogen gas; but in that state we know the smell to be very unpleasant.

"Sulphurated hydrogen gas, or hepatic air, smells very disagreeably; but the hydrogen gas in which phosphorus has been dissolved, often smells most intolerably, resembling the refuse of blubber. The superabundant oxygen of the muriatic acid, unites with the hydrogen, and forms water; and the sulphur and phosphorus being no longer in a state of solution, become concrete, and the bad smell disappears.

"Though it is not yet proved, it seems very likely, from the experiments of Mr. Wall, and others, that the effluvia from the human body communicating infection, is hydrogen gas, charged with some animal substances.

"Is it not natural, from analogy, to conclude, that if these were deprived of their solubility in hydrogen gas, they would become innocent? and may we not reasonably suppose, that the oxygenated muriatic gas will deprive them, as well as other putrid effluvia, of their solubility?

"It certainly, I think, deserves a trial, which may be made at a very small expense, in the following manner.

"Take an ounce of the black oxyd of manganese in powder, and mix with it a quarter of a pound of common salt; put this mixture into an earthen vessel, and place it upon a chafing dish of coals in the room where the person labours under an infectious complaint; then pour upon it two ounces of sulphuric (vitriolic) acid, diluted with the same quantity of water.

"Oxygenated muriatic acid gas will be instantly disengaged, and perceived in all parts of the room, and will at least destroy any putrid or offensive smell; and I am inclined to hope, that it will likewise correct the contagious effluvia.

"When this gas is extricated in too great quantity, it will excite a cough; but I think that will not be the case with the quantity I have mentioned; if it should, a less portion must be used, or the mixture may be made without heat; but we know that bleachers are continually inhaling this gas in considerable quantity, without any disagreeable consequences. I should think there could be no difficulty in doing it in the house of any poor person; but it certainly might be very easily done in a fever-ward."

As connected with the same subject and illustrative of the condition of atmosphere produced by the introduction of an infectious ferment, and the means of destroying the noxious compound, I subjoin the following valuable communication from Dr. Samuel Bard, the venerable and learned President of the College of Physicians and Surgeons, in the University of the State of New York.

Hyde Park, July 27, 1808.

DEAR SIR,

I have read, with great pleasure, your arrangement and explanation of contagious diseases, and am, by your arguments, confirmed in the opinion I have always entertained of the agency of a ferment, sui generis, in the propagation of contagious and infectious diseases. I use these terms here indiscriminately: indeed, I believe the analogy between fermentation and its various products, and the progress and fomes of contagious distempers, applies more closely, and to a greater extent, than has been generally supposed.

For instance; under certain circumstances of heat and moisture, all vegetable and animal matters run into fermentation; that of vegetables passes through the stages of vinous and acetous to a species of putrefactive; that of animals likewise has its stages, as is evident in the fermentation of milk; and its products differ conspicuously according as flesh, fish, fat, blood, or eggs are the subjects of it.

An increased degree of heat always increases the rapidity with which fermentation goes on; and other circumstances of the atmosphere, a greater or less degree of moisture and dryness, a violent storm, thunder and lightning, moonlight, and probably many other less evident causes combining with the heat, variously change, hasten, or retard the process of fermentation, as every person conversant with these subjects well knows.

Again, the different constitutions, (or, if you please, a certain predisposition in vegetable and animal substances,) have the same effect in hastening, retarding, and variously modifying the products of fermentation: thus rich vegetable juices readily assume, and stop at, the *vinous* stage, and with difficulty can be made to go on to the *acetous;* poor and crude vegetable juices can hardly be arrested at the *vinous* stage, but run *rapidly* into the acetous; and cabbages, and all vegetables of that class, can hardly be made to produce either wine or vinegar, but quickly run into a kind of *putrefactive* fermentation. *Fish* putrefies more readily than *flesh;* and there is a great difference in this respect between the flesh of different animals.

All these circumstances are hastened, rendered more certain, and variously modified, by the addition of a ferment; and, lastly, a certain degree of cold prevents all fermentation, and stops it where it has already begun. So it is with infectious epidemics; certain de-

grees of heat and moisture in the atmosphere are necessary to their production; high degrees of heat always increase the spread, and add to the malignancy, of the disease; filth, and other qualities of the atmosphere less known, vary their types and exalt their grades; the constitution of the patient renders him more or less susceptible of particular diseases; the addition of a ferment (which is found wherever the disease exists) most certainly communicates and characterizes it; and lastly, under certain circumstances of purity and temperature of the atmosphere, it is found almost impossible to propagate those diseases, even by the aid of a ferment; and this observation applies, in some measure, even to small pox, measles, &c.

The sentiment, therefore, which you express, and so well illustrate, that the contagious diseases of your third class, although they may originate in circumstances of heat, moisture, and filth, and some other less evident qualities of the atmosphere, yet are unquestionably, and with more certainty, produced and propagated by the introduction of a ferment characterizing the type and grade of the disease, is, I believe, strictly and literally true; and upon this opinion only can a well-regulated police and quarantine laws be founded. The question you ask, why A after visiting B, ill of dysentery, plague, yellow fever, &c. is seized with the identical disease of B, when you consider the universality of the fact, is decisive as to the existence of a peculiar virus, or fomes sui generis, producing that particular form of disease; and the observation of Sydenham, that the prevailing epidemic swallows up all other diseases, confirms it; and I think you treat the non-contagionists with too much lenity, when you say they differ from us only in terms: far from it; they differ in fact, and most dangerously so; for, by denying the generation of a peculiar ferment in and about the bodies of the sick, and the propagation of contagion from patient to patient, they deny the utility, and are led to the neglect of some of the most important precautions against the introduction, importation, and propagation of such diseases.

Your first class of contagious diseases is strictly and clearly defined; they can be communicated by contact only: is not the materies morbi of these diseases always generated within the body; and whether it consists of animalculæ, or a chemical mixt, are they to be found anywhere else?

The fact that the diseases of the second class are communicable at every season of the year, during the heat of summer as well as during the severest cold of winter, in a pure as well as an impure atmosphere, forms the best distinction between this class and the third. But in as far as it is true that none of the second class can be suffered more than once in a lifetime, I am almost led to conclude, that in these, too, the materies morbi can be generated only in the bodies of the sick: the first origin of these diseases, and their occasional re-appearance in places where they had not been seen for years before, I confess forms a difficulty,

but I do not think a contradiction to this opinion; the materies morbi in these seems to be of a grosser nature, not so readily assuming a very elastic æriform state as that of the diseases of the third class; and hence the circle of contagion is much more restricted near the bodies of the sick.

Of the third class the fomes may certainly be, in some instances, in the first place generated in the atmosphere, and for that reason requires particular circumstances of heat, moisture, &c. but finding materials in the bodies of the sick to act upon, it readily assimilates a large portion to its own nature, which being very elastic and æriform, spreads to a wider extent, and contaminates the atmosphere, particularly a foul atmosphere, in which similar ingredients are found to a great extent.

Your's sincerely,

Dr. Hosack. SAMUEL BARD.